THURSDAY, JUNE 2, 1892.

THE GRAMMAR OF SCIENCE.

The Grammar of Science. By Karl Pearson, M.A., Sir Thomas Gresham's Professor of Geometry. "The Contemporary Science Series." (London: Walter Scott, 1892.)

ONE chief merit of this book is its exposition of the meaning of scientific law. There still exists, unfortunately, a type of mind which delights in such phrases as "the reign of law," the "immutable laws of Nature," and so on. The truly scientific mind has, however, been long familiar with the truth that a so-called law of Nature is simply a convenient formula for the co-ordination of a certain range of phenomena. It is this which Prof. Pearson so emphatically, if somewhat redundantly, expounds in the earlier chapters of the "Grammar." As he delights in putting it, a scientific law is a description in mental shorthand of certain sequences of sense-impressions. Through these sense-impressions alone can we gain any knowledge of what we are accustomed to call the external world. Thus the Universe as pictured by the scientific mind is a purely mental product. We can assert, scientifically, nothing regarding its constitution other than what we may validly infer from our perceptions and the conceptions based on these; and even then we must never forget that the reality to us is conditioned wholly by our powers of perception. This is the grand argument of the grammarian of science.

In developing his theme he introduces not a few interesting questions and analogies. Take, for example, his comparison of the brain to a telephone exchange. Here Reason, presiding as clerk, finds by experience that a certain subscriber always desires to correspond with a certain other subscriber. As soon as the call-bell from the former sounds, the clerk mechanically links him to the latter. "This corresponds to an habitual exertion following unconsciously on a sense-impression." Other analogies are obvious. Now, just as the clerk would obtain a very scrappy knowledge of the outside world if he had to trust simply to the messages which stream past him through the exchange, so (it is suggested) the picture our mind forms of the external world acting upon us through our sense-impressions may be very wide of the reality. Of course analogies must not be pressed too far. Yet it does seem that this analogy of the telephone exchange could be worked out most consistently by the despised teleologist. To Sir Thomas Gresham's Professor of Geometry, however, a telephone exchange evolving its own clerk is as simple a matter as an uninterrupted stream of sense-impressions creating Spencerianly a consciousness.

But Prof. Pearson is no mere preacher of familiar doctrines. He is a second Hercules, self-appointed to clear the scientific stables of all materialistic and metaphysical rubbish. He labours at the task of proving how illogical is the mind that passes to "the beyond" of the sense-impressions and the conceptions directly based on these. Thus he argues that, because the Universe is known only as our own mental product, we have no right to infer a mind in or above Nature as an explanation of the universality of the scientific law. Nevertheless, it behoves him to find a rational substitute for the law of continuity on which the authors of the Unseen Universe build their edifice. Consequently on p. 121 we read:—

"It is therefore not surprising that normal human beings perceive the same world of phenomena, and reflect upon it in much the same manner."

Why not surprising? Because, as we learn from the preceding sentence, human beings "in the normal civilized condition have perceptions and reflective faculties nearly akin." But why nearly akin? Well, it has to be so because "the world of phenomena must be practically the same for all normal human beings," or the universality of scientific law will fail. Putting in the definitions of the terms in the first quoted sentence we read:—It is therefore not surprising that beings, who have perceptive and reflective faculties capable only of producing practically the same world of phenomena, perceive the same world of phenomena and reflect upon it in much the same manner. In this exquisite cycle of reasoning what, we ask, is the logical work done?

Our grammarian poses as a logician of the straitest sect. Bad logic he cannot abide; and since apparently he cannot read a book without seeing the cloven hoof he must have rather a sorry time of it. His own logic must, of course, be flawless. So, when we are told with reiterated emphasis that time and space are but modes of perception, and are then asked to imagine our Universe in time and space without a consciousness to perceive it, we feel a sinking at the heart. Things, we find, can exist under certain modes of a non-existent perception. The laws of Nature are a mental product; yet a certain evolution theory logically based upon them quite eliminates the mental. We are reminded of the sagacious carpenter who sat high and lifted up on the end of the bracket beam he was sawing through; or of the small boy who spent his wealth in buying a purse to hold it in.

A large section of Prof. Pearson's book is destructive criticism. "Cause," "Force," and "Matter" are as red rags to him. Cursed be he who uses these words without clearly defining, in footnote or otherwise, their significance according to the definitions given in the "Grammar of Science." Sir Isaac Newton is severely visited for his sins; Thomson and Tait get a thorough drubbing; Maxwell is censured for his bad logic; and Prof. Tait especially, if we are to judge of him through the medium of this book, must have done more to retard the progress of science than any other single man of the century. Sound criticism is always welcome; but "smart" controversy of the hustings type is rarely sound in print. As a fair example of our grammarian's method, take his critique of Maxwell's descriptions (not definitions be it noted) of the intimate relation between matter and energy. Maxwell says, "We are acquainted with matter only as that which may have energy communicated to it from other matter, &c.," and "Energy, on the other hand, we know only as that which . . . is continually passing from one portion of matter to another." These are represented as meaning that "the only way in which we can understand matter is through the energy which it transfers," and "the only way to understand energy is through matter. Matter has been defined in terms of energy, and energy again in terms of matter." By what logic or grammar can *understand* be substituted for *are acquainted with* or *know;* and by what right is a description twisted into a definition? Words in their usual meanings may, however, be of little consequence to a writer who persists in using the English word *resume* in the French sense. It seems to us that Prof. Pearson has altogether missed the significance of the word "objective" as used by Prof. Tait, to whom, as everyone knows or should know, we owe the first clear presentation of the dogma that force has no objective existence. At any rate, we are surprised to find in the "Grammar of Science" no distinct reference to the two grand principles of all science—to wit, the conservation of matter and the conservation of energy. This omission by an avowed writer on the principles of science is certainly matter of surprise. As regards the views of force expounded in the book, the author is simply a disciple of Prof. Tait. If not, he must regard Tait as "that worst of plagiarists"—the man who made the discovery before he did. Prof. Pearson has, indeed, a certain fatality for having dealings with that most unsatisfactory kind of plagiarism. In Tait's "Properties of Matter," first edition (1885), paragraph 162, are written these words :—

"Sir W. Thomson has shown that if space be filled with an incompressible fluid, which comes into existence in fresh quantities at the surface of every particle of matter, at a rate proportional to its mass, and is swallowed up at an infinite distance, or, if each particle of matter constantly swallows up an amount proportional to its mass, a constant supply being kept up from an infinite distance,—in either case gravitation would be accounted for."

If this is not *essentially* the theory of "ether-squirts" which "the author has ventured to put forward," what then is the ether-squirt? The quotation just given occurs in Tait's seventh chapter, which, being empty of "red rags," probably failed to come within Prof. Pearson's sphere of perception.

Be it noted that we do not criticize our author's views as to the significance of such words as force and cause; but we cannot say we fancy his critical tone towards others. He himself uses the phrase "acceleration of A due to B," but warns the reader in a footnote against taking the phrase in its literal sense; yet anybody else from Newton down the centuries who has dared to use similar phrases is sneered at as a searcher after the unknowable "why."

For example, in his criticism of Newton's first law of motion, what right has he to say that Newton "was thinking of force in the sense of mediæval metaphysics as a cause of change in motion"? What is the perceptual or conceptual basis of this assumed certitude? Newton was probably thinking of *vis impressa*, the very grammatical form of which shows that there was nothing ultimate implied in the *vis*. After discussing the various kinds of *vires* that have to be dealt with, and pointing out clearly by definitions and descriptions their precise meanings, Newton concludes one paragraph in these words :—

"*Mathematicus duntaxat est hic conceptus: Nam virium causas et sedes physicas jam non expendo.*"

Then a little further on we read :—

"*Has vires non physice sed mathematice tantum considerando. Unde caveat lector, ne per hujusmodi voces cogitet me speciem vel modum actionis causamve aut rationem physicam alicubi definire vel centris (quæ sunt puncta mathematica) vires vere et physice tribuere; si forte aut centra trahere, aut vires centrorum esse dixero.*"

Can it be that Prof. Pearson has never read Newton's "Principia," and has he forgotten that the complete title is "Philosophiæ Naturalis Principia Mathematica"? To insinuate that Newton's laws of motion (which, it should never be forgot, are intimately associated with the *Definitiones*) are incomplete because they may not possibly apply to corpuscles other than those of "gross" matter, to corpuscles of all imaginable types in short, implies a complete misapprehension of the whole purpose and scope of the "Principia." Again, our grammarian pounces upon the word "body," or *corpus*, as used by Newton, who should at least have used particle or corpuscle. In Definition I. will be found the meaning intended by Newton to be attached to the word *corpus;* but in any case the whole phraseology of the first law is quite intelligible to the candid mind. Newton had a fine faith in his reader. He gave the *Definitiones* and *Axiomata* in a form that appealed at once to the common experiences of thoughtful minds; and what more do we need?

Prof. Pearson characterizes the second law as a "veritable metaphysical somersault. How the imperceptible cause of change in motion can be applied in a straight line surpasses comprehension, &c." This may be smart, but is it relevant? Where does Newton define *Vis Motrix* as the "imperceptible cause of change in motion"?

We have not space to enter upon a discussion of the five laws of motion suggested by Sir Thomas Gresham's Professor of Geometry as a true non-metaphysical basis for all science. They are good enough in their way; but they seem to lack that direct reference to ordinary facts of experience which is a desideratum of all physical axioms. They begin with a dance of molecules and end with a measure of force Their ostensible merits are their logical form and their comprehensiveness—ether corpuscles as well as matter corpuscles being nominally included. Yet we have to confess our inability to see that these laws of motion can effect more than Newton's. Dynamics, in all its branches, still is Newtonian.

In its discussion of the meaning of scientific law, in its presentation of kinematic principles, and in its treatment of certain present-day speculations as to the constitution of matter and of ether, Prof. Pearson's book is at once interesting and instructive. There is much in it fitted to arrest the materialistic tendency of many who are devotees of science to the exclusion of all other intellectual activities. Yet its own conclusions are as materialistic as they well can be. The automaton theory of the human will, and the spontaneous generation of life, are articles of its creed. In the second last chapter we are treated to a choice collection of charming dogmatisms. Perhaps the most charming of all is the author's "unwavering belief" that the hitherto undiscovered formulæ which are to make history a science "can hardly be other than those which so effectually describe the relations of organic to organic and of organic to inorganic phenomena in the earlier phases of their development." A curious assertion, surely, for one to make who objects to Newton's laws of motion because they don't include imaginable but still unknown types of corpuscular motion. The particular value, however, of this confession of faith is that it enables the confessor to convict of scientific heresy Prof. Robertson Smith and all others who cannot regard it as other than an assumption. To believe as Prof. Pearson believes is to believe scientifically; all other belief is rotten. As the "auld licht" dame said when telling over the number of the elect, "Ay, there's jist me and John; and whiles I'm no that sure o' John." C. G. K.

THE TEACHING OF THE PRINCIPLES OF CHEMISTRY.

Laboratory Practice: A Series of Experiments on the Fundamental Principles of Chemistry. A Companion Volume to "The New Chemistry." By Josiah Parsons Cooke, LL.D., Erving Professor, and Director of the Chemical Laboratory, Harvard University. Pp. 192. (London: Kegan Paul, Trench, Trübner, and Co., 1892.)

THIS little book represents another attempt to teach the theory of chemistry upon the basis of a narrowly restricted experience of facts and phenomena. Whether this is possible is a question debatable, and still, in fact, debated among teachers. That it is possible to make the study of chemistry by young people, as a form of intellectual exercise, more useful than has usually been the case there can be no doubt, and that much instruction could be got out of a course such as this which is indicated in Prof. Cooke's little work is certain. The book appears to be intended as a guide for the teacher as much as for the pupil, and much would depend upon the qualifications of the former for the work of demonstration and exposition. It contains directions for the performance of a system of experiments; and to do justice to the system the teacher ought carefully to study the instructions given in the introduction, and to act upon them. And to those who know anything of the manner in which chemistry is too often taught in the schools of this country, either by the visiting "science teacher," who knows little, or by the mathematical master, who usually knows nothing at all about the subject, such remarks as the following, taken from the introduction, will seem particularly welcome and appropriate.

The author says:

"Experiments are only of value as parts of a course of instruction logically followed out from beginning to end. In such a course there must be necessarily a great deal to be filled out by the teacher, and this can vastly better be taught from his lips, with such illustrations as he can command, than from any books."

And again,

"The best apparatus will be of no use unless the teacher stands before it and speaks to his pupils out of the fulness of his own knowledge. This is an essential condition of success, and without it the experimental method should never be attempted."

But after these things have all been duly noted and acted upon, a glance at the table of contents is apt to raise a doubt whether after all the erection of so large a superstructure is justifiable or practicable upon foundations so slender. The book begins at p. 13, and thence to p. 52, with the exception of three or four pages about water, the whole is devoted to the physical properties of liquids and solids represented by water and air. Then we come to oxygen, hydrogen, sulphur and its oxides, chlorine, carbon and the oxides of carbon, ethylene, nitrogen, nitric acid, ammonia, magnesium, zinc, sodium, copper, and iron, all of which are included in the fifty pages following. Then comes a chapter on general principles, a third on molecules and atoms, followed by chapters on symbols and nomenclature, molecular structure, and thermal relations.

This is not the first book which has appeared with similar objects. In this country there have been Prof. Ramsay's little book on "Chemical Theory," Muir and Carnegie's "Practical Chemistry," Shenstone's "Practical Introduction to Chemistry," and probably others, which seem to aim at dealing with chemistry in the same kind of way, which is intended to be a way of pleasantness and a short cut to rather exalted territory. The road, however, is bordered by precipices unseen by the young traveller.

The advocates of this kind of system, which consists in passing from one or two rough experiments, or observations, direct to great generalizations, anticipate great things from its general adoption. All the rising generation who come under its influence are to possess greatly developed powers of observation and reasoning. Some of those who have been accustomed to old-fashioned ways of getting a good grip of facts, and some stock of experience before proceeding to difficult investigation, are not convinced, and are inclined to doubt whether school boys and girls can be made to reason out for themselves problems which have cost for their elucidation the work of generations of men. And the logic of the process is often more than questionable. Here is an example (p. 110). The law of the conservation of mass is supposed to be established by a single experiment, which consists in burning a bit of phosphorus in a jar, and showing that there is no loss of weight.

"Hence it must be that, *The sum of the weights of the products of a chemical change is exactly equal to the sum of the weights of the factors.* We may conceive of any chemical process as taking place in an hermetically sealed space—indeed the earth is essentially such a space —and hence this law must be universally true."

Here the process of induction is reduced to collecting a single instance, which is itself imperfect. Surely this is not to stand as an example of the methods of physical science.

One would not wish to be hard upon Prof. Cooke's little book, but with many meritorious features it does not seem to represent a great improvement upon the books referred to above. The naïve statement at the end of the introduction, that the directions can in many cases be improved, cannot be held to excuse the rough and slipshod character of some of the forms of experiment recommended. The book will supply suggestions which will be found useful by some teachers, but the reference to apparatus unfamiliar on this side the Atlantic may be a slight bar to its adoption here. W. A. T.

OUR BOOK SHELF.

Elementary Geography of the British Colonies. By George M. Dawson, LL.D., F.R.S., and Alexander Sutherland, M.A. With Illustrations. (London: Macmillan and Co., 1892.)

THIS volume forms one of the well-known geographical series edited by Sir Archibald Geikie. The part of it for which Dr. Dawson is responsible is that which deals with the British possessions in North America, the West Indies, and the southern part of the South Atlantic Ocean. Mr. Sutherland describes the British colonies, dependencies, and protectorates in the northern part of the South Atlantic, Mediterranean Sea, Africa, Asia (exclusive of India and Ceylon, which are described in a separate volume of the series, by Mr. H. F. Blanford), Australasia, and Oceania. Both writers have enlightened ideas as to the needs of those for whom such books are prepared. They have carefully avoided the bringing together of masses of uninteresting detail, their chief object being to convey a good general idea of the physical features and resources of the British colonies, and of the various ways in which these have affected the distribution of the population and the growth of industry and commerce. The facts are presented simply and clearly, and every page contains statements which an intelligent teacher would have no difficulty in using as texts for pleasant and profitable instruction. Most of the illustrations are from photographs, but there are also several very effective engravings from original drawings by Mr. Pritchett.

Farmyard Manure. By C. M. Aikman, M.A., B.Sc. (Edinburgh and London: Blackwood, 1892.)

WE are told in the preface that this little work is in substance a chapter from a larger work on "Soils and Manures," on which the author is at present engaged. Perhaps we may be excused if we fail to see the necessity of publishing this chapter separately in advance. It certainly contains much information from German works, such as Heiden's "Düngerlehre," but the book is written mainly from the chemist's point of view and not from the farmer's. The pamphlet gives one the impression of having been hurriedly prepared, but no doubt its deficiencies will be remedied in the larger book.

LETTERS TO THE EDITOR

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Peripatus from St. Vincent.

SOME of the readers of NATURE will doubtless be interested to learn that, while collecting in St. Vincent on behalf of the Committee appointed for the investigation of the fauna and flora of the Lesser Antilles, Mr. H. H. Smith obtained five examples of the genus *Peripatus*.

The importance of the discovery, or rather rediscovery, of this Arthropod in St. Vincent rests upon the fact that the Rev. L. Guilding procured the first recorded examples of the genus in this same island. A description of these, under the name *juliformis*, was published by this naturalist in 1826, in vol. ii. of the *Zoological Journal*. But from that time until now, a period of 66 years, no additional specimens have been brought to light in this locality; and since Guilding's types have been lost sight of, and his description of them is wanting in detail, the identity of *juliformis* has been involved in considerable obscurity. There can, however, be little if any doubt that the examples collected by Mr. H. H. Smith are specifically identical with those that Guilding described. Nevertheless this assumption receives more support from identity of locality than from the agreement that obtains between the description of *juliformis* and the specimens before me. The largest of these measures 43 mm. in length and 6·5 in width; the smallest, on the contrary, is only 13 mm. long. One example has 34 pairs of legs, two of them 33, one 30, and one 29. The colour of the lower surface may be described as fawn; that of the dorsal side varies from fawn to blackish grey.

Those who are familiar with Mr. Smith's qualifications as a collector need hardly be told that the specimens are on the whole in a satisfactory state of preservation. I consequently hope to be able to prepare a detailed description of the species, to be incorporated in the report upon the Myriopoda of the Lesser Antilles, the identification of the species of this group, together with that of the Scorpions, Pedipalpi, and fresh-water Decapoda, having been kindly intrusted to my care by the members of the Exploration Committee. R. I. POCOCK.

Natural History Museum, May 27.

The Line Spectra of the Elements.

I QUITE agree with Prof. Stoney that Fourier's theorem can be applied to motions which approximate to non-periodic motions in any assigned degree, and for any assigned time. And so the co-ordinates of any arbitrary motion may approximately in any assigned degree and for any assigned time be represented by formulas of this kind:—

$$a_0 + a_1 \sin\left(\frac{m_1 t}{j} + \alpha_1\right) + a_2 \sin\left(\frac{m_2 t}{j} + \alpha_2\right) + \ldots + a_n \sin\left(\frac{m_n t}{j} + \alpha_n\right),$$

where $m_1, m_2, \ldots m_n$ are positive integers, and j must be chosen sufficiently large to suit the length of the assigned time. This is not the point in Prof. Stoney's reasoning to which I object.

What I want to say is this: If the motion is not periodical, the periods of the circular functions, as well as the amplitudes and phases, are not necessarily definite. That is to say, if we choose a larger value of j, to get a closer approximation for a longer time, the values of a, $\frac{m}{j}$, α do not necessarily approach definite values, but may become totally different.

