THURSDAY, AUGUST 4, 1892.

COAL-TAR COLOURING-MATTERS.

Tabellarische Uebersicht der künstlichen organischen Farbstoffe. Von Gustav Schultz und Paul Julius. R. Gaertner's Verlagsbuchhandlung, Hermann Heyfelder. (Berlin, 1891.)

DR. SCHULTZ is well known to "tar chemists" as the author of "Die Chemie des Steinkohlentheers," the most exhaustive work on coal-tar products which has hitherto been written, and of which the first edition appeared in 1882, and the second, enlarged to two thick volumes, in 1887–1890. His colleague Dr. Julius is the author of a useful little work on the same subject published in 1887. The volume before us is a remarkable production from every point of view, and well worthy of the reputation of the two authors who have collaborated in its production. Although nothing more than a tabulated catalogue of coal-tar colouring-matters, as it professes to be, the work is in reality a complete index to the literature of this rapidly growing branch of industry; complete, that is to say, to the date of its publication; but development is taking place even now at such a pace that a single year has sufficed to render a supplement necessary, and many of the most recently added colouring-matters are not included in the lists. The first edition of the "Tabellarische Uebersicht" was published in 1888 and contained 278 colouring-matters; the present edition contains 392 colouring-matters—a fact which speaks for itself with respect to the progress of chemical discovery in this direction. The volume is dedicated to the late Prof. von Hofmann, whose labours in this field in the early days of the industry will render his name inseparable from that band of pioneers who were the first to penetrate into the new regions opened up by the discovery of mauve by Dr. W. H. Perkin in 1856.

The volume of tables under consideration has become indispensable to every chemist engaged in the manufacture of, or in any way interested in, the coal-tar colouring-matters. To the general chemist it will be a matter of wonder that from three to four hundred distinct compounds, for the most part of known constitution, definite in character, often beautiful in crystalline form and appearance, and, in short, all well-characterised "chemical individuals," should be turned out of factories by hundredweights and tons for consumption in the tinctorial industries.

The authors group the colouring-matters under sixteen headings:—Nitro-derivatives, Azoxy-compounds, Hydrazones, Azo-compounds, Nitroso-compounds (quinone-oximes), Oxyketones, Diphenyl-methane derivatives, Triphenyl methane derivatives, Indophenols, Oxazines and Thiazines, Azines, Artificial Indigo, Quinoline colouring-matters, Acridine colouring-matters, Thiobenzenyl derivatives, and colouring-matters of unknown constitution. The tables are arranged in eight columns, the first containing the commercial name of the colouring-matter, the second its scientific name, the third its empirical formula, the fourth its constitutional formula, the fifth its mode of preparation, the sixth its date of discovery, the seventh the name of the discoverer and literary references, and the eighth its general properties and mode of application. From this analysis it will be seen that the work is, as we have stated, a complete epitome of the coal-tar colour industry. Its value as a work of reference for technologists will be appreciated by all who may have occasion to consult it; our own experience has been that the many thousand references to chemical literature, patents, and periodicals, are given with an accuracy that leaves nothing to be desired. One special feature to which attention must be directed is that the compounds tabulated are or have been actual articles of commerce. If the colouring-matter has been superseded, as must inevitably be the case with the progress of discovery, the authors announce the fact by stating *nicht mehr im Handel.* Thus the reader is made acquainted with the actual state of the industry, and the student with these tables at hand will be prevented from becoming a prey to the snares of the compilers of examinational text-books, who are only too frequently quite out of touch with the technology of their subject. Writers of this class are apt to set forth lists of compounds which are worthless to the manufacturer, and which are of value only to the examiner in technology by enabling him at once to separate the sheep from the goats among his candidates—to distinguish the students whose knowledge has been derived solely from books from those who are actually engaged in the factory.

One very forcible truth which is brought home on running the eye down the seventh column of the tables before us is the great preponderance of references to patents, chiefly German. It is evident that the chemist who wishes to keep abreast of modern discovery can no longer afford to neglect the literature of the Patent Office. Many discoveries of the greatest scientific importance are buried in these specifications, and it is long before they find their way into the text-books. This, so far as we are concerned, is much to be regretted, for, in the first place, the working chemist is already painfully overburdened with literature, and in the next place the statements in specifications require very judicious sifting before they can be admitted as part of scientific knowledge. The student who is not familiar with the coal-tar colour industry would be hopelessly entangled among the mazes of patent literature were it not for such practical guides as Drs. Schultz and Julius, who have evidently used the greatest judgment in giving their references. In other words, the patents quoted have reference to the production of compounds which are, or were, manufactured, and the reader who consults their work may feel assured that the "bogus" or "fishing" patent, which may be so innocently swallowed by the unwary, will not be obtruded on his notice.

So far as English technologists are concerned, it is to be regretted that such an overwhelming majority of German patents have to be referred to. This, of course, is only to be expected, when we consider the extraordinary activity which the Germans have displayed in the development of the industry of which the foundations were laid in this country about thirty or forty years ago. But the technological student is thereby placed at a disadvantage because German patents are not very readily obtainable. It is true that all capital discoveries are also patented in this country, but, on the other hand, there are many important chemical processes discovered and patented on the Continent which are not filed in our Patent Office, and which are so long in finding their way into the current literature that they are apt to be overlooked. Chemists who have occasion to consult the admirable series of tables by Schultz and Julius cannot but look with admiration—even if tinged with envy—at the brilliant series of discoveries which have emanated from the laboratories of German universities, technical schools, and factories. This is the fruit of technical education in the true sense; no system of cramming for an examination, no method of orthodox "test-tubing," not even the "recreative institute" line of technical training, which is so much in vogue at the present time, will enable us to recover our lost position in this or in any other branch of chemical technology.

R. MELDOLA.

RAM BRAMHA SÁNYÁL ON THE MANAGEMENT OF ANIMALS IN CAPTIVITY.

A Handbook on the Management of Animals in Captivity in Lower Bengal. By Ram Bramha Sányál, Superintendent of the Zoological Garden, Calcutta. (Calcutta, 1892.)

CONSIDERING the number of zoological gardens in Europe, and their long establishment, it is singular that it should have been left to the superintendent of a zoological garden at Calcutta, and to a native of India withal, to produce the first practical handbook on the management of animals in captivity. The author, who, we believe, is a member of the "Brahma Somaj," and one of the very few natives of British India that have exhibited any taste for natural history, has been for some years superintendent of the Zoological Garden at Calcutta, an excellent institution mainly kept up by the Government of Bengal, but under the control of a committee of the subscribers. This committee, at the suggestion of Sir Steuart Bayley, the Lieutenant-Governor of Bengal, came to the conclusion that, after thirteen years' experience of the management of animals, it might be possible to produce a handbook on the subject which "would be of interest to the scientific world," and at the same time "of great use to nobles and other persons who, on a smaller scale, keep a collection of animals in captivity."

Such was the origin of the present volume, which has been prepared by Babu Ram Bramha Sányál, on a plan drawn up by a sub-committee appointed for the purpose, and has been supervised by Mr. C. E. Buckland, C.S., who was at one time honorary secretary to the Calcutta Garden, and is now a member of the Council of the Zoological Society of London. It is certainly a work of considerable interest. In the first place it has the merit of giving us a complete classified list of all the mammals and birds that have been kept alive in the Calcutta Garden. These are, of course, mostly species of British India—241 of the class of mammals, and 402 birds—but there are a good many exotic forms among the birds. In the second place large numbers of notes on the treatment of the animals in health and in sickness, on their length of life in captivity, and generally on their habits as observed in confinement are introduced, which, although in some cases of an apparently trifling nature, are well worthy of study by those who are engaged in the custody of living animals. It is evident that the author has kept a regular journal, and has recorded his experiences very minutely. In a case of a fight between a lioness and a tiger, which were by some accident allowed to pass into the same compartment of the Carnivora house, the tiger was completely victorious and killed the lioness. The longest period during which a tiger has lived in the Calcutta Gardens is fourteen years. It is curious that the Lesser Fruit-bat of Bengal (*Cynopterus marginatus*) "does not appear to bear captivity well." A nearly allied African species (*C. collaris*) has completely established itself in our Regent's Park Garden, and has bred abundantly for the last twenty years. On January 30, 1889, a young rhinoceros was born in the Calcutta Gardens, "the second recorded instance" of this mammal having bred in captivity. Interesting details are given of this event. The parents were a male Sumatran rhinoceros and a female of the northern form of the same species, which has been separated as *Rhinoceros lasiotis*. The highest bliss of these animals, as the Babu points out, is to "lie undisturbed in a muddy hollow," besmeared with liquid dirt.

In 1886 the Calcutta Garden obtained from Dar-es-Salam, in Eastern Africa, a young hippopotamus, but it did not live for more than eighteen months. Probably its voyage from Zanzibar to Calcutta "in an ordinary box without water" materially affected its health, as the hippopotamus, if properly treated, does exceedingly well in captivity.

The authorities of the Calcutta Garden have not yet succeeded in keeping the pangolin alive for any lengthened period. The same has been the case in our Zoological Gardens, where, although several examples of this Edentate have been received, not one has survived many weeks. This is curious, as both the American ant-eater (*Myzmecophaga*) and the African ant-bear (*Orycteropus*) maintain excellent health in captivity. It is suggested that the difficulty of obtaining a supply of their proper food—the *termites*—is the cause of this failure. At the same time, when supplies of this insect have been placed within reach the Pangolin has been "known to take no notice of them." We cannot therefore suppose that the true solution of this difficulty has yet been hit upon. It may be stated that in a similar manner ant-eaters kept in this country will not eat ants, but thoroughly enjoy raw meat when minced up small in a sausage machine.

The second part of the handbook contains a list of the birds exhibited in the Calcutta Garden, and corresponding observations upon them, but naturally there is not so much to be said on this branch of the subject. Among the more interesting species of this order we notice the fine large Laughing-thrush of the Himalayas (*Garrulax leucolophus*), the gold-fronted chloropsis (*Chloropsis aurifrons*), several sorts of drongo (*Dicruridæ*), Gould's ouzel (*Merula gouldi*), and the pheasant-tailed jacana (*Hydrophasianus chirurgus*), all birds which are rarely, if ever, seen in European aviaries. On the whole we must allow that this volume is a remarkable production, considering the circumstances under which it has been prepared, and that its author deserves great credit for the pains bestowed on its composition, and for much valuable information contained in it.

OUR BOOK SHELF.

In Starry Realms. By Sir Robert S. Ball, D.Sc., LL.D., F.R.S. (London: Isbister and Co., 1892.)

THIS is another striking example of Sir Robert Ball's skill in popularizing the most fascinating of the sciences. Though the same story has been to a large extent told by him before, there are several new features which prevent the least suspicion of staleness. The author is perhaps most interesting in his homely illustrations of astronomical dimensions. Among these are the disc of the moon projected on the map of Europe, and three lunar craters similarly compared with the map of England. The history of a falling star, as told by a particularly intelligent meteorite, is also worth special notice.

The two final chapters consist of "An Astronomer's Thoughts about Krakatoa," and "Darwinism in its Relation to other Branches of Science." The former is a popular account of the Report of the Krakatoa Committee of the Royal Society. The moral of the last chapter is that the scientific method of Darwin is closely related to that employed in astronomy. "Astronomers were the first evolutionists: they had sketched out a majestic scheme of evolution for the whole solar system, and now they are rejoiced to find that the great doctrine of Evolution has received an extension to the whole domain of organic life by the splendid genius of Darwin" (p. 349). We can confidently recommend the book to all classes of readers. Those who are already familiar with the subject will find much to delight them.

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.*]

Basset's Physical Optics.

I DESIRE to make a few remarks on Prof. Schuster's review of my treatise on physical optics.

The sentence in the preface to which he refers is not perhaps very happily arranged, and might be amended as follows:—

"I have a profound distrust of arguments based upon vague and obscure general reasoning instead of upon rigorous mathematical analysis." This, however, is a small matter; what I wished to protest against was, the practice which has crept into more than one recent work, of slurring over an investigation by means of a page or two of general talk, instead of writing out a careful mathematical demonstration; or at any rate making a serious attempt to grapple with mathematical difficulties, and trying to arrive at a definite result.

I fully admit, that when a subject is in a state of growth it is often impossible to dispense with hypothesis. But whenever this is necessary, the hypothesis should be expressed in clear and definite language; the evidence and arguments for and against the hypothesis should be properly marshalled and discussed; the reader should be plainly informed that the proposition which forms the basis of the investigation is a hypothesis and not an established fact, and that consequently further research may show that the hypothesis must either be abandoned or modified. When an investigation is conducted on these lines, all obscurity and vagueness will be avoided; for the reader will be thereby enabled to clearly understand the exact nature of the assumptions which are made, and will be able to discriminate between those portions of the investigation which consist of hypothesis, and those which constitute results deduced from hypothesis by the aid of mathematical analysis.

The dangerous character of arguments based upon general reasoning is well illustrated by the theory of the deformation of thin elastic plates and shells. When a thin shell is deformed by means of bodily forces, and stresses applied to its edges, the effect produced is extension, change of curvature, and torsion; and it might be argued from this, that the potential energy due to deformation is a homogeneous quadratic function of the quantities by which extension, change of curvature, and torsion are specified. But if the expressions for the potential energy of a cylindrical or of a spherical shell be examined (Phil. Trans. 1890, pp. 443, 467), it will be found that they contain certain terms which involve the *differential coefficients* of quantities by which extension is specified.

With regard to the concluding portion of the review, I must point out that one of the difficulties with which the author of an advanced treatise is confronted is *where to draw the line.* Upon this subject there is necessarily room for a wide difference of opinion. As my object was to write a book on physical optics, I considered that the reader might properly be expected to obtain his information respecting the various theories of the electromagnetic field, from the treatises and original memoirs on that subject; and for that reason I abstained from discussing purely electromagnetic theories, further than was necessary for the explanation of optical phenomena. A. B. BASSET.

July 25.

Causes of the Deformation of the Earth's Crust.

THE communication from E. Reyer in NATURE of July 7 (p. 224) "On the Causes of the Deformation of the Earth's Crust" is interesting from several points of view. It is an indication that the theory which looks upon mountain ranges as the effects of the earth's contraction does not satisfy the conditions of the geologist.

It is welcome to me individually as in the main accepting the principles of which I happen to be the exponent, and have systematized in the "Origin of Mountain Ranges," published in 1886. It is, however, the addition to this theory explaining the folding of strata by what Mr. Reyer aptly calls "gliding" that calls for examination. It is shown very clearly by experiment and otherwise that under certain conditions strata, when they reach a certain degree of inclination on the flanks of a mountain chain during elevation, must glide downwards by gravitation and produce folds and disturbances towards the lowlands. We have only to consider the effects of land-slides such as occur in the chalk districts in the south of England, and their effects on the shore deposits, to admit the truth of this. This aspect of the problem, though always present, has grown on me since my work was published, and I have little doubt that the "foot-hills" usually formed of the newer strata which flank most great mountain ranges are to a considerable extent due to gravitation and "gliding." I may point to the foot-hills of the Canadian Rockies and of the Himálayas as illustrations. The cases of folded lying upon undisturbed strata mentioned by Reyer are, as he clearly shows, explanatory on this view, but not by general contraction.

There are no doubt other effects traceable to the gravitation of masses of the earth's crust during elevation such as the lateral spreading of the plastic cores of mountain ranges in fan-like form, and the consequent shouldering of the strata on either side intensifying the effects of the folding of the strata by thermal expansion, as explained in the "Origin of Mountain Ranges."

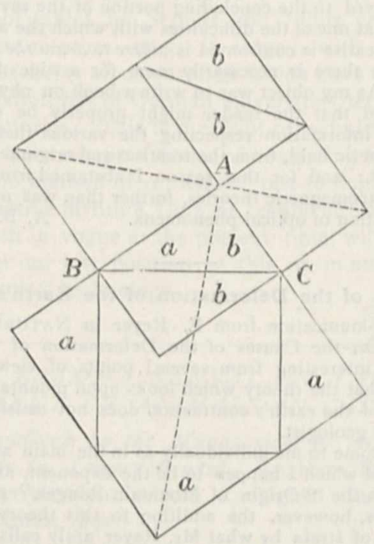
I cannot, however, follow Mr. Reyer if he considers "gliding" an explanation of all folding. I am not sure that this is his meaning, though the last paragraph would seem to bear such an interpretation. It seems obvious to me, to mention only one of numerous examples, viz., the folds of Jurassic strata caught up in the gneiss of the Central Alps, as shown in Heim's section, reproduced in "Prestwich's Geology," and in plate xiv., "Origin of Mountain Ranges."

While looking upon "gliding" as only a partial explanation of folding, I welcome Mr. Reyer's fresh and vigorous treatment of the important problem of the causes of the deformation of the earth's crust. It is evidence that geologists and physicists are now allowing their minds to play freely round the subject of the orogenic changes of the earth's crust, and of the growth of philosophical conceptions on the geological evolution of our planet.

Park Corner, Blundellsands, T. MELLARD READE.
July 11.

An Obvious Demonstration of the 47th Proposition of Euclid.

SOME years ago in trying for a simpler demonstration of this theorem I worked out the following. Its extreme simplicity suggested that it could scarcely be original; but as some years have elapsed, and as none of my friends have seen it elsewhere, I send it to you as possibly of interest to some and perhaps of use where practical geometry is being taught. It is evident that the two larger squares are equal, the side of each being equal to the sum of the sides AB, AC of the triangle ABC. It is also clear that the four triangles marked "*a*" are equal to one another. Again, the four triangles marked "*b*" are equal to one another, and to the four triangles marked "*a*."



Hence taking four times the triangle "*a*" from one of the large squares and four times triangle "*b*" from the other, there remain in the one case the square on BC, and in the other case the squares on AB and AC, and these remainders are equal. Therefore the square on the hypothenuse is equal to the sum of the squares on the other two sides.

A. J. BICKERTON.

Canterbury College, New Zealand University,
June 15.

[The principle of the above solution is not new. A proof, by dissection, depending on it is given in several text-books. The novelty of it consists in the position of the squares by means of which the truth of the property is seen in one figure.]

Musical Sand. Lava in the Bournemouth Drift.

IN reference to the note in NATURE (July 21) respecting musical sand in Australia, permit me to say that the subject has long since received attention there. I am away from references at present, but I should think it must be over two years since Mr. Sidney Olliff kindly sent me samples from Botany Bay. The samples sent were enclosed in small canvas bags, and, though there was probably not more than half-an-ounce of each, they were very musical on reaching me. For purity and musical effect, the Botany Bay samples were more like the Eigg sand than any other kinds I had previously examined.

During the last five years I have been collecting the various kinds of rock found in the Bournemouth high-level gravels (Codrington). A section has lately been exposed at the head of Alum Chine. Here a bed of angular and sub-angular flint gravel 5 ft. (varying) in thickness rests on the Bagshots, and is covered by sand, humus, and peat. At the base of the gravel bed I disinterred (on the 17th inst.) a small piece of vesicular lava, much decomposed in places, but retaining more than sufficient of its original structure for purposes of identification.

The specimen will be sliced for the microscope; in the meantime I draw attention to it because it is, to my knowledge, the first specimen of vesicular lava that has been found in these gravels. CECIL CARUS-WILSON.

Oxford, July 27.

The Flora and Fauna of Bromley.

THE Bromley Naturalists' Society have recently appointed a Special Committee to draw up lists of the flora and fauna of the Bromley Union District. This district comprises the parishes of Beckenham, Bromley, Chelsfield, Chislehurst, Cudham, Down, Farnborough, Foots Cray, Hayes, Keston, Knockholt, Mottingham, North Cray, Orpington, St. Mary Cray, St. Paul's Cray, and West Wickham.

I am desired to ask you to allow me to state that the Special Committee will be glad to receive from your readers any information which in their opinion might be of service to the Committee. J. FRENCH.

Hon. Sec. Special Committee.

99, Widmore-road, Bromley, Kent, July 27.

THE BRITISH ASSOCIATION.

EDINBURGH.

AN Edinburgh meeting of the British Association seems almost a home meeting. At every turn we are reminded of some of those who bore their part in founding and building up the Parliament of Science. Sir David Brewster meets us in the University quadrangle. The chair now set apart for the President of Section A was occupied for many years by James David Forbes, while for one brief year Natural History in Edinburgh was identified with Edward Forbes, to whom the Association owes, among many greater things, the evolution of the Red Lion. Viewed through the vista of years, the intellectual life of Edinburgh seems to have been marked by the combination of the love of science and letters with the full enjoyment of social intercourse, and we have before us such evidence of the persistence of this trait as bodes well for the success of the meeting.

The reception rooms are in keeping with the dignity of the Association, and afford every facility for the transaction of business. The programme of local arrangements which has been put in the hands of members indicates ample variety of occupation for hours of leisure. This pamphlet is of convenient size and easy of reference. In one point of detail it is worthy of remark; its maps do not require to be unfolded; these are two, one showing clearly, although on a small scale, Edinburgh and its suburbs, and the other giving, on a large scale, the part of the city which will be most frequently traversed by visitors. The Excursion Handbook has evidently been compiled with much care, and it will prove an interesting and artistic souvenir of the meeting.

Sir Archibald Geikie, the President of the Association, was President of the Geological section at the 1871 Edinburgh meeting. His address, suggested by the centenary of Hutton's "Theory of the Earth," deals with a subject in which Scottish geologists have ever been well to the front. The last decade of geological work in Scotland has done much to unlock the secrets of rock structure, and there could be no more fit exponent of the results than the president.

In the section programmes we hear promise of many welcome papers and several important discussions; in Section A, on Monday, the question of a National Physical Laboratory will be dealt with; while Tuesday will be devoted to a discussion on electrical units, in this Prof. von Helmholtz is expected to take part; Section B and D will consider bacteriology, with special reference to Brewing; Section D, "Fisheries"; Section F, "Old Age Pensions." In Section C, the feature of the meeting is likely to be the review of recent work in the geology of Scotland, and the presence of a considerable number of foreign geologists is sure to lead to interesting discussions. The Prince of Monaco will give in Section E the results of his observations on ocean currents. Section G will this year devote some attention to the subject on which there is much difference of opinion, the education of engineers.

Inaugural Address by Sir Archibald Geikie, LL.D., D.Sc., For.Sec.R.S., F.R.S.E., F.G.S., Director-General of the Geological Survey of the United Kingdom, President.

In its beneficent progress through these islands the British Association for the Advancement of Science now for the fourth time receives a welcome in this ancient capital. Once again, under the shadow of these antique towers, crowded memories of a romantic past fill our thoughts. The stormy annals of Scotland seem to move in procession before our eyes as we walk these streets, whose names and traditions have been made familiar to the civilized world by the genius of literature. At every turn, too, we are reminded, by the monuments which a grateful city has erected, that for many generations the pursuits which we are now assembled to foster have had here their congenial home. Literature, philosophy, science, have each in turn been guided by the influence of the great masters who have lived here, and whose renown is the brightest gem in the chaplet around the brow of this "Queen of the North."

