THURSDAY, MARCH 12, 1891.

HUYGENS AND HIS CORRESPONDENTS.

Œuvres Complètes de Christiaan Huygens. Tome Troisième, "Correspondance, 1660-61." (La Haye: Martinus Nijhoff, 1890.)

THE great edition of the works of Christian Huygens now in course of publication at the Hague (NATURE, vols. xxxviii. p. 103, xl. p. 591) continues to issue from the press, at a leisurely rate indeed, yet, all things considered, with creditable punctuality. A slight delay in the appearance of the third volume, now before us is fully accounted for by the discovery, in the National Library at Paris, of some documents bearing on the history of the pendulum-clock, which it was judged expedient to incorporate with it in the form of an appendix. Their interest is considerable, although their import be not subversive of received ideas. They serve to confirm the originality of Huygens, while illustrating the zeal displayed in contesting it. Such debates recur in every chapter of scientific history. They sometimes rouse impatience by their apparent triviality, but seldom fail, none the less, to bring out curious and suggestive facts. It is well when they are conducted with as little acrimony as in the present case. Huygens's brilliant success in applying the pendulum to the regulation of clocks in 1657 gave the signal for the raising of adverse claims to priority. Those of Galileo were the best founded; and they were championed by Prince Leopold de' Medici, brother of Ferdinand II., the reigning Grand Duke of Tuscany. Several of his letters on the subject to Boullaud are now published for the first time; he employed Viviani, a disciple of Galileo, to draw up a "statement of claim" on his behalf; and he sent to Paris, for communication to Huygens, a drawing of a model for a time-piece begun under Galileo's directions in the last year of his life (1641). Its reproduction (at p. 8 of the work under notice) shows indeed a pendulum connected with wheelwork, but no clock in the proper sense—means for continuing the motion, either in the shape of weights or springs, being totally absent. Galileo, in fact, was on the track of an invention which eluded him. He saw that the thing was to be done, but never quite achieved the doing of it. Old age and infirmity precluded him from this final triumph. He died, *re infectâ*, leaving his ideas to be perfected by his son Vincenzo, one of the "about to be's" of the world, whose dilatoriness, as usual in such cases, marred his ingenuity; his over-drawn account with time being at last peremptorily closed by the intervention of death. Huygens succeeded to the task ignorant of these previous attempts to deal with it; to which, indeed, attention was only directed by the effective completeness of his resulting discovery.

The present volume includes, besides supplementary matter, the correspondence of two years, 1660-61. Should the thirty-three still to be traversed prove equally productive, we may look forward to seventeen further volumes—making twenty in all—devoted to letters written by, to, or about the Dutch Archimedes. The practically endless vista of huge tomes thus opened out before us is no phantom of the imagination. There seems no reason to expect that the treasures of the Leyden archives will be found less rich as they are more deeply delved into. For the genius of Huygens never lapsed into inertness. Some of its most splendid fruits were gathered late in life. His career continued to the end closely interwoven with the seething scientific activity of his age; and his eagerness for information as to the fruits of that activity doubtless retaining its keen edge, his communications with the learned are not likely to have become less frequent or less copious. Their value, moreover, judging from the samples thus far printed, is so great as to render suppression or selection undesirable, so that we can only wonder at and admire the prodigious scale of this rising literary monument to the memory of a great man. And while bearing in mind that superfluity of information, as of other things, becomes at a certain point equivalent to penury, we willingly admit that the evils of extreme voluminousness are, in this stately publication, reduced to a minimum by the admirable care with which its contents are indexed and assorted.

The figure of Saturn continued to be the leading topic of astronomical discussion for several years after the publication of the Huygenian "System." The hypothesis there expounded still lacked some final touches of confirmation by fact; and its validity was accordingly regarded in many quarters as problematical. Experience alone could answer the question whether the predicted phases of the ring would manifest themselves as time went by; and observers eagerly scanned the ill-defined planetary contour displayed by their imperfect instruments for evidential *pros* and *cons*. Chief among them was the discoverer himself. He had described what was to be expected; it remained for him to announce what was to be seen; and he had the advantage over his contemporaries in the command of superior defining powers, both mental and telescopic. Eustachio Divini, it is true, was then popularly supposed to "hold the field" in optical art; yet his best glasses, favoured by the clear Roman air, still exhibited the "triple planet" under the aspect it had assumed to Galileo, as a globe with two detached spherical appendages. He naturally regarded his own observations as conclusive; but his "Brevis Annotatio," designed for the annihilation of a fictitious ringed Saturn, served only to demonstrate their deceptiveness; and Huygens, in his "Brevis Assertio Systematis Saturni," made a triumphant reply.