Take, for instance, the equation—

$$t = 2j\left[\sin\frac{t}{j} - \tfrac{1}{2}\sin\frac{2t}{j} + \tfrac{1}{3}\sin\frac{3t}{j} - \ . \ . \ . \ \right],$$

which holds good for all values of t between $-j$ and $+j$. Prof. Stoney may say that Fourier's theorem can be applied to the function t. So it can, certainly, if an interval is assigned. But the amplitudes and periods of the single terms are not independent of the length of the interval, and do not approach definite values when the interval increases indefinitely.

The time during which the approximation is to hold good need not be indefinitely long. But the time must be long in comparison with the longest of the periods. Motions of the ether that are represented by such functions will be resolved by a diffraction grating into different rays, but others will not. Prof. Stoney has not noticed that a distinct property of the function is wanted in order to get a proper resolution into a sum of circular functions. His reasonings in chapter iv. of his memoir on the cause of double lines, &c. (Transactions of the Royal Dublin Society, 1891), refer to all functions with or without this property, and therefore do not seem to me to be correct. But I admit that my expression in the passage quoted by Prof. Stoney might have been clearer. C. RUNGE.

Techn. Hochschule, Hannover, May 19.

Maxwell's Law of Distribution of Energy.

IN the current number of the *Philosophical Magazine*, Lord Kelvin describes a dynamical system in which when in stationary motion Maxwell's law of distribution of energy would fail, assuming that law to consist in the ultimate equality of the energy of different parts of the system. He has thus shown the necessity for more accurate language than is commonly employed in the enunciation of that law, and a consideration of his problem may help to determine the limits to which it is subject.

The following statement, whether co-extensive with Maxwell's law or not, will probably be accepted as true as far as it goes—

If there exist a very great number of material systems, the state of each being defined by certain co-ordinates and momenta, and if at a given instant all combinations of the co-ordinates and momenta are represented among them with frequency proportional to $\epsilon^{-h(\chi+T)}$, then that distribution will be permanent—that is, will not be disturbed by the mutual action of the systems, or by any forces in the field of which they are placed, provided all the forces concerned be conservative.

The further question as to how far the solution thus found for the permanent state is unique, has been treated by Boltzmann. He shows that a certain function, which in stationary motion must be positive and constant, necessarily diminishes with the time, so long as any small deviations exist from the above described state. It is obvious that this proposition of Boltzmann's cannot be applicable to all cases of stationary motion. Periodic motions are exceptions, and so is the system described by Lord Kelvin. The question is what assumptions underlie Boltzmann's demonstration. It will be of great advantage if one speaking with Lord Kelvin's authority will assist in defining the limits to which the proposition is subject.

Maxwell, although he may at times have expressed himself incautiously, was aware that the theory was subject to limitations. The statistical, as distinguished from the historical, method was from his point of view of the essence of the theory. A distinction may be drawn between systems, such as Lord Kelvin's, to which the statistical method is inapplicable, and those in which the stationary motion, when attained, is what is called thermal motion—that is, the relative motions are in all directions indifferently, and of that irregular character in which heat is supposed to consist.

It may be that we shall be driven to the conclusion that Maxwell's law has no application except to this class of systems; that it is, in fact, only the limiting state to which a material system approaches as we increase indefinitely the number of its degrees of freedom.

It does, at all events, appear that in cases where the law fails, its failure is due to the introduction of some restrictions on freedom of motion, especially as regards direction. Maxwell pointed out that dæmons—or, shall we say, beings endowed with free will—might by directing the courses of individual molecules cause a system to violate, not only the law of distribution of energy, but even the second law of thermodynamics. What these beings might be supposed to do, that Lord Kelvin in fact does once for all for his system, by prescribing *a priori* the directions of motion and other conditions of the problem to suit his purpose.

H. W. WATSON.
S. H. BURBURY.

The Former Connection of Southern Continents.

WITH reference to the very interesting question treated in Mr. Mellard Reade's letter of your issue of May 26 (p. 77), as to the former connection of southern continents, it may be worth while calling attention to the fact that a great circle, which I may call the Kaffraria Great Circle, connects that coast line with the Falkland Island and the South Georgia Island. It may be presumed that these two islands are the remaining summits of what was once a chain of mountains in connection with the continent of South America. Some of the points through which or near which this great circle passes are as follow — the above-mentioned islands, Port de Sta. Cruz, Patagonia; it traverses the Pacific, runs parallel to the southern branch of the Aleutian Islands, and cuts Kamtchatka somewhat south of Klienchewskaia Volcano, and traversing Asia emerges by the Island of Cutch, so interesting on account of the earthquakes which occurred there. It is of interest to note that South Georgia Island is antipodal to the northern extremity of Saghalian Island.

J. P. O'REILLY.

Royal College of Science for Ireland,
Stephen's Green, Dublin, May 30.

ON THE RELATIVE DENSITIES OF HYDROGEN AND OXYGEN.[1]

IN a preliminary notice upon this subject (Roy. Soc. Proc., vol. xliii. p. 356, February 1888), I explained the procedure by which I found as the ratio of densities 15·884. The hydrogen was prepared from zinc and sulphuric, or from zinc and hydrochloric, acid, and was liberated upon a platinum plate, the generator being in fact a Smee cell, inclosed in a vessel capable of sustaining a vacuum, and set in action by closing the electric circuit at an external contact. The hydrogen thus prepared was purified by corrosive sublimate and potash, and desiccated by passage through a long tube packed with phosphoric anhydride. The oxygen was from chlorate of potash, or from mixed chlorates of potash and soda.

In a subsequent paper "On the Composition of Water" (Roy. Soc. Proc., vol. xlv. p. 425, February 1889), I attacked the problem by a direct synthesis of water from weighed quantities of the two component gases. The ratio of atomic weights thus obtained was 15·89.

At the time when these researches were commenced, the latest work bearing upon the subject dated from 1845, and the number then accepted was 15·96. There was, however, nothing to show that the true ratio really deviated from the 16 : 1 of Prout's law, and the main object of my work was to ascertain whether or not such deviation existed. About the year 1888, however, a revival of interest in this question manifested itself, especially in the United States, and several results of importance have been published. Thus, Prof. Cooke and Mr. T. W. Richards found a number which, when corrected for an error of weighing that had at first been overlooked, became 15·869.

The substantial agreement of this number with those obtained by myself, seemed at first to settle the question, but almost immediately afterwards there appeared an account of a research by Mr. Keiser, who used a method presenting some excellent features, and whose result was as high as 15·949. The discrepancy has not been fully explained, but subsequent numbers agree more nearly with the lower value. Thus, Noyes obtains 15·896, and Dittmar and Henderson give 15·866.

I had intended further to elaborate and extend my observations on the synthesis of water from weighed quantities of oxygen and hydrogen, but the publication of Prof. E. W. Morley's masterly researches upon the "Volumetric Composition of Water" (*Amer. Journ. Sci.*, March 1891) led me to the conclusion that the best contribution that I could now make to the subject would be by the further determination of the relative densities of the two gases. The combination of this with the number 2·0002,[2] obtained by Morley as the mean of astonishingly concordant individual experiments, would give a better result for the atomic weights than any I could hope to obtain directly.

In the present work two objects have been especially kept in view. The first is simplicity upon the chemical side, and the second the use of materials in such a form that the elimination of impurities goes forward in the normal working of the process. When, as in the former determinations, the hydrogen is made from zinc, any impurity which that material may contain and communicate to the gas cannot be eliminated from the generator; for each experiment brings into play a fresh quantity of zinc,

[1] "On the Relative Densities of Hydrogen and Oxygen. II." Abstract of a paper by Lord Rayleigh, Sec.R.S., read at the Royal Society on February 18, 1892.

[2] It should not be overlooked that this number is difficult to reconcile with views generally held as to the applicability of Avogadro's law to very rare gases. From what we know of the behaviour of oxygen and hydrogen gases under compression, it seems improbable that volumes which are as 2·0002 : 1 under atmospheric conditions would remain as 2 : 1 upon indefinite expansion. According to the formula of Van der Waals, a greater change than this in the ratio of volumes is to be expected.

with its accompanying contamination. Moreover, the supply of acid that can be included in one charge of the generator is inadequate, and good results are only obtained as the charge is becoming exhausted. These difficulties are avoided when zinc is discarded. The only material consumed during the experiments is then the water, of which a large quantity can be included from the first. On the other hand, the hydrogen liberated is necessarily contaminated with oxygen, and this must be removed by copper contained in a red-hot tube. In the experiments to be described the generator was charged with potash,[1] and the gases were liberated at platinum electrodes. In the case of a hydrogen filling, the oxygen blew off on one side from a mercury seal, and on the other the hydrogen was conveyed through hot tubes containing copper. The bulk of the aqueous vapour was deposited in a small flask containing strong solution of potash, and the gas then passed over solid potash to a long tube packed with phosphoric anhydride. Of this only a very short length showed signs of being affected at the close of all operations.

With respect to impurities, other than oxygen and oxides of hydrogen, which may contaminate the gas, we have the following alternative. Either the impurity is evolved much more rapidly than in proportion to the consumption of water in the generator, or it is not. If the rate of evolution of the impurity, reckoned as a fraction of the quantity originally present, is not much more rapid than the correspondingly reckoned consumption of water, the presence of the impurity will be of little importance. If, on the other hand, as is probable, the rate of evolution is much more rapid than the consumption of water, the impurity is soon eliminated from the residue, and the gas subsequently generated becomes practically pure. A similar argument holds good if the source of the impurity be in the copper, or even in the phosphoric anhydride; and it applies with increased force when at the close of one set of operations the generator is replenished by the mere addition of water. It is, however, here assumed that the apparatus itself is perfectly tight.

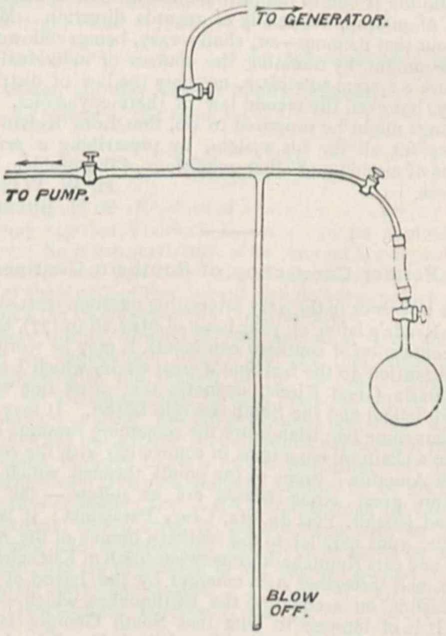
Except for the reversal of the electric current, the action of the apparatus is almost the same whether oxygen or hydrogen is to be collected. In the latter case the copper in the hot tubes is in the reduced, and in the former case in the oxidized, state. For the sake of distinctness we will suppose that the globe is to be filled with hydrogen.

The generator itself is of the U-form, with unusually long branches, and it is supplied from Grove cells with about 3 amperes of electric current. Since on one side the oxygen blows off into the air, the pressure in the generator is always nearly atmospheric. Some trouble has been caused by leakage between the platinum electrodes and the glass. In the later experiments to be here recorded these joints were drowned with mercury. On leaving the generator the hydrogen traverses a red-hot tube of hard glass charged with copper,[2] then a flask containing a strong solution of potash, and afterwards a second similar hot tube. The additional tube was introduced with the idea that the action of the hot copper in promoting the union of the hydrogen with its oxygen contamination might be more complete after removal of the greater part of the oxygen, whether in the combined or in the uncombined state. From this point onward the gas was nearly dry. In the earlier experiments the junctions of the hard furnace tubes with the soft glass of the remainder of the apparatus were effected by fusion. One of these joints remained in use, but the others were replaced by india-rubber connexions *drowned in mercury*. It is believed that no leakage occurred at these joints; but as an additional security a tap was provided between the generator and the furnace, and was kept closed whenever there was no forward current of hydrogen. In this way the liquid in the generator would be protected from any possible infiltration of nitrogen. Any that might find its way into the furnace tubes could easily be removed before the commencement of a filling.

Almost immediately upon leaving the furnace tubes the gas arrives at a tap which for distinctness may be called the regulator. In the generator and in the furnace tubes the pressure must be nearly atmospheric, but in the globe there is (at the commencement) a vacuum. The transition from the one pressure to the other takes place at the regulator, which must be so adjusted that the flow through it is approximately equal to the production of gas. At first the manipulation of the regulator was a source of trouble, and required almost constant attention, but a very simple addition gave the desired control. This was merely a long wooden arm, attached to the plug, which served both as a lever and as an indicator. Underneath the pointed extremity was a small table to which its motions could be referred. During the first two-thirds of a filling very little readjustment was needed, and the apparatus could be left for half an hour with but little fear of displacing too much the liquid in the generator. Towards the close, as the motive force fell off, the tap required to be opened more widely. Sometimes the recovery of level could be more conveniently effected by insertion of resistance into the electric circuit, or by interrupting it altogether for a few minutes. Into details of this kind it is hardly necessary to go further.

From the regulator the gas passed to the desiccating tubes. The first of these was charged with fragments of solid potash, and the second with a long length of phosphoric anhydride. Finally, a tube stuffed with glass wool intercepted any suspended matter that might have been carried forward.

The connection of the globe with the generator, with the Töppler, and with the blow-off, is shown in the accompanying figure. On the morning of a projected



filling the vacuous globe would be connected with the free end of the stout-walled india-rubber tube, and secured by winding wire. The generator being cut off, a high vacuum would be made up to the tap of the globe.

[1] At the suggestion of Prof. Morley, the solution was freed from carbonate or nearly so, by the use of baryta, of which it contained a slight excess.

[2] The copper must be free from sulphur; otherwise the contamination with sulphuretted hydrogen is somewhat persistent.

After a couple of hours' standing the leakage through the india-rubber and at the joints could be measured. The amount of the leakage found in the first two hours was usually negligible, considered as an addition to a globe-ful of hydrogen, and the leakage that would occur in the hours following would (in the absence of accidents) be still smaller. If the test were satisfactory, the filling would proceed as follows:—

The electric current through the generator being established, and the furnace being heated, any oxygen that might have percolated into the drying tubes had first to be washed out. In order to do this more effectively, a moderate vacuum (of pressure equal to about 1 inch of mercury) was maintained in the tubes and up to the regulator by the action of the pump. In this way the current of gas is made very rapid, and the half-hour allowed must have been more than sufficient for the purpose. The generator was then temporarily cut off, and a high vacuum produced in the globe connection and in the blow-off tube, which, being out of the main current of gas, might be supposed to harbour impurities. After this the pump would be cut off, the connection with the generator re-established, and, finally, the tap of the globe cautiously opened.

The operation of filling usually occupied from two to three hours. When the gas began to blow off under an excess of pressure represented by about half an inch of mercury, the blow-off cistern was lowered so as to leave the extremity of the tube free. For two minutes the current of gas from the generator was allowed to flow through, after which the generator was cut off, and the globe left in simple communication with the atmosphere, until it was supposed that equilibrium of pressure had been sufficiently established. Doubts have at various times been felt as to the interval required for this purpose. If too little time is allowed, there will remain an excess of pressure in the globe, and the calculated weight of the filling will come out too high. On the other hand, an undue prolongation of the time might lead to a diffusion of air back into the globe. In a special experiment no abnormal weight was detected after half an hour's communication, so that the danger on this side appeared to be small. When the passages through the taps were free from grease, one or two minutes sufficed for the establishment of equilibrium, but there was always a possibility of a partial obstruction. In the results to be presently given, four minutes were allowed after the separation from the generator. It may be remarked that a part of any minute error that may arise from this source will be eliminated in the comparison with oxygen, which was collected under like conditions.

The reading of the barometers and thermometers at the moment when the tap of the globe was turned off took place as described in the former paper. The arrangements for the weighings were also the same.

In the evacuations the process was always continued until, as tested by the gauge of the Töppler after at least a quarter of an hour's standing, the residue could be neglected. Here, again, any minute error would be eliminated in the comparison of the two gases.

In the case of oxygen, the errors due to contamination (even with hydrogen) are very much diminished, and similar errors of weighing tell very much less upon the proportional agreement of the final numbers. A comparison of the actual results with the two kinds of gas does not, however, show so great an advantage on the side of the oxygen as might have been expected. The inference appears to be that the individual results are somewhat largely affected by temperature errors. Two thermometers were, indeed, used (on opposite sides) within the wooden box by which the globe is surrounded, and they could easily be read to within $\frac{1}{20}$° C. But in other respects, the circumstances were unfavourable, in consequence of the presence in the same room of the furnace necessary to heat the copper. An error of ± 0°·1 C. in the temperature leads to a discrepancy of 1 part in 1500 in the final numbers. Some further elaboration of the screening arrangements actually employed would have been an improvement, but inasmuch as the circumstances were precisely the same for the two gases, no systematic error can here arise. The thermometers were, of course, the same in the two cases.

The experiments are grouped in five sets, two for oxygen and three for hydrogen. In each set the work was usually continued until the tap of the globe required re-greasing, or until, owing to a breakage or to some other accident, operations had to be suspended.

The means are as follow:—

HYDROGEN.

1891.	Weight.	Bar. temp., F.	Globe temp., C.	Corrected to 12°.
	gram.	°	°	gram.
July	0·15808	65	18	0·158056
September	0·15797	61	17	0·157950
October	0·15804	53	12	0·158040
Mean		60	16	0·158015

OXYGEN.

1891.	Weight.	Bar. temp., F.	Globe temp., C.	Corrected to 12°.
	grams.	°	°	grams.
June...............	2·51785	68	20	2·51735
November	2·51720	55	13	2·51713
Mean		61½	16½	2·51724

The means here exhibited give the weights of the two gases as they would be found with the globe at 12° C., and the barometers at 60° F. and at 30 inches. The close agreement of the mean temperatures for the two gases shows how little room there is for systematic error dependent upon imperfections in the barometers and thermometers. But the results still require modification before they can be compared with the view of deducing the relative densities of the gases.

In the first place, there is a systematic, though minute, difference in the pressures hitherto considered as corresponding. The terminal of the blow-off tube is 33 inches below the centre of the globe at the time of filling. In the one case this is occupied by hydrogen, and in the other by oxygen. If we treat the latter as the standard, we must regard the hydrogen fillings as taking place under an excess of pressure equal to $\frac{15}{16}$ of the weight of a column of oxygen 33 inches high; and this must be compared with 30 inches of mercury. Hence, if we take the sp. gr. of oxygen under atmospheric conditions at 0·0014, and that of mercury at 13·6, the excess of pressure under which the hydrogen was collected is as a fraction of the whole pressure

$$\frac{33}{30} \cdot \frac{15}{16} \cdot \frac{0{\cdot}0014}{13{\cdot}6} = 0{\cdot}000106\,;$$

and $0{\cdot}000106 \times 0{\cdot}158 = 0{\cdot}000017$. This, then, is what we must subtract from the weight of the hydrogen on account of the difference of pressures due to the gas in the blow-off tube. Thus

$$H = 0{\cdot}157998, \qquad O = 2{\cdot}51724.$$

But there is still another and a more important correction to be introduced. In my former paper it was shown that when the weighings are conducted in air the true weight of the gas contained in the globe is not given by merely subtracting the weight of the globe when empty from the weight when full. When the globe is empty, its external volume is less than when full, and thus, in order to obtain the true weight, the apparent weight of the gas must be increased by the weight of air whose volume is equal to the change of volume of the globe.

In order to determine the amount of this change of volume, the globe is filled to the neck with recently boiled distilled water, and the effect is observed upon the level in the stem due to a suction of, say, 20 inches of mercury. It is not advisable to carry the exhaustion much further, for fear of approaching too nearly the point at which bubbles of vapour may be formed internally. In the earlier experiments, described in the preliminary note, the upper surface of the liquid was in the stem of the globe itself (below the tap), and the only difficulty lay in the accurate estimation of a change of volume occurring in a wide and somewhat irregular tube. The method employed was to produce, by introduction of a weighed quantity of mercury, a rise of level equal to that caused by the suction.