Lingering for a moment over these local associations, we shall find a peculiar appropriateness in the time of this renewed visit of the Association to Edinburgh. A hundred years ago a remarkable group of men was discussing here the great problem of the history of the earth. James Hutton, after many years of travel and reflection, had communicated to the Royal Society of this city, in the year 1785, the first outlines of his famous "Theory of the Earth." Among those with whom he took counsel in the elaboration of his doctrines were Black, the illustrious discoverer of "fixed air" and "latent heat"; Clerk, the sagacious inventor of the system of breaking the enemy's line in naval tactics; Hall, whose fertile ingenuity devised the first system of experiments in illustration of the structure and origin of rocks; and Playfair, through whose sympathetic enthusiasm and literary skill Hutton's views came ultimately to be understood and appreciated by the world at large. With these friends, so well able to comprehend and criticize his efforts to pierce the veil that shrouded the history of this globe, he paced the streets amid which we are now gathered together; with them he sought the crags and ravines around us, wherein Nature has laid open so many impressive records of her past; with them he sallied forth on those memorable expeditions to distant parts of Scotland, whence he returned laden with treasures from a field of observation which, though now so familiar, was then almost untrodden. The centenary of Hutton's "Theory of the Earth" is an event in the annals of science which seems most fittingly celebrated by a meeting of the British Association in Edinburgh.

In choosing from among the many subjects which might properly engage your attention on the present occasion, I have thought that it would not be inappropriate nor uninteresting to consider the more salient features of that "Theory," and to mark how much in certain departments of inquiry has sprung from the fruitful teaching of its author and his associates.

It was a fundamental doctrine of Hutton and his school that this globe has not always worn the aspect which it bears at present; that, on the contrary, proofs may everywhere be culled that the land which we now see has been formed out of the wreck of an older land. Among these proofs the most obvious are supplied by some of the more familiar kinds of rock, which teach us that, though they are now portions of the dry land, they were originally sheets of gravel, sand, and mud, which had been worn from the face of long-vanished continents, and after being spread out over the floor of the sea, were consolidated into compact stone, and were finally broken up and raised once more to form part of the dry land. This cycle of change involved two great systems of natural processes. On the one hand, men were taught that by the action of running water the materials of the solid land are in a state of continual decay and transport to the ocean. On the other hand, the ocean-floor is liable from time to time to be upheaved by some stupendous internal force akin to that which gives rise to the volcano and the earthquake. Hutton further perceived that, not only had the consolidated materials been disrupted and elevated, but that masses of molten rock had been thrust upward among them, and had cooled and crystallized in large bodies of granite and other eruptive rocks which form so prominent a feature on the earth's surface.

It was a special characteristic of this philosophical system that it sought in the changes now in progress on the earth's surface an explanation of those which occurred in older times. Its founder refused to invent causes or modes of operation, for those with which he was familiar seemed to him adequate to solve the problems with which he attempted to deal. Nowhere was the profoundness of his insight more astonishing than in the clear, definite way in which he proclaimed and reiterated his doctrine, that every part of the surface of the continents, from mountain-top to sea-shore, is continually undergoing decay, and is thus slowly travelling to the sea. He saw that no sooner will the sea-floor be elevated into new land than it must necessarily become a prey to this universal and unceasing degradation. He perceived that, as the transport of disintegrated material is carried on chiefly by running water, rivers must slowly dig out for themselves the channels in which they flow, and thus that a system of valleys, radiating from the water-parting of a country, must necessarily result from the descent of the streams from the mountain crests to the sea. He discerned that this ceaseless and widespread decay would eventually lead to the entire demolition of the dry land; but he contended that from time to time this catastrophe is prevented by the operation of the underground forces, whereby new continents are upheaved from the bed of the ocean. And thus in his system a due proportion is maintained between land and water, and the condition of the earth as a habitable globe is preserved.

A theory of the earth so simple in outline, so bold in conception, so full of suggestion, and resting on so broad a base of observation and reflection, ought, we might think, to have commanded at once the attention of men of science, even if it did not immediately awaken the interest of the outside world; but, as Playfair sorrowfully admitted, it attracted notice only very slowly, and several years elapsed before any one showed himself publicly concerned about it, either as an enemy or a friend. Some of its earliest critics assailed it for what they asserted to be its irreligious tendency—an accusation which Hutton repudiated with much warmth. The sneer levelled by Cowper a few years earlier at all inquiries into the history of the universe was perfectly natural and intelligible from that poet's point of view. There was then a widespread belief that this world came into existence some six thousand years ago, and that any attempt greatly to increase that antiquity was meant as a blow to the authority of Holy Writ. So far, however, from aiming at the overthrow of orthodox beliefs, Hutton evidently regarded his "Theory" as an important contribution in aid of natural religion. He dwelt with unfeigned pleasure on the multitude of proofs which he was able to accumulate of an orderly design in the operations of nature, decay and renovation being so nicely balanced as to maintain the habitable condition of the planet; but as he refused to admit the predominance of violent action in terrestrial changes, and on the contrary contended for the efficacy of the quiet, continuous processes which we can even now see at work around us, he was constrained to require an unlimited duration of past time for the production of those revolutions of which he perceived such clear and abundant proofs in the crust of the earth. The general public, however, failed to comprehend that the doctrine of the high antiquity of the globe was not inconsistent with the comparatively recent appearance of man—a distinction which seems so obvious now.

Hutton died in 1797, beloved and regretted by the circle of friends who had learnt to appreciate his estimable character and to admire his genius, but with little recognition from the world at large. Men knew not then that a great master had passed away from their midst, who had laid broad and deep the foundations of a new science; that his name would become a household word in after generations, and that pilgrims would come from distant lands to visit the scenes from which he drew his inspiration.

Many years might have elapsed before Hutton's teaching met with wide acceptance, had its recognition depended solely on the writings of the philosopher himself. For, despite his firm grasp of general principles and his mastery of the minutest details, he had acquired a literary style which, it must be admitted, was singularly unattractive. Fortunately for his fame, as well as for the cause of science, his devoted friend and disciple, Playfair, at once set himself to draw up an exposition of Hutton's views. After five years of labour on this task there appeared the classic "Illustrations of the Huttonian Theory," a work which for luminous treatment and graceful diction stands still without a rival in English geological literature. Though professing merely to set forth his friend's doctrines, Playfair's treatise was in many respects an original contribution to science of the highest value. It placed for the first time in the clearest light the whole philosophy of Hutton regarding the history of the earth, and enforced it with a wealth of reasoning and copiousness of illustration which obtained for it a wide appreciation. From long converse with Hutton, and from profound reflection himself, Playfair gained such a comprehension of the whole subject that, discarding the non-essential parts of his master's teaching, he was able to give so lucid and accurate an exposition of the general scheme of Nature's operations on the surface of the globe, that with only slight corrections and expansions his treatise may serve as a text-book to-day. In some respects, indeed, his volume was long in advance of its time. Only, for example, within the present generation has the truth of his teaching in regard to the origin of valleys been generally admitted.

Various causes contributed to retard the progress of the Huttonian doctrines. Especially potent was the influence of the teaching of Werner, who, though he perceived that a definite order of sequence could be recognized among the materials of the earth's crust, had formed singularly narrow conceptions of the great processes whereby that crust has been built up. His enthusiasm, however, fired his disciples with the zeal of proselytes, and they spread themselves over Europe to preach everywhere the artificial system which they had learnt in Saxony. By a curious fate Edinburgh became one of the great head-quarters of Wernerism. The friends and followers of Hutton found themselves attacked in their own city by zealots who, proud of superior mineralogical acquirements, turned their most cherished ideas upside down and assailed them in the uncouth jargon of Freiberg. Inasmuch as subterranean heat had been invoked by Hutton as a force largely instrumental in consolidating and upheaving the ancient sediments that now form so great a part of the dry land, his followers were nicknamed Plutonists. On the other hand, as the agency of water was almost alone admitted by Werner, who believed the rocks of the earth's crust to have been chiefly chemical precipitates from a primeval universal ocean, those who adopted his views received the equally descriptive name of Neptunists. The battle of these two contending schools raged fiercely here for some years, and though mainly from the youth, zeal, and energy of Jameson, and the influence which his position as Professor in the University gave him, the Wernerian doctrines continued to hold their place, they were eventually abandoned even by Jameson himself, and the debt due to the memory of Hutton and Playfair was tardily acknowledged.

The pursuits and the quarrels of philosophers have from early times been a favourite subject of merriment to the outside world. Such a feud as that between the Plutonists and Neptunists would be sure to furnish abundant matter for the gratification of this propensity. Turning over the pages of Kay's "Portraits," where so much that was distinctive of Edinburgh's society a hundred years ago is embalmed, we find Hutton's personal peculiarities and pursuits touched off in good-humoured caricature. In one plate he stands with arms folded and hammer in hand, meditating on the face of a cliff, from which rocky prominences in shape of human faces, perhaps grotesque likenesses of his scientific opponents, grin at him. In another engraving he sits in conclave with his friend Black, possibly arranging for that famous banquet of garden-snails which the two worthies had persuaded themselves to look upon as a strangely neglected form of human food. More than a generation later, when the Huttonists and Wernerists were at the height of their antagonism, the humorous side of the controversy did not escape the notice of the author of "Waverley," who, you will remember, when he makes Meg Dods recount the various kinds of wise folk brought by Lady Penelope Pennfeather from Edinburgh to St. Ronan's Well, does not forget to include those who "rin uphill and down dale, knapping the chucky-stanes to pieces wi' hammers, like sae mony road-makers run daft, to see how the warld was made."

Among the names of the friends and followers of Hutton there is one which on this occasion deserves to be held in especial honour, that of Sir James Hall, of Dunglass. Having accompanied Hutton in some of his excursions, and having discussed with him the problems presented by the rocks of Scotland, Hall was familiar with the views of his master, and was able to supply him with fresh illustrations of them from different parts of the country. Gifted with remarkable originality and ingenuity, he soon perceived that some of the questions involved in the theory of the earth could probably be solved by direct physical experiment. Hutton, however, mistrusted any attempt "to judge of the great operations of Nature by merely kindling a fire and looking into the bottom of a little crucible." Out of deference to this prejudice Hall delayed to carry out his intention during Hutton's lifetime. But afterwards he instituted a remarkable series of researches which are memorable in the history of science as the first methodical endeavour to test the value of geological speculation by an appeal to actual experiment. The Neptunists, in ridiculing the Huttonian doctrine that basalt and similar rocks had once been molten, asserted that, had such been their origin, these masses would now be found in the condition of glass or slag. Hall, however, triumphantly vindicated his friend's view by proving that basalt could be fused, and thereafter by slow cooling could be made to resume a stony texture. Again, Hutton had asserted that under the vast pressures which must be effective deep within the earth's crust, chemical reactions must be powerfully influenced, and that under such conditions even limestone may conceivably be melted without losing its carbonic acid. Various specious arguments have been adduced against this proposition, but by an ingeniously devised series of experiments, Hall succeeded in converting limestone under great pressure into a kind of marble, and even fused it, and found that it then acted vigorously on other rocks. These admirable researches, which laid the foundations of experimental geology, constitute not the least memorable of the services rendered by the Huttonian school to the progress of science.

Clear as was the insight and sagacious the inferences of these great masters in regard to the history of the globe, their vision was necessarily limited by the comparatively narrow range of ascertained fact which up to their time had been established. They taught men to recognize that the present world is built of the ruins of an earlier one, and they explained with admirable perspicacity the operation of the processes whereby the degradation and renovation of land are brought about. But they never dreamed that a long and orderly series of such successive destructions and renewals had taken place, and had left their records in the crust of the earth. They never imagined that from these records it would be possible to establish a determinate chronology that could be read everywhere, and applied to the elucidation of the remotest quarter of the globe. It was by the memorable observations and generalizations of William Smith that this vast extension of our knowledge of the past history of the earth become possible. While the Scottish philosophers were building up their theory here, Smith was quietly ascertaining by extended journeys that the stratified rocks of the West of England occur in a definite sequence, and that each well-marked group of them can be discriminated from the others and identified across the country by means of its enclosed organic remains. It is nearly a hundred years since he made known his views, so that by a curious coincidence we may fitly celebrate on this occasion the centenary of William Smith as well as that of James Hutton. No single discovery has ever had a more momentous and far-reaching influence on the progress of a science than that law of organic succession which Smith established. At first it served merely to determine the order of the stratified rocks of England. But it soon proved to possess a world-wide value, for it was found to furnish the key to the structure of the whole stratified crust of the earth. It showed that within that crust lie the chronicles of a long history of plant and animal life upon this planet, it supplied the means of arranging the materials for this history in true chronological sequence, and it thus opened out a magnificent vista through a vast series of ages, each marked by its own distinctive types of organic life, which, in proportion to their antiquity, departed more and more from the aspect of the living world.

Thus a hundred years ago, by the brilliant theory of Hutton and the fruitful generalization of Smith, the study of the earth received in our country the impetus which has given birth to the modern science of geology.

To review the marvellous progress which this science has made during the first century of its existence would require not one but many hours for adequate treatment. The march of discovery has advanced along a multitude of different paths, and the domains of Nature which have been included within the growing territories of human knowledge have been many and ample. Nevertheless, there are certain departments of investigation to which we may profitably restrict our attention on the present occasion, and wherein we may see how the leading principles that were proclaimed in this city a hundred years ago have germinated and borne fruit all over the world.

From the earliest times the natural features of the earth's surface have arrested the attention of mankind. The rugged mountain, the cleft ravine, the scarped cliff, the solitary boulder, have stimulated curiosity and prompted many a speculation as to their origin. The shells embedded by millions in the solid rocks of hills far removed from the sea have still further pressed home these "obstinate questionings." But for many long centuries the advance of inquiry into such matters was arrested by the paramount influence of orthodox theology. It was not merely that the Church opposed itself to the simple and obvious interpretation of these natural phenomena. So implicit had faith become in the accepted views of the earth's age and of the history of creation, that even laymen of intelligence and learning set themselves unbidden and in perfect good faith to explain away the difficulties which Nature so persistently raised up, and to reconcile her teachings with those of the theologians. In the various theories thus originating, the amount of knowledge of natural law usually stood in inverse ratio to the share played in them by an uncontrolled imagination. The speculations, for example, of Burnet, Whiston, Whitehurst, and others in this country, cannot be read now without a smile. In no sense were they scientific researches; they can only be looked upon as exercitations of learned ignorance. Springing mainly out of a laudable desire to promote what was believed to be the cause of true religion, they helped to retard inquiry, and exercised in that respect a baneful influence on intellectual progress.

It is the special glory of the Edinburgh school of geology to have cast aside all this fanciful trifling. Hutton boldly proclaimed that it was no part of his philosophy to account for the beginning of things. His concern lay only with the evidence furnished by the earth itself as to its origin. With the intuition of true genius he early perceived that the only solid basis from which to explore what has taken place in byegone time is a knowledge of what is taking place to-day. He thus founded his system upon a careful study of the processes whereby geological changes are now brought about. He felt assured that Nature must be consistent and uniform in her working, and that only in proportion as her operations at the present time are watched and understood will the ancient history of the earth become intelligible. Thus, in his hands, the investigation of the Present became the key to the interpretation of the Past. The establishment of this great truth was the first step towards the inauguration of a true science of the earth. The doctrine of the uniformity of causation in Nature became the fruitful principle on which the structure of modern geology could be built up.

Fresh life was now breathed into the study of the earth. A new spirit seemed to animate the advance along every pathway of inquiry. Facts that had long been familiar came to possess a wider and deeper meaning when their connection with each other was recognized as parts of one great harmonious system of continuous change. In no department of Nature, for example, was this broader vision more remarkably displayed than in that wherein the circulation of water between land and sea plays the most conspicuous part. From the earliest times men had watched the coming of clouds, the fall of rain, the flow of rivers, and had recognized that on this nicely adjusted machinery the beauty and fertility of the land depend. But they now learnt that this beauty and fertility involve a continual decay of the terrestrial surface; that the soil is a measure of this decay, and would cease to afford us maintenance were it not continually removed and renewed; that through the ceaseless transport of soil by rivers to the sea the face of the land is slowly lowered in level and carved into mountain and valley, and that the materials thus borne outwards to the floor of the ocean are not lost, but accumulate there to form rocks, which in the end will be upraised into new lands. Decay and renovation, in well-balanced proportions, were thus shown to be the system on which the existence of the earth as a habitable globe had been established. It was impossible to conceive that the economy of the planet could be maintained on any other basis. Without the circulation of water the life of plants and animals would be impossible, and with that circulation the decay of the surface of the land and the renovation of its disintegrated materials are necessarily involved.

As it is now so must it have been in past time. Hutton and Playfair pointed to the stratified rocks of the earth's crust as demonstrations that the same processes which are at work to-day have been in operation from a remote antiquity. By thus placing their theory on a basis of actual observation, and providing in the study of existing operations a guide to the interpretation of those in past times, they rescued the investigation of the history of the earth from the speculations of theologians and cosmologists, and established a place for it among the recognized inductive sciences. To the guiding influence of their philosophical system the prodigious strides made by modern geology are in large measure to be attributed. And here in their own city, after the lapse of a hundred years, let us offer to their memory the grateful homage of all who have profited by their labours.

But while we recognize with admiration the far-reaching influence of the doctrine of uniformity of causation in the investigation of the history of the earth, we must upon reflection admit that the doctrine has been pushed to an extreme perhaps not contemplated by its original founders. To take the existing conditions of Nature as a platform of actual knowledge from which to start in an inquiry into former conditions was logical and prudent. Obviously, however, human experience, in the few centuries during which attention has been turned to such subjects, has been too brief to warrant any dogmatic assumption that the various natural processes must have been carried on in the past with the same energy and at the same rate as they are carried on now. Variations in energy might have been legitimately conceded as possible, though not to be allowed without reasonable proof in their favour. It was right to refuse to admit the operation of speculative causes of change when the phenomena were capable of natural and adequate explanation by reference to causes that can be watched and investigated. But it was an error to take for granted that no other kind of process or influence, nor any variation in the rate of activity save those of which man has had actual cognizance, has played a part in the terrestrial economy. The uniformitarian writers laid themselves open to the charge of maintaining a kind of perpetual motion in the machinery of Nature. They could find in the records of the earth's history no evidence of a beginning, no prospect of an end. They saw that many successive renovations and destructions had been effected on the earth's surface, and that this long line of vicissitudes formed a series of which the earliest were lost in antiquity, while the latest were still in progress towards an apparently illimitable future.

The discoveries of William Smith, had they been adequately understood, would have been seen to offer a corrective to this rigidly uniformitarian conception, for they revealed that the crust of the earth contains the long record of an unmistakable order of progression in organic types. They proved that plants and animals have varied widely in successive periods of the earth's history, the present condition of organic life being only the latest phase of a long preceding series, each stage of which recedes further from the existing aspect of things as we trace it backward into the past. And though no relic had yet been found, or indeed was ever likely to be found, of the first living things that appeared upon the earth's surface, the manifest simplification of types in the older formations pointed irresistibly to some beginning from which the long procession had taken its start. If then it could thus be demonstrated that there had been upon the globe an orderly march of living forms from the lowliest grades in early times to man himself to day, and thus that in one department of her domain, extending through the greater portion of the records of the earth's history, Nature had not been uniform but had followed a vast and noble plan of evolution, surely it might have been expected that those who discovered and made known this plan would seek to ascertain whether some analogous physical progression from a definite beginning might not be discernible in the framework of the globe itself.

But the early masters of the science laboured under two great disadvantages. In the first place, they found the oldest records of the earth's history so broken up and effaced as to be no longer legible. And in the second place, they lived under the spell of that strong reaction against speculation which followed the bitter controversy between the Neptunists and Plutonists in the earlier decades of the century. They considered themselves bound to search for facts, not to build up theories; and as in the crust of the earth they could find no facts which threw any light upon the primeval constitution and subsequent development of our planet, they shut their ears to any theoretical interpretations that might be offered from other departments of science. It was enough for them to maintain, as Hutton had done, that in the visible structure of the earth itself no trace can be found of the beginning of things, and that the oldest terrestrial records reveal no physical conditions essentially different from those in which we still live. They doubtless listened with interest to the speculations of Kant, Laplace, and Herschel, on the probable evolution of nebulæ, suns, and planets, but it was with the languid interest attaching to ideas that lay outside of their own domain of research. They recognized no practical connection between such speculations and the data furnished by the earth itself as to its own history and progress.

This curious lethargy with respect to theory on the part of men who were popularly regarded as among the most speculative followers of science would probably not have been speedily dispelled by any discovery made within their own field of observation. Even now, after many years of the most diligent research, the first chapters of our planet's history remain undiscovered or undecipherable. On the great terrestrial palimpsest the earliest inscriptions seem to have been hopelessly effaced by those of later ages. But the question of the primeval condition and subsequent history of the planet might be considered from the side of astronomy and physics. And it was by investigations of this nature that the geological torpor was eventually dissipated. To our illustrious former President, Lord Kelvin, who occupied this chair when the Association last met in Edinburgh, is mainly due the rousing of attention to this subject. By the most convincing arguments he showed how impossible it was to believe in the extreme doctrine of uniformitarianism. And though, owing to uncertainty in regard to some of the data, wide limits of time were postulated by him, he insisted that within these limits the whole evolution of the earth and its inhabitants must have been comprised. While, therefore, the geological doctrine that the present order of Nature must be our guide to the interpretation of the past remained as true and fruitful as ever, it had now to be widened by the reception of evidence furnished by a study of the earth as a planetary body. The secular loss of heat, which demonstrably takes place both from the earth and the sun, made it quite certain that the present could not have been the original condition of the system. This diminution of temperature with all its consequences is not a mere matter of speculation, but a physical fact of the present time as much as any of the familiar physical agencies that affect the surface of the globe. It points with unmistakable directness to that beginning of things of which Hutton and his followers could find no sign.

Another modification or enlargement of the uniformitarian doctrine was brought about by continued investigation of the terrestrial crust and consequent increase of knowledge respecting the history of the earth. Though Hutton and Playfair believed in periodical catastrophes, and indeed required these to recur in order to renew and preserve the habitable condition of our planet, their successors gradually came to view with repugnance any appeal to abnormal, and especially to violent manifestations of terrestrial vigour, and even persuaded themselves that such slow and comparatively feeble action as had been witnessed by man could alone be recognized in the evidence from which geological history must be compiled. Well do I remember in my own boyhood what a cardinal article of faith this prepossession had become. We were taught by our great and honoured master, Lyell, to believe implicitly in gentle and uniform operations, extended over indefinite periods of time, though possibly some, with the zeal of partisans, carried this belief to an extreme which Lyell himself did not approve. The most stupendous marks of terrestrial disturbance, such as the structure of great mountain chains, were deemed to be more satisfactorily accounted for by slow movements prolonged through indefinite ages than by any sudden convulsion.