The Accademia del Cimento from the first unanimously supported him. At Florence it was said, "tutti erano Hugeniani." Borelli, less prejudiced, or better optically armed, than Divini, succeeded in tracing the connection of the ansæ with the disk; and a model was constructed of the strange Saturnian machine, by which, for the special benefit of Prince Leopold, all its varying appearances, well or ill observed in the sky, were experimentally and convincingly reproduced. Premature speculations as to the nature of the ring, indulged in, among others, by Magalotti and Roberval, made it the result of condensed vaporous exhalations from the body of the planet; while Neuræus recognized in it a unique contrivance for rendering habitable an otherwise desolate world at the confines of the solar system. The bizarre idea of its composition like a lens out of some highly refractive material enabling it to concentrate light and heat upon an underlying zone, thereby brought up to the terrestrial standard of comfortable accommodation for living beings, appeared then scarcely more extravagant than any other supposition regarding a structure of which the existence, antecedently to proof, might well have been thought incredible. Fine distinctions of probability vanished in the presence of so astounding a specimen of celestial workmanship.

An imperfect anticipation of "this kind of Saturn was hatched" (it is curious to learn) early in 1658, at White-Waltham, in Berkshire, by the combined exertions of Sir Christopher Wren and Sir Paul Neile. An elliptical "corona" was fitted to the planetary globe, meeting it at two places, and rotating with it once in its period of revolution round the sun, on an axis coinciding with the plane of that revolution. Thus, after a fashion, the observed phases were accounted for; but the superiority of the Huygenian *rationale*, divulged in the following year, was perceived by none more clearly than by Wren himself; and he wisely allowed his own abortive attempt to sink into quasi-oblivion. The paper embodying it is now printed in full (p. 419), accompanied by drawings testifying very creditably to the skill of early English opticians.

Huygens was the first to make definite observations of the markings on Mars. Some of his drawings have even proved available in the most recent determination of the planet's rotation, roughly estimated by him, from his views of the Kaiser Sea, November 28 and 30, and December 1, 1659, to occur in a period of twenty-four hours. The desire to confirm a discovery which would have been the earliest of its kind quickened his zeal for the further improvement of telescopes. The main obstacle lay, he thought, in the defective quality of the glass then fabricated. And the best, which was procured from Venice, was certainly bad, judging from the collection of Huygenian lenses preserved at Leyden (Kaiser, *Astr. Nach.*, No. 592). Nor did his tubeless telescopes, when actually brought into operation, realize all that was expected from them. Concurrent evils outweighed their theoretical superiority. The disk of Mars was, however, measured by Huygens with remarkable accuracy on December 25, 1659, by means of a metal slip, of graduated width, inserted at the telescopic focus. This was the first Continental example of the use of a micrometer.

An amusing glimpse behind the scenes of pseudo-scientific life in the seventeenth century is afforded by some communications included in the present volume relative to the horoscope of one of the Princesses of Orange. Huygens was the intermediary; Boullaud the prophet, whose dignity as an interpreter of mystic influences contrasts ludicrously with his childish petition for an "Indian jewel" as the guerdon of his services. On both sides, too, there is evidence of failing faith. Huygens, apprehensive of a fiasco if the enjoined secrecy were observed as to the name and quality of the lady, imparted them, under the rose, to an astrologer who had at least the merit of choosing a low level for his pretensions. He made no claim to the divination of particular circumstances. "Temperaments" only, in his opinion, were rained down, according to the rules of science, from the skies; and temperaments are vague entities, intangible, undefinable, defiant of positive affirmations or denials; to say nothing of the saving possibility of their lying latent for any convenient length of time. The horoscope of Albertina Agnes, Princess of Nassau, was, then, at any rate safe from confutation by facts.

In the spring of 1661, Huygens visited London. He was, however, far from sharing his brother Ludovic's enthusiasm for the city by the Thames. The smell of smoke there he found "insupportable," and concluded to be insalubrious; mean architecture, narrow and ill-paved streets, unstable dwelling-houses, shabby public gardens, constituted the chief part of his impressions. The lower orders struck him as melancholy, the upper classes as naturally unsocial, their affability to strangers notwithstanding, the women as deficient in conversational charm, and falling a long way behind their French sisters in sprightliness; and he echoed the hope that an advance in refinement of manners would ensue upon the re-establishment of the Court. He was presented at Windsor to the King, whom he found somewhat curt and pre-occupied; but he left it to Pepys to chronicle the "brave sight" of the coronation in Westminster Abbey on May 3, choosing for his own share the competing celestial spectacle of Mercury's transit across the sun. It was the first phenomenon of the kind which had been at all generally observed, and Huygens watched its progress from Long Acre with one of Reeves's excellent telescopes. Some of his own were set up in the garden behind Whitehall, and occasionally served to display Saturnian marvels to the gaze of the Duke and Duchess of York. The method of their fabrication was a secret until Huygens gratified the Royal Society with its disclosure; and he was, on the other hand, deeply interested in the experiments on vacua exhibited before him at Gresham College. The hospitality and politeness, indeed, with which he was received both in public and private, did not fail to win his acknowledgments; nor could he remain insensible to the high capacity of many of his learned entertainers, "most of whom," he added, "had travelled in France and elsewhere." Unmistakably, he had by this time fallen under the spell of our neighbours' subtle charm. French had become a second native language to him, and, although he acquired enough English to make himself understood when occasion required, he did not prosecute the study very zealously. Nor was it necessary. His correspondence with Sir Robert Moray, Boyle, and Oldenburg, was carried on in French; Wallis used Latin by preference; Boyle's new tracts were promptly conveyed into French or Latin. Even his Dutch vernacular was in a measure discarded by the astronomer of the Hague in favour of more cosmopolitan tongues. Insensibly and inevitably he had grown beyond the range of a single country. He belonged henceforth primarily to Europe; only secondarily, and by a tie which was soon to be still further loosened, to Holland.