The advantage of this procedure lay in the avoidance of joints and of the tap itself, but, for the reasons given, the readings were not quite so accurate as might be desired. I wished, therefore, to supplement, if possible, the former determination by one in which the change of volume occurred in a tube narrower and of better shape. With this object in view, the stem of the globe was prolonged by a graduated tubular pipette attached with the aid of india-rubber. The tubes themselves were treated with gutta-percha cement, and brought almost into contact. It had hardly been expected that the joint would prove unyielding under the applied suction, but it was considered that the amount of the yielding could be estimated and allowed for by operations conducted *with tap closed*. The event, however, proved that the yielding at the joint was scarcely, if at all, perceptible.

The pipette, of bore such that 16 cm. corresponded to 1 c.c., was graduated to 0·01, and was read by estimation to 0·001 c.c. In order the better to eliminate the changes due to temperature, readings under atmospheric pressure, and under a suction of 20 inches of mercury, were alternated. On January 28, 1892, a first set gave 0·648 − 0·300 = 0·348; a second gave 0·6645 − 0·316 = 0·3485; and a third gave 0·675 − 0·326 = 0·349. Similar operations with tap closed[1] gave no visible movement.

The result of the day's experiments was thus 0·3485 for 20 inches, or 0·523 for 30 inches, suction. Similar experiments on January 28, at a different part of the graduation, gave 0·526. On this day the yielding with tap closed was just visible, and was estimated at 0·001. As a mean result, we may adopt 0·524 c.c. The graduation of the pipette was subsequently verified by weighing a thread of mercury that occupied a measured length.

A part of the above-measured volume is due to the expansion of the water when the pressure is relieved. We may take this at 0·000047 of the volume per atmosphere. The volume itself may be derived with sufficient accuracy for the present purpose from the weight of its oxygen contents. It is 2·517/0·00137, or 1837 c.c. The expansion of the water per atmosphere is thus 0·000047 × 1837, or 0·087 c.c. This is to be subtracted from 0·524, and leaves 0·437 c.c. This number applies strictly to the volume inclosed within the glass, but the change in the external volume of the globe will be almost the same.

The correction now under consideration is thus the weight of 0·437 c.c. of air at the average temperature of the balance room. The density of this air may be estimated at 0·00122; so that the weight of 0·437 c.c. is 0·000533 gram. This is the quantity which must be added to the apparent weights of the gases. The former estimate was 0·00056 gram. The finally corrected weights are thus—

$$H = 0·158531, \qquad O = 2·51777;$$

and for the ratio of densities we have

15·882.

This corresponds to a mean atmospheric condition of pressure and temperature.

If we combine the above ratio of densities with Prof. Morley's ratio of volumes, viz. 2·0002 : 1, we get, as the ratio of atomic weights, 15·880.

If we refer to the table, we see that the agreement of the first and third series of hydrogen weighings is very good, but that the mean from the second series is decidedly lighter. This may have been in part fortuitous, but it is scarcely probable that it was so altogether. Under the circumstances we can hardly reckon the accuracy of the final results as closer than $\frac{1}{3000}$.

The accompanying table of results, found by various experimenters, may be useful for comparison:—

Name.	Date.	Atomic weights.	Densities.
Dumas	1842	15·96	—
Regnault	1845	—	15·96
Rayleigh	1888	—	15·884
Cooke and Richards ...	1888	15·869	—
Keiser	1888	15·949	—
Rayleigh	1889	15·89	—
Noyes	1890	15·896	—
Dittmar	1890	15·866	—
Morley	1891	15·879	—
Leduc	1891	—	15·905
Rayleigh	1892	—	15·882

[1] For greater security the tap was turned while the interior was under suction.

THE ORIGIN OF THE YEAR.[1]

II.

Difficulties.

THERE no doubt was a time when the Egyptian astronomer-priests imagined that, by the introduction of the 365-days year, beginning at the solstice or the nearly contemporaneous Nile flood (there is an interval of three days between them in the present Coptic calendar[2]), and by marking the commencement, in addition, by the heliacal rising of one of the host of heaven, they had achieved finality. But alas! the dream must soon have vanished.

Even with this period of 365 days, the true length of the year had not been reached; and soon, whether by observations of the beginning of the inundation, or by observations of the solstice in some of the solar temples which, beyond all doubt, were then in existence, it was found that there was a difference of a day every four years between the beginning of the natural and of the newly-established year, arising, of course, from the fact that the true year is 365 days *and a quarter of a day* (roughly) in length.

The true year and this established year of 365 days, then, behaved to each other as follows. Let us take a year when the solstice, representing the beginning of the

[1] Continued from vol. xlv. p. 490.

[2] The calendar in question (given both by Brugsch and De Rougé) is, doubtless, a survival from old Egyptian times It is good for the neighbourhood of Cairo, and the relation of the important days of the inundation to the solstice, in that part of the river, is as follows:—

Night of the drop	11 Payni		
	15 ,,	...	Summer solstice.
Beginning of the inundation ...	18 ,,	...	3 days after.
Assembly at the Nilometer ...	25 ,,	...	10 ,,
Proclamation of the inundation ...	26 ,,	...	11 ,,
Marriage of the Nile	18 Mesori	...	63 ,,
The Nile ceases to rise...	16 Thoth	...	96 ,,
Opening of the dams	17 ,,	...	97 ,,
End of the greater inundation ...	7 Phaophi	...	117 ,,

true year, occurred on the 1st Thoth of the established year. We should have, in the subsequent years, the state of things described in the diagram. The solstice

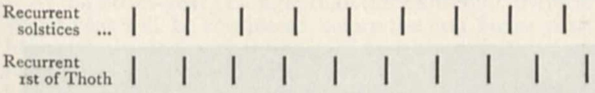


FIG. 2.—Showing the relation between the recurrences of the solstices and the 1st of Thoth.

would year by year occur *later* in relation to the 1st of Thoth. The 1st of Thoth would occur *earlier*, in relation to the solstice; so that in relation to the established year the solstice would sweep forwards among the days; in relation to the true year the 1st of Thoth would sweep backwards.

Let us call the true natural year a *fixed* year: it is obvious that, the months of the 365-day year would be perpetually varying their place in relation to those of the fixed year. Let us, therefore, call the 365-day year a *vague* year.

Now if the fixed year were exactly 365¼ days long, it is quite clear that, still to consider the above diagram, the 1st of Thoth would again coincide with the solstice in 1460 years, since in 4 years the solstice would fall on the 2nd of Thoth, in 8 years on the 3rd of Thoth, and so on $(365 \times 4 = 1460)$.

But the fixed year is not 365¼ days long *exactly*. In the time of Hipparchus 365·25 did not really represent the true length of the solar year; instead of 365·25 we must write 365·242392—that is to say, the real length of the year is a little *less* than 365¼ days.

Now the length of the year being a little *less*, of course we should only get a second coincidence of the 1st of Thoth vague with the solstice in a *longer* period than the 1460-years cycle; and, as a matter of fact, 1506 years are required to fit the months into the years with this slightly shortened length of the year. In the case of the solstice and the vague year, then, we have a cycle of 1506 years.

The variations between the fixed and vague years were known perhaps for many centuries to the priests alone. They would not allow the established year of 365 days, since called the *vague* year, to be altered; and so strongly did they feel on this point that, as already stated, every king had to swear when he was crowned that he would not alter the year. We can surmise why this was. It gave great power to the priests; they alone could tell on what particular day of what particular month the Nile would rise in each year, because they alone knew in what part of the cycle they were; and in order to get that knowledge they had simply to continue going every year into their Holy of Holies one day in the year as the priests did afterwards in Jerusalem, and watch the little patch of bright sunlight coming into the sanctuary. That would tell them exactly the relation of the true solar solstice to their year; and the exact date of the inundation of the Nile could be predicted by those who could determine observationally the solstice, but by no others.

But now suppose that instead of the solstice we take the heliacal rising of Sirius, and compare the successive risings at the solstice with the 1st of Thoth.

But why, it will be asked, should there be any difference in the length of the cycles depending upon successive coincidences of the 1st of Thoth with the solstice and the heliacal rising of Sirius? The reason is that stars change their places, and the star to which they trusted to warn them of the beginning of a new year was, like all stars, subject to the effects brought about by the precession of the equinoxes. Not for long could it continue to rise heliacally either at the solstice or the Nile flood.

Among the most important contributors to the astronomical side of this subject are M. Biot and Prof. Oppolzer. It is of the highest importance to bring together the fundamental points which have been made out by their calculations. We have determinate references to the heliacal rising of Sirius, to the 1st of Thoth, to the solstice, and to the rising of the Nile in connection with the Egyptian year; but, so far as I have been able to make out, we find nowhere at present any sharp reference to the importance of their correlation with the times of the *tropical* year at which these various phenomena took place. The question has been complicated by the use by chronologists of the Julian year in such calculations; so the Julian year and the use made of it by chronologists have to be borne in mind. Unfortunately, many side issues have in this way been raised.

The heliacal rising of Sirius, of course—if in those days a true *tropical* year was being dealt with—would have given us a more or less constant variation in the time of the rising over a long period, *on account of its precessional movement;* but M. Biot and others before him have pointed out that the variation in the time of the year at which the heliacal rising took place, produced by that movement, was almost exactly equal to the error of the *Julian* year as compared with the true tropical or Gregorian one. Biot showed by his calculations, using the solar tables extant before those of Leverrier, that from 3200 B.C. to 200 B.C. in the Julian year of the chronologists, Sirius had constantly, in each year, risen heliacally on July 20 Julian = June 20 Gregorian. Oppolzer, more recently, using Leverrier's tables, has made a very slight correction to this, which, however, is practically immaterial for the purposes of a general statement. He shows that in the latitude of Memphis, in 1600 B.C., the heliacal rising took place on July 18·6, while in the year 0 it took place on July 19·7, both Julian dates.

The variation from the true tropical year brought about by the precessional movement of Sirius or any other star, however, can be watched by noting its heliacal rising in relation to any physical phenomenon which marks the true length of the tropical year. Such a phenomenon we have in the rising of the Nile, which, during the whole course of historical time, has been found to rise and fall with absolute constancy in each year, the initial rise of the waters, some little way above Memphis, taking place very nearly at the summer solstice.

Again, M. Biot has made a series of calculations from which we learn that the heliacal rising of Sirius AT THE SOLSTICE occurred on July 20 (Julian) in the year 3285 B.C., and that in the year 275 B.C. the *solstice* occurred on June 27 (Julian), while the heliacal *rising of Sirius* took place, as before, on July 20 (Julian), so that in Ptolemaic times, at Memphis, there was a difference of time of about 24 days between the heliacal rising of Sirius and the solstice, and therefore the beginning of the Nile flood in that part of the river. This, among other things, is shown in Fig. 3.

We learn from the work of Biot and Oppolzer then that the precessional movement of the star caused successive heliacal risings of Sirius at the solstice to be separated by almost exactly 365¼ days—that is, by a greater period than the length of the true year. So that, in relation to this star, two successive heliacal risings at the 1st of Thoth vague are represented by a period of $(365\frac{1}{4} \times 4 =)$ 1461 years, while in the case of the solstices we want 1506.

Now in books on Egyptology the period of 1461 years is termed the Sothic period, and truly so, as it very nearly correctly measures the period elapsing between two heliacal risings at the solstice (or the beginning of the Nile flood) on the 1st of Thoth in the *vague* year.

But it is merely the result of *chance* that $365\frac{1}{4} \times 4$ represents it. It has been stated that this period had not any ancient existence, but was calculated back in later times. This seems to me very improbable. I look upon it rather as a true result of observation, the more so as the period was shortened *in later times*, as Oppolzer has shown.

It will be seen that our investigations land us in several astronomical questions of the greatest interest, and that the study is one in which modern computations, with the great accuracy which the work of Leverrier and others give to them, can come to the rescue, and eke out the scantiness of the ancient records.

To consider the subject further, we must pass from the mere question of the year to that of chronology generally, but in doing so I shall limit myself to the more purely astronomical part. To go over the already vast literature is far from my intention, nor is it necessary to attempt to settle all the differences of opinion which exist, and which are so ably referred to by Krall in his masterly analysis, to which I own myself deeply indebted. The tremendously involved state of the problem may be gathered from the fact that the authorities are not yet decided whether many of the dates really belong to a fixed or a vague year!

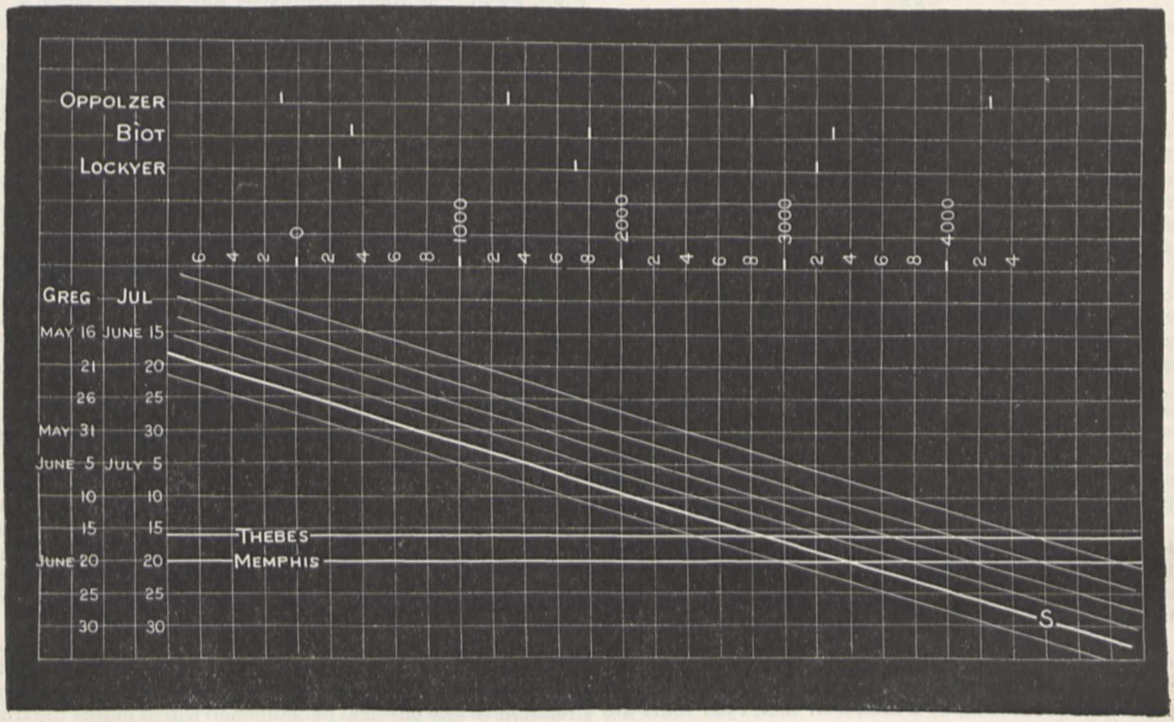


Fig. 3.—The conditions of the heliacal rising of Sirius from 4000 B.C. to 600 A.D. The diagram shows (1) by white horizontal lines, the Gregorian and Julian dates for the rising at Thebes and at Memphis. (2) By the full diagonal line the *Julian* date of the solstice or beginning of the inundation in each century, at a point of the river near Memphis. The fainter lines show the Julian dates for other places where the time of the beginning of the flood differs by three days from the Memphis dates. The interval between each line represents a difference of three days in the arrival of the flood (3) The interval in days between the heliacal rising and the inundation at different periods and at different points on the river. This can be determined for each century by noticing the interval between the proper diagonal line and that indicating the heliacal rising. (4) By dots at the top of the diagram the commencement of the Sothic period as determined by Oppolzer, Biot, and the author.

Let us, rather, put ourselves in the place of the old Egyptians, and inquire how, out of the materials they had at hand, a calendar could be constructed in the simplest way.

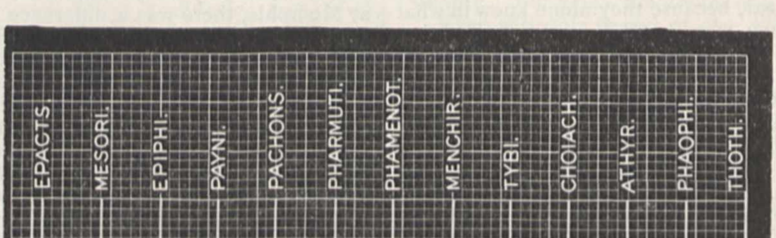


Fig. 4.—The distribution of the 1st of Thoth (representing the rising of Sirius) among the Egyptian months in the 1460-year Sothic cycle.

To make what follows clearer, it will be well to construct another diagram somewhat like the former one.

Let us map out the 1460 years which elapsed between two successive coincidences between the 1st of Thoth in the vague year and the heliacal rising of Sirius at the solstice, so that we can see at a glance the actual number of years from any start point (= 0) at which the 1st of Thoth in the vague year occurred successively further and further from the heliacal rising, until at length, after a period of 1460 years, it coincided again.

As the Sirius-year is longer than the vague one, the first vague year will be completed before the first Sirius-year, hence the second vague year will commence just before the end of the fixed year, and that is the reason that I have reversed the order of months in the diagram (Fig. 4).

Now it is clear that, if the Egyptians really worked in this fashion, any special day in the vague year given as the date of the heliacal rising of Sirius would enable us to determine the number of years which had elapsed from the beginning of the cycle. This will help us to determine whether or not they acted on this principle, or used one widely different. In such an investigation as this, however, we are terribly hampered by the uncertainty of Egyptian dates; while, as I have said before, there is great divergence of opinion among Egyptologists as to whether, from very early times, there was not a true fixed year.

But let us suppose that the vague year was in use, and that the rising of Sirius started the year; then, if we can get any accepted date to work with, and use the diagram to see how many years had elapsed between that date and the start-point of the cycle, we shall see if there be any cyclical relation, and if we find it, it will be evidence, so far as it goes, of the existence of a vague year.

Now it so happens that there are three references, with dates given, to the rising of Sirius in widely different times; and, curiously enough, the month references are nearly the same. I begin with the most recent, as in this case the date can be fixed with the greater certainty. It is an inscription at Philæ, described by Brugsch (p. 87), who states that, when it was written, the 1st of Thoth = 28th of Epiphi. That is, according to the view we are considering, the heliacal rising of Sirius occurred on the 28th of Epiphi in the vague year. He fixes the date of the inscription between 127 and 117 B.C. Let us take it as 122. Next, referring to our diagram to find how many years had elapsed since the beginning of the cycle, we have—

Days.
5 Epacts.
30 Mesori.
2 Epiphi.
—
37 × 4 = 148 years elapsed.

The cycle, then, began in (148 + 122 =) 270 B.C.

We next find a much more ancient inscription recording the rising of Sirius on the 28th of Epiphi. Obviously, if the Sothic cycle had anything to do with the matter, this must have happened 1458 years earlier, *i.e.* about (1458 + 122 =) 1580 B.C. Under which king? Thotmes III., who reigned, according to Lepsius, 1603–1565 B.C.; according to Brugsch, 1625–1577. Now, the inscription in question is stated to have been inscribed by Thotmes III., and, it may be added, on the temple (now destroyed) at Elephantiné.

There is yet another inscription, also known to be of a still earlier period, referring to the rising of Sirius on the 27th of Epiphi. We may neglect the difference of one day; and again, if the use of the Sothic cycle were the origin of the identity of dates, we have this time, according to Oppolzer, a period of 1460 years to add: this gives us (1584 + 1460 =) 3044 B.C. Again under which king? Here we are face to face with one of the difficulties of these inquiries. It may be stated, however, that the inscription is ascribed to Pepi, and that, according to various authorities, he reigned some time between 3000 and 3700 B.C.