What the more extreme members of the uniformitarian school failed to perceive was the absence of all evidence that terrestrial catastrophes even on a colossal scale might not be a part of the present economy of this globe. Such occurrences might never seriously affect the whole earth at one time, and might return at such wide intervals that no example of them has yet been chronicled by man. But that they have occurred again and again, and even within comparatively recent geological times, hardly admits of serious doubt. How far at different epochs and in various degrees they may have included the operation of cosmical influences lying wholly outside the planet, and how far they have resulted from movements within the body of the planet itself, must remain for further inquiry. Yet the admission that they have played a part in geological history may be freely made without impairing the real value of the Huttonian doctrine, that in the interpretation of this history our main must be a knowledge of the existing processes of terrestrial change.

As the most recent and best known of these great transformations, the Ice Age stands out conspicuously before us. If any one sixty years ago had ventured to affirm that at no very distant date the snows and glaciers of the Arctic regions stretched southwards into France, he would have been treated as a mere visionary theorist. Many of the facts to which he would have appealed in support of his statement were already well known, but they had received various other interpretations. By some observers, notably by Hutton's friend, Sir James Hall, they were believed to be due to violent debacles of water that swept over the face of the land. By others they were attributed to the strong tides and currents of the sea when the land stood at a lower level. The uniformitarian school of Lyell had no difficulty in elevating or depressing land to any required extent. Indeed, when we consider how averse these philosophers were to admit any kind or degree of natural operation other than those of which there was some human experience, we may well wonder at the boldness with which, on sometimes the slenderest evidence, they made land and sea change places, on the one hand submerging mountain-ranges, and on the other placing great barriers of land where a deep ocean rolls. They took such liberties with geography because only well-established processes of change were invoked in the operations. Knowing that during the passage of an earthquake a territory bordering the sea may be upraised or sunk a few feet, they drew the sweeping inference that any amount of upheaval or depression of any part of the earth's surface might be claimed in explanation of geological problems. The progress of inquiry, while it has somewhat curtailed this geographical license, has now made known in great detail the strange story of the Ice Age.

There cannot be any doubt that after man had become a denizen of the earth, a great physical change came over the northern hemisphere. The climate, which had previously been so mild that evergreen trees flourished within ten or twelve degrees of the north pole, now became so severe that vast sheets of snow and ice covered the north of Europe and crept southward beyond the south coast of Ireland, almost as far as the southern shores of England, and across the Baltic into France and Germany. This Arctic transformation was not an episode that lasted merely a few seasons, and left the land to resume thereafter its ancient aspect. With various successive fluctuations it must have endured for many thousands of years. When it began to disappear it probably faded away as slowly and imperceptibly as it had advanced, and when it finally vanished it left Europe and North America profoundly changed in the character alike of their scenery and of their inhabitants. The rugged rocky contours of earlier times were ground smooth and polished by the march of the ice across them, while the lower grounds were buried under wide and thick sheets of clay, gravel, and sand, left behind by the melting ice. The varied and abundant flora which had spread so far within the Arctic circle was driven away into more southern and less ungenial climes. But most memorable of all was the extirpation of the prominent large animals which, before the advent of the ice, had roamed over Europe. The lions, hyænas, wild horses, hippopotami, and other creatures either became entirely extinct or were driven into the Mediterranean basin and into Africa. In their place came northern forms—the reindeer, glutton, musk ox, woolly rhinoceros, and mammoth.

Such a marvellous transformation in climate, in scenery, in vegetation and in inhabitants, within what was after all but a brief portion of geological time, though it may have involved no sudden or violent convulsion, is surely entitled to rank as a catastrophe in the history of the globe. It was probably brought about mainly if not entirely by the operation of forces external to the earth. No similar calamity having befallen the continents within the time during which man has been recording his experience, the Ice Age might be cited as a contradiction to the doctrine of uniformity, and yet it manifestly arrived as part of the established order of Nature. Whether or not we grant that other ice ages preceded the last great one, we must admit that the conditions under which it arose, so far as we know them, might conceivably have occurred before and may occur again. The various agencies called into play by the extensive refrigeration of the northern hemisphere were not different from those with which we are familiar. Snow fell and glaciers crept as they do to-day. Ice scored and polished rocks exactly as it still does among the Alps and in Norway. There was nothing abnormal in the phenomena save the scale on which they were manifested. And thus, taking a broad view of the whole subject, we recognize the catastrophe, while at the same time we see in its progress the operation of those same natural processes which we know to be integral parts of the machinery whereby the surface of the earth is continually transformed.

Among the debts which science owes to the Huttonian school, not the least memorable is the promulgation of the first well-founded conceptions of the high antiquity of the globe. Some six thousand years had previously been believed to comprise the whole life of the planet, and indeed of the entire universe. When the curtain was then first raised that had veiled the history of the earth, and men, looking beyond the brief span within which they had supposed that history to have been transacted, beheld the records of a long vista of ages stretching far away into a dim illimitable past, the prospect vividly impressed their imagination. Astronomy had made known the immeasurable fields of space; the new science of geology seemed now to reveal boundless distances of time. The more the terrestrial chronicles were studied the farther could the eye range into an antiquity so vast as to defy all attempts to measure or define it. The progress of research continually furnished additional evidence of the enormous duration of the ages that preceded the coming of man, while, as knowledge increased, periods that were thought to have followed each other consecutively were found to have been separated by prolonged intervals of time. Thus the idea arose and gained universal acceptance that, just as no boundary could be set to the astronomer in his free range through space, so the whole of bygone eternity lay open to the requirements of the geologist. Playfair, re-echoing and expanding Hutton's language, had declared that neither among the records of the earth nor in the planetary motions can any trace be discovered of the beginning or of the end of the present order of things; that no symptom of infancy or of old age has been allowed to appear on the face of Nature, nor any sign by which either the past or the future duration of the universe can be estimated; and that although the Creator may put an end, as He no doubt gave a beginning, to the present system, such a catastrophe will not be brought about by any of the laws now existing, and is not indicated by anything which we perceive. This doctrine was naturally espoused with warmth by the extreme uniformitarian school, which required an unlimited duration of time for the accomplishment of such slow and quiet cycles of change as they conceived to be alone recognizable in the record of the earth's past history.

It was Lord Kelvin who, in the writings to which I have already referred, first called attention to the fundamentally erroneous nature of these conceptions. He pointed out that from the high internal temperature of our globe, increasing inwards as it does, and from the rate of loss of its heat, a limit may be fixed to the planet's antiquity. He showed that so far from there being no sign of a beginning, and no prospect of an end to the present economy, every lineament of the solar system bears witness to a gradual dissipation of energy from some definite starting-point. No very precise data were then, or indeed are now, available for computing the interval which has elapsed since that remote commencement, but he estimated that the surface of the globe could not have consolidated less than twenty millions of years ago, for the rate of increase of temperature inwards would in that case have been higher than it actually is; nor more than 400 millions of years ago, for then there would have been no sensible increase at all. He was inclined, when first dealing with the subject, to believe that from a review of all the evidence then available, some such period as 100 millions of years would embrace the whole geological history of the globe.

It is not a pleasant experience to discover that a fortune which one has unconcernedly believed to be ample has somehow taken to itself wings and disappeared. When the geologist was suddenly awakened by the energetic warning of the physicist, who assured him that he had enormously overdrawn his account with past time, it was but natural under the circumstances that he should think the accountant to be mistaken, who thus returned to him dishonoured the large drafts he had made on eternity. He saw how wide were the limits of time deducible from physical considerations, how vague the data from which they had been calculated. And though he could not help admitting that a limit must be fixed beyond which his chronology could not be extended, he consoled himself with the reflection that after all a hundred millions of years was a tolerably ample period of time, and might possibly have been quite sufficient for the transaction of all the prolonged sequence of events recorded in the crust of the earth. He was therefore disposed to acquiesce in the limitation thus imposed upon geological history.

But physical inquiry continued to be pushed forward with regard to the early history and the antiquity of the earth. Further consideration of the influence of tidal friction in retarding the earth's rotation, and of the sun's rate of cooling, led to sweeping reductions of the time allowable for the evolution of the planet. The geologist found himself in the plight of Lear when his bodyguard of one hundred knights was cut down. "What need you five-and-twenty, ten or five?" demands the inexorable physicist, as he remorselessly strikes slice after slice from his allowance of geological time. Lord Kelvin is willing, I believe, to grant us some twenty millions of years, but Prof. Tait would have us content with less than ten millions.

In scientific as in other mundane questions there may often be two sides, and the truth may ultimately be found not to lie wholly with either. I frankly confess that the demands of the early geologists for an unlimited series of ages were extravagant, and, even for their own purposes, unnecessary, and that the physicist did good service in reducing them. It may also be freely admitted that the latest conclusions from physical considerations of the extent of geological time require that the interpretation given to the record of the rocks should be rigorously revised, with the view of ascertaining how far that interpretation may be capable of modification or amendment. But we must also remember that the geological record constitutes a voluminous body of evidence regarding the earth's history which cannot be ignored, and must be explained in accordance with ascertained natural laws. If the conclusions derived from the most careful study of this record cannot be reconciled with those drawn from physical considerations, it is surely not too much to ask that the latter should be also revised. It has been well said that the mathematical mill is an admirable piece of machinery, but that the value of what it yields depends upon the quality of what is put into it. That there must be some flaw in the physical argument I can, for my own part, hardly doubt, though I do not pretend to be able to say where it is to be found. Some assumption, it seems to me, has been made, or some consideration has been left out of sight, which will eventually be seen to vitiate the conclusions, and which when duly taken into account will allow time enough for any reasonable interpretation of the geological record.

In problems of this nature, where geological data capable of numerical statement are so needful, it is hardly possible to obtain trustworthy computations of time. We can only measure the rate of changes in progress now, and infer from these changes the length of time required for the completion of results achieved by the same processes in the past. There is fortunately one great cycle of movement which admits of careful investigation, and which has been made to furnish valuable materials for estimates of this kind. The universal degradation of the land, so notable a characteristic of the earth's surface, has been regarded as an extremely slow process. Though it goes on without ceasing, yet from century to century it seems to leave hardly any perceptible trace on the landscapes of a country. Mountains and plains, hills and valleys, appear to wear the same familiar aspect which is indicated in the oldest pages of history. This obvious slowness in one of the most important departments of geological activity, doubtless contributed in large measure to form and foster a vague belief in the vastness of the antiquity required for the evolution of the earth.

But, as geologists eventually came to perceive, the rate of degradation of the land is capable of actual measurement. The amount of material worn away from the surface of any drainage-basin and carried in the form of mud, sand, or gravel, by the main river into the sea, represents the extent to which that surface has been lowered by waste in any given period of time. But denudation and deposition must be equivalent to each other. As much material must be laid down in sedimentary accumulations as has been mechanically removed, so that in measuring the annual bulk of sediment borne into the sea by a river, we obtain a clue not only to the rate of denudation of the land, but also to the rate at which the deposition of new sedimentary formations takes place.

As might be expected, the activities involved in the lowering of the surface of the land are not everywhere equally energetic. They are naturally more vigorous where the rainfall is heavy, where the daily range of temperature is large, and where frosts are severe. Hence they are obviously much more effective in mountainous regions than on plains; and their results must constantly vary, not only in different basins of drainage, but even, and sometimes widely, within the same basin. Actual measurement of the proportion of sediment in river water shows that while in some cases the lowering of the surface of the land may be as much as $\frac{1}{730}$ of a foot in a year, in others it falls as low as $\frac{1}{6800}$. In other words, the rate of deposition of new sedimentary formations, over an area of sea-floor equivalent to that which has yielded the sediment, may vary from one foot in 730 years to one foot in 6,800 years.

If now we take these results and apply them as measures of the length of time required for the deposition of the various sedimentary masses that form the outer part of the earth's crust, we obtain some indication of the duration of geological history. On a reasonable computation these stratified masses, where most fully developed, attain a united thickness of not less than 100,000 feet. If they were all laid down at the most rapid recorded rate of denudation, they would require a period of seventy-three millions of years for their completion. If they were laid down at the slowest rate they would demand a period of not less than 680 millions.

But it may be argued that all kinds of terrestrial energy are growing feeble, that the most active denudation now in progress is much less vigorous than that of bygone ages, and hence that the stratified part of the earth's crust may have been put together in a much briefer space of time than modern events might lead us to suppose. Such arguments are easily adduced and look sufficiently specious, but no confirmation of them can be gathered from the rocks. On the contrary, no one can thoughtfully study the various systems of stratified formations without being impressed by the fulness of their evidence that, on the whole, the accumulation of sediment has been extremely slow. Again and again we encounter groups of strata composed of thin paper-like laminæ of the finest silt, which evidently settled down quietly and at intervals on the sea bottom. We find successive layers covered with ripple-marks and sun-cracks, and we recognize in them memorials of ancient shores where sand and mud tranquilly gathered as they do in sheltered estuaries at the present day. We can see no proof whatever, nor even any evidence which suggests, that on the whole the rate of waste and sedimentation was more rapid during Mesozoic and Palæozoic time than it is to-day. Had there been any marked difference in this rate from ancient to modern times, it would be incredible that no clear proof of it should have been recorded in the crust of the earth.

But in actual fact the testimony in favour of the slow accumulation and high antiquity of the geological record is much stronger than might be inferred from the mere thickness of the stratified formations. These sedimentary deposits have not been laid down in one unbroken sequence, but have had their continuity interrupted again and again by upheaval and depression. So fragmentary are they in some regions, that we can easily demonstrate the length of time represented there by still existing sedimentary strata to be vastly less than the time indicated by the gaps in the series.

There is yet a further and impressive body of evidence furnished by the successive races of plants and animals which have lived upon the earth and have left their remains sealed up within its rocky crust. No one now believes in the exploded doctrine that successive creations and universal destructions of organic life are chronicled in the stratified rocks. It is everywhere admitted that, from the remotest times up to the present day, there has been an onward march of development, type succeeding type in one long continuous progression. As to the rate of this evolution precise data are wanting. There is, however, the important negative argument furnished by the absence of evidence of recognizable specific variations of organic forms since man began to observe and record. We know that within human experience a few species have become extinct, but there is no conclusive proof that a single new species has come into existence, nor are appreciable variations readily apparent in forms that live in a wild state. The seeds and plants found with Egyptian mummies, and the flowers and fruits depicted on Egyptian tombs, are easily identified with the vegetation of modern Egypt. The embalmed bodies of animals found in that country show no sensible divergence from the structure or proportions of the same animals at the present day. The human races of Northern Africa and Western Asia were already as distinct when portrayed by the ancient Egyptian artists as they are now, and they do not seem to have undergone any perceptible change since then. Thus a lapse of four or five thousand years has not been accompanied by any recognizable variation in such forms of plant and animal life as can be tendered in evidence. Absence of sensible change in these instances is, of course, no proof that considerable alteration may not have been accomplished in other forms more exposed to vicissitudes of climate and other external influences. But it furnishes at least a presumption in favour of the extremely tardy progress of organic variation.

If, however, we extend our vision beyond the narrow range of human history, and look at the remains of the plants and animals preserved in those younger formations which, though recent when regarded as parts of the whole geological record, must be many thousands of years older than the very oldest of human monuments, we encounter the most impressive proofs of the persistence of specific forms. Shells which lived in our seas before the coming of the Ice Age present the very same peculiarities of form, structure, and ornament which their descendants still possess. The lapse of so enormous an interval of time has not sufficed seriously to modify them. So too with the plants and the higher animals which still survive. Some forms have become extinct, but few or none which remain display any transitional gradations into new species. We must admit that such transitions have occurred, that indeed they have been in progress ever since organized existence began upon our planet, and are doubtless taking place now. But we cannot detect them on the way, and we feel constrained to believe that their march must be excessively slow.

There is no reason to think that the rate of organic evolution has ever seriously varied; at least no proof has been adduced of such variation. Taken in connection with the testimony of the sedimentary rocks, the inferences deducible from fossils entirely bear out the opinion that the building up of the stratified crust of the earth has been extremely gradual. If the many thousands of years which have elapsed since the Ice Age have produced no appreciable modification of surviving plants and animals, how vast a period must have been required for that marvellous scheme of organic development which is chronicled in the rocks!

After careful reflection on the subject, I affirm that the geological record furnishes a mass of evidence which no arguments drawn from other departments of Nature can explain away, and which, it seems to me, cannot be satisfactorily interpreted save with an allowance of time much beyond the narrow limits which recent physical speculation would concede.

I have reserved for final consideration a branch of the history of the earth which, while it has become, within the lifetime of the present generation, one of the most interesting and fascinating departments of geological inquiry, owed its first impulse to the far-seeing intellects of Hutton and Playfair. With the penetration of genius these illustrious teachers perceived that if the broad masses of land and the great chains of mountains owe their origin to stupendous movements which from time to time have convulsed the earth, their details of contour must be mainly due to the eroding power of running water. They recognized that as the surface of the land is continually worn down, it is essentially by a process of sculpture that the physiognomy of every country has been developed, valleys being hollowed out and hills left standing, and that these inequalities in topographical detail are only varying and local accidents in the progress of the one great process of the degradation of the land.

From the broad and guiding outlines of theory thus sketched we have now advanced amid ever-widening multiplicity of detail into a fuller and nobler conception of the origin of scenery. The law of evolution is written as legibly on the landscapes of the earth as on any other page of the Book of Nature. Not only do we recognize that the existing topography of the continents, instead of being primeval in origin, has gradually been developed after many precedent mutations, but we are enabled to trace these earlier revolutions in the structure of every hill and glen. Each mountain-chain is thus found to be a memorial of many successive stages in geographical evolution. Within certain limits, land and sea have changed places again and again. Volcanoes have broken out and have become extinct in many countries long before the advent of man. Whole tribes of plants and animals have meanwhile come and gone, and in leaving their remains behind them as monuments at once of the slow development of organic types, and of the prolonged vicissitudes of the terrestrial surface, have furnished materials for a chronological arrangement of the earth's topographical features. Nor is it only from the organisms of former epochs that broad generalizations may be drawn regarding revolutions in geography. The living plants and animals of to-day have been discovered to be eloquent of ancient geographical features that have long since vanished. In their distribution they tell us that climates have changed, that islands have been disjoined from continents, that oceans once united have been divided from each other, or once separate have now been joined; that some tracts of land have disappeared, while others for prolonged periods of time have remained in isolation. The present and the past are thus linked together not merely by dead matter, but by the world of living things, into one vast system of continuous progression.

In this marvellous increase of knowledge regarding the transformations of the earth's surface, one of the most impressive features, to my mind, is the power now given to us of perceiving the many striking contrasts between the present and former aspects of topography and scenery. We seem to be endowed with a new sense. What is seen by the bodily eye—mountain, valley, or plain—serves but as a veil, beyond which, as we raise it, visions of long-lost lands and seas rise before us in a far-retreating vista. Pictures of the most diverse and opposite character are beheld, as it were, through each other, their lineaments subtly interwoven and even their most vivid contrasts subdued idto one blended harmony. Like the poet, "we see, but not by sight alone;" and the "ray of fancy" which, as a sunbeam, lightened up his landscape, is for us broadened and brightened by that play of the imagination which science can so vividly excite and prolong.

Admirable illustrations of this modern interpretation of scenery are supplied by the district wherein we are now assembled. On every side of us rise the most convincing proofs of the reality and potency of that ceaseless sculpture by which the elements of landscape have been carved into their present shapes. Turn where we may, our eyes rest on hills that project above the lowland, not because they have been upheaved into these positions, but because their stubborn materials have enabled them better to withstand the degradation which has worn down the softer strata into the plains around them. Inch by inch the surface of the land has been lowered, and each hard rock successively laid bare has communicated its own characteristics of form and colour to the scenery.

If, standing on the Castle Rock, the central and oldest site in Edinburgh, we allow the bodily eye to wander over the fair landscape, and the mental vision to range through the long vista of earlier landscapes which science here reveals to us, what a strange series of pictures passes before our gaze! The busy streets of to-day seem to fade away into the mingled copsewood and forest of prehistoric time. Lakes that have long since vanished gleam through the woodlands, and a rude canoe pushing from the shore startles the red deer that had come to drink. While we look, the picture changes to a polar scene, with bushes of stunted Arctic willow and birch, among which herds of reindeer browse and the huge mammoth makes his home. Thick sheets of snow are draped all over the hills around, and far to the north-west the distant gleam of glaciers and snow-fields marks the line of the Highland mountains. As we muse on this strange contrast to the living world of to-day the scene appears to grow more Arctic in aspect, until every hill is buried under one vast sheet of ice, 2,000 feet or more in thickness, which fills up the whole midland valley of Scotland and creeps slowly eastward into the basin of the North Sea. Here the curtain drops upon our moving pageant, for in the geological record of this part of the country an enormous gap occurs before the coming of the Ice Age.

When once more the spectacle resumes its movement the scene is found to have utterly changed. The familiar hills and valleys of the Lothians have disappeared. Dense jungles of a strange vegetation—tall reeds, club-mosses, and tree-ferns—spread over the steaming swamps that stretch for leagues in all directions. Broad lagoons and open seas are dotted with little volcanic cones which throw out their streams of lava and showers of ashes. Beyond these, in dimmer outline and older in date, we descry a wide lake or inland sea, covering the whole midland valley and marked with long lines of active volcanoes, some of them several thousand feet in height. And still further and fainter over the same region, we may catch a glimpse of that still earlier expanse of sea which in Silurian times overspread most of Britain. But beyond this scene our vision fails. We have reached the limit across which no geological evidence exists to lead the imagination into the primeval darkness beyond.

Such in briefest outline is the succession of mental pictures which modern science enables us to frame out of the landscapes around Edinburgh. They may be taken as illustrations of what may be drawn, and sometimes with even greater fulness and vividness, from any district in these islands. But I cite them especially because of their local interest in connection with the present meeting of the Association, and because the rocks that yield them gave inspiration to those great masters whose claims on our recollection, not least for their explanation of the origin of scenery, I have tried to recount this evening. But I am further impelled to dwell on these scenes from an overmastering personal feeling to which I trust I may be permitted to give expression. It was these green hills and grey crags that gave me in boyhood the impulse that has furnished the work and joy of my life. To them, amid changes of scene and surroundings, my heart ever fondly turns, and here I desire gratefully to acknowledge that it is to their influence that I am indebted for any claim I may possess to stand in the proud position in which your choice has placed me.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. ARTHUR SCHUSTER, PH.D., F.R.S., F.R.A.S., PRESIDENT OF THE SECTION.

IN opening the proceedings of our Annual Meeting the temptation is great to look back on the year which has passed and to select for special consideration such work published during its course as may seem to be of the greatest importance. I fear, however, that a year is too short a time to allow us to form a fair estimate of the value of a scientific investigation. The mushroom, which shoots up quickly, only to disappear again, impresses us more than the slow-growing seedling which will live to be a tree, and it is difficult to recognize the scientific fungus in its early stage. But, although I do not feel competent to give you a review of the progress made in our subject during the last twelve months, there is one event to which some allusion should be made. It has been the sad duty of many of my predecessors to announce the death of successful workers in the field of science, but I believe I am unique in having the pleasure of recording the birth of a scientific man. At the beginning of this year there came into the world a being so brilliant that he could, without preparation, take up the work of the most eminent man amongst us. Believers in the transmigration of souls have speculated on the fact that Galileo's death and Newton's birth fell within a year of each other; but no event has ever happened so striking as that which took place on the 1st of January, when the mantle of Sir William Thomson fell on the infant Lord Kelvin. Those who have attended these meetings will feel with me that the honour done to our foremost representative, an honour which has been a source of pride and satisfaction to every student of science, could not altogether remain unnoticed in the section which owes him so much.