A. M. Clerke.

FORCE AND DETERMINISM.

The Philosophical Basis of Evolution. By James Croll, LL.D., F.R.S. (London: Edward Stanford, 1891.)

AGAIN and again is the physicist, in the course of his researches, brought face to face with philosophical questions. It then depends upon the bent and bias of his mind whether he is content to leave these questions as he finds them, or is impelled to adapt them to some more or less plausible metaphysical solution. Dr. Croll, whose death we have so recently had occasion to lament, made his mark by a skilful application of physical reasoning to sundry difficult problems connected with climate and time. It need hardly be said that the whole tendency of his thought and work was in support of the doctrine of evolution in its widest sense. In the volume before us he discusses, after forty years of meditation, the fundamental principles which underlie this doctrine.

At the outset, Dr. Croll draws and emphasizes what he terms "the radical and essential distinction between force and the determination of force." The *production* of motion, he says, is one thing; the *determination* of its direction, another and perfectly distinct thing. When a molecule is to be moved, there is an infinite number of directions in which force may be conceived to move it. But, out of the infinite number of different paths, what is it that directs the force to select the right path? Force produces motion, but what determines it and gives it its thusness? "In the formation of, say, the leaf of a tree, no two molecules move in identically the same path. But each molecule must move in relation to the objective idea of the leaf, or no leaf would be formed. The grand question therefore is, What is it that selects from among the infinite number of possible directions the proper one in relation to this idea?"

Dr. Croll states in his preface that his volume is not of a speculative or hypothetical nature. But we venture to think that, notwithstanding this disclaimer, the "objective idea" of a leaf comes perilously near a metaphysical as opposed to a scientific conception. But let that pass. We venture to think, further, that physicists may be a little impatient with the "radical and essential distinction between force and the determination of force." This, too, they may be disposed to regard as rather a metaphysical than a physical distinction. And this the more, since physicists are accused of attributing everything to force, and little or nothing to the far more important determination of force. Let us, however, endeavour to see what Dr. Croll's contention really amounts to. And let us take the simpler case of a crystal of alum, instead of the more complex case of a leaf, involving, as this latter does, complicating elements such as heredity and natural selection.

From a solution of potassium aluminium sulphate, octohedral crystals of alum are obtained on evaporation. Concerning them, Dr. Croll says, in effect: Granted that the molecules run into crystalline figure under the stress of certain forces, *why this particular figure?* Force accounts for their motion, but what determines the direction of motion so as to give rise to this particular form and not a scalenohedron or a rhombic pyramid? Many of us are content, at this point, to confess our ignorance, and to say that it is a way they have; it is part of the constitution of Nature as presented for our study. But possibly, others may descend to more recondite physical principles. Let us, then, for the nonce, grant that if we only knew the full bearings of that fundamental physical principle, that force is the product of mass into acceleration, we should find the crystalline figure of the alum inevitably contained therein. Few, if any, are likely to go so far. But if such there be, even of them Dr. Croll will still ask: What, then, has determined that force should be the product of mass into acceleration, rather than the product of mass into momentum, or an indefinite number of other conceivabilities? If it be replied that such is the constitution of things, the answering question will still be, But what determined that the world should be so constituted?

It will now be seen that Dr. Croll's question, though couched in new terms, is an old, old question. Nor are the solutions suggested other than those which have of old been put forward. And if Dr. Croll adopts a Theistic solution, he does not adduce other than the well-worn arguments, which it is not our province to discuss.

Two chapters are devoted to "Determinism in relation to Spencerianism," in which Dr. Croll inquires whether a rational explanation of evolution can be derived from the persistence of force. The conclusion arrived at is that no such rational explanation can be deduced from this vaguely definite axiom; and in this we are disposed to agree. With all our admiration of Mr. Herbert Spencer's power as a thinker, we believe that a synthetic philosophy, *quâ* synthetic, is of little, if any, practical or speculative value. If the synthesis leads us back to the universe from which our analysis started, nothing is gained thereby; if it brings us to a different universe, we can afford to neglect it. Other chapters are devoted to "Determinism in relation to Darwinism," in which Dr. Croll points out that natural selection does not and cannot explain the origin of variations; but this, though true enough, is an old story.