We come, then, to this: that one of the oldest dated inscriptions known seems to belong to a system which continued in use at Philæ up to about 100 B.C., and it was essentially a system of a vague year.

Now, assuming that the approximate date of the earliest inscription is 3044 B.C., and that it represented the heliacal rising of Sirius on the 27th of Epiphi; the year 3044 must have been the [(5 + 30 + 3) × 4 =] 152nd after the beginning of the cycle. The cycle, then, must have commenced (3044 + 152 =) 3196 B.C.

According to Biot's calculation, the first heliacal rising of Sirius at the solstice took place in the year 3285 B.C.,

If we assume that the real date of Pepi, who, it is stated, reigned 100 years, included the year 3044 B.C., it may be that the inscriptions to which I have directed attention give us three Sothic cycles beginning—

122 + 148 = 270 B.C.
1580 + 148 = 1728 B.C.
3044 + 148 = 3192 B.C.

J. NORMAN LOCKYER.

(*To be continued.*)

NOTES.

THE list of those on whom honorary degrees are to be conferred at Cambridge on the occasion of the installation of the Duke of Devonshire as Chancellor shows that culture, and especially scientific culture, goes for very little among the classes of distinction recognized by the University. Eminence in the political world and in society seems to be the claim chiefly recognized.

SCIENCE was well represented at the annual dinner of the Incorporated Society of Authors on Tuesday. The chair was occupied by Prof. Michael Foster, and Sir Archibald Geikie was one of those who responded to the toast of "Literature."

DR. A. F. BATALIN has been appointed Director of the Imperial Botanic Garden at St. Petersburg, in succession to the late Dr. E. Regel.

THE ninety-seventh meeting of the Yorkshire Naturalists' Union will be held on Whit Monday, June 6. Some interesting notes on the physical geography and geology, botany, entomology, conchology, and vertebrate zoology of the district have been issued for the benefit of those who intend to be present. We are glad to see that members are expected to "do all in their power to discourage the uprooting of ferns and rare plants, or the too free collection of rarities of any kind."

THE Botanical Society of France has held its annual meeting at Algiers, commencing April 16, under the presidency of the Algerian botanist, M. Battandier. In addition to the reading of papers, excursions were made to Biskra, and other spots on the border of the Sahara.

WE have received the programme of the ninth International Congress of Orientalists. It is to be held in London from September 5 to 12, Prof. Max Müller acting as President. The Duke of Connaught has accepted the office of Honorary President. The following are the Vice-Presidents: the Marquis of Ripon, Lord Northbrook, Lord Reay, Major-Gen. Sir Henry Rawlinson, the Rt. Hon. Sir M. E. Grant Duff, Sir John Lubbock, Sir William Muir, Sir William W. Hunter, Sir George Birdwood, Sir William Markby, Sir Edwin Arnold, the Provost of Oriel College, Oxford, the Master of Balliol College, Oxford, the Master of Christ's College, Cambridge, H. S. King, and M. M. Bhownuggree. The Treasurer is Mr. E. Delmar Morgan. The Honorary Secretaries are: the Rev. C. D. Ginsburg, D.D., Prof. T. W. Rhys Davids, the Rev. E. W. Bullinger, D.D., Prof. A. A. Macdonell, M. M. Bhownuggree, the Raja Peari Mohan Mukharji (for Bengal), Prof. Peterson (for Bombay). Many eminent foreign scholars and members of former Congresses have signified their adhesion, and several important Societies have undertaken to send delegates. The sections into which the work of the Congress has been provisionally divided, are the following (the name of the President being in each case given first, that of the Secretary second):—I. Aryan, Prof. Cowell, Prof. A. A. Macdonell; II. Semitic (*a*) Assyrian and Babylonian, Prof. A. H. Sayce, T. G. Pinches, (*b*) General, Prof. Robertson Smith, A. A. Bevan; III. China and the Far East, Sir Thomas Wade, (for China) Prof. Douglas, (for Japan) Prof. B. H. Chamberlain; IV. Egypt and Africa, Prof. Le Page Renouf, E. Budge; V. Australasia and Oceania, Sir Arthur Gordon, Rev. R. H. Codrington, D.D.; VI. Anthropological and Mythological, Dr. E. B. Tylor; VII. Indian, Lord Reay, Prof. T. W. Rhys Davids; VIII. Geographical, Sir M. E. Grant Duff, Halford J. Mackinder; IX. Archaic Greece and the East, the Rt. Hon. W. E. Gladstone.

The Committee of the two International Congresses of Prehistoric Archæology and Zoology, which will be held at Moscow this summer in connection with the Geographical and Anthropological Exhibition, has announced, in accordance with a decision of the Russian Railway Department, that all members of the Congresses and exhibitors at the Exhibition may obtain tickets with a 50 per cent. reduction for travelling to Moscow and back. Exhibits may be sent and will be returned on the same terms. As there are at Moscou two different Societies, the Société des Naturalistes de Moscou and the Society of Friends of Natural Science (*Obschestvo Lubitelei Estestvoznaniya*), it may be worth while to note that it is the latter which is organizing the Exhibition and the two Congresses, and to which all applications for the Exhibition must be made.

It is stated that the Secretary of State for the Colonies has appointed Miss Doberck, formerly Government Meteorological Observer in Sligo, to be Assistant Meteorologist in Hong Kong. Miss Doberck's father has for some years past been the head of the Meteorological Observatory in Hong Kong.

Lieutenant-Colonel Holdich, of the Survey of India, will, it is said, personally superintend the mapping out of Captain Bower's journey across Tibet. The work will be done in the Survey drawing offices at Simla, where Captain Bower is at present engaged in preparing the report of his journey.

It is bad news for farmers that the diamond-back moth has made its appearance in Yorkshire and Northumberland. Specimens from both counties have been identified by Miss Ormerod.

The weather during the past week has been noteworthy for the occurrence of thunderstorms, copious rainfall at nearly all places, and excessive temperatures at most of the English stations. In London a severe thunderstorm was experienced on Thursday morning, May 26 (succeeding one that occurred the previous evening), with a heavy downpour of rain varying from 0·7 inch to 1·0 inch in different parts of the metropolis. At 8h. a.m. on Saturday the thermometer registered 76° in London, being the highest recorded at that hour this year. The type of wind has been cyclonic, with light or moderate south-westerly breezes generally. The Meteorological Office report for the week ending May 28, shows that the rainfall was equal to the normal value in the south and east of England, and exceeded it in all other districts; while in the northern parts, in Ireland and in Scotland, the fall was about three times as much as the mean. On Sunday the temperature was considerably lower, but since then it has again become abnormally high, the maxima in the shade registering 75° and upwards in places over the southern parts of the kingdom, 83° being registered in London on Tuesday; and thunder-showers occurred in various places on that day.

The detailed despatches brought to Marseilles from Port Louis by the mail steamer *Australien* confirm all that was stated in the telegrams relating to the hurricane which devastated Mauritius on April 29. A Reuter's telegram from Marseilles, giving a summary of the despatches, says that the total number of lives lost amounted to 1200, while the list of persons injured exceeded 4000. Strong magnetic disturbances were noticed on April 25, and continued with increasing intensity on the three following days. Several well-defined groups of sun-spots were also noticed at the same time. On the afternoon of the 28th, the eve of the hurricane, there was a vivid display of lightning and a good deal of thunder, while the air grew peculiarly heavy. On the following morning the tempest broke over the island in all its fury, the velocity of the wind at times reaching 112 miles an hour. The sea rose 9 feet above its usual level, a thing unknown since the terrible cyclone of 1818, when the water rose nearly four metres. In Port Louis itself houses fell to the ground in nearly every street. In the Tringlar quarter not a single house was left standing. In fact, there is scarcely a house in the entire colony which does not show some signs of the fury of the storm. Half the sugar crop has been destroyed. An immense number of persons were overwhelmed and killed by the ruins of the falling houses, or were stricken down in the streets, as they fled, by the falling stones and wreckage.

A very destructive cyclone passed over various towns in Kansas, on May 27. The storm gave no signs of its approach. Travelling in a north-easterly direction, it struck Wellington (a town containing a population of 10,000) at nine o'clock in the evening, when most people were indoors. Within a few seconds the central parts of the town coming within its track were devastated from end to end. Wellington Avenue, the principal business street, is lined on both sides with ruins, whole blocks of buildings having been shaken and overthrown as violently as if the place had been rocked by an earthquake. Numbers of victims were buried in the ruins, and of those who momentarily survived many were found struggling for their lives in order to escape from the flames which broke out in all directions in consequence of the sudden escape of gas. The towns of Harper and Argona were also visited by the cyclone. In the former town seven people were killed in the wreck of the buildings, and five at the latter. It is estimated that between twenty and thirty people lost their lives in the cyclone; while seventy others have been more or less injured.

On Tuesday, May 3, a fall of hail mixed with foreign particles was observed in Stockholm, and appears to have extended as far as Christiania. The fall of dust lasted from 1 to 8 p.m., and was abundant enough to allow of considerable quantities being collected. At a meeting of the Geologiska Förening in Stockholm, remarks were made by Baron Nordenskiöld, and Messrs. N. Holst, E. Svedmark, and Törnebohm, from which it appears that the dust contained glassy, isotropic, and various anisotropic particles, hornblende, magnetite, minute scales of mica, metallic iron, and some diatoms.

The Tiflis *Kavkaz* gives the following description of a meteor of great brilliancy which was observed at Tiflis, on May 10. It appeared at 11 p.m. in the west part of the sky, was of a round shape, and very brilliant. Three seconds after its appearance a part of it separated, moving towards the Mtatsminda Mountain, and disappeared below the horizon, after lighting the slopes of the mountain, the central meteor continuing to move, but having lost for a few seconds its great brilliancy, which, however, soon reappeared. In about 30 seconds after the first appearance of the meteor, a second small part separated from it, increasing in size as it approached the earth. This also disappeared in the west, behind the same mountain, after having brilliantly lighted for two or three seconds its slopes and gorges. After that, the meteor took first a milky coloration, but soon became bright again, and of phosphoric aspect. A third part separated from it, but it was much smaller and not so brilliant as the two former. Finally the meteor disappeared behind the clouds—a white, lighted blot being seen through them—and gradually faded away. The phenomenon lasted altogether about three minutes.

WE learn from the *Pioneer Mail* that a smart shock of earthquake was felt at Madras on May 6, about ten minutes to ten o'clock. The sound heard was at first like distant thunder, and afterwards like a railway train, running close by. The shock was distinctly felt. The weather was cloudy and the atmosphere still at the time.

THE *Annuaire Géologique universel*, founded by Dr. Dagincourt in 1885, and continued under the editorship of Dr. L. Carez for geology, and of M. H. Douvillé for palæontology, has now reached its seventh volume. Each year the work has increased in value, and it now affords an admirable *résumé* of geological literature. Hitherto each volume has been issued in a complete form, but the latest has appeared in four parts. By the arrangement adopted there is some repetition, but this enables information required to be readily obtained. There is first a fairly complete list of papers and other publications, then a systematic account of the various main chronological divisions of formations; this is followed by a description of separate districts; and finally we have a summary of palæontological work. The stratigraphical notes are not always complete in each volume, sometimes two years are grouped in one yearly issue; for instance, this volume contains no account of the Triassic and Tertiary rocks, whilst the Cretaceous works of 1890–91 are here included. The volume contains lists of geologists in France, Belgium, and the British Isles; next year we are promised lists for other European countries. The editors are assisted by a large staff of workers in various countries.

M. E. RIGAUX, of Boulogne-sur-Mer, who has devoted many years to the study of the geology of the Bas Boulonnais, has published an excellent account of this region in the *Mémoires de la Soc. Académ. de Boulogne* (vol. xiv., 108 pp.). The district is of especial interest to English geologists because of the fine development there of the Devonian and Carboniferous rocks, and of the Jurassic series from the Great Oolite upwards. The coal, formerly supposed to lie within the Carboniferous Limestone series, is now known to be true coal measures, over which the older rocks have been thrust. The paper gives an account of several important deep borings, in some of which Silurian rocks have been reached beneath the Jurassic series. Thirteen new species of fossils are described.

DURING the past season, Dr. Sheldon Jackson, the Government Agent of Education in Alaska, introduced into Alaska from Siberia sixteen reindeer. Next year he proposes to establish a herd of reindeer in the neighbourhood of Fort Clarence, and he expects to begin with 100 animals. The *Scientific American*, which records these facts, is of opinion that from an economical point of view the experiment is of the highest interest, because the reindeer is useful as a draught animal for sledges, as well as for its milk, its meat, and its skin. As it flourishes in Siberia, there seems to be no reason why it should not also flourish in Alaska, where the conditions of climate and vegetation are very similar to those of Siberia.

THE editors of the *Entomological Monthly Magazine* note that at the sale of the late Mr. Arthur Naish, of Bristol, at Stevens's Rooms on May 16, some of the extinct (or nearly extinct) species of British Lepidoptera fetched high prices. Seven examples of *Lycæna dispar* (the long extinct British form of *L. Hippothoë*) realized £16 8*s*., or an average of nearly £2 7*s*. each. A lot containing four *Polyommatus Acis* (perhaps extinct) was sold for 18*s*. Eight *Lælia cœnosa* (apparently recently extinct) were sold for £3 17*s*. 6*d*. Two *Cleora viduaria* (not found very recently) were knocked down for a guinea. Seven *Noctua subrosea* (long extinct as British, and the continental form of which, *subcærulea*, is very different in appearance) obtained £6 12*s*., one very fine example realizing £2 10*s*.

MR. C. W. DALE, writing from Glanvilles Wootton, records, in the June number of the *Entomologist's Monthly Magazine*, that the effect of the weather upon insect life in Dorsetshire during April was remarkable. Butterflies were unusually plentiful, moths unusually scarce. The conclusion he draws is that easterly winds, with frosts at night, are injurious to moth life, but do not affect butterfly life, so long as there is plenty of blue sky and sunshine. These were the general meteorological conditions in Dorsetshire during April.

MESSRS. LONGMANS, GREEN, AND CO. have issued a new and revised edition (the third) of Mr. W. A. Shenstone's "Practical Introduction to Chemistry." It contains the practical introductory course of work in use at Clifton College. In this edition the author has made several changes which have been suggested by his own experience and that of various friends.

THE forty-fourth part of Cassell's "New Popular Educator" has been published. It includes two coloured maps, one of Asia Minor, the other of Palestine.

CYANIDE of arsenic, $As(CN)_3$, has been prepared by M. Guenez, and is described by him in the current number of the *Comptes Rendus*. It has been obtained by the action of finely divided elementary arsenic upon iodide of cyanogen, CNI, a substance which is usually obtained crystallized in delicate, transparent needles, frequently attaining the length of several inches. About thirty grams of perfectly dry cyanogen iodide were placed in a strong Wurtz flask, together with seven grams of powdered arsenic and sixty to seventy cubic centimetres of carbon bisulphide previously dried over phosphoric anhydride. The air contained in the flask was then displaced by dry carbon dioxide and the flask sealed. The reaction was found to commence in the cold, crystals of tri-iodide of arsenic soon making their appearance. But, in order to complete the conversion of the iodide of cyanogen into arsenic cyanide, it was found necessary to heat the flask for about twenty-four hours over a water-bath. The heating is best carried out in successive periods of seven or eight hours, allowing the flask to cool after each period and subjecting the contents to brisk agitation. Under these circumstances a quantitative yield of arsenic cyanide was obtained, in accordance with the following equation:—

$$2As + 3CNI = AsI_3 + As(CN)_3.$$

In order to isolate the cyanide, advantage was taken of its insolubility in carbon bisulphide, arsenic iodide being readily soluble. The product of the reaction was therefore placed in a continuous extracting apparatus, in which it was thoroughly exhausted with pure carbon bisulphide. The residual cyanide was subsequently dried in a current of carbon dioxide, and preserved in sealed tubes previously filled with the same indifferent gas.

CYANIDE of arsenic obtained in the manner above indicated is a slightly yellow substance consisting of small crystals, which under the microscope are observed to be well formed, and to possess a deep yellow colour by transmitted light. The crystals are extremely deliquescent, being instantly decomposed by water with production of arsenious oxide and prussic acid:

$$2As(CN)_3 + 3H_2O = As_2O_3 + 6HCN.$$

When heated, arsenic cyanide evolves about a third of its cyanogen in the form of gaseous di-cyanogen, the residue consisting of a mixture of free arsenic and paracyanogen. When brought in contact with concentrated sulphuric acid and slightly warmed, mutual decomposition occurs, with liberation of sulphur dioxide and carbon monoxide, the nitrogen remaining in the form of ammonium sulphate. Iodine reacts with arsenic cyanide in an energetic manner, even in the cold, forming iodides of arsenic and cyanogen without the volatilization of any iodine. With potassium chlorate, arsenic cyanide forms a mixture which detonates with considerable violence when struck.

THE additions to the Zoological Society's Gardens during the past week include two Black-backed Jackals (*Canis mesomelas*) from South Africa, presented by Master Logan; two North African Jackals (*Canis anthus*), four —— Gerbilles (*Gerbillus* sp. inc.), an Egyptian Jerboa (*Dipus ægyptius*), six Leith's Tortoises (*Testudo leithii*), five Common Skinks (*Scincus officinalis*), an Egyptian Eryx (*Eryx jaculus*), a Schneider's Skink (*Eumeces schneideri*), two Crowned Snakes (*Zamenis diadema*), a Hissing Sand-Snake (*Psammophis sibilans*) from Egypt, presented by Dr. J. Anderson, F.R.S., F.Z.S.; a Cinerous Vulture (*Vultur monachus*) from Aden, presented by Mr. W. H. Still; a Common Peafowl (*Pavo cristatus* ♂) from India, presented by Colonel Bagot-Chester; two African Love-Birds (*Agapornis pullaria*) from West Africa, presented by Lady McKenna; a Chinese Goose (*Anser cygnoides* ♂) from China, presented by Miss Hill; two Common Vipers (*Vipera berus*), four Common Snakes (*Tropidonotus natrix*), a Slowworm (*Anguis fragilis*), British, presented by Mr. C. Browne; a Mocassin Snake (*Tropidonotus fasciatus*) from North America, presented by Master Denny Stradling; two Purple-capped Lories (*Lorius domicella*) from Moluccas, two Scaly-breasted Lorikeets (*Trichoglossus chlorolepidotus*) from Timor, deposited; four Common Sheldrakes (*Tadorna vulpanser*, 2 ♂, 2 ♀), four Ringed Doves (*Columba palumbarius*, 2 ♂, 2 ♀), European, purchased; two Black-eared Marmosets (*Hapale penicillata*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

WINNECKE'S PERIODIC COMET, 1892.—The following ephemeris for this comet has been extracted from *Astronomische Nachrichten*, No. 3083. The comet itself is becoming decidedly brighter, and will be found just between the Great Bear and Leo Minor.

Berlin Midnight.

1892.	App. R.A. h. m. s.	App. Decl. ° ′ ″	log Δ.	log r.	Br.
June 2	10 50 23·4	+43 11 46·0	9·9949	9·5376	8·61
3	48 52·4	4 31·4			
4	47 16·4	42 56 59·7			
5	45 34·6	49 10·6			
6	43 46·4	41 2·7	9·9837	9·5012	10·72
7	41 50·7	32 34·0			
8	39 47·0	23 42·0			
9	37 34·4	14 23·5			

SATURN'S RINGS.—At the present time the earth may be said to be very nearly in the plane of Saturn's Rings, thus affording observers an opportunity of examining the ring from the sectional point of view. M. Bigourdan communicates to the *Comptes rendus* (No. 21) the results of a study he has just made, with reference to some peculiarities he has found to exist. The preceding arm of the ring, he says, presented nothing very abnormal, but it appeared to thin rather than thicken whilst approaching the planet. The amount of thinning in the case of the following arm was much more striking. At a distance of two-thirds of the length of the arm it commenced to leave the outside edge, continuing gradually, and producing "the appearance of a luminous angle, regular and very pointed, the apex of which joined to the disk of the planet." Observed again on May 21, the following arm showed a protuberance situated near Cassini's division. In M. Bigourdan's own words, the above appearances could be produced by an "elevation of the level edging the separation of Cassini, and producing on the opposite face a luminous pad which would nearly double the apparent thickness of the ring."