We are chiefly concerned here with the increase of scientific knowledge, and we derive pleasure in contrasting the minor state of ignorance of our own time with that which prevailed a hundred years ago. But when we contrast at the same time the refined opportunities of a modern research laboratory with the crude conditions under which the experimentalist had to work at the beginning of the century, we may fairly ask ourselves whether it is possible by means of any systematic course of study or by means of any organisation to accelerate our progress into the dark continent of science. A number of serious considerations arise in connection with this subject, and though I am not going to weary you by attempting an exhaustive discussion, I should like to draw your attention to a few matters which seem to me to be well worthy of the consideration of this Association. Changes are constantly made and proposed in our existing institutions, or new ones are suggested which are to serve the purpose of a more rapid accumulation of knowledge. I need only allude to the alterations in the curriculum of the science schools in our old Universities, made partly for the purpose of fitting their graduates for the conduct of original research, or to the national laboratory proposed by my predecessor in this chair for carrying out a certain kind of scientific investigation, which at present is left undone, or is done by private enterprise. Even our own Association has not escaped the evil eye of the reformer, and, like other institutions, it may be capable of improvement. But in choosing the direction in which a change may best be made, I think we may learn something from the way in which Nature improves its organisms. We are taught by biologists that natural selection acts by developing those qualities which enable each species best to survive the struggle for existence; useless organs die off or become rudimentary. Nature teaches us, therefore, how a beautiful complex of beings, mutually dependent on each other, is formed by improving those parts which are best and most useful, and letting the rest take care of itself. But in many of the changes which have been made or are proposed the process of reform is very different. The weakest points are selected, our attention is drawn to some failure or something in which we are excelled by other nations, and attempts are made to cure what perhaps had better be left to become rudimentary. The proceeding is not objectionable as long as the nourishment which is applied to develop the weaker organs is not taken from those parts which we should specially take care to preserve. To apply these reflections to the questions with which we are specially concerned, I should like to see it more generally recognized that although there is no struggle for existence between different nations, yet each nation, owing to a number of circumstances, possesses its own peculiarities, which render it better fitted than its neighbours to do some particular part of the work on which the progress of science depends. No country, for instance, has rivalled France in the domain of accurate measurement, with which the names of Regnault and Amagat are associated, and the International Bureau of Weights and Measures has its fitting home in Paris.[1] The best work of the German Universities seems to me to consist in the following up of some theory to its logical conclusions and submitting it to the test of experiment. I doubt whether the efforts to transplant the research work of German Universities into this country will prove successful. Does it not seem well to let each country take that share of work for which the natural growth of its character and its educational establishment best adapt it? Is it wise to remedy some weak point, to fill up undoubted gaps, if the soil that fills the gaps has to be taken from the hills and elevations which rise above the surrounding level?

As far as the work of this section is concerned the strongest domain of this country has been that of mathematical physics. But it is not to this that I wish specially to refer. Look at the work done in Great Britain during the last two centuries; the work not only in physics, but in astronomy, chemistry, biology. Is it not true that the one distinctive feature which separates this from all other countries in the world is the prominent part played by the scientific amateur, and is it not also true that our modern system of education tends to destroy the amateur?

By amateur I do not necessarily mean a man who has other occupations and only takes up science in his leisure hours, but rather one who has had no academical training, at any rate in that branch of knowledge which he finally selects for study. He has probably been brought up for some profession unconnected with science, and only begins his study when his mind is sufficiently developed to form an entirely unbiassed opinion. We may, perhaps, best define an amateur as one who learns his science as he wants it and when he wants it. I should call Faraday an amateur. He would have been impossible in another country; perhaps he would be impossible in the days of the Science and Art Department. Other names will occur to you, the most typical and eminent being that of Joule. It is not my purpose to discuss why distinguished amateurs have been so numerous in this country, but I am anxious to point out that we are in danger of losing one great and necessary factor in the origination of scientific ideas.

One of the distinctive features of an amateur is this, that he carries the weight of theories, often not the weight of knowledge, and, if I am right, there is a distinct advantage in having one section of scientific men beginning their work untrammelled by preconceived notions, which a systematic training in science is bound to instil. Whatever is taught in early age must necessarily be taught in a more or less dogmatic manner, and, in whatever way it is taught, experience shows that it is nearly always received in a dogmatic spirit. It seems important, therefore, to confine the early training to those subjects in which preconceived notions are considered an advantage. It is to me an uncongenial task to sound a note of warning to our old Universities, for the chief difficulties in which they are placed at present are due to the fact that they have given way too much to outside advice; but I cannot help expressing a strong conviction that their highly specialised entrance examinations are a curse to all sound school education, and will prove a still more fatal curse to what concerns us most nearly, the progress of scientific knowledge. If school examinations could be more general, if scientific theories could only be taught at an age when a man is able to form an independent judgment, there might be some hope of retaining that originality of ideas which has been a distinctive feature of this country, and enabled our amateurs to hold a prominent position in the history of science. At present a knowledge of scientific theories seems to me to kill all knowledge of scientific facts.

It is by no means true that a complete knowledge of everything that has a bearing on a particular subject is always necessary to success in an original investigation. In many cases such knowledge is essential, in others it is a hindrance. Different types of men incline to different types of research, and it is well to preserve the dual struggle. The engine which works out the great problems of nature may be likened to a thermodynamic machine. The amateur supplies the steam and the Universities supply the cold water; the former, boiling over often with ill-considered and fanciful ideas, does not like the icy douche, and the professional scientist does not like the latent heat of the condensing steam, but nevertheless the hotter the steam and the colder the water the better works the machine. Sometimes it happens that the boiler and cooler are both contained in the same brain, and each country can boast of a few such in a century, but most of us have to remain satisfied with forming only an incomplete part of the engine of research.

But while it is necessary to recognize the great work done by the unprofessional scientists, it seems not untimely to draw their attention to the damage done to themselves if they overstep their legitimate boundaries, and especially if they seek popular support for their theories, which have not received the approval of those who are competent to judge. An appeal from Alexander sober to Alexander drunk will not prove successful in the end.

The gradual disappearance of the amateur may be a necessary consequence of our increased educational facilities, and we must inquire whether any marked advantages are offered to us in exchange. There is one direction in which it would seem at first sight, at any rate, that a proper course of study could do much to facilitate the progress of research.

On another occasion I pointed out that two parties are necessary for every advance in science, the one that makes it and the one that believes in it. If the discoverer is born, and cannot be made, would it not be possible at any rate to train the judgment of our students so that they may form a sound opinion on the new theories and ideas which are presented to them? It is too early as yet to judge in how far our generation is better in this respect than the one that has gone before them, but on closer examination it does not seem to me to be obvious that any marked improvement is possible. Every new idea revolutionizing our opinions on some important question must necessarily take time before it takes a proper hold on the scientific world. Is it not true that anyone who can at once see the full importance of a new theory, and accept it in place of the one in which he has been brought up, must stand at a height almost equal to that of the originator? The more startling and fresh the new conception the fewer must be those who are ready to adopt it. But looking back at the history of science during the present century, is there much evidence that great discoveries have been seriously delayed by want of proper appreciation? We may hear of cases where important papers have been rejected by scientific societies, and occasionally a man of novel ideas may have been too much neglected by his contemporaries. I doubt whether such cases of apparent injustice can ever be avoided, and, simply looking

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[1] Much of the good work done by this Bureau remains unknown, owing to the miserly way in which their publications are circulated. No copies are supplied even to the University libraries. The explanation, of course, is "want of funds." In other words, England, France, and Germany, together with other nations, unite to do a certain kind of work, but cannot afford to distribute a few copies of the publication to the public for whose benefit the work is undertaken.

back on the great changes involved in matters of primary importance, such as the undulatory theory of light, the conservation of energy, and the second law of thermodynamics, I cannot admit that there is much reason to be dissatisfied with the rate at which new theories have been received. Those who experience a temporary check, owing to the fact that public opinion is not ripe for their ideas, are often amply rewarded after the lapse of a few years. The disappointment which Joule may have felt during the time his views met with adverse criticisms from the official world of science was no doubt amply compensated by the pleasure with which he watched the subsequent progress of research in the new domain which his discoveries have opened out.

The point is not one of academic interest only, for the fear of repressing some important new discovery has a detrimental influence in another direction. The judgment of the scientific world seems to me to be tending too much towards leniency to apparently absurd theories, because there is a remote chance that they may contain some germ of real value. A new truth will not be found to suffer ultimately by adverse and even unreasonable criticism, while bad theories and bad reasoning, supported by the benevolent neutrality of those to whose judgment the scientific world looks for guidance, are harmful in many ways. They block the way to an independent advance and encourage hasty and ill-considered generalizations. The conclusions I should draw from the considerations I have placed before you are these: I believe that a reasonable censorship exercised by our scientific societies is good and necessary; that those whose fate it is to be called on to express an opinion on some work or theory should do so fearlessly according to their best judgment. Their opinion may be warped by prejudice, but I think it is better that they should incur the risk of being ultimately found to be wrong than that they should help in the propagation of bad reasoning. There is one matter, however, on which all opinions must agree. Worse than bad theory or logic is bad experimental work. Should we then not rigorously preserve any influence or incentive which encourages the beginner to avoid carelessness and to consider neither time nor trouble to secure accuracy? There is no doubt to my mind that the prospect of admission to the Royal Society has been most beneficial in this respect, and that the honourable ambition to see his paper published in the "Transactions" of that Society has preserved many a student from the premature publication of unfinished work.

One of the principal obstacles to the rapid diffusion of a new idea lies in the difficulty of finding suitable expression to convey its essential point to other minds. Words may have to be strained into a new sense, and scientific controversies constantly resolve themselves into differences about the meaning of words. On the other hand, a happy nomenclature has sometimes been more powerful than rigorous logic in allowing a new train of thought to be quickly and generally accepted.

A good example is furnished by the history of the science of energy. The principle of the conservation of energy has undoubtedly gained a more rapid and general acceptance than it would otherwise have had by the introduction of the word potential energy. A great theorem, which in itself seems to me to be an intricate one, has been simplified by calling something energy which, in the first place, is only a deficiency of kinetic energy. The only record I can find on the history of the expression is given in Tait's "Thermodynamics," wherein the term statical energy is ascribed to Lord Kelvin, and that of potential energy to Rankine. It would be of interest to have a more detailed account on the origin of an expression which has undoubtedly had a marked influence not only on the physics, but also on the metaphysics of our time. But while fully recognizing the very great advantage we have derived from this term "Potential Energy," we ought not, at the same time, to lose sight of the fact that it implies something more than can be said to be proved. It is easy to overstep the legitimate use of the word. Thus, when Professor Lodge[1] attempts to prove that action at a distance is not consistent with the doctrine of energy, he cannot, in my opinion, justify his position except by assuming that all energy is ultimately kinetic. That is a plausible but by no means a necessary theory. Efforts have been made to look on energy as on something which can be labelled and identified through its various transformations. Thus we may feel a certain bit of energy radiating from a coal-fire, and if our knowledge was complete, we ought to be able to fix the time at which that identical bit of energy left the sun and arrived on the surface of the earth, setting up a chemical action in the leaves of the plant from which the coal has been derived. If we push this view to a logical conclusion, it seems to me that we must finally arrive at an atomic conception of energy which some may consider an absurdity.

Let, for instance, a number of particles P_1, P_2, &c., in succession, strike another particle Q. How can we in the translatory energy of the latter identify the parts which P_1, P_2, &c., have contributed? According to Professor's Lodge's view, we should be able to do so, for if the particle Q in its turn gives up its energy to others, say R_1, R_2, R_3, &c., we ought to be able to say whether the energy of P_1 has ultimately gone into R_1 or into R_2, or is divided between them. It is only by imagining that all energy is made up of a finite number of bits, which pass from one body to another, that we can defend the idea of considering energy as capable of being "labelled."

In the expressions we adopt to prescribe physical phenomena we necessarily hover between two extremes. We either have to choose a word which implies more than we can prove, or we have to use vague and general terms which hide the essential point, instead of bringing it out. The history of electrical theories furnishes a good example. The terms positive and negative electricity committed us to something definite; we could reckon about quantities of electricity, and form some definite notion of electrical currents as a motion of the two kinds of electricity in opposite directions. Now we have changed all that; we speak of electric displacements, but safeguard ourselves by saying that a displacement only means a vector quantity, and not necessarily an actual displacement. We speak of lines and tubes of force not only as a help to realize more clearly certain analytical results, but as implying a physical theory to which, at the same time, we do not wish to commit ourselves. I do not find any fault with this, for it is a perfectly legitimate and necessary process to state the known connection between physical phenomena in some form which introduces the smallest number of assumptions. But the great question "What is electricity?" is not touched by these general considerations. The brilliant success with which Maxwell's investigations have been crowned is apt to make us overrate the progress made in the solution of that question. Maxwell and his followers have proved the important fact that optical and electrical actions are transmitted through the same medium. We may be said to have arrived in the subject of electricity at the stage in which optics was placed before Young and Fresnel hit on the idea of transverse vibrations, but there is no theory of electricity in the sense in which there is an elastic solid theory of light.

If the term electrical displacement was taken in its literal sense, it would mean that the electric current consists of the motion of the ether through the conductor. This is a plausible hypothesis, and one respecting which we may obtain experimental evidence. The experiments of Rayleigh and others have shown that the velocity of light in an electrolyte, through which an electric current is passing, is, within experimental limits, the same with and against the current. This result shows that if an electrical current means a motion of the ether the velocity of the medium cannot exceed ten metres a second for a current density of one ampère per square centimetre. This, then, is the upper limit for a possible velocity of the medium; can we find a lower limit? The answer to that question depends on the interpretation of a well-known experiment of Fizeau's, who found that the speed of light is increased if it travels through water which moves in the same direction as the light. If this experiment implies that the water carries the ether with it, and if a motion of the ether means an electric current, we should be led to the conclusion that a current of water should deflect a magnet in its neighbourhood. An experiment made to that effect would almost certainly give a negative result, and would give us a lower limit for the velocity of the medium corresponding to a given current. Such an experiment, together with that of Rayleigh, would probably dispose of the theory that an electric current is due to a translatory velocity of the medium. This would be an important step, and it would be worth while to arrive at a final settlement of the question.[1] The whole question of the relation between the

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[1] *Phil. Mag.* vol. xi. p. 36 (1881).

[1] Fizeau's results must either be due to the motion of matter through the medium or to the fact that moving matter carries the ether with it. If it is due to the former cause, and matter does *not* carry the ether with it, may we not consider that matter moving through the ether, that is a relative motion of matter and ether, must produce effects equal and opposite to those of ether moving through matter? In that case the reasoning in the text would, *mutatis mutandis*, hold good.

motion of matter and motion of the medium is a vital one, and we shall probably not make any serious advances until experiment has found a new opening. But we must expect many negative results before some clue is discovered. Nor can we attach much importance to negative results unless they are made by some one in whose care and judgment we place full reliance. We should all the more, therefore, recognize the courage and perseverance of those who spend their valuable time in such investigations as Prof. Lodge has recently undertaken. That ultimately some relation will be found between moving matter and electrical action there is no reasonable doubt.

One of the most hopeful openings for new investigations has always been found in the pursuing of a theory to its logical conclusions, and there is one result of the electromagnetic theory of light which has not, in my opinion, received the share of attention which it deserves.

When sound passes through air it is propagated more quickly with the wind than against it, and we may easily find the velocity relative to the earth by combining the ordinary sound velocity with the velocity of the wind. Similarly, when any waves pass through a medium moving with uniform velocity, the waves being due to internal stresses in the medium, we may treat of the velocity of the waves independently of that of the medium, and say that the wave-velocity in the direction of motion of the medium, and relative to a fixed body, is the sum of the wave-velocity, calculated on the supposition that the medium is at rest and the velocity of the medium. Prof. J. J. Thomson,[1] applying Maxwell's equations, has arrived at a different result for electromagnetic waves, and has come to the conclusion that in order to get the velocity of light along a stream of flowing water we have to add to the velocity of light only half the velocity of water. The following considerations suggest themselves to me with respect to this result. Maxwell's theory is founded on certain observed effects, which all depend on the relative motion of matter. A result such as the one referred to implies actions depending on absolute motion, and appears therefore to point to something which has been introduced into the equations for which there is no experimental evidence. The only assumption clearly put down by Maxwell is that electromagnetic actions are transmitted through the medium, and it is possible that that assumption necessarily carries Prof. J. J. Thomson's result with it. If a careful examination of the subject should show that this is the case, we are brought face to face with a serious difficulty. It is said, with justice, to be one of the great advantages of Maxwell's theory that it does away with action at a distance; but what do we gain if we replace action at a distance by something infinitely more difficult to conceive, namely, internal stresses of a medium depending on the velocity of the medium through space? I can only see one loophole through which to escape, namely, that Maxwell's medium is not homogeneous, but consists of two parts, and that if we speak of the medium as moving, we mean the motion of one of these parts relative to the other.

While we may hope to obtain important results from an investigation of the relation between what we call electricity and the medium, we must not lose sight of another avenue, namely, the relation between electricity and chemical effects. The passage of electricity through gases presents us with a complicated problem to which a number of physicists have given their attention of late years. There seems no reasonable doubt that electricity in a gas is conveyed by the diffusion of particles conveying high charges, probably identical with those carried by the electrolytic ion. The fact that this convection is a process of diffusion with comparatively small velocity is shown by the experimental result that the path of the discharge is affected by any bodily motion of the gas which conveys the current. Even the convection currents due to the heat produced by the discharge itself are sufficient to deflect the luminous column which marks the passage of the current.

The most puzzling fact, however, connected with the discharge of electricity through gases consists in the absence of symmetry at the positive and negative poles. There must be some difference between a positively and negatively charged atom which seems of fundamental importance in the relation between matter and what we call electricity. A discussion of the various phenomena attending the discharge of electricity through gases seems to me to point to a conclusion which may possibly prove a step in the right direction.

A surface of separation between bodies having different conductivities becomes electrified by the passage of a current, while at the surface between two chemically distinct bodies we have, according to Helmholtz, a sheet covered at the two sides with opposite electricities. These surface electrifications are not merely imaginary layers invented to satisfy mathematical surface conditions. They can be proved to be realities. Thus, when one electrolyte floats on another, the specific resistances being different, we often observe secondary chemical effects due to the action of the ions which carry the surface electrification.

If the passage of electricity from the solid to the gas involves some work done, we must expect a double sheet of electricity at the boundary, the gas in contact with the kathode becoming positively, and that in contact with the anode negatively, electrified. *A priori* we can form no idea how a layer of gas, the atoms of which carry charges, will behave. The ordinary proof that all electrification must be confined to the surface implies that all forces act according to the law of the inverse square, but where we have also to consider molecular forces, I see no reason why the electrification at a surface may not stretch across a layer having a thickness comparable with the mean free path of the molecule. It is here that there seems to be the fundamental difference between positive and negative electricity. A negative electrification of the gas, like that of a solid or a liquid, seems always confined to the surface, and no one has ever observed a volume electrification of negative electricity. The case is different for the positively electrified part of the gas. Wherever from other considerations we should expect a positively electrified surface sheet, we always get a layer of finite thickness. The result implies a different law of impact between positively and negatively electrified ions, but I see no inherent improbability in this. That the kathode let into a gas is surrounded by a positively electrified layer of finite thickness extending outwards must be considered as an established fact, and several of the characteristic features of the discharge are explained by it. The large fall of potential at the kathode can also be explained on the view which I have put forward, for in order to keep up the discharge there must be a sufficient normal force at the surface, and if this force is not confined to the surface, but necessarily stretches across a finite layer, the fall of potential must be multiplied a great number of times. Similarly Goldstein has shown that some of the phenomena of the kathode are observed at every place at which the positive current flows from a wide to a narrow part of a column of gas. At such places we should expect a positive surface electrification, and here, again, the whole appearance tends to show that we are dealing with a positive volume electrification. No corresponding phenomena are observed when the current passes from the narrow to the wide part.

The fact that in all cases experimented upon positive volume electrifications are observed but never similar negative electrifications is surely of significance.

Some of the results recently brought to light by investigations on the discharge of electricity have interesting cosmical applications. Thus it is found that such a discharge through any part of a vessel containing a gas converts the whole gas into a conductor.[1] The dissociation which we imagine to take place in a liquid before electrolytic conduction takes place must be artificially produced in a gas by the discharge itself. We may imitate in gases which have thus been rendered conductive many of the phenomena hitherto restricted to liquids: thus I hope to bring to the notice of this meeting cases of primary and secondary cells in which the electrolyte is a gas. There are other ways in which a gas can be put into that sensitive state in which we may treat it as a conductor, and we have every reason to suppose that the upper regions of our atmosphere are in this state. The principal part of the daily variation of the magnetic needle is due to causes lying outside the surface of the earth, and is in all probability only an electro-magnetic effect due to that bodily motion in our atmosphere which shows itself in the diurnal changes of the barometer. A favourite idea of the late Prof. Balfour Stewart will thus probably be confirmed. The difference in the diurnal range between times of maximum and times of minimum sun-spots is accounted for by the fact that the atmosphere is a better conductor at times of maximum sun-spots.

The mention of sun-spots raises a point not altogether new to this section. Careful observation of celestial phenomena may

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[1] *Phil. Mag.*, vol. ix. p. 284 (1880).

[1] An experiment by Hittorf (*Wied. Ann.* vii., p. 614) suggested the probability of this fact, which was proved independently by Arrhenius and myself.

suggest to us the solution of many mysteries which are now puzzling us. Consider, for instance, how long it would have taken to prove the universal property of gravitational attraction if the record of planetary motion had not come to the philosopher's help. And surely the most casual observation of cosmical effects teaches us how much we have yet to learn.

The statement of a problem occasionally helps to clear it up, and I may be allowed, therefore, to put before you some questions, the solution of which seems not beyond the reach of our powers.

1. Is every large rotating mass a magnet? If it is, the sun must be a powerful magnet. The comets' tails, which eclipse observations show stretching out from our sun in all directions, probably consist of electric discharges. The effect of a magnet on the discharge is known, and careful investigations of the streamers of the solar corona ought to give an answer to the question which I have put.[1]

2. Is there sufficient matter in interplanetary space to make it a conductor of electricity? I believe the evidence to be in favour of that view. But the conductivity can only be small, for otherwise the earth would gradually set itself to revolve about its magnetic pole. Suppose the electric resistance of interplanetary space to be so great that no appreciable change in the earth's axis of rotation could have taken place within historical times, is it not possible that the currents induced in planetary space by the earth's revolution may, by their electromagnetic action, cause the secular variation of terrestrial magnetism? There seems to me to be here a definite question capable of a definite answer, and as far as I can judge without a strict mathematical investigation the answer is in the affirmative.