Some of the ablest sections of the work, including an appendix of three chapters, are devoted to "Determination in relation to Free-will." We fancy that some of those who may welcome Dr. Croll's argument for Theism will be unwilling to accept his argument for determinism in the matter of human conduct. They will, however, find his discussion of the matter well worthy of careful consideration, as, indeed, are many other parts of the little treatise.

In conclusion, we must state that we have felt some diffidence in dealing with this work of an author who has so recently passed beyond the reach of either praise or criticism. We could not, however, pass it by in silence; and since we were impelled to speak, we have not hesitated to speak frankly; and we trust that Dr. Croll's many friends will not blame us for the course we have adopted.

C. LL. M.

NATURAL HISTORY OF THE ANIMAL KINGDOM.

Natural History of the Animal Kingdom, for the Use of Young People. In Three Parts. With 91 Coloured Plates and numerous additional Illustrations in the Text. Adapted from the German of Prof. von Schubert by W. F. Kirby. (London: The Society for Promoting Christian Knowledge, 1889.)

THE number of books on natural history, written for the purpose of attracting persons young in years or knowledge is truly astonishing; in most cases they are adaptations from French or German sources, but sometimes they are "original" compilations. They are always illustrated; they come, and, what is more truly wonderful, they go, in the trade acceptation of this word. The demand for them would seem to be great—so great that few are able to resist the temptation of adding to their number, not knowing beforehand what pains of remorse are caused to the authors of such writings, in after days.

Times there were when, in the infancy of knowledge, such books as Patterson's "Zoology for Schools" and Milne Edwards's "Elementary Course of Natural History" served a useful purpose and had their day; but the rapid piling up of additions to that knowledge soon left it impossible for any single person to keep up with it, and even to write a good popular treatise on one small group of animals required the combined labour of a Kirby and a Spence. However, when the demand went on as before, the hint conveyed by such a fact was quite wasted, and the publishers took steps to meet it. Natural history for the people is now in as great favour as ever, and it must be brought out in a manner not only to attract the crowd, but it must be within their pecuniary re sources From some experience we have learned that works of the popular natural history class are not written with the view of being criticized; indeed, it would appear to be scarcely fair to subject them to any such ordeal, at least from a scientific point of view. A compiler who had a good working knowledge of, say, the mammals and birds, would be a more or less exceptional creature; if he knew something of the whole group of the Vertebrates, he would be far out of the common; but even such a one would flounder when he came to treat of the remaining great groups; we laugh at Oliver Goldsmith's "Animated Nature," but a learned entomologist might be as ignorant of the Vertebrates and the Mollusca as Goldsmith was of the difference between a cow and a horse. Therefore instead of criticism, we venture to think that commiseration were the more needed, and perhaps to this the advice might be added to beware not to repeat the folly.

In the "Natural History of the Animal Kingdom," for the use of young people, as adapted from the German of Prof. von Schubert, by Mr. W. F. Kirby, there are no such absolute blunders as are to be met with in Goldsmith's work; its shortcomings are more in the direction of omissions and lack of explanation of technical terms. If the adapter had handed say the first part, "On Mammals," to some young and fairly intelligent youth, and then examined him about what he read, he would probably have been astonished at the result; we tried the experiment, the critic perversely turned to the last page, about the "duckbill and spiny anteater," the position of the marsupial bones (nowhere described as having any connection with the pouch) puzzled him, and his ideas fell far from realizing the fact, but when he came to the description of the cavity of the mouth of the duckbill as "a closed weir," his speculations became hopelessly absurd, and we inquired no further. Mr. Kirby, we feel, is not accountable for the illustrations, which may amuse many, possibly instruct some.

The next time the Society makes an attempt in this direction we hope it will succeed better.

OUR BOOK SHELF.

Commercial Botany of the Nineteenth Century. By John R. Jackson, A.L.S. (London: Cassell and Co., 1890.)

THE general public are so little aware of the sources and history of the many familiar vegetable products which they use daily, that a short description of them in a readable form will naturally be welcome. To provide this is the object of the little book under review: it contains within its 160 pages an epitome of the results achieved by Kew and the colonial gardens in vegetable economics during the present century. The more important attempts to introduce plants of commercial value into new areas, their success or failure, and the consequent effect upon the imports of raw materials, and the prices of manufactured articles are discussed. The facts, in themselves interesting enough, will appeal with additional weight to the reader since they come from head-quarters, the author being the Curator of the Museums in the Royal Gardens at Kew.