GEOGRAPHICAL NOTES.

Petermann's Mitteilungen for June contains the long-expected account of Emin Pasha's return expedition to the equatorial lakes, written by his companion, Dr. Stuhlmann. Leaving Kahura in March 1891, they traversed an unknown region to the southern shore of Lake Albert Edward, which they followed round its western side, and marched as far north as the Ituri in 2° 13′ N. Here the party had to turn. Emin met a number of his old followers living near Kavalli, on Lake Albert, and many of them joined his expedition. The return journey was disastrous. An outbreak of small-pox made it necessary to divide the expedition; Emin was left behind, while Stuhlmann went on with those able to travel and reached the German station of Bukoba on Lake Victoria in February 1892. The scientific observations made necessitate certain corrections on the map of this part of Africa. Mount Mfumbiro is further west than originally supposed, and may even lie within the boundaries of the Congo State. It is a volcanic chain, one of the peaks of which, Mount Virungo, is apparently still active. Stuhlmann gives the level of Lake Albert Edward as 2750 feet instead of 3307 as determined by Stanley.

SIR WILLIAM MACGREGOR continues to give proofs of remarkable energy as an explorer, and of tact and skill as Administrator of British New Guinea. In the early part of this year he has been engaged in a series of journeys through the south-eastern districts of the possession, and everywhere he has found the natives peaceful and friendly. In a recent coasting trip he passed several islands, which at first sight appeared uninhabited, but on landing he discovered that this appearance was due to their singular configuration. A narrow belt of gently sloping land led from the sea to a steep wall of coral rock, from 300 to 400 feet high, from the summit of which an undulating plateau was seen dipping inland. Here the villages were built, from 50 to 100 feet below the level of the encircling rim, and sheltered from the trade-winds. Sir William considers these islands to be upraised atolls, modified in most cases by subsequent wave action on the shore strips.

LIEUTENANT T. H. BARNES, on behalf of the Bolivian Government and the Argentine Geographical Society, is investigating the navigability of the Rio Otuquis, a tributary of the Upper Paraguay, in the hope of opening a new route for Bolivian trade.

LIEUTENANT MIZON, compelled by the passive resistance of the Royal Niger Company to relinquish his projected journey by the Benué to Lake Chad, has (according to *La Politique Coloniale*) crossed the watershed to the Congo Basin, and on his way from the Benué to the Sangha, traversed the hinterland of the German Cameroon colony, one of the rapidly-shrinking blank spots on the map of Africa. Reports from Tripoli state that Captain Monteil is pushing on from Kano, in Sokoto, to Bornu and Lake Chad.

THE collection of educational appliances, books, atlases, &c., made by Mr. J. Scott Keltie on behalf of the Royal Geographical Society in 1885, has been lent to the Teachers' Guild of Great Britain and Ireland, in whose rooms at 74 Gower Street it has been admirably arranged by Miss Busk. This collection, which after its original appearance in London was shown in most of the large towns in the country, has never been more effectively displayed. When first brought together, the inferiority of the British school atlases and text-books to the German and French productions was very marked; but during the last seven years English publishers have made great advances, and several of the newer and better publications have been added. It would be most desirable if the department of English books, maps, and geographical appliances could be made thoroughly representative, so that teachers would have a real opportunity of comparing the best work of 1892 with that which was current in 1885.

MR. H. J. MACKINDER, Reader in Geography at Oxford, has recently returned from a brief visit to the United States, where he has been devoting special attention to the state of the higher geographical teaching. In many respects the American Colleges are in advance of the corresponding institutions in this country, and geography is attracting increased attention on the part of some of the most energetic and progressive educationists.

COUNT PFEIL, who recently passed through London, *en route* for the Cape of Good Hope, is about to conduct a party of emigrants to Walfisch Bay, in the hope of colonising the adjacent parts of German South-West Africa.

Two pillars, erected by Diogo Cão, the first Portuguese explorer on the west coast of Africa, have recently been brought back to Lisbon. An interesting circumstance is the discovery on the pillar brought from Benguela of an inscription showing that the coast had been traced so far in 1482, two years earlier than the date usually assigned.

THE IRON AND STEEL INSTITUTE.

THE annual meeting of this institution was held on Thursday and Friday of last week, at the Institution of Civil Engineers, Sir Frederick Abel, the President, occupying the chair. After the reading of the Council's annual report Sir Frederick Abel delivered his Presidential address. He began with a reference to the losses which the Institute had sustained during the year by the death of some of its eminent members. He spoke especially of the solid services rendered to science by the late Duke of Devonshire. The Duke's wise munificence in the establishment of the Cavendish Laboratory in Cambridge University, and the important part he took in the labours of the Royal Commission on scientific instruction and the advancement of science, were described as "illustrations of his active participation in a movement of most vital importance to the maintenance of our position and influence among nations." Sir Frederick also referred to the Duke's ready consent to fill the post of first president of the Iron and Steel Institute as a proof of his appreciation of the high importance to be attached to the successful foundation of an organization of which he predicted that it would prove "a powerful instrument for the advancement and progress of the iron and steel trade of Great Britain, by promoting intercourse and interchange of knowledge between its members"—a prediction which was speedily and amply fulfilled. In his introductory address the Duke had discussed the development of iron-manufacture in most interesting and comprehensive fashion. In referring to the most extraordinary mineral wealth of the United States, he pointed out that although in 1867 the production of pig iron in America had risen to nearly 1,350,000 tons (of 2240 lbs.), the price of labour did not warrant the belief that there was any immediate prospect of the United States competing with the iron-producing countries of Europe in the open markets of the world.

Sir Frederick continued :—

A very interesting report upon the state of iron manufacture was presented by Sir Lowthian Bell to the British Association at its meeting in Dundee in 1867. A critical examination was made therein of the relative position of ourselves and Continental nations as iron manufacturers, *à propos* of the Paris International Exhibition of that year ; but in the encouraging view which that eminent authority presented of our position at the period named he was not led to make any reference to the prominence which the United States were beginning to assume among iron-producing countries. After the lapse of twelve years, however, the production of pig iron in the States had been doubled, while in another ten years it had reached a figure approximating to the average production in Great Britain during the past ten years.[1]

Viewed from our present standpoint, the observations made by our first President in his opening address of 1867, regarding the development of the manufacture of steel, are very interesting. The Duke pointed out that, "owing to recent inventions and improvements, steel had acquired an importance greatly exceeding that which it previously possessed." After referring to the then prevalent views regarding the nature of steel, and to its production by the cementation process, the puddling and the mixing processes, and the partial decarbonisation of cast iron by blowing air into pig iron melted in a charcoal hearth, he dwelt upon the interest with which the development of the Bessemer process had been watched by the iron-making world, upon the *promise* "which that process afforded of furnishing a supply of steel suitable for many most important purposes upon a scale and at a price hitherto unknown," and upon the association of the names of Joseph Heath with the first employment of manganese in steel manufacture, and of Robert Mushet with the important part played by manganese-alloys in the development of the Bessemer process. While the approaching expiration of the first Bessemer patent was referred to as likely to tend to an increase in the demand for its products, the limits which the then existing knowledge placed upon the application of the process were pointed out, and the advantages of the puddling process dwelt upon. It is interesting to note that, at any rate in Germany, these advantages have not yet been dispelled, in spite of the great revolution which the Bessemer and open-hearth processes have effected in the applications of wrought iron and steel. On the other hand, the importance which steel had acquired through the practical development of the Bessemer-process, at the date of our first President's address, was but an indication of the new era upon which the iron and steel industries were about to enter. In that year the produce of Bessemer-steel in the United Kingdom was only 160,000 tons, while open-hearth steel was not yet a staple product ; in 1890 the British production of Bessemer steel exceeded two millions of tons, while that of open-hearth steel exceeded 1·5 millions of tons.[1]

A statement made in the Duke's address of 1869, that, so far as existing knowledge went, the Bessemer process was of limited application, as only certain kinds of iron were susceptible of successful treatment by it, affords, by a comparison with the present condition of things, an interesting illustration of the continuous progress made in the successful application of advances in scientific knowledge to practical purposes. The success which crowned the efforts of Thomas, Gilchrist, Snelus, and others to render the Bessemer- and open-hearth processes efficient in their application to ores, the successful treatment of which by them appeared well-nigh hopeless in the earlier days of the Iron and Steel Institute's existence, has recently been very prominently before the public, and the members will certainly receive with special interest the communication which the Director of Naval Construction has promised us on experiments with basic steel.

In the discussion which took place at the meeting of the Institute of Naval Architects last year, *à propos* to a paper by M. J. Barba on recent improvements in armour plates, it became evident that the public were far better instructed as to progress made in such directions as this by other nations than as to advances made by ourselves ; such information as Mr. White feels warranted in affording us with respect to our progress in practical experience on the merits of basic steel as applied to shipbuilding and other naval purposes will therefore be very welcome.

From the United States interesting accounts have reached us of a continuation of the experiments with armour plates 10½ inches thick, which were commenced at Annapolis in September 1890, when an all-steel and a nickel-steel plate, from the Creusot works, were contested in comparison with a compound plate of Cammel's make. Of these, the nickel-steel plate was considered to have shown itself somewhat superior to the all-steel plate, and very decidedly superior to the compound plate ; and it is stated that Congress showed its appreciation of the importance of this result by appropriating a million dollars to the purchase of nickel ore. The second and third series of trials have been carried out at the Naval Ordnance Proof Ground at Indian Island, near Washington. The plates fired at in October last, constituting the second series of three, are described as a high carbon nickel-steel plate from the Bethlehem Iron Company, one of low carbon nickel-steel from Carnegie, Phipps, and Co.'s works at Pittsburg, and a so-called "Harveyised" plate of low carbon steel from the Bethlehem works. The description given of the Harveyising process

[1] Mr. Robert S. M'Cormick, Resident Commissioner for Great Britain for the Chicago Exhibition of 1893, in a paper recently communicated to the Society of Arts upon the future trade-relations between Great Britain and the United States, gives the following figures as demonstrating that the British iron and steel industry has been outstripped in magnitude by that of the United States. In 1890 the produce of pig iron in America was 9,202,703 tons against 7,875,130 tons in the United Kingdom ; of manufactured iron, including rails, 2,820,377 tons were produced in America against 1,923,221 tons in Great Britain, and, of Bessemer steel, 3,688,871 tons were American produce, while the production in the United Kingdom amounted to 2,014,843 tons.

[1] 1891 appears to have witnessed a very remarkable falling off in the production of Bessemer steel, to the extent indeed of about 35 per cent., while the production of open-hearth steel exhibits a reduction in the past year of only 3 per cent.

identifies it as being a case-hardening or partial cementation treatment, the surfaces of the steel plate being hardened by carbonization (and by a supplementary chilling process), and the increase in carbon dying away towards the interior of the mass. In the trials of these plates, that of high carbon nickel-steel appears to have stood the best, but the effect of the Harveyising process upon the powers of resistance of the low carbon steel plate seems to have afforded indications of beneficial effect such as to warrant the application of the process to nickel-steel plates included in the third series, fired at last November, and which comprised a high carbon nickel-steel plate from Carnegie, Phipps, and Co., a low carbon Harveyised nickel-steel plate from the same makers, and a high cabon Harveyised nickel-steel plate from the Bethlehem Company. In all the nickel-steel plates, including that from Creusot tried in 1890, the amount of nickel in the metal appears to have been a little above 3 per cent.

Care seems to have been taken to render all conditions attending the trials as uniform as practicable, with this not unimportant difference, that very much less time was allowed to elapse, in the second and third trials, between the firing of the successive rounds than in the first experiments.

A careful consideration of the results led the Board, of which Admiral Kimberley was President, to the unanimous conclusion that the high carbon Harveyised nickel-steel plate was the best, but that one part of the plate was much superior in resisting powers to the other, which was ascribable apparently to a want of uniformity in the Harveyising or carbonising treatment. The official report is also said to have recorded the unanimous opinion of the Board that both the high carbon nickel-steel Harveyised plate and the high carbon nickel-steel untreated plate were superior to the Creusot nickel-steel plate tried in 1890.

Further trials will shortly be made of high and low carbon nickel-steel Harveyised plates, to be supplied by Carnegie, Phipps, and Co. From published analyses it appears that the high carbon nickel-steel plate manufactured by that Company contained 0·45 per cent. of carbon and 0·65 per cent. of manganese, and the low carbon plates of nickel-steel, 0·26 per cent. of carbon and 0·75 per cent. of manganese.

The *New York Sun*, with what appears, from the reported results, to be very justifiable sentiments of pride, winds up an account of the results arrived at with the remark that they show that America now stands at the top in the excellence of her ship-armour; and certainly our friends of the Bethlehem and Pittsburg Steel-works are to be warmly congratulated upon their achievements in this new direction.

Although the trials in the United States seemed to establish a marked superiority of nickel-steel plates over the compound plates of J. Brown and Co.'s manufacture, it is interesting to notice that this eminent firm is gallantly striving to maintain the high position which exhaustive trials had secured to that form of plate, as efficient armouring for ships of war, and that recent trials at Shoeburyness and at Portsmouth of experimental compound plates which have been submitted to a supplemental process devised by Captain Tressider, late Royal Engineers, seem, so far as I can learn, to have demonstrated that powers of resistance and endurance, much exceeding those of the compound plates tried in the United States and in the Ochta experiments of last year, can be secured to these structures. I have reason to hope that we shall receive a communication ere long on the interesting results which are being obtained in this direction.

My reference to the rapid advance which has been made in the United States in the manufacture of armour plates will recall to the minds of many here present the memorable visit of the Institute to America in 1890. The valuable record of that visit presented to us a few months ago by the American Institute of Mining Engineers, in the form of a portly volume, embracing full accounts of the proceedings and the papers read and discussed at the international meetings at New York and Pittsburgh, constitutes an important work of reference, as well as an interesting memento of one of the most notable events in the history of our Institute. And now, thanks mainly to the self-sacrificing exertions of our much-esteemed past President, Sir Lowthian Bell, we have been able to match this American volume with a companion work, the interest and value of which it would, I venture to say, be difficult to over-estimate. The series of monographs by Sir Lowthian Bell and other highly competent authorities which are embraced in this special volume (aptly named "The Iron and Steel Institute in America"), will certainly receive careful study—productive both of profit and of pleasure, at the hands not only of members of the Institute, but also of our transatlantic friends who so cordially received us; and our warmest thanks are due to the joint authors of this work, and especially to Sir Lowthian Bell, who, besides contributing some of the most valuable of its contents, undertook the arduous task of editing the volume; and I beg leave heartily to congratulate him upon the realization of his wish, that the completed work should make its first appearance in public at this meeting.

An interesting illustration afforded by the inaugural address of the Duke of Devonshire in 1869 of the advance made in the knowledge at the disposal of the iron and steel maker, is found in his observations on the relations of carbon to iron and steel, a subject which I had occasion to discuss in some detail in my opening address last year, in reference to the then recent interesting investigations in that direction; a subject on which we may still hope to have further light thrown by the continued researches of Osmond and others. The very complete and systematic manner in which existing knowledge on this subject is treated in the first part, recently published, of Dr. Hermann Wedding's new edition of his *Handbuch der Eisenhüttenkunde*, calls for the highest commendation.

The importance of pursuing the investigation of problems such as the conditions most favourable to economy in the use of fuel in the blast furnace, and conditions to be fulfilled in the form and dimensions of the furnace for ensuring efficiency and economy of working, were dwelt upon by the Duke of Devonshire in his comprehensive address, and reference was made to the elaborate inquiry into the chemical operations occurring in the blast furnace upon which Sir Lowthian Bell had then for some time been engaged. The interesting results arrived at by him, and the instructive discussions to which they and the conclusions based upon them gave rise, are memorable illustrations of the progress in the application of scientific research and reasoning to the study of metallurgic operations, in the promotion of which the Iron and Steel Institute has of late years played an important part, and the most recent outcome of which, in connection with the production of pig iron, is to be found in the remarkable achievements recounted by Mr. Gayley in his paper on the development of American blast furnaces, which was one of the most noteworthy communications dealt with at the New York meeting in 1890.

In directing attention to that paper in my address a year ago, I spoke of the reference made by Mr Gayley to the importance of the elaborate series of investigations carried out, nearly a quarter of a century ago, by Sir Lowthian Bell on the chemistry of the blast furnace. I have had occasion since then to refresh my memory with regard to the ground covered by the work which our ex-President then carried out, and I must freely confess that I had no recollection of its extent nor of the mass of interesting and important experimental data accumulated by him, until I lately referred to the comprehensive and systematic investigations on the chemical phenomena of iron-smelting, which he communicated in detail to the meeting of the Institute at Merthyr in 1870. His paper on the chemistry of the blast furnace, to which I had the pleasure of listening at the Chemical Society in 1869, deals with the circumstances and conditions attending the union of iron with carbon in the blast furnace, and gives interesting results bearing upon the question of the temperature at which carbon is deposited in a finely-divided state in ironstone when it is exposed to the waste gases, rich in carbonic oxide, of the blast furnace; and the subject is discussed more definitely, by the light of additional experimental information, in Sir Lowthian Bell's admirable work on the "Principles of the Manufacture of Iron and Steel," published in 1884. He there demonstrates the readiness with which carbon is desposited in iron sponge from carbonic oxide at temperatures up to a dull red heat, examines into the question whether the presence of iron in the metallic state in iron ore is indispensable for determining the dissociation of carbonic oxide, and, after concluding in the negative, and demonstrating that metallic iron is not more active in this respect than its oxide, he refers to trials made by him of the power of several other metals and metallic oxides to effect the dissociation of carbonic oxide.

In the researches communicated in 1870 to the Merthyr meeting, the whole of these experiments are given and discussed, and it is to be regretted that the interesting results arrived at did not receive greater publicity than they met with in the Transactions of a young technical Institute, then comparatively unknown in the scientific world, and in which the results of original scientific research would scarcely be sought for.

Without venturing to enter into the details of these researches, I may mention that experimental evidence favoured the conclusion that the carbon-impregnation of an iron ore by dissociation of carbonic oxide takes place at as low a temperature as de-oxidation, which, in the case of Cleveland ore, occurs at 200° to 210° C. (392° to 410° F.), and that freshly reduced spongy iron, at about that temperature, reduces carbon from carbonic oxide to an extent corresponding to 20–24 per cent. of its weight, but that, as the temperature approaches a red heat, the deposition of carbon diminishes considerably in amount.

The results of many experiments with other metals and their oxides showed that zinc, tin, chromium, and silicon, and their oxides are neither reduced by carbonic oxide at a temperature of about 420° (that of melted zinc), nor give rise to deposition of carbon; that copper and lead are reduced at temperatures up to a red heat, without deposition of carbon; that the higher manganese oxides are reduced to protoxide below a red heat without impregnation by carbon; but that nickel, and in a smaller degree cobalt, suffer reduction from their oxides, with deposition of carbon.

Sir Lowthian Bell, while conducting his experiments at temperatures considerably below those prevailing at the particular positions in the blast furnace where the production of pig iron was believed to be effected, did not have recourse to so low a temperature as that at which Messrs. Mond, Langer, and Quincke, after having demonstrated (what appeared, in the absence of an acquaintance with Bell's results, a novel observation) that carbon was separated from carbonic oxide by passing the gas over nickel at a high temperature, found that this metal actually entered into the composition of the gas. Thus they started from the point, in this particular direction, up to which Sir Lowthian Bell had carried his observations twenty years previously, and obtained the remarkable nickel-carbon-oxide compound referred to by me in my address last year, which they have since succeeded in producing upon so considerable a scale as to afford prospect of its acquiring industrial importance.