3. What is a sunspot? It is, I believe, generally assumed that it is analogous to one of our cyclones. The general appearance of a sunspot does not show any marked cyclonic motion, though what we see is really determined by the distribution of temperature and not by the lines of flow. But a number of cyclones clustering together like the sunspots in a group should move round each other in a definite way, and it seems to me that the close study of the relative positions of a group of spots should give decisive evidence for or against the cyclone theory.

4. If the spot is not due to cyclonic motion, is it not possible that electric discharges setting out from the sun, and accelerating artificially evaporation at the sun's surface, might cool those parts from which the discharge starts, and thus produce a sunspot? The effects of electric discharges on matters of solar physics have already been discussed by Dr. Huggins.

5. May not the periodicity of sunspots, and the connection between two such dissimilar phenomena as spots on the sun and magnetic disturbances on the earth, be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the sun? Such an increase of conductivity might be produced by meteoric matter circulating round the sun.

6. What causes the anomalous law of rotation of the solar photosphere? It has long been known that groups of spots at the solar equator perform their revolution in a shorter time than those in a higher latitude; but spots are disturbances which may have their own proper motions. Duner[2] has shown, however, from the displacement of the Fraunhofer lines, that the whole of the layer which produces these lines follows the same anomalous law, the angular velocity at a latitude of 75° being 30 per cent. less than near the equator.[3] As all causes acting within the sun might cause the angular velocity of the sun to be smaller at the equator than at other latitudes, but could not make it greater, the only explanation open to us is an outside effect either by an influx of meteoric matter, as suggested by Lord Kelvin, or in some other way. If we are to trust Dr. Welsing's result that faculæ which have their seat below the photosphere revolve in all latitudes with the same velocity, which is that of the spot velocity in the equatorial region, we should have to find a cause for a retardation in higher latitudes rather than for an acceleration at the equator. The exceptional behaviour of the solar surface seems to me to deserve very careful attention from solar physicists. Its explanation will probably carry with it that of many other phenomena.

In conclusion, I should like to return for an instant to the question whether it is possible by any means to render the progress of science more smooth and swift. If there is any truth in the idea that two types of mind are necessary, the one corresponding to the boiler and the other to the cooler of a steam-engine, it must also be true that some place must be found where the two may bring their influence to bear on each other. I venture to think that no better ground can be chosen than that supplied by our meetings. We hear it said that the British Association has fulfilled its object; we are told that it was originally founded to create a general interest in scientific problems in the towns in which it meets; and now that popular lectures and popular literature are supposed to perform that work more satisfactorily, we are politely asked to commit the happy despatch. There is no need to go back to the original intention of those who have founded this institution, which has at any rate adapted itself sufficiently well to the altered circumstances to maintain a beneficial influence in scientific research.

The free discussion which takes place in our sections, the interchange of ideas between men who during the rest of the year have occupied their minds, perhaps too much, with some special problem, the personal intercourse between those who are beginning their work with sanguine expectations, and those who have lost the first freshness of their enthusiasm, should surely one and all ensure a long prosperity to our meetings. If we cannot claim any longer to sow the seeds of scientific interest in the towns we visit, because the interest is established, we can at any rate assure those who so kindly offer us hospitality that they are helping powerfully in the promotion of the great object which we all have at heart.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. HERBERT MCLEOD, F.R.S., F.C.S., PRESIDENT OF THE SECTION.

IN endeavouring to prepare myself to properly fulfil the duties of President of this Section, to which I have been elected, and for which honour I am much indebted to the council and members of the Association (although I am only too well aware that the position might have been more efficiently filled by many others), I naturally looked at the reports of the previous meetings held in Edinburgh in 1834, 1850, and 1871, and it appears that on the first two occasions an address was not given by the president, a custom the discontinuance of which I have, at the present moment, much reason to regret.

At the meeting in 1834 a committee was appointed consisting of Dr. Dalton, Dr. Hope, Dr. T. Thomson, Mr. Whewell, Dr. Turner, Prof. Miller, Dr. Gregory, Dr. Christison, Mr. R. Phillips, Mr. Graham, Prof. Johnston, Dr. Faraday, Prof. Daniell, Dr. Clark, Prof. Cumming, and Dr. Prout, to report at the next meeting their opinion on the adoption of an uniform set of chemical symbols. Dr. Turner to be secretary.

In the following year the report contains: "Report of the Committee on Chemical Notation. Dr. Turner, the chairman of the committee appointed to take into consideration the adoption of an uniform system of chemical notation, made a report to the following effect:—

"1. That the majority of the Committee concur in approving of the employment of that system of notation which is already in general use on the Continent, though there exists among them some difference of opinion on points of detail.

"2. That they think it desirable not to deviate in the manner of notation from algebraic usage except so far as convenience requires.

"3. That they are of opinion that it would save much confusion if every chemist would always state explicitly the exact *quantities* which he intends to represent by his symbols.

"Dr. Dalton stated to the Chemical Section his reasons for preferring the symbols which he had himself used from the commencement of the atomic theory in 1803, to the Berzelian system of notation subsequently introduced. In his opinion regard must be had to the arrangement and equilibrium of the atoms (especially elastic atoms) in every compound atom, as well as to

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[1] The efforts of Mr. Bigelow have a bearing on this point, also some remarks which I have made in a lecture before the Royal Institution (*Proc. Roy. Inst.* 1891), but nothing decisive can be asserted at present.

[2] Oefvers. af Kongl. Veterrk. Ak. Forhandl., 47, 1890.

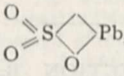
[3] Although the importance of M. Duner's results would make an independent investigation desirable, the measurements of Mr. Crew, who by a much inferior method arrived at other results, cannot have much weight as compared with those of Duner.

their number and weights. A system either of *arrangements* without *weights*, or of *weights* without *arrangements*, he considered only half of what it should be."

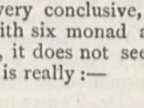
We can all sympathize with the members of the section of 1834 in their desire to obtain an uniform system of chemical notation, for at that time several very different systems seem to have been in use. Although the report is a short one, it probably directed the attention of chemists to the desirability of avoiding confusion by the use of various systems, and since that period many advances have been made.

There is now little necessity for every chemist to "state explicitly the exact *quantities* which he intends to represent by his symbols" for the accurate determinations of atomic weights by many chemists—and we must not omit to mention the work of Stas (whose death we have had to deplore since the last meeting of the British Association)—have given us a series of numbers which are in the hands of all chemists, so that, except in the cases where great refinement is requisite (or when the atomic weight has not been universally accepted) there is no need to state the values of the symbols.

That great advances have been made in chemical notation is well known to all; even in my own short experience I have had to learn several different methods. When I began to work at chemistry I was told that sulphate of lead was to be expressed by the formula PbO,SO_3. Hofmann taught me that it should be $PbSO_4$; then Gerhardt doubled the atomic weights of oxygen and sulphur and the formula became Pb_2SO_4; Cannizzaro showed that the atomic weight of lead should also be doubled, and the formula again became $PbSO_4$, but representing twice as much as formerly; then Frankland taught me to write SO_2Pbo'' as the expression of the graphic formula—



which not only states that the compound contains 207 of lead, 32 of sulphur, and 64 of oxygen, but that the sulphur is hexad, and is combined with two atoms of dyad oxygen, and with a dyad compound radical containing one atom of lead and two of oxygen; and of all the formulæ just given this is the only one which satisfies the requirements which Dalton thought necessary in 1835, namely, to indicate not only the weights of the elements present, but also their arrangement. It may be objected that we do not know that this formula really represents the arrangements of the atoms in plumbic sulphate, but there can be very little doubt that the four atoms of oxygen in the compound are not all in the same condition, for if we examine the properties of sulphuric acid (from which the sulphate of lead is derived by the replacement of the hydrogen by lead), we find that two of the atoms of oxygen are more closely associated with the hydrogen than are the other two, and, as there is some evidence, although perhaps not very conclusive, that sulphur may be capable of combining with six monad atoms, although no such compound is yet known, it does not seem unreasonable to suppose that sulphuric acid is really:—



What the nature of the attraction that holds the atoms together may be is not known, but it is more probably of a character similar to that of gravity which holds together sun and planets, than of the nature of cohesion which would hold the atoms rigidly together; the atoms in each molecule are therefore most probably in a state of rotation around, or of vibration to and from, the central atom which holds them together. The pictorial representation in a plane does not therefore truly express the position of the atoms, but merely the relations existing between them. In organic chemistry the use of formulæ expressing such a relation has become indispensable, and in inorganic chemistry I believe such a system is very useful.

Recently this system has been found insufficient for the requirements of organic chemistry, and recourse has been had to the figure of a tetrahedron to represent the atom of carbon, other atoms being attached to the solid angles; in this way the position of the atoms in space is more or less expressed.

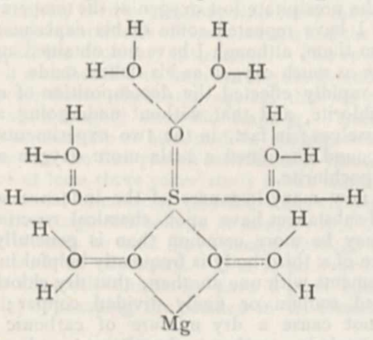
There are many cases, however, in which the atomicity theory fails us. At first it seemed probable that the atomicity of an element varied in pairs of attractions, that is, an element might be monad, triad, or pentad, but not dyad or tetrad; or it might be dyad, tetrad, or hexad, but not triad or pentad; but some great difficulties have been encountered. Thus nitrogen, which is pentad in ammonic chloride and triad in ammonia, forms the compound nitric oxide, NO, in which it would appear to be dyad; it has been suggested, however, that in this body the nitrogen is really triad, and that it possesses a "free bond." Now the idea of a "free bond" seems contrary to the principles of atomicity, since it is on the belief that such a free bond is impossible that the explanation of the existence of elementary molecules is formed, for it is said that when hydrogen is liberated two atoms unite to form a molecule, so that their mutual attractions may be satisfied. Nevertheless nitric oxide is a very active body, uniting readily with other substances, so the free bond seems to be on the look out for other kinds of matter, but to have no attraction for the free bond of another molecule of nitric oxide. As the molecule of nitric peroxide is variable by alterations of temperature, being N_2O_4 at low and NO_2 at high temperatures, it seemed not impossible that at the ordinary atmospheric temperature nitric oxide was a simplified or dissociated molecule, and that if the temperature were sufficiently reduced it would be found that its molecule would be N_2O_2, and thus it would contain triad nitrogen without a free bond. The density of the gas has, however, been determined at a temperature as low as $-73°$ and the molecule is still NO. Another important exception to the variation of the atomicity of an element in pairs was furnished by the investigations of Sir Henry Roscoe on the chlorides of vanadium; this element which, from analogy, should be a triad or a pentad, appears to form a chloride of the composition VCl_4. Again, the molecule of peroxide of chlorine is ClO_2, which would make chlorine a tetrad or the compound must have a free bond.

Another set of phenomena which the atomicity theory will not explain is the existence of well-defined crystalline salts containing what is called water of crystallization. This water is in many cases held with considerable pertinacity, the body appearing to be a veritable chemical compound. But water appears to be a saturated body, the attractions of the oxygen being satisfied by those of the hydrogen. It is true that water acts vigorously on other compounds, as on metallic oxides to form hydrates, and on some anhydrides to form acids, but these appear to be phenomena of double decomposition; thus the combination of water with sodic oxide and nitric anhydride respectively may be expressed by the equations

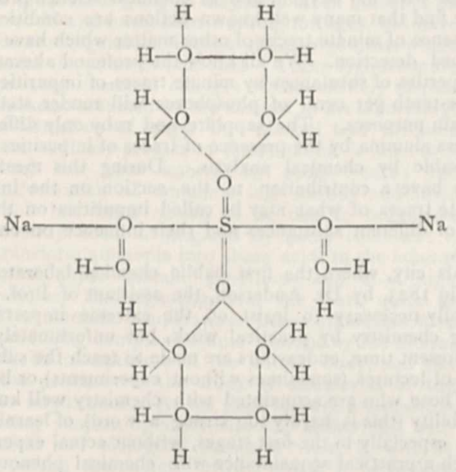
$$OH_2 + ONa_2 = OHNa + ONaH \text{ and}$$
$$OH_2 + O(NO_2)_2 = OH(NO_2) + O(NO_2)H.$$

In the combination of water with an anhydrous salt, a phenomenon often accompanied by great rise of temperature, there does not appear to be a double decomposition. That there is a chemical combination of some sort is shown by the changes of properties produced, crystalline form and colour being both sometimes altered. Compounds so produced have been called "molecular compounds" to imply that saturated molecules are in some way or another combined, the combination being different from "atomic combination," in which the atoms are directly united according to their valencies. Another explanation has been suggested by assuming that there is some "residual affinity" not saturated by the constituents of the body, and that this residual affinity enables bodies to unite in a less stable manner than in most compounds. But are not these terms—"molecular combination" and "residual affinity"—analogous to the term "catalysis," merely *words* to express—not to explain—what we do not understand? If "residual affinity" really exists, it must reside in the oxygen of the water, or in the hydrogen, or both; if so, what will happen to some of the complex constitutional formulæ of the organic chemist in which the carbon is tetrad, the oxygen dyad, and the hydrogen monad? If any of these elements have a residual affinity should we not expect to find additional unions between some of the atoms of the same molecule over and above those represented by the formula?

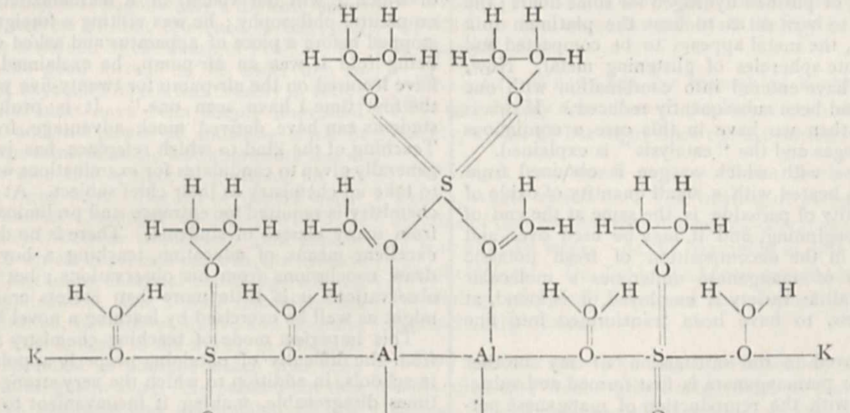
Oxygen may be tetrad, for which there is evidence in OAg_4. Under these circumstances water is by no means a saturated compound, and there would be no difficulty in explaining the combination of water with oxygen salts. Thus crystallized magnesic sulphate, $MgSO_5$, $7OH_2$ or SOHoMgo″, $6OH_2$ would be—



and sodic sulphate, Na_2SO_4, $10OH_2$:—



Even alum, with its 24 molecules of water of crystallization, may be expressed by an appalling formula :—



There is certainly a symmetry about the formula, and it will be found that 16 of the molecules of water are in a different position from the remaining 8 ; this probably has no significance, although Graham found that crystallized alum at a temperature of 61° lost 18 molecules of water ; if he had used a temperature a few degrees lower he might have found that only 16 passed off !

By a little stretching of the imagination and altering the atomicities of the elements to suit each particular case, no doubt graphic formulæ might be made for all crystalline salts, but they would be perfectly artificial, and not much good is likely to come from the attempt.

I fear we are driven to the conclusion, that, notwithstanding all the progress that has been made in chemical science during the last fifty-eight years, we have not yet reached a method of notation that would have satisfied Dr. Dalton in 1834.

But since that time we have learnt that our formulæ ought to show even more than the number and position of the atoms of a compound ; we should like them to indicate the amount of potential energy residing in a body, and our equations ought to indicate the amount of heat generated by a chemical change. Let us hope that before the next meeting of the British Association in Edinburgh these desirable developments will have been accomplished.

A short time ago I mentioned the word catalysis as being employed to express certain chemical actions which cannot be explained. It is applied to those phenomena which take place in the presence of a body which appears to be entirely unchanged by the action. Happily these catalytic actions are being explained one after another, so that soon the name itself may become obsolete. An example of this action of presence may be given. When a mixture of sulphuric acid and alcohol is heated to a temperature of about 140° to 150°, ether passes over. Now alcohol contains C_2H_6O, and if from two molecules of alcohol one molecule of water is subtracted a molecule of ether results :—$2C_2H_6O = OH_2 + C_4H_{10}O$. As sulphuric acid is known to have a great attraction for water, it is easy to imagine that the acid combines with the water and ether passes off. But it is found that a small quantity of sulphuric acid at the temperature of 140°–150° will transform a very large amount of alcohol into ether and water, much more than can be explained by assuming that the acid has combined with the water. If a mixture of sulphuric acid and alcohol is heated to a temperature of 140°–150°, and alcohol allowed to flow into the liquid, a mixture of ether and water vapours passes over, and after a large quantity of alcohol has been transformed, the amount of sulphuric acid is found to be unaltered. At first glance this seems very difficult to explain, but on further investigation it is found that alcohol and sulphuric acid act one on another to form ethyl-sulphuric or sulphovinic acid :—

$$SO_2Ho_2 + EtHo = SO_2HoEto + OH_2$$

but when ethyl-sulphuric acid is heated with alcohol, ether is formed with the reproduction of sulphuric acid—

$$SO_2HoEto + EtHo = OEt_2 + SO_2Ho_2$$

the sulphuric acid is then able to produce ethyl-sulphuric acid by acting on more alcohol, so a continuous production of ether and water takes place without loss of sulphuric acid. Another well-known action is the combination of oxygen and hydrogen under the influence of spongy platinum. In this case the platinum remains apparently unaltered, and is capable of causing the combination of any quantity of mixed gases. As spongy platinum possesses the power of absorbing large quantities of gases, it is usually said that the molecules of oxygen and hydrogen are so much condensed in the platinum that they are brought within the sphere of one another's attractions, and consequently combine.

Another instance of an action of this kind is afforded by the oxidation of ammonia in the presence of chromic oxide. When ammonic dichromate is heated an evolution of gas occurs, and a residue of chromic oxide is left which bears a striking resemblance to a mixture of black and green tea; when some of this substance is placed on a piece of wire gauze, heated and then supported over a vessel containing a strong solution of ammonia, the oxide glows in a manner similar to the glowing of spongy platinum under the influence of a mixture of hydrogen and air. Under these conditions the chromic oxide facilitates the oxidation of the ammonia, but it becomes changed during the process; instead of having the appearance above described, it acquires a bright green colour. Now, we know that chromium is capable of forming several combinations with oxygen. Is it therefore too much to suppose that the chromium is alternately oxidized by the oxygen of the air, and reduced by the hydrogen of the ammonia, so that, although in the end it has the same composition as at the beginning, nevertheless it has been continuously decomposed and reproduced? Now, may not a similar change take place during the action of spongy platinum on a mixture of hydrogen and oxygen? The alteration of the platinum is very slight, but I believe I have observed a slight modification of the appearance of a fragment of spongy platinum that was kept glowing by a small jet of purified hydrogen for some hours; the gas not being allowed to burn so as to heat the platinum to a very high temperature, the metal appears to be compacted and to be covered by minute spherules of glistening metal. Now, may not the platinum have entered into combination with one or other of the gases and been subsequently reduced? If this is the true explanation, then we have in this case a continuous series of chemical changes and the "catalysis" is explained.

We all know the ease with which oxygen is obtained from potassic chlorate when heated with a small quantity of oxide of manganese; the quantity of peroxide is the same at the end of the process as at the beginning, and it may be used over and over again to assist in the decomposition of fresh potassic chlorate. The oxide of manganese undergoes a molecular alteration; if a crystalline variety is employed, it is found, at the end of the process, to have been transformed into fine powder.

I hope I have proved to the satisfaction of my brother chemists that potassic permanganate is first formed and subsequently decomposed with the reproduction of manganese peroxide.

Oxide of cobalt possesses the remarkable property of decomposing solutions of hypochlorites at moderate temperatures with evolution of oxygen. For some time I have been endeavouring to find the explanation of the change, but hitherto without complete success. At first it seemed probable that an unstable cobaltate, analogous to a ferrate, was formed and decomposed at the temperature of the experiment. In fact oxygen is evolved when chlorine is passed through a boiling solution of sodic hydrate containing ferric hydrate in suspension. But no evidence of the existence of a cobaltate could be found. When a cobaltous salt is added to an alkaline solution of a hypochlorite, a black precipitate is formed which is usually stated to be cobaltic hydrate, Co_2Ho_6, but Vortmann has shown that, when a cobaltous salt is mixed with a solution of iodine in potassic iodide, and the liquid rendered alkaline by sodic hydrate, the precipitate formed at a temperature between 50° and 60° approaches in composition the dioxide of cobalt, CoO_2. He also found that the precipitate lost oxygen at the temperature of boiling water. I have repeated some of his experiments and can quite confirm them, although I have not obtained an oxide containing quite as much oxygen as his richest oxide. The oxides I prepared rapidly effected the decomposition of a solution of sodic hypochlorite, and that without undergoing any loss of oxygen themselves; in fact, in the two experiments made, the cobalt compound contained a little more oxygen after boiling with the hypochlorite.

We have now many instances of the influence which small quantities of substances have upon chemical reactions. These influences may be more common than is generally supposed. The presence of a third body is frequently helpful in the combination of elements with one another: thus dry chlorine will not attack melted sodium or finely divided copper; an electric spark will not cause a dry mixture of carbonic oxide and oxygen to explode; carbon, phosphorus, and sulphur will not unite with dry oxygen, and as chemical science progresses we may find that many well-known actions are conditioned by the presence of minute traces of other matter which have hitherto escaped detection. We all know the profound alterations of the properties of substances by minute traces of impurities; less than one-tenth per cent. of phosphorus will render steel unfit for certain purposes. The sapphire and ruby only differ from colourless alumina by the presence of traces of impurities hardly recognisable by chemical analysis. During this meeting we hope to have a contribution to the section on the influence of minute traces of what may be called impurities on the properties of different substances and their influence on chemical changes.

In this city, where the first public chemical laboratory was started in 1823, by Dr. Anderson, the assistant of Prof. Hope, it is hardly necessary to insist on the extreme importance of teaching chemistry by practical work, but unfortunately, even at the present time, endeavours are made to teach the subject by means of lectures (sometimes without experiments) or by reading. Those who are acquainted with chemistry well know the impossibility (this is hardly too strong a word) of learning the science, especially in the first stages, without actual experiment, by which a practical acquaintance with chemical phenomena is obtained. The attempt to learn chemistry without practical experience reminds one of the well-known story (for the truth of which I will not vouch) of a mathematician who lectured on natural philosophy; he was visiting a foreign laboratory, and stopped before a piece of apparatus and asked what it was: on being told it was an air-pump, he exclaimed, "Dear me! I have lectured on the air-pump for twenty-five years, and this is the first time I have seen one." It is problematical if his students can have derived much advantage from his lectures. Teaching of the kind to which reference has just been made is generally given to candidates for examinations who do not intend to take up chemistry as their chief subject. At the present time chemistry is required for entrance and preliminary examinations from many classes of students. There is no doubt that it is an excellent means of education, teaching a boy to observe and draw conclusions from his observations; but if he makes no observations it is little more than useless cram, the memory might as well be exercised by learning a novel by heart.