Mr. Jackson has wisely avoided the dictionary form, which makes books of this nature so dry and disconnected. He has devoted separate chapters to distinct classes of products, *e.g.* india-rubber, drugs, oils, dyes, fibres, &c., each chapter thus gives a succinct account of the steps taken to advance the interests of a separate industry. Perhaps the most interesting are the pages which describe the rise and progress of the trades in india-rubber (pp. 10–26) and quinine (pp. 60–71); these illustrate admirably the methods pursued by the Directorate of Kew, and it is highly desirable that such statements should be put before the general reading public. It is desirable, not merely for their information in matters which more or less directly concern every one of them, but in order that they may duly appreciate the importance of the work carried on by Kew, in the introduction of economic plants into new areas, and the effect which such experiments have already had upon supply and prices. Mr. Jackson is to be congratulated on having produced a book at once short, interesting, and useful: the facts which he puts forward so closely affect the whole community that they lose little or nothing in weight by the plainness of the style in which the book is written.

F. O. B.

Fresenius's Quantitative Analysis. Translated by Chas. E. Groves, F.R.S. Vol. II., Part 3. (London: J. and A. Churchill.)

THIS third part is especially welcome after the long time that has elapsed since the second was to hand, as we hope it indicates that the rest of the volume is likely to follow without delay. The present part continues the subject of acidimetry, and goes on to alkalimetry, compounds of the alkalies, and alkaline earths (including bleaching powder), aluminium compounds, silicates, and chromium and zinc ores.

The Design of Structures. By S. Auglin, C.E. (London: Charles Griffin and Co., 1891.)

THIS work can be confidently recommended to engineers. The author has wisely chosen to use as little of the higher mathematics as possible in his treatment of the different branches of the subject, and has thus made his work of real use to the practical engineer. It must not be imagined that the author has not thoroughly dealt with his subject. The work is a very good example of the way in which the subject can be adequately treated without the use of abstruse formulæ and complicated calculations. In a volume of 500 pages, we find most of the usual points dealt with, and illustrated by a large number of practical examples such as occur in the every-day experience of the engineer.

The volume is divided into thirty-one chapters, and concludes with a good index. Although the work has been designed for students of engineering and architecture—at least this is the modest claim of the author—he also hopes that it may prove a useful book of reference to those engaged in the profession generally. There is little doubt that these hopes will be fulfilled, for after careful perusal we have nothing but praise for the work.

On pp. 409 and 414, "Mr. B. Baker" is quoted. In a future edition it will be as well to give this eminent engineer his proper title. N. J. L.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

I CANNOT but think that my friend Mr. Bottomley is a little hard on Prof. Van der Waals. I am not aware that there is any dispute as to the fact that the methods he employed are open to criticism, and that his formula is only approximately true. In spite of its defects the treatise was regarded by Maxwell at the time of its publication as of very great interest. If, however, Van der Waals is accused of not showing a "proper appreciation of the work of Andrews," the following facts should be considered before judgment is passed:—

(1) The celebrated Bakerian Lecture of Andrews is not directly referred to, but the full account of it which appeared in *Poggendorff's Annalen* (Ergänzungsband v. p. 64, 1871) is quoted (p. 406).

(2) This reference is followed by a long section headed "Experiments of Andrews" (p. 407).

(3) On p. 420 the following passage occurs:—"The signification of the temperature—the critical temperature of Andrews—is clear from what precedes. Below it the substance can exist in the so-called gaseous as well as in the so-called liquid state, &c. The honour of this remarkable discovery, which alters our views as to the so-called permanent gases, and the liquefaction of gases generally, belongs to Andrews. That it was not so easy to reach this conclusion from experiments appears, amongst other circumstances, from Regnault giving in good faith maximum pressures for carbonic acid above 40°."

(4) The phrase, "I have borrowed this remark from Maxwell," which follows the description of the continuous transformation from gas to liquid, is at all events a proof that Van der Waals did not claim priority in the conception of the possibility of such a transformation.

He can therefore have had no possible reason for desiring to credit Maxwell, rather than Andrews, with this idea, especially in view of the facts that Maxwell himself (p. 119, first edition, "Theory of Heat") laid no claim to it, and that it is most clearly expressed in the abstract of the work of Andrews (*Pogg. Ann., loc. cit.*), to which Van der Waals himself refers his readers.

(5) The preface is not happily worded, but I think that the phrases employed do not necessarily bear the interpretation which Mr. Bottomley attaches to them.

The context shows that the "connection between the gaseous and liquid condition," which Van der Waals claims to have established, is not the possibility of a continuous transformation from one to the other through a series of stable states, but that "both portions of the isotherms belong to one curve, even in the case in which these portions are connected by a part which cannot be realized."

He is referring to the work of James Thomson, not to that of Andrews, and his claim, as I read it, is to have deduced "from theoretical considerations" a form of the isothermal which, as the passage on p. 416 shows, he fully admits that James Thomson was the first to suggest and to support by sound argument. Again, I do not understand that Van der Waals claims to be the originator of the "conception" of the continuity of the liquid and gaseous states. He only says that his conception of their *identity*, which, in the sense in which he uses the word, he admits to be doubtful, has proved a "fruitful" hypothesis. He defines *identity* to mean that the molecule is not more complex in the liquid than in the vaporous state. His calculations are based on this assumption, and he fully admits that they only apply in cases where it is justified.