In the description of their earliest results, they stated that attempts to produce similar combinations of carbon oxide with other metals, including iron, had failed. By persevering with research in many very varied directions, and especially with iron, they at length succeeded in volatilizing notable, although small, quantities of the latter metal in a current of carbonic oxide, by using the finely-divided pure iron obtained by reducing the oxalate in a current of hydrogen at the lowest possible temperature (about 400° C.), by allowing the product to cool in hydrogen to 80°, and then by passing a current of carbonic oxide over the spongy metal. The gas, after this treatment, was found to impart a yellow colour to a colourless flame, and if conducted through glass tubes heated to between 200° and 350° C., it deposited a metallic mirror; at a higher temperature it furnished black flakes, which analysis showed to contain 79·30 per cent. of carbon. The quantity of iron- and carbon-oxide compound produced in this way was very small; by passing 2½ litres of carbonic oxide per hour over the metal (the latter being from time to time reheated in a current of hydrogen), the issuing gas contained not more than 0·01 grm. of iron, equal to less than 2 c.c. of the gaseous iron-compound in a litre of the carbonic oxide.

The gas-mixture, when passed through benzine or heavy mineral- or tar-oils, was partially deprived of the iron compound, and the result of examination of solutions of this kind led to the conclusion that the gaseous iron combination, analogous in composition to the nickel-carbon-oxide compound (or nickel-tetra-carbonyl), had been found, and that its formula was $Fe(CO)_4$.

In continuing their researches, Mr. Mond and Dr. Langer have succeeded in obtaining the iron compound in the form of a liquid of spec. grav. 1·4664 (at 18° C.), which distils without decomposition at 102°·8 C., and solidifies below 21° into yellow needle-shaped crystals. It is slowly decomposed by exposure to air, and when its vapour is heated to 180°, it is completely decomposed into iron and carbonic oxide. Analysis and the determination of its vapour density show the composition of the liquid to be represented by the formula $Fe(CO)_5$, and Messrs. Mond and Langer have therefore called the compound *ferro-penta-carbonyl*.

If exposed to light in a sealed vessel for several hours, it deposits gold-coloured tabular crystals having a metallic lustre like gold when dry, but becoming brown by gradual decomposition when exposed to air. These crystals appear to contain a slightly smaller amount of carbonic oxide than the liquid compound from which they are deposited. Mr. Mond and his co-laborateurs are still actively engaged in pursuing the researches which have brought to light the formation of these remarkable metallic compounds, whose discovery and properties suggest possibilities in several directions of technical chemistry which will doubtless lead to interesting investigations.

The first report made by Prof. Roberts-Austen to the Alloys Research Committee of the Institution of Mechanical Engineers, bearing upon the particular investigation undertaken by him at their request, although little more than introductory in its character, is full of interest, and of importance not only on account of the valuable information it furnishes regarding the method of investigation adopted by him and of the preliminary results attained by its agency, but also because of the interesting discussion elicited by its presentation to the Institution of Mechanical Engineers at their meeting last autumn.

At our annual gathering a year ago we had the advantage of receiving from Prof. Roberts-Austen a description of the autographic method adopted by him for recording the results indicated by the Le Chatelier pyrometer, the efficient operation of which I had an opportunity of witnessing as a member of the Alloys Research Committee. We also heard from Sir Lowthian Bell that he had already successfully and very usefully applied this pyrometer to determine the temperature of the blast entering a furnace at a considerable distance from the point of observation. We shall, I trust, have the advantage of learning the results of further experience by Sir Lowthian and others in the practical application of this much-needed instrument in conjunction with the automatic recording system used by Prof. Roberts-Austen; the observations made by the present President of the Institution of Mechanical Engineers, by Mr. Henry M. Howe, of Boston, and by others at the discussion of the Professor's report, demonstrated that several valuable applications were already being made of the Le Chatelier pyrometer and the system of continuous record of its indications. While it is satisfactory to me, as one of the earliest to use the ingenious pyrometer designed by my distinguished friend the late Sir William Siemens, to note that its trustworthiness as an indicator of temperatures up to 500° C. has been vindicated by the work of Messrs. Callendar and Smith, the accuracy and sharpness of the indications of Le Chatelier's pyrometer, the simplicity of its character, and the well established trustworthiness of its results at temperatures of over 1000° C., render it decidedly more valuable to the practical metallurgist, as well as to the scientific investigator, than any instrument of the class hitherto available. We shall none the less be glad to hear what Mr. Callendar has to tell us on the present occasion with regard to the results of his persevering and, I believe, successful labours in discovering and eliminating the defects of construction which served to destroy the confidence placed, in the first instance, on the indications afforded by platinum pyrometers.

The tendency of the discussion following the reading of Prof. Roberts Austen's report, which was shared in by Mr. Robert Hadfield, Mr. Henry M. Howe, and by some others whose right to criticism was beyond dispute, was to emphasise the necessity for caution in the application of theoretical views, regarding the laws which regulate the mechanical or physical properties of metals, to predictions as to the influence upon the properties of metals, such as iron, of particular impurities. I believe no one will be disposed to differ from the view expressed on that occasion by Prof. Arnold, that, for a thoroughly comprehensive examination into "the effects of small admixtures of certain elements on the mechanical and physical properties of iron, copper, lead, and other metals," it is indispensable to combine different lines of investigation with the particular one which Prof. Roberts Austen has so far prosecuted with very promising results.

The fame which Mr. Gruson has acquired in connection with the production of chilled iron structures, for land defence, presenting marvellous powers of resistance, must cause members of the Institute to look forward with much interest to the communication which has been promised us by the director of the Gruson Works, Mr. E. Reimers, on the manufacture and application of chilled cast iron, a subject with which, especially in regard to the selection of varieties and mixtures of iron suitable for securing a structure of metal essential to the attainment of combined toughness and hardness in armour-piercing projectiles, I was much concerned in the days of my much-lamented friend, the late Sir William Pallisser.

Another subject to which I devoted considerable attention twenty-three years ago, in co-operation with the late Dr. Matthiessen, bears directly upon some very interesting results which will be brought to your notice by my old and valued friend Colonel Dyer, of the Elswick Ordnance Works. In 1863 Dr. Matthiessen communicated to the British Association the results of some interesting researches into the chemical nature of alloys, which he followed up in 1866 with a preliminary report on the chemical nature of cast iron. In this memoir, after drawing a comparison between the physical deportment of what he terms the alloys of carbon and iron, and those of such alloys as are produced by copper with zinc and with tin, he discusses in some detail the question whether carbon exists in combination with iron, in cast iron, and expresses himself in favour of the view that white iron is not actually a chemical combination of carbon with the metal, but rather a solidified solution in it of carbon, while grey iron is a solidified solution of the same kind, with carbon mechanically intermixed. But while he supports this hypothesis by certain analogies between the specific electric conducting power of different varieties of iron and of alloys of other metals, he proposes to test the validity of his views by preparing pure iron, alloying it with various proportions of carbon, examining the physical and chemical properties of these alloys, and afterwards investigating the properties of alloys of the pure metal with various other metals and non-metals. Matthiessen's persevering endeavours to elaborate a process for the preparation of pure iron, which extended over three years, were at length crowned with success, and in 1869 I was engaged with him upon experiments with metal, obtained in the form of sponge, containing as its only impurity a minute trace of sulphur. This iron was prepared by fusing together perfectly pure and dried ferrous sulphate and sodium sulphate, completely washing the crystalline oxide thus obtained, and then reducing it to metallic sponge by heating it in thoroughly purified hydrogen. The sponge metal was welded together in the cold by powerful compression, for certain experiments; for others it was fused in very carefully prepared lime-crucibles. The experiments which it was hoped to undertake with this pure material, in the directions I have indicated, were arrested almost at their commencement by Dr. Matthiessen's death, and by the all-engrossing nature of my official labours. The process, which was elaborated with such trouble, may perhaps prove useful in connection with the investigation which Prof. Roberts-Austen has undertaken for the Alloys Research Committee of the Institution of Mechanical Engineers; but it appears to me that there is good prospect of procuring iron sufficiently pure, at any rate for certain of the experiments (when carried out upon a practical scale) which will form part of these investigations, by the very simple mode of procedure which Colonel Dyer has adopted in the production of iron containing only traces of carbon and silicon, no phosphorus, and less than two-hundreths of one per cent. of sulphur, and which affords a very interesting instance of the application of the basic furnace.

In concluding these few observations, I cannot forbear once more referring to the interesting address delivered to the members by our first President twenty-three years ago, in order to point out how strikingly its peroration illustrates the progress which has been made in the development of the steel industry during the past twenty-two years. While forcibly dwelling upon "the extraordinary influence which the manufacture of iron had come to exercise on the condition of society throughout the civilized world," the writer gives no indication of the part then played, or destined to be played, by *steel* in that civilizing influence. Even seven years later, when steel-manufacture had advanced with rapid strides, there was still great hesitation in adopting it for some of the most important purposes to which iron was applied; thus Sir Nathanial Barnaby wrote at that time, "Our distrust of steel is so great that the material may be said to be altogether unused by private shipbuilders." Yet, a few years afterwards, it had come to pass that the examples of the marvellous development in the applications of iron, to which the Duke referred in illustration of his statements, constituted the very directions in which the steel manufacturer has accomplished his most prominent achievements, and in which the use of iron is becoming a memory of the past.

The following was the list of papers to be read:—On experiments with basic steel, by W. H. White, C.B., F.R.S., Director of Naval Construction; on the production of pure iron in the basic furnace, by Colonel H. S. Dyer, of Elswick; on experiments on the elimination of sulphur from iron, by E. J. Ball, Ph.D., and A. Wingham, F.I.C.; on platinum pyrometers, by H. L. Callendar; on the manufacture and application of chilled cast iron (Gruson's system), by E. Reimers, of Magdeburg; on valves for open hearth furnaces, by J. W. Wailes; on the calorific efficiency of the puddling furnace, by Major Cubillo, of Trubia Arsenal, Spain; on a practical slide rule for use in the calculiation of blast furnace charges, by A. Wingham, F.I.C.; notes on fuel, and its efficiency in metallurgic operations, by B. H. Thwaite.

The whole of these papers were read except that by Major Cubillo.

Mr. White's was the first contribution taken. His paper was founded on a number of experiments made at Pembroke Dockyard, with a view to determine the suitability of steel made by the basic process for ship-building purposes. It would be useless to attempt to summarise the results of the large amount of information contained in the paper, and in the tables, which formed an appendix to it. As a general fact, it may be said that basic steel no longer labours under the disadvantages that attended its early days, when it was undoubtedly unfit to be used as a ship-building material. The importance of the basic process to this country can hardly be overrated. The manufacture of steel on the original acid process demands a pig low in phosphorus, and this can only be prepared from a special ore, such as the hematites of Cumberland and other parts. Unfortunately, the deposits of such ore in the British Isles are very limited in extent, and it is for this reason that we have been, for years past, importing vast quantities of steel-making ore from the neighbourhood of Bilboa in Spain. This means a heavy item for freight; and it is a question whether we could, in England, stand the competition of Spain, if that country once organized her steel-making resources on a sound footing. But in any case it is desirable we should depend, as little as possible, on foreign countries for raw material, when we have such vast stocks within our own borders. In the ores of the Cleveland district and other parts, we have such deposits, but the ore contains a comparatively large percentage of phosphorus, which entirely unfits it for the old acid process of steel-making. The basic process, however, is designed to enable phosphoric pig to be used, and to judge by Mr. White's paper a fair measure of success has been attained in this direction. It would have been interesting if the paper had given details as to the pig from which the steel was made. Mr. Martell has said that no steel to meet Lloyd's requirements has been made from pig containing 3 per cent. of phosphorus, and that the basic steel which has been successful has been made from an ore low in phosphorus. It is, however, not the bulk of the phosphorus which is difficult to eliminate, but the last part, and Sir Lowthian Bell stated that he would be glad if the pig of his district di l contain 3 per cent. of phosphorus instead of about half that quantity, as it would then produce a slag more valuable for fertilizing purposes. However this may be, it would have been satisfactory to have had full analyses of pig to attach to particulars of physical tests so well authenticated as those now given to the engineering world by Mr. White's paper. Another point upon which it is desirable to get information is, which process gives the best results in working on the basic principle? We have always considered it a settled matter that the open-hearth furnace was superior to the converter in this respect, so far as the quality of the product is concerned, and the discussion of last Thursday, on the whole, tended to confirm this opinion. The opposite view, however, was advanced by more than one speaker whose words should carry weight; and there is also the question of cost and quickness of production to consider. On the whole, it would seem, therefore, that the problem as to whether the converter or the open-hearth furnace should be used is still an open one; doubtless it will be settled in this case, as before, by the special requirements of the metal to be produced. As we have said, we cannot reproduce even a brief abstract of Mr. White's paper, but we can give one or two figures. One sample of basic Bessemer had a tensile strength of 30·6 tons per square inch, and an extension in 8 inches of 26 per cent. Of some pieces tested after annealing the tensile strength was about 28 tons, with an elongation of 25 per cent. One sample of basic open-hearth showed 31·3 tons per square inch tensile strength and 26·2 per cent. extension in 8 inches. We quote these figures as showing the best results, and to serve as a guide ; it is doubtless unnecessary to say that they are not conclusive standing alone. The riveting tests given are valuable, but these are of a nature which cannot be epitomized.

Colonel Dyer's paper might well have been longer, as the subject of it is one of considerable importance. Pure iron is a substance at any rate difficult to get. Sir Lowthian Bell has said he has never met with absolutely pure iron. Commercially pure iron, or what might be called practically pure iron, is not uncommon. Colonel Dyer's object was to obtain a pure iron in order to determine the value of alloys. By working on the lines which he had followed, the author hoped that pure iron and steel may be produced at reasonable cost. In the first experiments the furnace was charged in the ordinary manner with pig and scrap of fairly good quality, and the charge was worked slowly, care being taken to keep the slag well saturated with lime by liberal additions of limestone. The phosphorus was reduced during the process, but the result left much to be desired in other respects. Charges composed of from one-half to four-fifths of good scrap, and one-half to one-fifth of good Swedish pig were then worked very quickly, and a remarkably pure iron was obtained, of which the following was the result of analysis :—

Combined carbon	trace
Silicon	·005
Manganese	trace
Phosphorus	trace
Sulphur	·015

This iron could only be forged in small pieces, even with the greatest care, and therefore no results could be given as to its mechanical properties. Dr. Hopkinson had determined the magnetic properties of the metal, but the results are to be reserved for the Royal Society. Speaking generally, it has been found that the metal is more easily magnetized for small magnetizing forces than any other metal hitherto tested ; its coercive force is less, its magnetization is greater, than any other sample experimented with. The next stage of Colonel Dyer's experiments had for their object the utilization of ordinary scrap steel, and the production, in the basic furnace, of steel high in carbon and low in phosphorus, and at the same time to decrease the wear and tear of the furnace. The principle of the process consists in melting scrap with carbonaceous material, and the results of the experiments have shown that when a pure carbonaceous material and ferro-manganese free from phosphorus can be obtained there will be no difficulty in producing a pure carbide of iron containing only sufficient manganese for forging. The author next described the method by which the process was carried out. Nine consecutive charges were worked, with the object of producing steel containing varying percentages of carbon, to test the value of the process. The following table gives the chemical analyses and the mechanical properties of the steel of these charges :—

Tensile Tests.					Chemical Analysis.				
No.	Yield.	Break.	Elongation.	Fracture.	C.C	Si.	Mn.	P.	S.
			Per cent.						
1	14·0	22·7	44·0	F.	·11	trace	·21	trace	·030
2	14·0	23·0	41·5	F.	·10	,,	·21	,,	·030
3	18·0	27·5	32·0	F.	·16	·020	·40	·018	·022
4	21·0	30·0	33·0	F.	·21	trace	·39	·014	·026
5	20·0	31·2	32·5	F.	·25	·014	·43	trace	·019
6	23·0	34·0	26·0	F.	·24	·018	·50	·019	·024
7	25·0	35·4	20·0	F.	·30	trace	·38	·019	·017
8	25·0	43·2	18·5	F.	·53	·012	·54	·016	·028
9	24·0	45·3	14·5	F.G.	·50	·031	·60	·009	·026

The paper by Messrs. Ball and Wingham, on the elimination of sulphur, contained the results of experiments thoughtful and suggestive in themselves, even if they do not show the iron and steel maker any immediate results which he may apply. The authors found that potassium cyanide placed on the surface of molten cast iron almost completely removed the sulphur. Owing to the extreme volatility of the cyanide, it was not found possible to reduce the quantity required to within practical limits, and efforts were therefore made to find some flux which would retain, when molten, a quantity of cyanide sufficient to effect the desulphurisation. Sodium carbonate, lime, and blast-furnace slag were in turn tried. It was found that the desulphurising action was greater when the flux consisted mainly of sodium carbonate than when a less basic lime slag was used ; and that in the latter case the diminution in the percentage of sulphur varied directly with the amount of added cyanide. A table is given of the results of the experiments, the best condition being obtained when 200 grains of sodium carbonate and 100 grains of potassium cyanide were used to 2000 grains of metal, when the sulphur was reduced from 0·46 per cent. to 0·06 per cent. A further experiment with sodium carbonate alone—400 grains being added to 4000 grains of metal—the sulphur was reduced from 1·11 to 0·15 per cent. With caustic soda the sulphur was reduced from 0·15 per cent. to 0·02 per cent., which is a satisfactory result. The experiments also showed the facility with which sulphur is reduced when present in large quantities, and that it is the last part which gives difficulty in removing. Metallic sodium was introduced into the bath in the form of an alloy with lead, and this had the effect of entirely removing 0·18 per cent. of sulphur.

The paper by Mr. Reimers, which was the first read on Friday, the second day of the meeting, does not call for notice, excepting, perhaps, to remark that the Council of the Institute were to blame for not taking care that the author was informed beforehand that his contribution was not of a nature which should have been submitted in the form in which it was read. Mr. Callendar's paper on "Platinum Pyrometers" is a great contrast to the last-mentioned. The prominence given to the Le Chatelier pyrometer in this country, by Prof. Roberts-Austen chiefly, has led to renewed hope on the part of those who desire to measure higher temperatures. Mr. Callendar has been amongst those who have been giving attention to the subject, and the results of his labours, which are distinctly valuable, are given in his paper, to which we would refer all practically interested in the matter. His introductory remarks on air pyrometers are interesting, and may be read with advantage by those not already acquainted with this branch of the subject ; but it is of the Siemens electrical resistance thermometer, known generally as the "platinum pyrometer," that he has most to say. It has been hitherto accepted that the platinum pyrometer was subject to the serious defect of changing its zero with use. The British Association Committee of 1874 discovered this, and it has since been amply confirmed as a fact. The Committee experimented chiefly with a pyrometer in an ordinary fire at moderate temperatures of about 800° C., and they found that the resistance increased continuously with heating, and that the wire underwent rapid deterioration. They also made some experiments and suggestions with a view to remedy this defect, but they did not succeed in overcoming it. This continuous change of zero is certainly the most serious practical defect that a pyrometer can have, and there can be no doubt that the report of the British Association did a great deal to destroy confidence in this method of measuring temperature.

We cannot do better than continue Mr. Callendar's communication on this part of the subject in his own words :—

"About seven years ago, when I began making experiments on this subject at the Cavendish Laboratory, Cambridge, I was at first very much surprised to find that the platinum wires which I used did *not* undergo continuous change, even when subjected to much more severe tests than those applied to the Siemens pyrometer by the Committee of the British Association. By making further experiments, however, with a sort of imitation Siemens pyrometer, I succeeded in reproducing at pleasure the effects they had observed, and in proving to my own satisfaction that these defects were not inherent to the method, but merely incidental to the particular form of instrument on which they experimented. I found that if the wire were properly protected from strain and from contamination, the pyrometers could be made practically free from change of zero, even at very high temperatures.