This imperfect mode of teaching chemistry arises principally from the difficulty of obtaining properly appointed laboratories in schools, in addition to which the very strong fumes are sometimes disagreeable, making it inconvenient to have them in or near a house, to say nothing of the possible dangers to the clothes and their contents; but there is no help for it, the teaching must be accompanied by experimental demonstration, as was indicated in the Reports on the teaching of chemistry which have been presented to this Association in former years. It must be admitted that examinations do not always discover the best student; many are capable of preparing for examinations with a small knowledge of their subject, others, with a good knowledge, fail from nervousness or other causes, but at the present time examination, though far from perfect, is almost the only means we have of judging the fitness of the candidate. By properly selecting questions the examiner may, to a considerable extent, discourage cram; he should endeavour to find out what the pupils have actually seen, and to make them draw conclusions from facts which they have either themselves observed, or which have been described to them; it is only in this manner that chemistry can be used as a means of mental training.

These remarks do not apply to the education of students intending to make chemistry their profession, who have many opportunities, in the large laboratories of Great Britain and the Continent, of obtaining all the necessary instruction. The Institute of Chemistry, which was founded to improve the status and also the education of professional chemists, requires that its members should have a thoroughly scientific training. Before a candidate for the associateship is admitted to examination, he must bring evidence that he has passed satisfactorily through a systematic course of at least three years' study in the subjects of theoretical and practical chemistry, physics, and elementary mathematics, in some recognized college or school; and before admission to the fellowship he must have passed through three additional years of work in chemistry. It is to be hoped that an example of this kind will ultimately have a good effect in improving the modes of teaching the science in its elementary stages.

There is another class of workers in chemistry who must not be forgotten at the present time, as they have much influence on the life of the world and have been working for ages, but have only recently been recognized. I mean those organisms which are included under the name of microbes. These organisms are capable of producing chemical changes which entirely surpass all the results hitherto obtained by the chemist in his laboratory. That the transformation of sugar into alcohol and carbonic anhydride in the ordinary process of fermentation is due to a living organism, has been known for some years; the important transformation of ammonia into nitrous and nitric acids in the soil has been shown to be due to organisms which have recently been investigated by many chemists; it is possible to transform ammonia into these acids in the laboratory by oxidation under certain conditions and at a high temperature, whereas the organism does the work quite as efficaciously at the common temperature. Other organisms have the power of producing complex organic poisons by the alteration of some of the constituents of the animal body, and the relation of these products to the study of diseases is of the highest possible importance. As we hope to have a discussion on this interesting subject by many eminent authorities, both from the chemical and biological points of view, it will be unnecessary to pursue the subject further, unless it be to urge some of the younger chemists to work at the chemical aspect of bacteriology. They must be prepared for hard work and many disappointments, for the subject is undoubtedly a difficult one.

I cannot conclude this address without reference to the great loss which chemistry has sustained by the death of Prof. A. W. von Hofmann. I had the good fortune to be under him as student and assistant from 1856 until he left this country in 1865; all who worked with him must have been deeply impressed by his capacity for work and his power of inducing work in others. Although perhaps some of us did not appreciate this at the time, yet we feel we owe him a debt of gratitude for his having started us in the right way. The list of papers under his name in the Royal Society Catalogue up the year 1883 is 299, written by himself alone, besides twenty-two joint papers. One of his characteristics which impressed me was his investigation for the purpose of furthering chemical knowledge without any view to practical applications, and I well remember his lecture at the Royal Institution, in 1862, on Mauve and Magenta (which owed so much of their success to his work), in which he produced the original specimen of benzene which had been obtained by Faraday from oil-gas in 1825. He pointed out that Faraday had prepared this substance and investigated its properties without ever supposing that it could have any practical application. The following is the concluding paragraph of the lecture:—

"Need I say any more? The moral of Mauve and Magenta is transparent enough; I read it in your eyes. We understand each other. Whenever in future one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly-discovered compounds, you will not cool his noble ardour by asking him that most terrible of all questions, 'What is its use? Will your compound bleach or dye? Will it shave? May it be used as a substitute for leather?' Let him quietly go on with his work. The dye, the lather, the leather will make their appearance in due time. Let him, I repeat, perform his task. Let him indulge in the pursuit of truth—of truth pure and simple—of truth not for the sake of Mauve, not for the sake of Magenta, let him pursue truth for the sake of truth."

This seems to me the true spirit of the scientific investigator and in many cases the reward consists solely in the consciousness that the investigator has done his duty; in some cases the reward may take a more substantial form, and since the above paragraphs were written I have been informed that Prof. von Hofmann has left a large fortune, the result of the applications of his discoveries in technical chemistry.

NOTES.

WE hope to publish shortly, in the series of "Scientific Worthies," a portrait of Sir Archibald Geikie, whose address as president of the British Association we print to-day. The portrait will be accompanied by a sketch of Sir Archibald's career as a man of science.

THE International Congress of Experimental Psychology began work at University College, Gower Street, on Monday, when an address was delivered by Prof. H. Sidgwick. We propose to give on a future occasion some account of the proceedings.

THE Helvetic Society of the Natural Sciences will hold its seventy-fifth annual meeting at Basel from September 5 to 7. The Basel Society of the Natural Sciences will celebrate its seventy-fifth anniversary at the same time.

MR. J. BRETLAND FARMER, M.A., Fellow of Magdalen College, Oxford, and Demonstrator of Botany in the University, has been appointed Assistant-Professor in Botany at the Royal College of Science, London, as successor to Dr. D. H. Scott, who becomes Honorary Keeper of the Jodrell Laboratory, at the Royal Gardens, Kew.

MR. H. M. BERNARD, M.A., has been elected to the Marshall Scholarship, Royal College of Science, South Kensington, for the ensuing year, in place of Mr. G. Biebner, whose term of office has expired.

MR. J. P. HILL, of the Royal College of Science, South Kensington, and the University of Edinburgh, has been appointed to the Demonstratorship of Biology in the University of Sydney.

MR. SILVA WHITE has, for reasons of health, resigned his office as secretary and editor to the Royal Scottish Geographical Society, a post he has filled since the institution of the society.

WE regret very much to hear of the death of Dr. H. J. Tylden, whose article on "The bearing of pathology upon the doctrine of the transmission of acquired characters" was printed in NATURE last week. At the beginning of last week he died of typhoid fever. Dr. Tylden had been engaged in investigating the etiology of typhoid fever, and there is no doubt that he thus contracted the disease.

TWO eminent men who had been intimately connected with India died last week—Dr. Forbes Watson and Dr. H. W. Bellew. Dr. Bellew was well known as an Oriental linguist and as the author of various works in which he made important contributions to ethnology. Dr. Forbes Watson acted for many years as Reporter on the Products of India and Director of the India Museum. He did much to give the English people a wider and more accurate knowledge both of the races and the material resources of India.

THE death of Dr. Felice Giordano, of Rome, is announced. He was the head of the Geological Survey of Italy and Chief Inspector of Mines.

THE Glasgow and West of Scotland Technical College has issued its calendar for the year 1892-93.

ON July 27 the eruption of Mount Etna, which on the previous day had increased considerably in activity, was again as violent as during the first few days of the outbreak. Rocks and masses of volcanic *débris* were ejected from the crater to a great height, as well as a quantity of fine ash, which fell in showers over the country. The cloud of smoke over the summit increased, and the subterranean rumblings were so loud and frequent as to make the windows in the houses rattle. The lava streams were also extending. Similar reports were issued on the three following days; but on July 31 a general decrease in the volume of the lava was noted. On August 1 it was stated that the eruption seemed to be subsiding. No underground rumblings were heard, the smoke issuing from the crater was white, and the lava streams moved very slowly, and, in fact, almost stopped. On August 2 the volcano showed some signs of renewed activity, and the lava streams began to flow afresh. The underground rumblings were not, however, so loud as before.

SOME information as to the volcanic eruption in Great Sangir is given in letters sent from Menado, the chief Dutch settlement in the north of the Celebes, from which Sangir is about 300 miles distant. The letters are dated June 12, and were printed in the *Handelsblad*, of Amsterdam, on July 27. According to a summary in a Reuter's telegram, the disaster came with appalling suddenness. At ten minutes past six on the evening of June 7, unannounced by the slightest shock of earthquake, subterranean rumblings, or other seismic warning, a terrific eruption began from the great volcano Gunona Awa, which is not far from Tarvena, the capital of the island. Ashes in immense masses and stones of considerable size soon fell all over the island. Hundreds were killed by this shower, and even those who reached the shelter of their homes were not safe, for nearly everywhere in the country districts the light wooden houses collapsed under the weight of the stones and ashes which quickly settled on the roofs. In the immediate vicinity of the mountain, on the slopes of which are numerous farms and villages with extensive plantations, immense destruction was caused by the great streams of lava, which flowed with astonishing rapidity down into the surrounding valleys. Houses were carried away with all their contents, and many of the occupants met a terrible death in these rivers of molten rock. Besides the hundreds who are known to have lost their lives on the lowlands, between five hundred and a thousand more who were engaged in the rice-fields on the mountain slopes have not been heard from. The crops have been destroyed, the cocoa-nut trees have suffered severely, and in many parts of the island the wells have become dry.

AT the time of our last issue the weather was very settled, and the air very dry, scarcely any rain having fallen for some days. On Friday, however, July 29, the anticyclone began to give way, and the low pressure over the Bay of Biscay extended northwards and over the eastern parts of England, causing thunderstorms in the southern counties. By Sunday, the disturbed weather had extended over the whole country, and rain had fallen at most places, but the area of low barometer was passing away to the eastward, and during the early part of this week the type of weather again became anticyclonic generally, but the sky became cloudy, and rain fell in places; while on Wednesday a depression lay over the North of Scotland, which appeared likely to spread southwards. Temperatures have ranged from 70° to 75° and upwards in the southern districts, but have been considerably lower in the north; the daily maxima frequently not reaching 60°. The *Weekly Weather Report* showed that for the week ending July 30 the temperature only slightly exceeded the average in the North of Scotland. Rainfall was much below the mean, amounting to six to nine-tenths of an inch in most districts, while reckoning from the beginning of the year there is a deficit in every district, amounting to as much as 7·4 inches in the south-west of England.

THE Austrian Meteorological Society has issued an appeal for contributions towards the support of the meteorological observatory on the summit of the Sonnblick. The observatory was established by M. Rojacher in 1886, and completed at the expense of the Austrian Society and the German and Austrian Alpine Club; it has since been maintained at the expense of these two institutions, together with a subvention from the Ministry of Instruction, and aided by a small reserve from the original building fund. The recent death of M Rojacher, and the removal of the Alpine Club from a house on the summit, has thrown such additional expense on the Austrian Society as to endanger the efficient maintenance of the Observatory. The station has already rendered good service to science and has somewhat modified the theory of the nature and origin of storms; several physicists have also conducted experiments there on radiation, atmospheric electricity, and other subjects of considerable importance. We hope, therefore, that the appeal of the Society for funds for the efficient maintenance of the station will meet with entire success.

THE trustees of the South African Museum, in their report for the year 1891, record a serious loss in the mineralogical series of the Museum's collection. On the night of September 7 and 8 the Museum was robbed of the Stonestreet collection of rough diamonds, a separate diamond in singularly hard rock, and several very interesting nuggets of South African gold. The exhibition hall was broken into through one of the small upper windows opening on the higher of the two galleries, and the specially protected table-case, containing the diamonds and gold, forced by shattering the lock. Two men—whose names, A. McEwen and E. Cohen respectively, were already too well known in the criminal records—were convicted of the robbery at the Supreme Court session on the 13th November, and sentenced to four years' hard labour. The police succeeded in recovering 49 of the 173 diamonds belonging to the Stonestreet collection, including most of the larger stones, but among the missing majority are many unusual and abnormal crystalline forms of much interest, collected with great pains by the late Mr. Stonestreet, during the earlier years of mining in Griqualand West. The Du Toit's Pan diamond in indurated rock and the gold nuggets have not been recovered.

IN the course of an interesting address delivered lately at the opening of the new chemical laboratory of the Case School of Applied Science, Prof. C. F. Mabery called attention to the fact that notwithstanding America's abundant supply of crude materials, with cheap fuel in unlimited quantities, and a ready market with an increasing demand, she continues to pay enormous sums for imported products which should be produced at home. Prof. Mabery thinks, however, that the outlook for the immediate future is encouraging. In several directions the manufacture of chemical products has begun, and others, he believes, will follow. There are certain lines along which rapid development may evidently soon be expected, and one of the most promising is sal-soda. Until quite recently the Le Blanc process, which was invented in France to manufacture soda-ash when the supply from natural sources was largely cut off during the French Revolution, has supplied the world since early in the present century. In utilizing all bye-products the great Le Blanc works of Europe have been able to produce soda-ash at a trifling cost. A Le Blanc plant has never been established in America, and probably one never will be. Such a plant requires immense capital, and, besides, a combination of coal, salt, and limestone, that can be found close at hand in but few localities. Within a few years another method, known as the ammonia-soda process, has been put into operation in Europe. The first cost of a plant for this process is not large, and since it furnishes a purer product than the Le Blanc method, it will probably supply a considerable portion of the sal-soda of the future, especially in the United States. The newer method has the especial advantage that it forms bicarbonate of soda direct and very pure. Two plants for this process have been erected in America, one of which has been in operation at Syracuse, N.Y., for several years, and the other has recently been erected in Cleveland. As additional illustrations of the possibilities in store for the United States, Prof. Mabery mentioned the manufacture of porcelain, and the production of artificial dyes and colours from coal-tar.

AN interesting report on the pearl fishery of the Gulf of California is contributed by Mr. C. H. Townsend to the new Bulletin of the United States Fish Commission. The season for pearl fishing begins about the first part of May near Cape St. Lucas, whence operations are gradually carried into the Gulf of California, which is usually entered by May 15. During the summer the entire eastern coast of the peninsula is worked, and in October the base of operations is removed from La Paz, the headquarters of the Pearl Shell Company of Lower California, to Acapulco, where the fishery is continued for two or three months longer. Whatever of romance may hitherto have enshrouded the diver for pearls in the sea, he is now, as described by Mr. Townsend, practically a submarine labourer, who uses all the modern diving paraphernalia available. No longer plunging for sixty seconds into the sun-lit green water that covers a coral bank, he puts on a rubber suit with glass-fronted helmet, and, suitably weighted with lead, descends for hours to gather pearl-oysters, which are hoisted in a wire basket by his companions in the boat above, who supply him through a rubber tube with the air he breathes. The best year at the fisheries in comparatively recent times was 1881. During that year many pearls of extraordinary size and great value were obtained; among them was a black one weighing twenty-eight carats, which sold in Paris for 10,000 dollars.

A VALUABLE report on the petroleum trade of the Caucasus has been sent to the Turkish Government by Aassib, the Turkish Consul-General at Tiflis, and some interesting extracts from it are quoted in the *Board of Trade Journal*. The petroleum springs of the peninsula of Apcheron, not far from the place at present occupied by the town of Baku, were known according to the writer, several centuries before the Christian era, and the phenomena produced by them, totally inexplicable in those barbaric ages, gave rise, he says, to the worship of the Guebres, followers of Zoroaster, which lasted into the nineteenth century, for the temple of the worshippers of eternal fire is seen to the present day. The springs of Balakhani are situated 20 kilometres from Baku on a bare and arid plateau, swept by the winds, at an elevation of about 60 metres above the level of the Caspian Sea. The petroleum lands occupy an area of about 8 kilometres. At the present time Balakhani and Sabountchi possess more than 1000 wells, some of them newly bored, producing in twenty-four hours as much as 400,000 pouds. An era was marked in the history of the naphtha industry by the house of M. Nobel, which started at Baku in 1874, and in the following year purchased a small business and undertook the production of petroleum on a small scale. At that time the conveyance of petroleum to Baku was effected by means of carts and leather bottles. M. Nobel endeavoured to show the absurdity of this primitive method of transport, and recommended that pipes should be constructed, but the majority of the merchants rejected the proposal. He then constructed the first pipe at his own cost, and demonstrated the utility of it to his colleagues, several of whom very soon imitated his example, and Baku has to-day a dozen lines of pipes, each of which costs more than 100,000 roubles. The same house, dissatisfied with the system of shipping petroleum in barrels, proposed to the Kavkaz and Mercury Navigation Company of the Caspian and the Volga that they should build tank boats for the exclusive conveyance of petroleum. This proposal having been rejected, the firm constructed several of these vessels at their own expense. This innovation, of which even the Americans had not yet thought, was accepted by the two petroleum-producing countries, and tank boats, the number of which is constantly increasing, are to be found on all the waters of the civilized world. It is also to M. Nobel that those gigantic reservoirs of iron which contain hundreds of thousands of naphtha products are due. They are to be seen in large numbers at Baku, Batoum, and everywhere else where petroleum is carried in bulk. The series of innovations by M. Nobel do not stop there. With a desire to improve land carriage he proposed to the Griazi-Tsaritsine Railway Company the construction of special tank waggons for the transport of the petroleum, guaranteeing a load for them for several years. The railway authorities scoffed at the idea, and it was by the expenditure of very large sums that the Swedish merchant constructed for his own use the first tank waggons. Scorn was immediately changed to enthusiasm, and to-day thousands of these waggons circulate on the railways of Caucasia and Griazi Tsaritsine.

IN Part xxi. of the Zoological Reports of the Norwegian North Atlantic Expedition, Christiania, 1892, Dr. D. C. Danielssen gives an account of the Crinoids and Echinoids of the North Atlantic. Chief among the former is the beautiful *Bathycrinus carpenteri*, first described as *Ilycrinus carpenteri* by Koren and Danielssen in 1877 from specimens collected by the expedition, and thought to be a new genus, but a careful study and comparison with Herbert Carpenter's description in the report of the *Challenger* Crinoids proved it to belong to *Bathycrinus*. The morphology of this species is very fully described and figured; very interesting are the statements about the apparent formation of "new crown" on specimens which had apparently lost their first crowns; in one of these "the stalk was 110 mm. in height, the crown was 2·5 mm. high, and the root was 20 mm. in length. The radials of the crown were attached to the basals by a pretty broad seam, the basals being concreted and forming a firm ring as upon old individuals; which distinctly showed that while the radials were a new formation, the basals pertained to the old detached crown and formed the true calyx from which the new crown issued." In this specimen the tentacles could not be seen, and it was very difficult to observe the disc, as it was covered by the closed arms which could not without damage be separated from each other, but that a new crown was in course of formation seemed indubitable. In addition to this species of Bathycrinus, *Rhizocrinus lofotensis*, and the following species of Antedon were found:—*A. tenella*, Retzius; *A. petasus*, D. and K.; *A. prolixa*, Dun. and Sladen; *A. quadrata*, Carp.; and *A. eschrichti*, Muller. Fourteen species of Echinida are mentioned, of which *Echinus alexandri*, Dan. and Kor., is redescribed and figured.

THE additions to the Zoological Society's Gardens during the past week include a Hainan Gibbon (*Hylobates hainanus*) from Southern China, presented by Mr. Julius Newman; a Humboldt's Lagothrix (*Lagothrix humboldti*) from the Upper Amazons, presented by Mr. Chas. Clifton Decconson, F.Z.S.; a Red Howler (*Mycetes seniculus*) from New Granada, presented by Mr. John F. Chittenden, C.M.Z.S.; a Garnett's Galago (*Galago garnetti*) from East Africa, presented by Commander H. J. Keene, R.N.; a Bennett's Wallaby (*Halmaturus bennettii* ♂) from Tasmania, presented by Lieutenant E. A. Findlay, R.N.R.; a Raccoon (*Procyon lotor*) from North America, presented by Mr. A. C. Cooke; a Short-toed Eagle (*Circaetus gallicus*) from Southern Europe, presented by Mr. B. Vincent; a Leadbeater's Cockatoo (*Cacatua leadbeateri*), a Slender-billed Cockatoo (*Licmetis tenuirostris*) from Australia, presented by Mrs. Phillips; a Rock Thrush (*Monticola saxatilis*), two Solitary Thrushes (*Monticola cyanus*), European; a Common Jay

(*Garrulus glandarius*), an Ortolan Bunting (*Emberiza hortulana*), a Blackbird (*Turdus merula*), a Nightingale (*Daulias luscinia*), British, presented by Mr. E. Cossavella; a Common Jay (*Garrulus glandarius*), a Natterjack Toad (*Bufo calamita*), six Crested Newts (*Molge cristata*), three Palmated Newts (*Molge palmata*), British; three Sand Lizards (*Lacerta agilis*), five Yellow-bellied Toads (*Bombinator bombinus*), an Edible Frog (*Rana esculenta*), European, presented by Mr. G. B. Coleman; four Common Snakes (*Tropidonotus natrix*), British, presented by Count Pavoleri, F.Z.S.; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, a Barbary Wild Sheep (*Ovis tragelaphus* ♂) from North Africa, two Common Squirrels (*Sciurus vulgaris*), British, deposited; two black Apes (*Cynopithecus niger*) from Celebes, purchased.

OUR ASTRONOMICAL COLUMN.

Solar Observations at the R. Osservatorio del Collegio Romano.—Prof. Tacchini, in the *Memorie della Società degli Spettroscopisti Italiani*, gives a tabular statement of the prominences, faculæ, and spots visible on the sun's surface during the first three months of the present year. Taking the case of the number of prominences, no less than 300 were observed during this period, 161 appearng in northern and 139 in southern latitudes. During the first two months prominences were more numerable in the south hemisphere, amounting to an excess of 7 and 5 respectively, but in March as many as 78 were recorded for the northern as against 44 for the southern. The latitudes for the regions of greatest frequency were + 40° + 30° and − 20° − 30°.

For the faculæ 28, 24, and 18 (total 70) were recorded for the northern latitudes, while very nearly the same number (76 = 20 + 18 + 38) was observed on the southern hemisphere. In both cases the record for latitudes ± 50° ± 40° was one, the greatest number appearing in latitudes ± 10° ± 30°.

The total number of groups of spots recorded was 80, of which 38 were observed north of the equator. Curiously enough the month of February only contributed 21 out of this number, 34 being recorded for January; the region of greatest frequency occupied the zones ± 10° ± 30°.

Allowing for the very unfavourable season for observations, a considerable increase over the preceding quarter will be at once noticed. The relative amount of spotted area shows an enormous increase for February, the numbers for the months commencing with January being 79.79, 153.61, and 61·57.