While, then, I agree with Mr. Bottomley that an explicit tribute in the preface to Andrews and to James Thomson would have been graceful on the part of Van der Waals, I do not think that there is any evidence of an attempt to claim for himself credit which is due to others. A. W. RÜCKER.

SINCE my letter which was published in your last issue was written, I have found that the first edition of Maxwell's "Theory of Heat" contains a diagram, intended to represent the isothermals of carbonic acid substance, with all, or almost all, the faults of the diagram of Prof. Van der Waals; and from this, no doubt, Van der Waals's diagram was taken. Consequently I beg leave to withdraw absolutely the words used in my letter, viz. "The curves seem certainly not taken from Maxwell," and also a succeeding sentence which gave my reason for this opinion. I am sorry for my error; but I was not aware, or rather had quite forgotten, that Maxwell's first edition contained this faulty diagram.

My criticism of Van der Waals's essay is in no way altered, however, unless perhaps it is a little strengthened. Maxwell became alive to the faultiness of his diagram, at any rate prior to 1875, and corrected it. Unfortunately, Prof. Van der Waals and the translators had not reached a clear understanding of the physical meaning of these curves in 1890, even with the aid of Maxwell's second edition. J. T. BOTTOMLEY.

13 University Gardens, Glasgow, March 10.

Surface Tension.

I SHALL be obliged if you can find space for the accompanying translation of an interesting letter which I have received from a German lady, who with very homely appliances has arrived at valuable results respecting the behaviour of contaminated water surfaces. The earlier part of Miss Pockels' letter covers nearly the same ground as some of my own recent work, and in the main harmonizes with it. The later sections seem to me very suggestive, raising, if they do not fully answer, many important questions. I hope soon to find opportunity for repeating some of Miss Pockels' experiments. RAYLEIGH.

March 2.

Brunswick, January 10.

MY LORD,—Will you kindly excuse my venturing to trouble you with a German letter on a scientific subject? Having heard of the fruitful researches carried on by you last year on the hitherto little understood properties of water surfaces, I thought it might interest you to know of my own observations on the subject. For various reasons I am not in a position to publish them in scientific periodicals, and I therefore adopt this means of communicating to you the most important of them.

First, I will describe a simple method, which I have employed for several years, for increasing or diminishing the surface of a liquid in any proportion, by which its purity may be altered at pleasure.

A rectangular tin trough, 70 cm. long, 5 cm. wide, 2 cm. high, is filled with water to the brim, and a strip of tin about 1½ cm. wide laid across it perpendicular to its length, so that the under side of the strip is in contact with the surface of the water, and divides it into two halves. By shifting this partition to the right or the left, the surface on either side can be lengthened or shortened in any proportion, and the amount of the displacement may be read off on a scale held along the front of the trough.

No doubt this apparatus suffers, as I shall point out presently, from a certain imperfection, for the partition never completely shuts off the two separate surfaces from each other. If there is a great difference of tension between the two sides, a return current often breaks through between the partition and the edge of the trough (particularly at the time of shifting). The apparatus, however, answers for attaining any condition of tension which is at all possible, and in experiments with very clean surfaces there is little to be feared in the way of currents breaking through.

I always measured the surface tension in any part of the trough by the weight necessary to separate from it a small disk (6 mm. in diameter), for which I used a light balance, with unequal arms and a sliding weight.

I will now put together the most important results obtained with this apparatus, most of which, though perhaps not all, must be known to you.

I. *Behaviour of the surface tension of water.*—The surface tension of a strongly contaminated water surface is *variable*—that is, it varies with the size of the surface. The minimum of the separating weight attained by diminishing the surface is to the maximum, according to my balance, in the ratio of 52 : 100.

If the surface is further extended, after the maximum tension is attained, the separating weight remains *constant*, as with oil, spirits of wine, and other normal liquids. It begins, however, to diminish again, directly the partition is pushed back to the point of the scale at which the increase of tension ceased.

The water surface can thus exist in two sharply contrasted conditions; the *normal* condition, in which the displacement of the partition makes no impression on the tension, and the *anomalous* condition, in which every increase or decrease alters the tension.

II. *Mobility.*—Upon the purity of the surface depends its mobility, and in consequence the persistence of a wave once set in motion. So long, however, as the water surface is in its anomalous condition, the damping of the waves is constant,[1] and just at the degree of purity at which *the tension ceases to alter the decrease of the damping begins.*

If the balance is loaded with just the maximum weight which the surface tension can hold, and the normal surface is contracted till the weight breaks away, a measure is obtained of the relative amount of contamination by the ratio of the length of the surface before and after contraction; for, the purer the surface, the smaller must be the fraction to which it is reduced before it begins to enter the anomalous state. By counting, with different relative contaminations, how often a wave excited by a small rod at the end of the trough passed along the surface adjusted to a length of 30 cm. before it ceased to be visible, I obtained approximately the following values for the number of the passages:—

Relative contamination...	0	5	10	15	20	25	30
Number of visible wave passages	17	17	17	17	12	8	3

The numbers of the upper row indicate the length at which the surface becomes anomalous in 30ths of its whole length; those of the second row are, as may be imagined, rather uncertain, particularly the greater ones, although they are the mean of many observations.