"The construction of the Siemens pyrometer has not, so far as I am aware, undergone any material change since 1874. The coil of platinum wire, which forms the sensitive part of the instrument, is wound on a clay cylinder, and packed in an iron tube from 5 to 8 feet long, and about an inch or so in diameter. I have here the fine wire and the clay cylinder from a pyrometer which was recently in use at the Royal Arsenal, Woolwich. I was informed that it had never been heated above 900° C., or 1600° F., but its resistance had increased some 15 per cent., corresponding to an error of about 100° F. in the temperature measurements. When the instrument was taken to pieces it was found that the wire was quite rotten and brittle in some places, and sticking to the clay cylinder. This, I think, is sufficient evidence that the clay, or some impurity contained in it, attacks the wire, otherwise the local nature of the action could not be explained, unless the quality of the wire used was very inferior.

"I have tried several materials on which to wind the wire, but have found nothing that answers so well as mica. The plan I generally adopt is to double the wire on itself, and wind it round a very thin plate of mica, in such a way that it only touches the mica at the edges. This method gives very good insulation, even at high temperatures, and, so far as I can discover, the mica has no action on the wire even at temperatures of 1200° C.

"Another defect of the Siemens pyrometer is the iron-containing tube. Metallic vapours of any kind will attack the wire readily, and will ruin the pyrometer. It is not probable that the iron itself will be appreciably volatile at temperatures below 1000° C., but it is very likely to contain several more volatile impurities. Vapours of copper, tin, zinc, &c., rapidly render the wire brittle and useless. A comparatively small trace suffices."

Mr. Callendar's wires were inclosed in glass, a material which naturally cannot be used for high temperatures. He finds that a hard-glazed porcelain tube does very well to protect the wire, at least up to temperatures of 1200° C. A silica tube would be better, but that the author has not succeeded in obtaining. He pointed out, however, that good porcelain is not so fragile as it is generally thought to be. He has only broken one tube, and that with a hammer. He hopes, however, ultimately to be able to produce a satisfactory silica tube. The remainder of the paper was taken up with a description of the indicating apparatus, but here, again, we must refer our readers to the original paper.

Mr. Thwaites's paper is of far too formidable proportions for us to deal with in anything like detail in this notice. He describes calorimeters, pyrometers, &c., and their uses. A good deal of the matter put forward is not altogether new.

Mr. Wingham's paper on the slide rule is of value to those interested in the practical working of blast furnaces.

Mr. Wailes's gas furnace valve has been designed to give an absolutely air-tight closing, an effect which is obtained by a water seal. Illustrations were given by means of wall diagrams.

The meeting was brought to a close with the usual votes of thanks.

The autumn meeting will be held in Liverpool, but the date is not yet fixed.

THE YEARLY ADMISSIONS TO THE ROYAL SOCIETY.[1]

THE discussions that arose in connection with the revision of the Statutes of the Royal Society during the years 1890 and 1891, led me to endeavour to obtain definite data on which to found a trustworthy opinion as to the effect of the existing limitation of the number of yearly admissions on the eventual total strength of the Society, and the probable result of increasing the number beyond fifteen, the present limit.

The facts bearing on this subject, so far as I have been able to collect them from the records of the Society, are embodied in the tables annexed to this communication, for the proper appreciation of the significance of the figures in which a few preliminary explanations are necessary.

The anniversary of the Society being fixed for November 30 in each year, the customary record of the number of Fellows for any year refers to the number on that date. I have throughout regarded the date to which this number applies as being January 1 of the following year.

The annual election of Ordinary Fellows usually takes place in the first or second week of June in each year. I have considered the date to be January 1 of the same year.

The lapses, whether from death or other causes, have been treated as having occurred at the end of the calendar year in which they take place.

These assumptions have been made to simplify the various computations that the investigation required (which have been sufficiently troublesome as it is), and owing to the considerable period dealt with, forty-three years, the results will not, I believe, be sensibly affected thereby.

Unless it is otherwise specifically stated, the numbers refer exclusively to the *Ordinary Fellows*, elected at the regular annual meetings fixed for the purpose.

So far as I have been able to ascertain (for the earlier records in many particulars are defective), the number of Ordinary Fellows elected since 1848 has been 15 in each year, except on four occasions; in two years the number having been 14, and in two years 16: the average, therefore, is 15 yearly.

During the period since 1848, the number of *Royal* and *Honorary* Fellows has been about 5, and the *Foreign* Members about 50; these are included in the total number of Fellows shown in the annual reports of the Council, but will not be further considered in what follows.

The rules under which certain privileged classes have been admitted as Fellows, in addition to the *Ordinary* Fellows, have varied somewhat since 1848, but at present, apart from the persons eligible for the classes of Fellows above excluded, the only persons so privileged are Privy Councillors. The total number of *Privileged* Fellows elected since 1848 seems to have been 75, which for 43 years gives an average of 1·75 per annum.

Table I. contains a summary of the available data relating to the total number of Fellows since 1848.

The total number, excluding Royal, Honorary, and Foreign Fellows, at the commencement of 1848 was 768. I am not able to say how many of these were Fellows elected in the ordinary way, and how many were privileged, but this has no importance for my present object. From 1860 onwards the distinction between the three classes, those elected before 1848, Privileged Fellows, and Ordinary Fellows, is exhibited.

At the end of 1890, the total number of Fellows, excluding the Royal, Honorary, and Foreign Classes, was 463; of whom 26 were Fellows elected before 1848, 36 were Privileged Fellows elected since 1848; and 401 Ordinary Fellows elected since 1848.

Hence it appears that the reduction of number of Fellows, of the three classes last referred to, has been 305, and as the number of admissions of the Privileged class has not been very materially affected by the changes in the rules relating to them, it follows that virtually the whole of this large reduction is a consequence of the restriction, to 15, of the number of Ordinary Fellows elected yearly.

As the ages of the 768 Fellows who constituted the bulk of the Society in 1848 are not known, and as the conditions of election before that year differed materially from what they have been since, no very useful conclusions can be drawn from the rate of their diminution since 1848.

Assuming, however, that the number of Privileged Fellows in 1848 was, as is probable, about 50, there would remain 718 Ordinary Fellows, of whom in 43 years 692 lapsed, or at an average yearly rate of 2·24 per cent., that is rather more than 16 a year. This rate, as I shall show subsequently, does not differ greatly from that which has prevailed among the Ordinary Fellows elected since 1848, and it may therefore be presumed that the average age of the Fellows in that year did not differ greatly from the average age since.

Table II. gives, as far as available data admit, the ages at the time of election of all Fellows elected since 1848; and shows the number of years they severally survived, the average age at election, the number and average age of those who were alive in 1891, and the greatest and least ages of Fellows elected in each year.

From this table it will be seen that there has been a gradual small increase in the age at election; the average for the first 10 years having been 42·2; for the second 10 years, 43·0; for the third 10 years, 44·8; and for the last 13 years, 45·2.

The accuracy of these conclusions may be somewhat affected by the greater number of unknown ages in the earlier years, the age when unknown having been taken at the average of the group of years in which the election took place.

The least age at which any Fellow has been elected is 24, one such case being recorded. The average minimum at any election is slightly under 30, and the average maximum is rather over 63; one election at an age of 87 is recorded, and several above 70.

The oldest survivor of the Fellows elected since 1848, who alone are dealt with in this table, was 86 years of age in 1891.

[1] "On the Probable Effect of the Limitation of the Number of Ordinary Fellows elected into the Royal Society to Fifteen in each Year on the eventual Total Number of Fellows." By Lieut.-General R. Strachey, R.E., F.R.S. Read at the Royal Society on May 12, 1892. This paper was accompanied by four tables, presenting summaries of the author's results.

The average age at election was 43·9, and the average age of all the Fellows in 1891 was 58·4.

Table III. records the numbers of Ordinary Fellows elected in each year, and remaining alive in each year after election, until 1891.

From this it will be seen that during the last ten years the numbers have increased by 46; in the previous ten years the increase was 68, or 22 more; and in the ten years still earlier the increase was 111, or 43 more than the last. If the decrease of growth for the ten years after 1890 takes place in a similar ratio to that which took place between 1870–80 and 1880–90, we might anticipate an increase of only 11 up to 1900, or probably a smaller number.

In order to obtain a satisfactory comparison between the lives of the Fellows, and those of the general population as shown in the accepted life tables, I have calculated, from the known ages of the Fellows at election, and the known dates of the deaths that have occurred among them, the average age of the Fellows remaining alive in each year. From these ages I have computed, from Dr. Farr's tables, the probable number of Fellows that would survive from year to year, assuming the initial number to be 15.

From Table III., above referred to, has been ascertained the number of Fellows surviving in each successive year after election, and thence has been obtained the average number surviving from an initial number 15.

The results of these computations will be found in Table IV.

The second column in this table shows the number of lives dealt with for each year after election. The first entry, 645, is the total number of Fellows elected in the whole 43 years. The next column to the right gives their aggregate ages, and the next their average age, 44·9, in their first year. Following the same line to the right, we find the average number of Fellows elected, and in their first year.

Passing to the second line of the table, 619, immediately below 645, is the total number of Fellows remaining in their second year from the elections of 42 years; this is succeeded, in the columns to the right, by their aggregate ages in their second year and their average age, and the average number in their second year, out of 15, the average number elected.

The third line gives the same data for the third year of Fellowship, and so on throughout, the last line but one showing that in their 42nd year there remained 6 Fellows from the elections of 2 years, with an aggregate age of 444 years, and an average age of 74·0, the average number surviving in their 42nd year, out of the 15 elected, being 3.

The sixth column of the table gives the successive sums of the numbers in the fifth column, and therefore indicates the aggregate number of Fellows that will, on the average, be surviving in each successive year of Fellowship, the number elected in each year being always supposed to be 15.

It will be seen that the total for the 43rd year is 397·0, whereas the actual number surviving, shown in column XI., is 401. This difference is of course due to the number 397 representing what the result would be if the average rates of election and decrease prevailed, instead of the actual rates for the separate years; and it is probably sufficiently accounted for by the fact, already pointed out, of the gradually increasing age at election in the later years, which will lead to the lives in the earlier years of the series being somewhat better than the average. Column XI. shows the *actual* results for successive years corresponding to the average results given in column VI. The differences will be seen to be somewhat irregular, but nowhere to be of importance.

Column VII. gives the aggregate ages of the numbers surviving in successive years, as shown in column V., and from it is deduced the average age of the whole number of Fellows shown in column VI., 397, which is seen to be 57·7 years, a result differing slightly from that obtained from the actual ages of the Fellows surviving in 1891, which was shown to be 58·4. The cause of this difference has already been indicated.

Columns VIII. and IX. supply the results that would be obtained by applying to an initial number of 15, the rates of mortality in Dr. Farr's tables, for the ages in successive years given in column IV. Column X. contains the ratio of column VI. to column IX., and indicates that throughout the whole period of 43 years the actual results are somewhat better than the tabular results, or that the lives of the Fellows are better than the ordinary lives, and that this advantage leads in the 43rd year to the actual number of survivors being rather more than 5 per cent. in excess of that which would be given by the life tables, or of about 20 on a total of 400.

An examination of this table will show that, with the exception of the last six or eight years, in which the number of lives dealt with at last becomes very small, the figures indicate a very regular and consistent progression, and it will practically be quite safe to assume that the series in column VI. may be extended on the basis of the ordinary life tables, subject to the addition of 5 per cent. on the total amounts obtained from these last.

Hence it will be found that in 10 years after 1891 the aggregate number of Fellows is not at all likely to be increased by more than 15, that the final result may be as little as 410, but is not likely to be more than 420, or at the outside 425.

In an earlier part of this paper, I mentioned that the rate of decrease of the Ordinary Fellows elected before 1848 did not appear to differ materially from that which has prevailed subsequently.

Taking the number of Ordinary Fellows elected before 1848, and then alive, at 718, it will be found that in 12 years (1860) the number was reduced to 422, which is about 60 per cent. of the original number; after 24 years (1872) the number fell to 206, which is about 30 per cent. of the original; and in 36 years (1884) there remained only 65, which is about 9 per cent. of the first number.

Assuming that the average age of the 718 Fellows elected before 1848, and then alive, was not materially different from (58) the average age of the Fellows elected after 1848 and alive in 1891, when it has probably become nearly stationary, it may be inferred that the lapses among a body of Fellows of that age will correspond to the lapses among the Fellows alive in 1848. Now, from Table IV. it will be seen that of the Fellows elected after 1848, the average age in their 17th year was 58·3 years, which is almost exactly the average age of the whole body. Further, it is shown that of the supposed original 15 there remained 10·9 in the 17th year of the age above mentioned, 58·3. This number was reduced in 12 years to 6·7, which is nearly 60 per cent. of the number in the 17th year, and again falls after 12 years more to 3·7, which is not very different from 30 per cent. of the starting number, and after 12 years more the number will be seen to be likely to be less than 1·0, which again will not differ materially from 9 per cent. of the original 10·9. These proportions, it will have been observed, are those above shown to hold in the case of the Fellows elected before 1848.

On the whole, it seems to be established that the present restriction to 15 of the number of Ordinary Fellows elected in any year will lead to an eventual maximum number not exceeding 420; and that the ultimate increase of the total strength of the Society, for each additional Fellow elected in excess of 15 may be taken at 28, so that an increase to 18 of the annual number of Ordinary Fellows elected would lead to an ultimate total of 500 such Fellows.

THE ERUPTIONS OF VULCANO (AUGUST 3, 1888, TO MARCH 22, 1890).[1]

THERE are some 180 (nominally 212) pages and 11 plates. Of these latter 4 are reproductions of Silvestri's beautiful whole-plate photographs [one of Vulcano at rest (with Vulcanello), and the other three instantaneous views of the volcano in eruption]. A fifth reproduces, half-size, two of Dr. Johnston-Lavis's instantaneous views of eruptions taken from the crater's edge.[2] Two other plates give 14 excellent photographs of the "bombs," and of the rest two are sketches of Stromboli crater, one petrographical, and the last the map of Vulcano (1/50,000).

1 "Le Eruzione dell' Isola di Vulcano, incominciate il 3 agosto, 1888, e terminate il 22 Marzo, 1890: Relazione scientifica della Commissione incaricata degli studi dal R. Governo," *Annali dell' Ufficio Centrale di Meteorologica e Geodinamica*, Parte 4, vol. x., 1888 (Rome, 1891).

The Commission was originally as follows:—President, Prof. O. Silvestri (Catania), Prof. G. Mercalli (Milan), Prof. Grablovitz (Seismological Observatory, Ischia), and as engineer, V. Clerici (Messina), with A. Cerati, Prof. Ponte, and A. Silvestri, as assistants.

As is known, Prof. Silvestri died before the publication of the Report, but not till some months after the end of the eruptions, on which he had elsewhere published various papers. The Commission must thus have had full time to profit by his experience both in the field and afterwards, and his name appears as author or joint author of a number of sections. After Silvestri's death, Prof. Mercalli, the largest contributor to this Report, took his place, and brought the work to its completion.

2 For others by Dr. Lavis and Silvestri, see "South Italian Volcanoes."

A table of contents may be found at the end.

Appended to the various sections are the names of the authors responsible. The 180 pages of text necessarily vary in character. Thus, 50 pages are devoted to an almost daily record of the state of the volcano during the twenty months of the eruption. For not a few days we have a record of the times and degree of violence of all the explosions which (February 12 and 14, 1889) might number more than 100 between 10 a.m. and 6 p.m. On the other hand, we find between pp. 207 and 210, a *résumé* of the chief facts observed, and the conclusions to which they point.

There are 20 pages (9–29) on the topography and geology of the island of Vulcano. Details are given as to the rocks collected at various localities, and the conclusion (expressed with some reserve) as to the history of the island, is very similar to the view stated (Proc. Geol. Assoc., vol. xi. pp. 395–96, 1890) by Dr. Johnston-Lavis.

The author (Mercalli) only recognizes one crater (with lavas of andesitic and basaltic type) in the "Piano" district, which forms the southern half of the island. The Serro di Capo and Monte Lentia represent the western part of a second old (north-west) crater of more "acid" type, which may have had its centre almost coincident with that of the present active cone, and which, judging by the weathering of the rocks, may be older than the Piano crater. The author notices that the straight north and south line, drawn from *Vulcanello* through the *hot springs* by the "Faraglioni" and the two overlapping "*Forgia Vecchia*"-*s* (on the north flank of Vulcano) to the present crater (or "*Fossa di Vulcano*"), if continued, strikes *Monte Saraceno* (a lateral cone on the north-west edge of the Piano crater). It is then pointed out that, assuming Monte Saraceno to be situated over a continuation of the crack which most probably runs from Vulcano to Vulcanello, and assuming the present eruptive centre to coincide with that of the old north-west crater, that then the present "Fossa di Vulcano" is situated on the point of meeting of two cracks, viz. a north to south one from Vulcanello to Monte Saraceno, and a no th-west to south-east one joining the more ancient craters. The present crater would then be situated over a weak point. Whether or no Monte Saraceno be situated over a crack extending south from Vulcano rather than over some other, there is nothing at any rate in the above against the view expressed in Prof. Judd's "Volcanoes" (see Fig. 81), according to which there is one main crack beneath the island of Vulcano, the crack from Vulcano to Vulcanello being but a continuation of that on which the more ancient craters lie. As to the number of craters more ancient than the main modern cone, it will be seen that the Report takes a view intermediate between that of Scrope ["Volcanos," 2nd edit., p. 192, Fig. 47] and that of Judd ["Volcanoes," p. 196, and Figs. 77 and 85].

The twenty pages (30–50) devoted to the records of previous eruptions are naturally full of interest. A number of quotations from older writers are given. The conclusion is that the eruptions of Vulcano in the historic period have been on the whole very similar.

As interesting dates may be noticed:—475 B.C., Vulcano in activity (Thucydides); 183 B.C., Vulcanello formed; about 1550 A.D., strait between Vulcanello and Vulcano filled up by eruption of the latter; 1727, Forgia Vecchia (on north slope of Vulcano) in eruption (D'Orville); 1771, "Pietre Cotte" obsidian stream (on north flank of Vulcano) poured out; 1878, Fumaroles still visible on Vulcanello. [1]

From p. 53 to p. 174 is devoted to (1) detailed record of the eruptions, as to which a valuable *résumé* is given, pp. 112–14; (2) seismological and various other physical observations; and (3) the description of the erupted products.

The following epitome is based on that given by Silvestri, pp. 207–208:—

(*a*) The recent activity of Vulcano lasted 20 months, viz. August 3, 1888, to March 22, 1890 (with final explosions, May 17), the most violent explosions (p. 113) occurring on August 4, 1888, December 26, 1889, and March 15, 1890. There had previously been a period of repose (1832–72), followed by minor premonitory eruptions in 1873–7–8–9 and 1886.

(*b*) Just as we have the "*Plinian*" or "*Vesuvian*" eruptions of Vesuvius accompanied by violent outbursts of "ashes" and welling out of lava, and the incessant, milder "*Strombolian*" type of eruption, so we may distinguish a "*Vulcanian*" type (pp. 58–59). Characteristic of this are—

(i.) Intermittent explosions with discharge of bombs, ash, dust, and vapours. Each of these explosions resembles the first outburst of "Plinian" ("Vesuvian") eruptions (p. 112).

(ii.) The absence of lava streams.

(iii.) The absence of noteworthy earthquake shocks.

(*c*) The more violent of the explosions burst out suddenly, discharging clouds of vapour, with dust lapilli, and more or fewer bombs and fragments of compact lava, and such an explosion was then followed at short intervals by feebler ones, which merely discharged the smaller materials, or vapours only.