A Remarkable Prominence.—Mr. J. Fényi, in the *Memorie della Società degli Spettroscopisti Italiani*, gives an account of an unusually large prominence that was visible at Kalocsa, on May 5 last. At 10h. 25m., Kalocsa mean time, the prominence was very small, but later it developed very considerably, forming itself into a set of small bands, clearly inclined towards the equator. At 11h. 55m. the observed height was 139″, there being no indication of a rapid ascent. At 12h. 11m. a very rapid upward motion had already begun to make itself visible, and by 12h. 17m. 34s. the height reached was 287″, extending to 317″ 1m. 11s. later, when the velocity of ascent was 306 km. per second. After a few minutes the lower parts to the height of 360″ became invisible, but the smooth portions ascended at 12h. 21m. 4s., with a velocity of 368 km. per second to a height of 531″. This latter measurement was made at 12h. 29m. 25s., and soon after the object was no more seen. The actual height attained, then, may be reckoned about 381,800 km., or 237,126 miles. At the terminaton of this eruption, it was noticed that the prominences at 127° and 79°, and even the one at 106°, which very nearly coincided with the position of the eruption itself, still retained the same forms, having apparently suffered no change by this enormous disturbance; no faculæ or spots either were recorded which could in any way be connected with this outbreak.

The Trapezium in the Orion Nebula.—During the first three months of the present and preceding year Dr. L. Ambronn, of the Göttingen Observatory, has undertaken a measurement of the distances and position angles between the four bright stars forming the trapezium in the great nebula of Orion. The results which he has obtained are recorded in the 3103 number of *Astronomische Nachrichten*.

Commencing with the star θ′ Orionis, which is here designated *a*, and taking the others in cyclic order following the direction opposite to that of the motion of the hands of a watch, we find these designated by *b*, *d*, and *c* respectively. The accompanying table, for the sake of comparison, shows the position angles and distances for the equinox 1870 from the measurements of W. Struve, Dembowski, O. Struve, Hall, and Ambronn.

	W. Struve. 1836·15	Dembowski. 1867·04.	O. Struve. 1870·0.	Hall. 1877·7.	Ambronn. 1891·6.
	° ′	° ′	° ′	° ′	° ′
ab	311 45	311 22	311 32	311 4	311 15
ac	60 29	61 38	60 22	61 8	60 58
ad	340 20	342 23	342 5	342 15	342 31
bc	95 35	96 2	95 36	95 34	95 26
bd	31 48	32 11	31 43	32 55	33 1
cd	299 34	299 33	299 34	299 18	299 15
	″	″	″	″	″
ab	13·002	12·907	13·049	13·116	13·250
ac	13·344	13·385	13·276	13·453	13·698
ad	16·854	16·681	16·876	16·768	16·997
bc	21·414	21·582	21·410	21·758	22·038
bd	8·706	8·706	8·705	8·772	8·915
cd	19·227	19·340	19·237	19·363	19·576

New Variable Stars.—A short note communicated by Prof. Pickering to *Astronomische Nachrichten*, No. 3104, informs us that six new variable stars in the southern sky have been discovered on examination of the photographs of stellar spectra taken at Arequipa in Peru. The following are the constellations, positions, and the dates on which the photographs were taken:—

Constell.	α 1900 h. m.	δ 1900 ° ′	Date.
Horologium	2 49·5	− 50 10	Sept. 10, 1891
Octans	6 0	− 86 30	Sept. 11, 1891
Bootes	14 22·1	+ 5 2	April 26, 1892
Octans	17 30	− 86 45	Aug. 31, 1891
Sagittarius	19 49·8	− 29 27	Oct. 3, 1891
Tucana	23 53·2	− 65 56	Aug. 25, 1891

All these stars when at a maximum are as bright or brighter than the 8th magnitude, but only one, that in Sagittarius, is a catalogue star (Cord. G.C. 27271, Mag. 8½).

THE BRITISH ASSOCIATION COMMITTEE ON ELECTRICAL STANDARDS.

In view of the hoped-for presence of Prof. von Helmholtz and other distinguished foreigners at this year's meeting of the British Association in Edinburgh, it will probably be recognized as suitable to take up and continue the discussion on new electromagnetic units for practical purposes, which was begun last year at Cardiff.

I therefore beg to contribute the following notes and to conclude by moving some resolutions.

One great fact brought into prominence by the practical development of electricity is the analogy or reciprocity between the electric and the magnetic circuit, and this is the fact which it behoves us to emphasize in the naming of fresh units.

The magnetic circuit has as yet no authorized names applied to it. The electric circuit is well provided, but perhaps one or two improvements can be made.

(1) The Electric Circuit.

The first point on which I consider that practical men would do well to insist is that names shall be given to the complete things dealt with, rather than to mere coefficients. Thus of all units with which they are concerned there can be no doubt but that *volt* and *ampere* are the most prominent. These are the active things with which Electrical Engineers have to deal, and these are the things for which meters exist on every wall in an electric lighting station. The ohm, or unit coefficient of resistance, is comparatively academic in character; it is a constant of a coil of wire or of an underground lead, it is nothing vivid and active. The engineering use of the term ohm is mainly in connection with insulation and other high resistances; for large conductors the equivalent "volt per ampere" is perhaps more often used. It is the drop of potential which a given conductor entails for a given current that is of real interest to an engineer, and it is this of which in large leads he consciously thinks.

A 6 ohm conductor means one that drops 6 volts for every ampere that is sent along it. If you send 3 amperes along such a line, the potential at the far end is 18 volts below that at the near end. The clear realization of this fact would be almost aided by the complete title, 6 volts per ampere, instead of the abbreviation, 6 ohms. Nevertheless, the name ohm is in common use and hence may be assumed useful.

A still more useful name, however, for good conductors would really be the reciprocal of an ohm—the ampere per volt. Suppose this called a mo, as Sir W. Thomson once suggested, then a cable of 20 mos would be one that conveyed 20 amperes with a drop of 1 volt. A thousand-mo cable would convey 500 amperes with a drop of half a volt, and so on. It is more directly practical to think of the amperes conveyed per drop of voltage, than of the drop of voltage per ampere. I believe that some authorized name for unit conductance would be welcomed.

Units of Inconvenient Size.

The authorized name "coulomb" for unit quantity is barely used by engineers, who are content with ampere-hour; thus proving that what is needed in practical units is not so much a consistent decimal system, as a set of units each of practicable magnitude.

Farad.

The effort after consistency has resulted in the useless "Farad"; and this should be a lesson not to try and fix units of unreasonable size. The c.g.s. units already exist as a consistent system; the only objection to them is that they are of unpractical size. The whole object of devising a practical system of units was to have things of every-day size to deal with. The volt, the ampere, and the ohm satisfy this condition. The coulomb, the farad, and the watt do not. Already they have practically given place to the ampere-hour, the micro-farad, and the kilowatt.

Considerably more progress would have been made in knowledge of ordinary capacities if the microfarad had been called the farad, so that easy submultiples of it would have been available to express the capacity of Leyden jars, and such like things. The capacity of an ordinary jar would then have been a few millifarads, and a microfarad would have been the capacity of a short bit of connecting wire. I ask whether this change would introduce serious confusion even now. I think not. Nobody cares the least about "coulombs per volt," and so there is no sense or use in the present farad. Telegraphists would surely soon consent to drop the useless prefix micro; and the factor of a million is too great to render doubt possible as to what was intended, even in the transition stage. It ought to be regarded as essential to have the practical unit somewhere not hopelessly away from the middle of the range of probable multiples and submultiples.

Coulomb.

A coulomb again is almost useless as a synonym for the ampere-second; it is so easy to speak of ampere-minutes or ampere-hours. If the name coulomb could be set free from its present superfluous meaning it could usefully be applied to the electrostatic unit of quantity, which wants a name. Teachers would find it convenient at once, and in the apparently imminent line of development engineers might find it useful before long. It is the charge on a two-centimetre sphere at a potential 300 volts (or on a one-foot sphere at 20 volts). The capacity of the two-centimetre sphere would be $\frac{9}{10}$ of a (new) microfarad.

Watt.

Lastly with regard to the watt. The name volt-ampere is almost as good as the name watt, especially since the watt is also one joule per second.

Both names, watt and joule, are not really wanted by electricians, to whom their coexistence is rather confusing. I believe it would be more convenient to use the term watt in the sense it gets so frequently used now, viz., energy, say a volt-ampere-hour; in which case a kilowatt would be synonymous with the present Board of Trade unit.

The rate of working, or power, could then be expressed in a rational and unforced way as so many watts per hour or so many volt-amperes. It is much more natural to give a name to a definite thing like a quantity of energy, than it is to give it to a mere rate of working. The latter is instinctively felt to need a reference to time; just as a velocity unit has not been practically found to need a name, being quite simply expressible in feet per second or miles per hour; and even when a name has been given, like "knot," instinct constrains people to practically get rid of it again by speaking of knots per hour, just as we find "kilowatts per hour" already often used in electrical workshops. I suggest, therefore, that the present watt is too small, that it is sufficiently expressed by a joule per second, and that it would be more useful if magnified 3,600 times, and turned into a unit of energy.

That we should thus have several energy units—the erg, the joule, and the watt, all of quite different sizes, is no objection, but an advantage, seeing the extreme importance of energy. Such things as length, mass, time, and energy demand a fair range of units. It would be tedious to express centuries in seconds.

(2) MAGNETIC CIRCUIT.

In speaking of the magnetic circuit I wish to refer back to my opening remarks concerning the electric circuit, and the class of things for which names should be found. In the magnetic circuit the only thing at present seriously attempted to be named is, in accordance with the historic parallel of the ohm, a coefficient or characteristic of a coil of wire—its coefficient of self-induction; the unit of which has been called variously a secohm, a quadrant, and a henry.

Total Induction.

But the real active thing with which engineers are concerned is total magnetic induction, total number of lines of force across an airgap: as between the polepieces or through the armature of a dynamo, or in the circuit of a transformer. It may be called the electromagnetic momentum per turn of wire; or the surface integral of B. This total induction is in some respects analogous to electric current, and has occasionally been called magnetic current (a bad name), or "magnetic flux." It is, however, more strictly analogous to the coulomb, and its time rate of variation is the more proper representative of electric current.

Its common practical name at present is "total lines," or "total induction," or "number of lines."

Now "one line" is awkward as a unit, besides being (if a c.g.s. line) inconveniently small. The earth, for instance, sends 4,400 such lines through every horizontal square metre about England; through a square inch it only sends a fraction of a line. A practically sized unit of induction badly wants a name, and "henry" would have done for it very well, and have been both more suitable and more useful for the actual quantity than for a coefficient. But "henry" has already been half appropriated to the secohm, so, for illustrative purposes at any rate, I propose to use the name "weber" for the unit magnetic flux.

Concerning the most convenient size for the weber, there is much to be said for making it 10^8 c.g.s. lines, though that is bigger than ordinarily occurs in practice; because then a wire which cuts one weber per second will have a volt difference of potential between its ends. Or a coil of twenty turns through which the magnetic induction changes at the rate of one weber per second will have an E.M.F. of twenty volts induced in it. The average E.M.F. in such a coil, spinning thirty turns a second, and enclosing a maximum total-induction of one weber, is 600 volts.

This is the dynamo use of the unit; the following is the motor use.

A wire carrying an ampere and cutting a weber per second, does work at unit rate, viz., one joule per second.

Probably the simplicity of all this compensates for the rather unwieldy size of the unit. A strongly magnetized piece of iron may have 20,000 lines to the square centimetre; so a weber could occur across a narrow airgap half a square metre in area.

The earth gives an induction of about one weber through every 23,000 square metres of England, or 100 webers per square mile. The earth induction through a horizontal square metre is 44 micro-webers, so with micro- and milli-webers the range would be fairly covered; though a smaller weber would have been better if it had been equally convenient as regards the volt.

The pull between two parallel surfaces joined by a weber is $\frac{10^{16}}{8\pi}$ dynes, or four hundred thousand tons. A milli-weber gives less than half a ton pull; and a micro-weber less than half a gramme.

Because of the property that the voltage excited in a circuit is equal to the webers cut by it per second, a weber might be called a sec-volt. It is equal to a secohm-ampere-turn; that is to say, if a single turn of wire can have a self-induction coefficient of one secohm, it will excite a weber of induction for every ampere passing through it.

[Such a circuit in the form of an anchor ring would be enormous, something like a mile across; but it could be made in the form of a solid cylinder of best iron ($\mu = 2500$), with an axial perforation for the wire, and 80 metres long.

If a secohm coil has n turns, then an ampere passing through it excites only $\frac{1}{n}$th of a weber; for, since every turn encloses the induction, the latter is effective n times over, and so the induction coefficient is n times the induction per ampere, or n^2 times the induction per ampere turn.]

No name is needed for intensity (or density) of induction (B), for that can always be expressed in webers per unit area.

[For instance, strongly magnetized iron, with say 10,000 lines to the square centimetre, has one-tenth of a weber per square foot, or 0·7 milli-webers per square inch.]

And there is a practical gain in thus leaving the specification of area open, for it enables British units of length to be employed in measuring air-gaps, yokes, cores, and polepieces.

So long as dynamo dimensions are commonly expressed in inches, there is no serious objection to specifying induction in fractions of a weber per square inch or per square foot.

Magnetomotive Force.

Now consider the magnetic analogue of the volt; the unit of magnetic potential or magnetomotive force. By this is understood the line integral of the magnetizing force H, the quantity $4\pi n C$, the step of potential once through and all round the circuit of a coil. It is a quantity most important in practice, and requires a name.

Mr. Heaviside has suggested the name "gaussage," as analogous to voltage; and, if this were adopted, the unit of magnetomotive force would be the gauss. The intensity of magnetizing force would be the gauss-gradient, or drop of gaussage per centimetre; no special name is needed for the unit of this quantity H.

The common practical unit of gaussage at present is the ampere-turn, and this has several advantages. It may, however, be found better to make some convenient number of ampere-turns into a gauss; for instance, the c.g.s. unit of gaussage would be $\frac{4\pi}{10}$ or 1·2566 ampere-turns. If that were adopted as the gauss, the horizontal component of the earth's magnetic intensity about here would be, say ·18 gauss per linear centimetre.

But this unit, whether the c.g.s. unit or the ampere-turn, is very small. The step of potential all round a single ampere-turn is only equivalent to a vertical step of about 2 centimetres in the earth's field.

Nevertheless, in spite of its smallness, the ampere-turn as practical unit of gaussage will probably commend itself by reason of its simplicity. Let us see how it works out.

Reluctance.

The ratio of gaussage to the induction excited by it, is a quantity characteristic of the magnetic circuit, and called its reluctance or magnetic resistance. This is the quantity $\frac{l}{A\mu}$ for simple circuits, or $\Sigma \frac{l}{A\mu}$ for complex ones; it is unfortunately not constant for any but air circuits. This constitutes one difficulty of naming its unit satisfactorily, else it might be expressed as so many "gilberts" or "sturgeons" (analogous to ohms). It is, however, fairly constant under many common conditions of practice, and it can always be expressed as gausses per weber; and perhaps this way is sufficient.

A magnetic circuit with unit reluctance is one that requires one gauss to induce in it one weber.

Permeability.

Permeability (μ), analogous to electric conductivity, would be measured by the webers induced through unit cube of the material between whose faces there is unit fall of gaussage. It has been suggested (by Prof. Perry) that the permeability of air had better be called $4\pi \times 10^{-9}$. But the whole electromagnetic system of units is based on the μ for air being called 1; so it would be rather confusing to change that. Moreover, it would be a retrograde step to affix another incorrect value to the constant μ, instead of waiting and trying to find out what its value really is. It is better to adhere for the present to the existing table of permeabilities, and to use whatever constant factor may be needed in order to turn $\frac{l}{\mu A}$ into practical units of reluctance.

Permeance.

But the reciprocal of reluctance, or the webers induced per gauss, may be the more instructive thing to attend to and name; just as conductivity is often more directly interesting than resistance. This reciprocal ratio, $\frac{\mu A}{l}$, has been called "permeance," and that is not a bad name for it; it is proportional to the inductance of a single-looped circuit. Permeability is the permeance of unit cube of the material. Permeance is the webers induced per unit drop of gaussage. Permeability is the webers per unit area induced by unit gauss gradient.

The permeance of the magnetic circuit enclosed by a solenoid of wire is the same as its appropriate self-induction-coefficient divided by 4π times the square of its number of turns.

The c.g.s. unit of permeance (or of reluctance) is that of a centimetre cube of air, and is not a bad-sized unit. But it is inconsistent with the weber as 10^8 and the gauss as a single ampere turn.

One of the three must give way.

On the whole I have no hesitation in suggesting that the derived unit (that of permeance) must give way, and be taken as $4\pi \times 10^7$ c.g.s. units, in order to harmonize with the other two as already defined.

The fact is that the great size of the weber renders a small gauss desirable, in order that their product may not represent too large a quantity of energy. For instance, if 1 c.g.s. unit were taken as the unit of permeance, the weber being fixed at 10^8, then the gauss would also be 10^8, and the gauss-weber would be 10^9 joules, or nearly 300 Board of Trade units; which is far too much.

Whereas if the unit of permeance is fixed high, and the gauss kept small, then the energy corresponding to a gauss-weber may be moderate. Thus with 10^8 c.g.s. as weber, and an ampere-turn as gauss, their product is only $\frac{10^9}{4\pi}$ ergs, or $\frac{100}{4\pi}$ or about 8 joules; which will be useful in energy considerations connected with the heating of transformers.

I therefore propose, in order to retain the ampere-turn as unit of gaussage, that the permeance of a cylinder of material of length l and area A be reckoned as $\frac{\mu A}{l}$ multiplied by $4\pi \times 10^7$, if dimensions of the cylinder are measured in centimetres; μ being its ordinarily tabulated value with air = 1. If dimensions are measured in inches, then the permeance of a cylinder will be $\frac{\mu A}{l}$ multiplied by $\frac{4\pi}{2{\cdot}54} \times 10^7$, that is by about $\frac{1}{2}$ 10^8.

The unit of permeance thus suggested is immensely big, and it requires a name of which easy sub-multiples could be formed. A slab of iron 1 centimetre thick, and with its $\mu = 2500$, would need an area of 5 square metres in order to have unit permeance; but a micro-unit would be possessed by an air-gap a millimetre thick and less than a decimetre square.

PROPOSED RESOLUTIONS.

(1) That the unnecessary prefix "micro" be dropped before the word farad, and that the farad be defined afresh as 10^{-15} c.g.s. electromagnetic units of capacity.

(2) That the name "mo" for the unit of conductance or the ampere per volt, be recognized and adopted. (Every *mo* in a cable enables it to carry an ampere with a drop of 1 volt.)

(3) That the ampere-hour be recognized as a convenient practical unit of electrical quantity.

(4) That the volt-ampere-hour be recognized as a convenient

practical unit of electrical energy, and be called the watt. (It equals 2640 foot-pounds, or a trifle over a foot-ton.)

(5) That the present Board of Trade unit be called a kilowatt.

(6) That the ordinary unit of power be a kilowatt per hour [It equals about 4/3rds of a horse-power, more accurately $\frac{1000}{746}$ HP.]

(7) That it is convenient to retain the name joule in its present sense of a volt-coulomb, or ten million ergs, for use in the science of heat; since heat-capacities are conveniently expressed in joules per degree; and it will be handy to remember that a volt-ampere generates one joule of heat per second.

(8) That the name coulomb be affixed to the electrostatic unit of quantity [for academic purposes].

(9) That a name be given to unit magnetic flux or total induction, and that the name weber is suitable.

(10) That the most convenient size for the weber is 10^8 c.g.s. units or "lines" (since the rate of change of this through a circuit is equal to the induced voltage).

(11) That a name be given to unit magnetic potential or magnetomotive force, and that the name gauss is suitable.

(12) That the handiest size for the gauss is one ampere-turn.

(13) That a name be given to the ratio of the weber to the gauss, or unit of permeance, or self-induction per turn of wire. [If the above resolutions were adopted, this unit would be $4\pi \times 10^7$ c.g.s. units, or $\frac{1}{8}$ secohm per turn.]

(14) That intensities of field be expressed in gausses per unit length, and densities of induction in webers per unit area (leaving the length or area unit open for practical convenience to arrange).

No doubt many of these recommendations have been made before. Mr. Preece has often urged the change of farad, so that I hope there will be no difficulty about that.

I find that my magnetic suggestions are very similar to those suggested by Prof. Perry in his modified letter to the Committee as published in the *Electrician*, vol. xxvii. p. 355 [July 31, 1891], and received there with approving editorial comments. The accordance between our suggestions is satisfactory, and makes it likely that they are such as engineers may be satisfied with and be willing to adopt. I need hardly say that I lay no stress upon the particular *names* here proposed. In choosing them I have been influenced by such trivial considerations as the selection of a monosyllable to correspond with volt, and a dissyllable to correspond with ampere or coulomb.

[With regard to Prof. Perry's footnote concerning college instruction and use of c.g.s. units, I suppose systems of teaching differ, but a senior student *ought* to be taught to deal with concrete quantities in so familiar a manner that no possible admixture of units can be any puzzle to him, nor involve anything worse than a little tiresome arithmetic.]

Mechanical Units.

There are several quantities in dynamics beside the joule and the watt for which brief names would be advantageous. I do not propose to discuss these fully now, but the present opportunity might be utilized by agreeing to at least one unit, that of pressure, viz., the "atmosphere"; which might be defined as 10^6 c.g.s., or dynes per square centimetre, and stated to be equal to the pressure of a column of mercury 75 centimetres high at a specified temperature. The inconvenient pressure, 76 centims., might be spoken of as a Regnault atmosphere. I believe that a smaller unit of pressure, for instance, the micro-atmosphere or "barad," might also be usefully named. These pressure units will be useful for expressing energies per unit volume also, and the "barad," or whatever other name is decided on for the erg per cubic centimetre, is of reasonable magnitude for many purposes.

Oliver J. Lodge.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE annual summer meeting of the Institution of Mechanical Engineers, held last week at Portsmouth, was a successful gathering in regard to numbers present and the attendance at the excursions; but the business part of the meeting, which consists of the sittings at which papers are read, was of a rather tame character. The following is a list of the papers on the agenda:—

On Shipbuilding in Portsmouth Dockyard, by Mr. William H. White, C.B., F.R.S., Director of Naval Construction and Assistant Controller of the Navy.

On the Applications of Electricity in the Royal Dockyards and Navy, by Mr. Henry E. Deadman, Chief Constructor, Portsmouth.

Description of the Lifting and Hauling Appliances in Portsmouth Dockyard, by Mr. John T. Corner, R.N., Chief Engineer, Portsmouth.

Description of the New Royal Pier at Southampton, by James Lemon, J.P., Mayor of Southampton.

Description of the Portsmouth Sewage Outfall Works, by Sir Frederick Bramwell, Bart., D.C.L., LL.D., F.R.S., Past-President.

Description of the New Floating Bridge between Portsmouth and Gosport, by Mr. H. Graham Harris, of London.

Description of the Southampton Sewage Precipitation Works and Refuse Destructor, by Mr. William B. G. Bennett, Borough Engineer and Surveyor.

Description of the Experimental Apparatus and Shaping Machine for Ship Models at the Admiralty Experiment Works, Haslar, by Mr. R. Edmund Froude, of Haslar.

Description of the Pumping Engines and Water Softening Machinery at the Southampton Water Works, by Mr. William Matthews, Waterworks Engineer.