A *perfectly clean* surface, whose tension remains constant, even under the greatest contraction, can be approximately produced with the adjustable trough, by placing the partition quite at the end, and pushing it from thence to the middle. The surface on one side is thus formed entirely afresh, from the interior of the liquid.

III. *Effect on a water surface of contact with solid bodies.*—Every solid body, however clean, which is brought in contact with a newly formed surface, contaminates it more or less decidedly, according to the substance of which the body consists. With many substances, such as camphor or flour, this effect is so strong that the tension of the surface is lowered to a definite value; with others (glass, metals) it is only shown by the increase of relative contamination. The contaminating current which goes out from the circumference of a body—for example, of a floating fragment of tinfoil—is easily made visible by dusting the water with Lycopodium or flowers of sulphur. I will call it, for the sake of brevity, "the solution current."

The solution current of a body which is introduced into a perfectly clean water surface lasts until the relative contamination produced by it has attained a definite value, which is *different for every substance.*

Thus the solution current for wax ceases at a relative contamination of 0·55; that of tinfoil at a still smaller one; but that of camphor, not until the surface has become decidedly anomalous, and the separating weight gone down to within 0·80 of the maximum. If, on the other hand, the surface surrounding a small piece of tinfoil be restored to its previous purity, the current begins again with renewed strength, and it appears that this process may be repeated as often as desired without the solution current ever quite disappearing.

From this effect of the contact of solid bodies, it follows that a perfectly pure surface cannot be maintained for long in any vessel, since every vessel will contaminate it. Whether the air and the matter contained in it have a share in the gradual increase of relative contamination which occurs on water left standing, I know not; but the influence of gases and vapours does not appear to me important in general. The contamination by the sides of the vessel does not, however, always go so far as to diminish the tension, which remains normal, for example, in a glass of water, after four days' standing.

With a rising temperature the contamination from all substances seems to increase considerably; but I have not yet investigated this in detail.

IV. *Currents between surfaces of equal tension.*—Between two normal surfaces, which are unequally contaminated by one and the same substance, a current sets in from the more to the less contaminated when the partition is removed; much weaker, indeed, than that exhibited in the anomalous condition by differences of tension, but, all the same, distinctly perceptible. With *equal* relative contamination by the same substance, no current of course sets in. It is otherwise when the contamination is produced by *different* substances.

I contaminated the surface on one side of the partition by repeated immersion of a metal plate, on the other by immersion of a glass plate, which had both been previously carefully cleaned and repeatedly immersed in fresh water surfaces. I then made the relative contamination on the two sides equal (*i.e.* = ½) by pushing in the outer partitions by which the surfaces were inclosed. After the water had been dusted with Lycopodium, the middle partition was removed. I repeated this experiment eight times, with different changes devised as checks.

On the removal of the partition a *decided current* set in each time, *from the surface contaminated by glass to that contaminated by metal*; and when I replaced the partition after the current had ceased, and investigated the contamination on both sides, I always found it greater on the metal than on the glass side.

Thus equal relative contamination by different substances does *not* indicate equality of that (osmotic?) pressure which is the cause of the current between surfaces of equal tension.

For further proof of this result I have made experiments with other substances; for example, with a floating piece of tinfoil on one side, and of wax on the other, when, after they had been acting for a long time, and then the relative contaminations had been equalized, a current resulted from the wax to the tinfoil; and again, with camphor on the one, and small pieces of wood and wax on the other side, which showed a current from the wax and wood to the camphor.

Since, therefore, the water surface assumes dissimilar qualities from contact with different substances, the conviction is forced upon me that it is these bodies themselves (glass, metal, wax, &c.) which are dissolved, though only feebly in the surface, and thereby render it capable under sufficient contraction of becoming anomalous.

V. *Further observations on solution currents.*—The following facts agree with this view. If a newly formed water surface be contaminated by small floating slices of wax until the latter cease to give solution currents, the relative contamination amounts to 0·55. If now another fresh surface is brought to the same relative contamination by tinfoil and a corresponding contraction, and then a slice of wax from the first surface be introduced, it will develop a considerable solution current. This therefore depends on the substance with which the surrounding surface was previously contaminated.

Substances which are properly soluble in water, such as sugar and soda, exhibit a similar behaviour when immersed in the surface, only they continue to act in the anomalous condition.