(*d*) The more violent explosions were generally separated by longish intervals, either of absolute repose, or with insignificant explosions; and, on the other hand, when eruptions took place every few minutes, they were generally feeble.

(*e*) (p. 113, 70) Observations of atmospheric pressure extending over a day, or short period of time, show no relation to the frequency or degree of violence of the explosions. But viewing the 20 months of the eruption as a whole, it is found that Vulcano enjoyed comparative repose during periods of high atmospheric pressure, or of small change, and was most active during periods of change from fair to stormy weather, with marked fall of barometer.

(*f*) Though during the 20 months of the eruption there were altogether a good many earthquakes recorded either by the seismoscope, or by some of the inhabitants (pp. 134–37), still these were but slight, and, as stated, formed no feature of the eruption, being very rare compared with the explosions.

It was found (pp. 125–28) that for making observations of the shocks or tremors accompanying the explosive eruptions, even close to the foot of Vulcano, seismoscopes were as a rule not sensitive enough. On the other hand, owing to the frequency of the explosions, a tromometer was never quiet. The simplest method is often the best, and recourse was had to pools of mercury (at once sensitive and stable). With the aid of a reflector it was then easy to keep an eye at once on the reflection of some object in the mercury, and on the lip of the crater, and so observe the time relation between the tremors and the explosive outbursts.

Observations made near the base of Vulcano showed that each eruption was preceded by a short tremor (apparently the result of a deep-seated explosion), followed after a short interval of calm, of from a few seconds to three-quarters of a minute, by another, the result, apparently, of the superficial explosion that made a vent for the vaporous and solid ejecta. The interval was shorter in the case of the more violent explosions.

(*g*) In the first three days of the eruption (pp. 54 and 152–58), August 3–5, 1888, the ejecta consisted mainly of a variety of old materials blocking the neck of the volcano. These, much of which was more or less altered by solfataric action, were discharged in pieces of all sizes from fine dust to large masses.[1]

(*h*) After an interval of thirteen days, the second main period of the eruption set in. The older materials soon became almost entirely replaced by newly elaborated matter in the form of dust, lapilli, lighter or heavier "bread-crust" bombs,[2] and masses of compacter lava. These, as opposed to the matter discharged during the first three days, were all, except for inclusions of older rocks, of essentially similar mineralogical and chemical constitution (of andesitic type (p. 165), with 62–67 per cent. of silica, the percentage of which might be greater in the centre than in the crust of the same bomb). The larger masses on leaving the crater had a high initial temperature, and were plastic, taking rounded, elongated, or flattened forms, and on reaching the ground melted various metallic wires—silver (1000° C.), and copper (perhaps 1200°, but, as we are cautioned, the copper might oxidize and then fuse lower).

From the preceding, Silvestri draws the following conclusions:—

(Excluding the ejecta of the first three days) The high temperature and plasticity, with the presence of inclusions of older rocks, and the uniform composition of the ejecta, point to their being derived from a molten magma of recent elabo-

[1] Dr. Lavis found these practically extinct in 1887 (see NATURE, vol. xxxviii. p. 13).

[1] The Report describes these as not so hot as the later ejecta. However, from Mr. Narlian's graphic account (*Times*, September 13, 1888, and Brit. Assoc. Report, 1888, p. 665) it would seem that, at the first outburst, some of the ejecta fell red-hot, so as to set hedges, &c., on fire.

[2] Dr. Lavis, in NATURE, vol. xxxix. p. 110.

ration. Of this, all things considered, there was probably a vast reservoir at a depth probably far below the bottom of the adjacent sea (which is 670 metres on the east, and 500 metres on the west). At intervals the steam included in this molten lava would acquire sufficient force to burst forth, producing the premonitory tremor (see (*f*) above, and pp. 125–28); and though it is not very clearly stated, I gather that the interval of calm between the first and second tremors is considered to have been occupied by the escape of such steam into the space between the surface of the deep-seated lava and the crater floor, till it acquired sufficient pressure to force an exit through the mouth of the crater (which became plugged by the fall of ejecta after each eruption). Then the visible explosion would take place, accompanied by the second tremor.

The "bread-crust" bombs (with pumiceous interior and cracked subvitreous crust) are said to commonly contain inclusions of older rock, and it is suggested (pp. 163 and 209) that the frothing up of the interior is *in part* due to these, for, says he, fragments of rock falling into a superhydrated molten lava may not improbably act as centres of ebullition, just as solids (in proportion to their extent of surface) cause rapid disengagement of gas when dropped into "soda"—or other aërated—waters.

The compact fragments and masses sometimes ejected (pp. 120, 160, and 209), notably in the last eruption, might be explained as pieces of the shells of domelike bubbles which had partly consolidated below the volcano, or as derived from less hydrated parts of the magma.

If there was this huge lake of lava and so much steam, why did not the lava appear at the surface? Silvestri points out that the rise of the lava will depend on the proportion of the compressed steam to the mass of the lava that contains it, and on the resistance offered to its escape. If the exit of the steam is blocked by lava, we may have all the phenomena of a "Plinian" ("Vesuvian") eruption. On the other hand, in 1888 Etna had eruptions of vapours only, and we get all stages between these two. In the case of a large space partly filled with lava at a great depth below the surface, the conditions might well be such that the steam would escape long before the lava overflowed.

Among other points dealt with in the Report may be noticed:—Pp. 143–45, the breaking three times during this eruptive period of the submarine cable between Lipari and Milazzo in Sicily. The first and third breaks were near the same spot, and near a place where the sea was seen to "boil," with pumice rising to the surface.

On p. 147 observations are recorded which support the view that the electric discharges accompanying eruptions depend principally on the friction of much dust and fine ejecta. Violent explosions discharging large masses, if unaccompanied by such finer matter, might be without the electric phenomena (p. 146).

There are ten pages on the state of Stromboli during the eruption of Vulcano, from which there appears to have been no relation between the two. Nor do the "secondary phenomena" (the hot springs and fumaroles) in these islands appear to have been markedly affected. Some of the fumaroles increased and some decreased in vigour, and some showed no change.

From what has been said as to the slight seismic effects, we are prepared to hear that no change in the level of the land was produced. In this connection there is a chapter on the tides, which have an amplitude at Lipari of about 30 cm.

On p. 120 are given four sections, in three, of the crater of Vulcano before, during, and after the eruption, from which it is seen that the crater has been much filled up.

On November 18, 1891, the writer found the crater still in the quiet solfatara condition, so that one could descend into it. The higher slopes were covered with white, and the lower, where the fumaroles were more marked, with yellow and red crystalline deposits. A little water lay at the bottom. The deepest part was a funnel-like depression, a little to the north of the middle, somewhat as shown in Mercalli's figure for April 1890. This marked the last eruptive vent.

In conclusion, then, the Report contains a great mass of facts, and in addition generalization and theory, which, as often based on long experience, are also welcome; and the Commission is to be congratulated.

G. W. BUTLER.

SOCIETIES AND ACADEMIES.

LONDON.

Anthropological Institute, May 10.—Dr. Edward B. Tylor, F.R.S., President, in the chair.—The election of the Duke of Devonshire and Dr. H. Colley March was announced.—Mrs. Bishop (Miss Isabella Bird) read a paper on the Ainos of Japan, amongst whom she had spent some time in a village near Volcano Bay. It is doubtful whether the Ainos were the aboriginal inhabitants of Japan; they say themselves that they conquered and exterminated an earlier race who dwelt in caves. The men are strongly built and muscular, and their stature varies from about 5 feet 4 inches to 5 feet 6 inches. The extreme hairiness ascribed to the Ainos applies only to the mountain tribes, and to the men only amongst them; the women, and the men of the coast tribes, not being more hairy than many people of other races. The houses are rectangular and built of wood; they are all constructed on the same plan, and have a large window at the east end opposite the door, and two smaller ones in the south side, below which is the shelf on which the boys of the family sleep; the girls occupy a similar shelf on the north side of the room, and during the night the sleeping-places are screened off by mats. The women are remarkable for their modesty, and the men are exceedingly gentle, obliging, and hospitable. They are a religious people, and offer copious libations of "saki" on the slightest provocation. The race is dying out, and will no doubt be quite extinct in the course of a few generations.

PARIS.

Academy of Sciences, May 24.—M. d'Abbadie in the chair.—Heats of combustion and formation of alcohol and of formic and acetic acids, by MM. Berthelot and Matignon.

$$C_2(\text{diamond}) + H_6 + O = C_2H_6O \begin{cases} \text{liquid} \ldots & +69{\cdot}9 \\ \text{gas} \ \ldots & +59{\cdot}8 \\ \text{solution} & +72{\cdot}4 \end{cases} \ \text{Cal.}$$

Heat of combustion of liquid alcohol at constant pressure = 324·5 cal.

$$C\,(\text{diamond}) + H_2 + O_2 = CH_2O_2 \begin{cases} \text{solid} \ \ldots & +103{\cdot}3 \\ \text{liquid} \ \ldots & +100{\cdot}8 \\ \text{gas at } 200^\circ & +\ 90{\cdot}0 \end{cases} \ \text{Cal.}$$

Heat of combustion of liquid formic acid at constant pressure = 62·5 cal.

$$C_2(\text{diamond}) + H_4 + O_2 = C_2H_4O_2 \begin{cases} \text{solid} \ \ldots & +119{\cdot}7 \\ \text{liquid} \ \ldots & +117{\cdot}2 \\ \text{gas at } 250^\circ & +107{\cdot}1 \end{cases} \ \text{Cal.}$$

Heat of combustion of liquid acetic acid at constant pressure = 209·4 cal.—Some facts in the chemical history of nickel, by M. P. Schützenberger.—Degradation products of the tissues, particularly of the muscles, separated from the living being: analytical methods, by MM. Armand Gautier and L. Landi.—On the *Bramus*, a new type of fossil rodent from the Quaternary phosphorites of Berberah, by M. A. Pomel.—On the flexure of Gambey's mural circle, by M. Périgaud.—On the appearance of Saturn's ring at the present time, by M. G. Bigourdan. On May 21 M. Bigourdan made some observations of Saturn's ring, with special reference to its thickness. He noted several uneven portions at different points, thus confirming the observations of previous workers.—On integrals in dynamics, by M. P. Painlevé.—On equations in dynamics, by M. R. Liouville.—Approximate equation to the trajectory of a projectile in air when the resistance is supposed to be proportional to the fourth power of the velocity, by M. de Sparre.—Experimental researches on the *matériel de la batellerie*, by M. F. B. de Mas.—On the characteristic equation of various vapours, by M. Ch. Antoine.—The two phases of the persistence of luminous impressions, by M. Aug. Charpentier.—Plastic sulphur formed from sulphur vapour, by M. Jules Gal.—Some basic nitrates, by MM. G. Rousseau and G. Tite.—On the preparation and properties of arsenic cyanide, by M. E. Guenez.—Occurrence of fluorine in modern and fossil bones, by M. Ad. Carnot.—Apocinchonine and diapocinchonine, by MM. E. Jungfleisch and E. Léger.—Monosodium pyrocatechol, by M. de Forcrand.—Substitution reactions in carbon or nitrogen nuclei: application to explosive compounds, by M. C. Matignon.—On dibromomalonic acid, by M. G. Massol. The heats of neutralization by each successive KOH are about 10 cal. superior to those of malonic acid. The general conclusion is drawn that the substitution of H by a haloid increases the thermal value of the acid function.—Alcohols superior to vinyl alcohol, by M. C. Bardy.—Action of esters of unsaturated acids on ethyl sodium cyanacetate, by M. P. Th. Muller.—On a tetramethyl-meta-diamidobenzidine, by M. Charles Lauth.—On the embryology of a Proneomenia, by M. G. Pruvot.—Researches on the general cavity and excretory apparatus of Cirrhipedes, by M. Kœhler.—Anatomical study of the secondary wood of certain apetalons, by M. C. Houlbert.—On the relations of the Trias in the south-east Paris basin, by M. A. de Grossouvre.—Variations in the mean temperature of the air in the region of Paris, by M. E. Renou.—On a natural ice-cave at Creux-Percé (Gold Coast), by M. E. A. Martel.

BERLIN.

Meteorological Society, May 3.—Prof. von Bezold, President, in the chair.—Dr. Schwalbe gave an account of observations on the extent and spread of anomalies of temperature in Germany based on synoptic weather-charts. He showed that the weather-types introduced by Teisserenc de Bort hold good for Germany, especially in winter, and that, as a result of the varying distribution of barometric pressure, they are the cause of very marked differences of temperature between the north and south, the east and west. These types are less frequently observed in summer, although both then and in the spring certain very marked distributions of pressure exist.—Prof. Spörer spoke on the recent magnetic storm of April 25, for which, as also for the great storms of February 13 and 14, he had been unable to discover any corresponding phenomena in the sun-spots at the same dates.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, JUNE 2.

ROYAL SOCIETY, at 4.—Election of Fellows.——At 4.30.—On the Method of Examination of Photographic Objectives at the Kew Observatory: Major Darwin.—Supplementary Report on Explorations of Erect Trees containing Animal Remains in the Coal Formation of Nova Scotia: Sir J. W. Dawson, F.R.S.—The Hippocampus: Dr. A. Hill.—On a New Form of Air-Leyden, with Application to the Measurement of Small Electrostatic Capacities: Lord Kelvin, P.R.S.—On Certain Ternary Alloys; Part VI., Alloys containing Aluminium, together with Lead (or Bismuth) and Tin (or Silver): Dr. Wright, F.R.S.—The Conditions of the Formation and Decomposition of Nitrous Acid: V. H. Veley.—On the Theory of Electrodynamics as affected by the Nature of the Mechanical Stresses in Excited Dielectrics: Dr. J. Larmor.—On Current Curves: Major Hippisley.

LINNEAN SOCIETY, at 8.—On the Disappearance of Desert Plants in Egypt: E. A. Floyer.—On Insect Colours: F. H. Perry Coste.—Lantern Demonstration.

CHEMICAL SOCIETY, at 8.—Ethylene Derivatives of Diazo-Amido Compounds: R. Meldola, F.R.S., and F. W. Streatfield.

ROYAL INSTITUTION, at 3.—Faust: R. G. Moulton.

FRIDAY, JUNE 3.

GEOLOGISTS' ASSOCIATION, at 8.

ROYAL INSTITUTION, at 9.—Metallic Carbonyls: Ludwig Mond, F.R.S.

SATURDAY, JUNE 4.

ROYAL INSTITUTION, at 3.—Some Modern Discoveries in Agricultural and Forest Botany; Prof. H. Marshall Ward, F.R.S.

TUESDAY, JUNE 7.

ROYAL INSTITUTION, at 3.—Some Aspects of Greek Poetry: Prof. R. C. Jebb, M.P.

WEDNESDAY, JUNE 8.

GEOLOGICAL SOCIETY, at 8.—The Tertiary Microzoic Formations of Trinidad: R. J. Lechmere Guppy. (Communicated by Dr. H. Woodward, F.R.S.)—The Bagshot Beds of Bagshot Heath (a Rejoinder): Rev. A. Irving.—Notes on the Geology of the Nile Valley: Johnson Pasha and H. D. Richmond. (Communicated by A. Norman Tate.).

THURSDAY, JUNE 9.

MATHEMATICAL SOCIETY, at 8.—On the Reflection and Refraction of Light from a Magnetized Transparent Medium: A. B. Basset, F.R.S.

ROYAL INSTITUTION, at 3.—Faust: R. G. Moulton.

FRIDAY, JUNE 10.

PHYSICAL SOCIETY, at 5.—Some Points connected with the Electromotive Force of Secondary Batteries: Dr. J. H. Gladstone and Mr. Hibbert.—Workshop Ballistic and other Shielded Galvanometers: Prof. W. E. Ayrton and Mr. Mather.

ROYAL ASTRONOMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 9.—Magnetic Properties of Liquid Oxygen: Prof. Dewar, F.R.S.

SATURDAY, JUNE 11.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—Some Modern Discoveries in Agricultural and Forest Botany: Prof. H. Marshall Ward, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Theoretical Mechanics: J. Spencer (Percival).—The First Part of Goethe's Faust, carefully revised, with Introduction by C. A. Buchheim (Bell).—Influenza, 2nd edition: Dr. J. Althaus (Longmans).—The Physiology of the Invertebrata: Dr. A. B. Griffiths (L. Reeve).—Annual Report of the Smithsonian Institution to July 1890 (Washington).—Lessons in Elementary Mechanics, new edition: Sir P. Magnus (Longmans).—Elements of Physics: C. E. Fessenden (Macmillan and Co.).—An Elementary Course in Theory of Equations: C. H. Chapman (Gay and Bird).—A Text-book in Retaining Walls and Masonry Dams: M. Merriman (Gay and Bird).—A Treatise on the Mathematical Theory of Elasticity, vol. i.: A. E. H. Love (Cambridge University Press).—Der Peloponnes, Abteilung ii.: Dr. A. Philippson (Berlin, Friedländer).—Topographische und Hypsometrische Karte des Peloponnes: Dr. A. Philippson (Berlin, Friedländer).—The System of Mineralogy of J. D. Dana, 1837–68; Descriptive Mineralogy, 6th edition: E. S. Dana (Kegan Paul).—Die Tägliche Gang der Temperatur und des Sonnenscheins auf dem Sonnblickgipfel: Dr. W. Trabert (Wien, Tempsky).—Bacteriologisches Practicum: Dr. W. Migula (Karlsruhe, O. Nemnich).—In Starry Realms: Sir R. S. Ball (Isbister).—A Hand-book of the Management of Animals in Captivity in Lower Bengal, Ram Bramha Sányál (Calcutta).—Mineralogy: Dr. F. H. Hatch (Whittaker).—Darwin and after Darwin; I. The Darwinian Theory: Dr. G. J. Romanes (Longmans).—Animal Coloration: F. E. Beddard (Sonnenschein).—The Discovery of America, 2 vols.: J. Fiske (Macmillan and Co.).—A Text-book of Geometrical Deductions: J. Blaikie and W. Thomson (Longmans).—Distinction and the Criticism of Beliefs: A. Sidgwick (Longmans).—Solutions of the Examples in a Treatise on Elementary Dynamics: S. L. Loney (Cambridge University Press).—Theoretical Mechanics, Division I.: J. C. Horobin (Bell).—A Hand-book of Practical Astronomy; W. W. Campbell (Ann Arbor, Michigan, *Register* Publishing Company).—Logarithmic and other Mathematical Tables, 2nd edition: W. J. Hussey (Ann Arbor, Michigan, *Register* Publishing Company).—The Universal Atlas, Part 15 (Cassell).—Life in Motion: Prof. J. G. McKendrick (Black).—An Elementary Text-book of Magnetism and Electricity: R. W. Stewart (Clive).—The Two Spheres of Truth: T. E. S. T. (Unwin).—The Essentials of Histology, 3rd edition: Prof. E. A. Schäfer (Longmans).—Planisphere for Latitudes 68° to 48°, and Key to Planets until 1901: M. W. Harrington (Ann Arbor, Michigan, *Register* Publishing Company).—The Optical Indicatrix and the Transmission of Light in Crystals: L. Fletcher (Frowde).—Tasmanian Official Record, 1892 (Hobart).

PAMPHLET.—The Rutherfurd Photographic Measures of the Group of the Pleiades: H. Jacoby (New York).

SERIALS.—Bulletin of the New York Mathematical Society, vol. i. No. 8 (New York).—Bulletin de la Société Impériale des Naturalistes de Moscou, No. 4 (Moscou).—Zeitschrift für Wissenschaftliche Zoologie, liii. Band, Supplement (Williams and Norgate).—Natural Science, June (Macmillan and Co.).—Annalen des k.k. Naturhistorischen Hofmuseums, Band vii. Nr. 1 u. 2 (Wien, Hölder).—Proceedings of the Royal Physical Society, Session 1890–91 (Edinburgh).—Internationales Archiv für Ethnographie, Band v. Heft 2 (Leiden, Trap).

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