Mr. Matthews' paper was adjourned, and that by Mr. Froude was not read, as time ran short. This was much to be regretted, as the Haslar experimental works are one of the most interesting of all our establishments set apart for scientific investigation. It is to be hoped, now Mr. Froude has broken the ice, that he will contribute a fairly complete descriptive paper to the Institution of Naval Architects, where he would naturally find a more appreciative audience than amongst the members of a society devoted more exclusively to mechanical engineering. Although there was not time for the reading of the paper, Mr. Froude very good-naturedly stopped and explained to some of those present the working of the apparatus which he had brought for the purpose of exhibition, together with the large wall diagrams that had been prepared expressly for illustrating the paper.

On the members assembling in the Town Hall on July 26, Dr. Anderson, the President, occupied the chair, and the usual formal business having been disposed of, Mr. White's paper was read. This was chiefly of a historical character, the author going back to the year 1212, when the sheriff of the county of Southampton was ordered to enclose the King's Dock by a strong wall, and to provide suitable storehouses. A dockyard, properly so called, was not, however, founded until the reign of Henry VIII., so it was second in point of antiquity to Woolwich Dockyard. The latter was closed in 1869, "so that Portsmouth Yard is now," Mr. White says, "the oldest as well as the most important in existence." We do not know whether Mr. White means by this that it is the oldest in existence in Great Britain, or in the whole world. In 1540 the total area was 8 acres. Until nearly the end of last century there was no basin in which ships could lie while completing or repairing, and the dock accommodation was poor, but about that time a basin of 2½ acres and six dry docks were constructed. At that time the yard area was 90 acres. In 1843-50 a steam factory was added, and another basin of 7 acres, besides four docks; the total area of the dockyard being 115 acres. The effect of steam on the navy is well illustrated by the extensions that took place about 1864, when the area of the Dockyard was more than doubled. A fitting-out basin of 14 acres, a rigging basin of the same size, and a repairing basin of 22 acres, were made. There is also a tidal basin of 10 acres. The extent of Portsmouth Dockyard is now nearly 300 acres.

Mr. Deadman's paper was also largely of an historical nature, giving many interesting details of the introduction of electricity into the navy. Among the most notable features in the application of electricity to naval purposes are the temporary installations used for interior lighting during the building and finishing of the vessel. The estimated cost of electric lighting during the period of building the *Royal Arthur* was £1200. This would be about the same sum as would be required were candles used, but naturally electricity affords a far superior light, and it is to its use that is due much of the quickness with which the

Royal Sovereign was finished. There was nothing very startling in Mr. Deadman's paper, which was none the less a useful record of facts. During the discussion, however, Mr. Crompton sounded a very stirring note. He roundly told the whole body of important dockyard officials and Admiralty officers present, including even the Director of Naval Construction, that they were altogether behind the age in the matter of electricity, that the French and German navies were far ahead of them, to say nothing of other powers, and that generally the English Government was the most benighted and non-progressive Government in all the world, so far as the matter of electricity was concerned; for they paid twice as much as they ought to do for an article that was not half as good as it should be. That was the purport of Mr. Crompton's speech, if not the exact words he used, and one cannot but acknowledge that he did not speak altogether without a text. It is hard to fully account for the want of enterprise in the Royal Navy, but there is one point to which we might draw attention. The paper read at the meeting was by a naval constructor, and electricity is, we understand, within the Constructor's department. Now electrical engineering is essentially an engineering question, and its consideration requires engineering knowledge and ability of a very high order. In the early days nothing kept electric lighting back more than the bad engineering that was associated with it; and thus it will always be so long as engineers are not employed in carrying out the plans which are founded on the researches of those more highly scientific investigators, upon whose experiments and deductions the practical applications are founded.

The next paper read was Mr. Corner's contribution, in which he described the lighting and hauling apparatus used at Portsmouth. These may be divided into the hydraulic installation, the compressed-air appliances, and the ordinary steam cranes. There are in the dockyard ninety-six boilers, which burn about 10,000 tons of coal per annum, but what proportion of this is used for lifting and hauling we do not know. In the hydraulic department there are nearly two miles of pressure pipes varying from 1½″ to 4″ in diameter. There are also some independent installations, as well as the coaling arrangements for the fleet at coaling point. There are here ten 30 cwt. cranes, and three 10 ton tips, with necessary capstan weigh-bridges. The more modern lifting and hauling appliances are by compressed air, the air being compressed to 60 lbs. With this pressure there is little or no trouble with frost, only a little forming at the exhaust in very damp weather, and altogether the pneumatic system seems to be preferred to the hydraulic. It must be remembered that the power required is variable, and this of course brings the advantage of the pneumatic system, in the matter of working expansively, to the fore. We understood Mr. Corner to say, during the discussion, that when the hydraulic motors and the air-engines were both worked at their full power the water system was the most economical, but working linked up, under the prevailing conditions, the air system was the best. The condensation of steam in the pipes is the objection to the steam motor when situated at some distance from the boiler, otherwise steam would be the best vehicle. The other papers read do not call for any special notice at our hands, their titles giving a sufficient indication of their scope, and there being no features of especial novelty in the matters they described.

A number of excursions had been arranged, and were carried out in a very satisfactory manner. On the first day, Tuesday, the 26th ult., the members visited the Dockyard, and were welcomed by the Admiral Superintendent in person. On Wednesday the Portsmouth Sewage Works were visited, and a trip was made to the Clarence Victualling Yard at Gosport. On Thursday a trip was made to Southampton, where the Docks were inspected, and a visit was paid to the Union Steamship Company's new engineering shops. There was an alternative visit to the Ordnance Survey Office. In the afternoon a visit was paid to the London and South Western Railway Company's new carriage and wagon shops at Eastleigh. Friday was devoted wholly to frivolity, the only item on the programme being a steamer trip round the Isle of Wight. On Saturday a good many of the members went to Brighton to visit the locomotive works of the London, Brighton and South Coast Railway. Largely owing to the exceptionally fine weather the meeting was a great success, and, for pleasantness, may rank with the Dublin meeting of three or four years back.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The fifth summer meeting of Oxford University students commenced on July 29, and will continue till August 27. The general outline of the programme has already been noticed in these columns, but we may notice here that the popularity which has attended these gatherings shows no signs of diminishing. It was announced by the Provost of Queen's College, who presided at the inaugural lecture given by Mr. John Addington Symonds, that upwards of 1250 students had come to attend the lectures it was proposed to deliver. In welcoming the students to the meeting, Dr. Magrath remarked that last winter 60,000 students (including 10,000 artisans) regularly attended the extension lectures of the various universities engaged in the work. There had been 312 courses of Oxford lectures. He also commended the co-operative societies of the North, and particularly the Co-operative Union, and mentioned the individual liberality of Mr. Dixon Galpin, who had founded scholarships for students from Dorset to attend this summer meeting. The munificence of Mr. Galpin had been supplemented by the Dorset County Council. A University Extension College had been recently established at Reading, under the presidency of Mr. MacKinder, an example which he hoped would be followed at other centres.

On Monday a conference was held in the Union Debating-room, under the presidency of Mr. J. G. Talbot, M.P., to consider the relations between the County Councils and the University extension movement. The president invited the lecturers under various County Councils to express their opinion as to the advantages, prospects, and difficulties which they had met or encountered in the course of their peripatetic teaching. His own opinion was that one very successful result of these lectures would be the amalgamation of the classes and the masses, and he noticed that one of the candidates to whom a County Council had awarded a scholarship was in the position of an agricultural labourer.

Mr. Hall, who had been a University Lecturer under the Surrey County Council, cautioned the meeting against entertaining any exaggerated views of the actual information that he had been able to convey to the agricultural labourers. He himself was satisfied if he could awaken a desire for knowledge in the rural mind and convince the extremely conservative agriculturist that he had something to learn.

Mr. Sells, of the Yorkshire College, Leeds, described the activity of that portion of the Victoria University, and believed that in the North they were in advance of the Oxford movement in meeting the actual and practical wants of the labouring section of the community. Coal-mining was taken up by them with eagerness, and the agricultural lecturers carried about with them the actual implements of husbandry in order to bring the matter practically before their audience. The discussion was continued by Mr. Sadler, secretary to the Delegacy, who said that alliances had been entered into with twelve large counties in the past year, and they should be proud of the achievement. In his opinion they ought to give a stimulus to learning to the masses, and for this reason they ought also to combine with the elementary teachers. Help should also be given to individuals, and it was necessary to secure the services of good men, by enabling the scheme to compete with other professions in the matter of the remuneration offered.

Mr. MacKinder (University Extension Lecturer) and Dr. Magrath agreed in deprecating any fixed cut and dried plan for the whole country, but thought that the scheme should be varied to meet the different circumstances of the various County Councils. At the same time, each County Council should have a definite policy.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for March 1892, contains:—On a new branchiate Oligochæte (*Branchiura sowerbyi*), by Frank E. Beddard, M.A. (plate xix.). This annelid, found in mud from the "*Victoria regia* tank" in the Royal Botanical Gardens, Regent's Park, London, is remarkable for the unusual contractility of its body, which suggested a leech or flat worm rather than a Chætopod. It consists of about 120 segments. When magnified the orange-coloured digestive tract traversed by the bright blood vessels is seen, and at the posterior end of the body there is a series of delicate dorsal and ventral processes; these latter are segmentally arranged, developed in pairs upon the last sixty segments or so of the body. There is no connection between the setæ and these processes, as in Bourne's Chætobranchus, also found in the same tank. This worm is referred to the Tubificidæ, without having any certain affinities to any of the known genera.—On the formation of the germ-layers in *Crangon vulgaris*, by W. F. R. Weldon, M.A. (plates xx. to xxii.). The author's conception of the early development differs widely from that of Kingsley.—On the pigment cells of the retina, by I. S. Boden and F. C. Sprawson. The retinal pigment cells are not, as usually represented, invariably hexagonal; in specimens taken from the eyes of sheep, ox, rabbit, kitten, pig, hen, and frog, while hexagonal cells were the most numerous, heptagonal cells were frequently found and scattered at intervals. Cells with four, five, eight, nine, ten, and eleven sides were found.—Observations upon the development of the segmentation cavity, the archenteron, the germinal layers, and the amnion in mammals, by Dr. Arthur Robinson (Plates xxiii. to xxvii.). There is a general description of the development of the ova of the rat and mouse up to the period of the completion of the blastodermic vesicle, and a comparison with the results obtained by Fraser, Duval, and Selenka: there is a description of the formation of the mesoblast and of the chorda dorsalis, followed by a comparison of the ova of the rat and mouse with the ova of other mammals and the lower vertebrates and by a description of the formation of the amnion and a discussion of the relation of amnion formation to "inversion," and by a description of the formation of the cœlom.

June.—Contains:—On the primitive segmentation of the vertebrate brain, by Bertram H. Waters, B.A. (Plate xxviii.); concludes that the fore-brain is composed of at least two well-marked neuromeres, possibly of three; that the mid-brain consists of two neuromeres, from which there is every reason to think that the third and fourth nerves take their origin, and hence these deserve to be recognized as segmental structures; and that the hind brain consists of six neuromeres. On the oscula and anatomy of *Leucosolenia clathrus*, O.S., by E. A. Minchin, B.A. (Plate xxix.). In this sponge, in the fresh and healthy condition, not only are there oscula, "but in the full-sized specimens larger oscula than in any other Leucosolenia known to me, whether from pictures or in the flesh." These oscula are provided with a sphincter, and can be so tightly closed as to escape notice. Hœckel's four varieties of the sponge are only different states of contraction.—Researches into the embryology of the Oligochæta, No. 1: on certain points in the development of *Acanthodrilus multiporus*, by Frank E. Beddard, M.A. (Plates xxx. and xxxi.).—On the Innervation of the Cerata of some Nudibranchiata, by Dr. W. A. Herdman and J. A. Clutt (Plates xxxii. to xxxiv.). If the cerata of Nudibranchs cannot all be said to be true epipodia innervated by the pedals, it would seem equally impossible to regard them in all cases as pallial outgrowths supplied by the pleural ganglia. It is possible that they may have been epipodial in origin, although there be now, in some, a connection with pleural nerves.—Notes on Elasmobranch development, by Adam Sedgwick, M.A. (Plate xxxv.). On the paired nephridia of Prosobranchs, the homologies of the only remaining nephridium of most Prosobranchs, and the relations of the nephridia to the gonad and the genital duct, by Dr. R. v. Erlanger (Plates xxxvi. and xxxvii.).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"The Physiological Action of the Nitrites of the Paraffin Series considered in connection with their Chemical Constitution. Part II. Action of the Nitrites on Muscular Tissue and Discussion of Results." By J. Theodore Cash, M.D., F.R.S., Professor of Materia Medica in the University of Aberdeen, and Wyndham R. Dunstan, M.A., Professor of Chemistry to the Pharmaceutical Society of Great Britain.

Continuing the examination of the physiological action of various pure organic nitrites of the paraffin series (Part I.; Roy. Soc. Proc., 1891), the authors have studied their effect on striated muscular tissue. When the vapours of these nitrites come into contact with the muscle a paralysant effect is observed. All the experiments were made with the triceps and gastrocnemius of *Rana temporaria*. The muscle was contained in a specially constructed air-tight chamber. A very extensive series of experiments was necessary in order to obtain reliable contrasts. The amounts of the nitrites employed varied between $\frac{1}{30}$ and $\frac{1}{400}$ c.c.

Several series of concordant results have thus been obtained which lead to two different orders of activity, viz. (1) with reference to the extent to which equal quantities of nitrites shorten the resting muscle, and (2) with reference to the rapidity with which the shortening is produced. The order of activity of the nitrites as regards the extent of the shortening they induce is as follows:—(i.) Iso-butyl, (ii.) tertiary amyl, (iii.) secondary butyl, (iv.) secondary propyl, (v.) propyl, (vi.) tertiary butyl, (vii.) butyl, (viii.) α-amyl, (ix.) β-amyl, (x.) ethyl, (xi.) methyl. The order representing the speed with which shortening occurs is (i.) methyl, (ii.) ethyl, (iii.) secondary propyl, (iv.) tertiary amyl, (v.) primary propyl, (vi.) tertiary butyl, (vii.) secondary butyl, (viii.) α-amyl, (ix.) β-amyl, (x.) primary butyl, (xi.) isobutyl.

The effect of these nutrites in interfering with the active contraction of a stimulated muscle has also been studied, and it has been ascertained that very minute doses, insufficient to cause passive contraction, interfere in a marked degree with the active contraction, and cause the muscle to fail in responding to stimulation, whilst the companion muscle, contained in a closed chamber free from nitrite vapour, still responded to stimulation.

The remainder of the paper is devoted to a discussion of the connection between the various phases of physiological action and the chemical constitution of the nitrites which gave rise to them. The principal conclusions which have been arrived at are briefly as follows:—The physiological action of these nitrites is not solely, and in some cases not even mainly, dependent on the amount of nitroxyl (NO_2) they contain.

In respect of all phases of physiological activity, the secondary and tertiary nitrites are more powerful than the corresponding primary compounds. This is to be chiefly attributed not to the direct physiological action of the secondary and tertiary groups, but to the great facility with which these compounds suffer decomposition mainly into the alcohol and nitrous acid. In respect of the acceleration of the pulse, the power of the nitrites is directly as their molecular weight, and inversely as the quantity of nitroxyl they contain. They, therefore, fall into an order of physiological activity which is identical with that in which they stand in the homologous series. This same relationship holds, though less uniformly, in their power of reducing blood-pressure, and of inducing muscular contraction.

This order appears to be the result not so much of the direct physiological influence of the substituted methyl groups as of the increased chemical instability which their presence confers on the higher members of the series. In respect of the duration of subnormal pressure, as well as of the rapidity with which muscular contraction ensues, the activity of the nitrites is expressed by an order which is for the most part the reverse of that representing their power in accelerating the pulse, reducing blood-pressure, and contracting muscular fibre, this order being in general contrary to that of the homologous series. In these respects the more volatile nitrites of low molecular weight which contain relatively more nitroxyl are the most active. It appears probable that these simpler nitrites more readily attach themselves to certain constituents of blood and muscle, and thus act more quickly than the higher compounds, whilst their greater stability causes their effects, *i.e.*, reduction of blood-pressure, &c., to endure for a greater length of time than that of the higher and more easily decomposed bodies.

A large proportion of an organic nitrite is changed into nitrate in its passage through the organism, and is excreted as an alkali nitrate in the urine.

The results which have been gained by this research have an important bearing on the therapeutic employment of the nitrites. It is proposed elsewhere to consider what the outcome of this investigation is for practical medicine.

PARIS.

Academy of Sciences, July 25.—M. d'Abbadie in the chair.—Some new observations on the employment of the calorimetric shell, by M. Berthelot. Different bodies must be treated differently, according as they are fixed, volatile, or gaseous. For fixed compounds, solid or liquid, the ratio between the weight of the combustible and the weight of oxygen ought to be such that the gas which remains after combustion contains at least 60 per cent. of free oxygen; otherwise some half-burnt gases will remain in the vessel, notably carbonic oxide. Excess of oxygen, especially if under a pressure of 25 atmospheres, ensures that the temperature of the centre of combustion should remain as high as possible. In the case of gases the oxygen should only be in very slight excess, and should be introduced by tenths of an atmosphere, until the most favourable pressure is reached. Volatile bodies should, if possible, be burnt in the liquid state.—Study of boron trisulphide, by M. Henri Moissan. Five new methods of obtaining this body are described: by the action of fused sulphur on boron iodide; by burning boron in sulphur vapour at 610°; by the action of hydrogen sulphide on pure boron; by the action of carbon bisulphide on boron; and by the action of the sulphides of arsenic, antimony, and tin upon boron. The substance thus obtained shows several remarkable properties.—Researches on the chemical constitution of the peptones, by M. P. Schutzenberger.—On two ruminants of the Neolithic epoch of Algeria, by M. A. Pomel.—The two candidates selected for the Directorship of the Paris Observatory were M. Tisserand and M. Loewy.—*Résumé* of solar observations made at the Royal Observatory of the Roman College during the second quarter of 1892. A letter from M. P. Tacchini to the President. The spots, faculæ, and prominences observed show a considerable increase since last quarter.—Sun observations made at the Lyons Observatory (Brunner equatorial) during the first half of 1892, by M. Em. Marchand. 125 groups of sunspots have been counted, as against 101 in the previous half-year. The southern hemisphere, which used to contain less spots, has lately shown nearly as many as the northern. The latitude of the groups continues to diminish.—New results with regard to hydrogen, obtained by the spectroscopic study of the sun. Similarity with the new star in the Charioteer, by M. Deslandres. In addition to the nine ultra-violet lines of hydrogen already known, five more have been photographed in the spectrum of a very brilliant prominence, extending up to the oscillation frequency 271,700. They correspond very closely with the frequencies calculated from Balmer's harmonic series. The interest of the discovery is augmented by the circumstance that the spectrum obtained shows a great similarity with that of the new star in the Charioteer.—On the velocity of propagation of the electromagnetic undulations in insulating media, and on Maxwell's relation, by M. R. Blondlot. Given an oscillator, the wave-length which it is susceptible of emitting remains the same, whatever may be the insulating medium in which the experiment is made.—On the heat of formation of permolybdic acid and the permolybdates, by M. E. Péchard.—On crystallized phosphide of mercury, by M. Granger.—On the mineralizing action of ammonium sulphate, by M. T. Klobb.—Micrographic analysis of the alloys, by M. Georges Guillemin.—On homopyrocatechine, and two derived nitrides of homopyrocatechine, by M. H. Cousin.—On a new class of combinations, the metallic nitrides, and on the properties of nitrogen peroxide, by MM. Paul Sabatier and J. B. Senderens.—The specific heat of the atoms and their mechanical constitution, by M. G. Hinrichs. On monopropyl urea and dissymmetrical dipropyl urea, by M. F. Chancel.—On the composition of fossil bones, and the variation in their percentage of fluorine during the various geological periods, by M. Adolphe Carnot.—Distribution and state of the iron in barley, by M. P. Petit.—On the comparative number of nerve fibres of cerebral origin serving as motor nerves for the upper and lower limbs of man respectively, by MM. Paul Blocq and M. J. Onanoff.—On the comparative toxic effects of the metals of the alkalies and of the alkaline earths, by M. Paul Binet.—Experimental regeneration of the sporogenic property of the *Bacillus anthracis*, previously deprived of it by heat, by M. C. Phisalix.—Excretion in the pulmonate gasteropods, by M. L. Cuénot.—On a colourless globuline which possesses a respiratory property, by M. A. B. Griffiths.—On the constitution of the cystoliths and of membranes encrusted with carbonate of lime, by M. Louis Mangin.—On a fresh-water perforating alga, by MM. J. Huber and F. Jadin.—On the causes of the catastrophe of St. Gervais (Haute-Savoie) on July 12, 1892, by MM. J. Vallot and A. Delebecque.—Contribution to the improvement of cultivated plants, by M. Schribaux.—The solar period and the last volcanic eruptions, by M. Ch. V. Zenger.

BERLIN.

Physiological Society, July 8.—Prof. Munck, President, in the chair.—Dr. Dessoir spoke on the sense of temperature regarded from the anatomical, psychological, and physiological, point of view. He did not believe in the existence of separate senses for heat and cold since he had failed to obtain sensations of heat and cold by either mechanical or electrical stimulation of certain points of the skin. The temperature sense is localized, since portions of the body-surface can be found which are quite insensitive. The above communication was followed by a lengthy discussion.

July 22.—Prof. Munck, President, in the chair.—Prof. Zuntz had long ago observed that strong muscular exertion has a different effect on the alkalinity of the blood of carnivora as compared with herbivora; thus in dogs the power of their blood to absorb carbon dioxide was practically unaltered by exercise, whereas in rabbits it was considerably lessened. This point had recently been reinvestigated in the speaker's laboratory by Dr. Cohnstein, who found that the blood of a dog at hard work on a treadmill showed no alteration of alkalinity. The result was unaffected by diet, since it was the same when the dog was fed with meat alone, or with rice and fat. During very prolonged exertion the blood was finally found to possess an increased alkalinity. Dr. Lilienfeld had recently discovered Prof. Kossel's "histon" in the leucocytes of blood, united to neuclein as "nucleo-histon." Histon prevents the clotting of blood, whereas nuclein promotes the formation of fibrin. These two facts were regarded as explaining the various phenomena connected with blood clotting. Thus the blood is fluid in the blood vessels because nucleo-histon is retained by the leucocytes. On the other hand, when the blood is shed some of the leucocytes or platelets die, whereupon the nucleo-histon escapes into the plasma, is decomposed by the calcium salts there present into nuclein and histon, and the former (nuclein) then causes clotting. These facts also explain the action of calcium salts in promoting clotting. Prof. Zuntz stated that, according to his researches, a taste-sensation, as of something sweet, is very markedly increased when some other stimulus is simultaneously applied to the organ of taste, even when the stimulus is too weak to alone produce any sensation. Thus, for example, a solution of sugar tastes more sweet if it is mixed with some solution of common salt so weak that it excites no saline taste. The same result was obtained by the addition of a solution of quinine, also too weak to itself give rise to any sensation of taste.

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