A crystal of sugar placed in a normal but not perfectly pure surface produces a great fall of tension. If the surface be then made normal again by immersing and withdrawing strips of paper, and if this process be repeated several times, a normal surface is at last attained, which is contaminated *by sugar only*, and on the tension of this the sugar produces no further effect. A piece of soda held in the surface containing sugar greatly lowers the tension; and on the other hand on a surface rendered repeatedly anomalous by soda, soda acts but slightly, and sugar powerfully.

[In this experiment the sugar and soda crystals being instantly

[1] This is not quite exact. I found the number of visible passages constant = 3 in the anomalous state; but the velocity of transmission varying in some degree with the tension, the time required for the vanishing of the wave must really become a little longer when the tension is lowered.—*February* 26.

wetted, they do not really act by solution-currents, for the latter can only be produced by a dry body. The action here is an indirect one by intervention of the deeper layers.—*February* 26.]

VI. *Behaviour of the surfaces of solutions.*—The effect of soluble matter on the surface tension has absolutely nothing to do with the change which the cohesion of the water undergoes, through matter dissolved in the body of the liquid, for both sugar and soda solutions have a *higher* maximum tension than pure water, and yet these same substances introduced into the surface produce a fall in the separating weight.

In order to investigate the behaviour of the surfaces of solutions more closely, I introduced a saturated solution of common salt into the adjustable trough. The freshly formed surface of the solution of salt maintained its normal separating weight, (1·154 of that of water) even when most contracted, though it must necessarily have contained as much salt as the interior of the liquid. The entrance of the anomalous condition, then, does not depend on the absolute quantity of the contaminating substance contained in the surface; but when I placed some salt in contact with the normal surface of the saturated solution, it gave a solution current and lowered the tension, as in the case of pure water. I obtained similar results with a solution of sugar. From these experiments I concluded (*a*) that the surface layer of water can take up *more* of soluble substances than the internal liquid; (*b*) that the surface of a solution is capable of becoming anomalous under contraction, always and only, when it contains more of the dissolved substance than the interior of the liquid.

That the surface layer really possesses a higher dissolving power is further shown by the experiment, which is well known to you, in which a thin disk of camphor, so hung that it is half immersed in the cleanest possible water surface, is cut through in the course of a few hours. I will add by the way, that a newly formed surface of a saturated solution of camphor is *normal* according to my observations, *i.e.* that its tension remains nearly constant under contraction, and that small pieces of camphor floating on it still give solution streams and have slight motions. The solution stream seems in this case to cease just when the surface begins to be anomalous.

What I have further observed regarding solutions in the surface and the like, seems to me less remarkable, and part of it still very uncertain. I therefore confine myself to these short indications, but I believe that much might be discovered in this field, if it were thoroughly investigated. I thought I ought not to withhold from you these facts which I have observed, although I am not a professional physicist; and again begging you to excuse my boldness, I remain, with sincere respect,

Yours faithfully,

(Signed) AGNES POCKELS.

Modern Views of Electricity.

DR. LODGE's doctrine of the slope of potential, explained in his note to my letter in NATURE of February 19 (p. 367), still presents great difficulties. A plate of zinc is covered by a film of air or oxygen in a different state from the surrounding atmosphere. We first consider a point outside of the film. Dr. Lodge says this point is influenced by the ordinary dielectric strain of a static charge imparted to the zinc in any adventitious manner. That is evident. Now, when the zinc was isolated, we had a negative charge upon it, or in the film, and therefore, at the point in question, a positive slope of potential upwards from the zinc. Call it R. When we make contact with copper we introduce a positive static charge on to the zinc. The effect of this at the point in question is a negative slope of potential—that is, downwards from the zinc. Call it $-R'$. Then, as the final result we have an upward slope of potential, $R - R'$, which is less than before contact was made.

Dr. Lodge further says that the static charge imparted to the zinc does not alter the slope of potential *within the film*. By that, I understand the average slope of potential over a line drawn from end to end of the film at right angles to the zinc. Now if the new static charge—suppose σ per unit of surface—be placed close upon the zinc, so as to have the film outside of it, it will diminish the upward slope of potential at all points within the film by exactly $2\pi\sigma$. If we are at liberty to place the new static charge at some distance from the zinc, we may modify this result in any way we please.

Mr. Chattock suggests that the essence of combination between zinc and oxygen is that the zinc atom is + and the oxygen −. By this, I understand him to mean that two atoms of zinc assume equal and opposite charges, and two atoms of oxygen assume equal and opposite charges; and then positive zinc combines with negative oxygen, forming a neutral compound as regards electrification; but the remaining zinc is negative, and the remaining oxygen positive; hence the step of potential from zinc to oxygen. But how would he explain the permanence of these states of electrification?

S. H. BURBURY.

The Flying to Pieces of a Whirling Ring.

HAVING had occasion lately to devise a high-speed whirling-machine, I examined the speed at which it might be safe to work, and some of the results surprise me. For instance, it is easy to show (by equating the normal component of the tension to the centrifugal force of any element) that the critical velocity at which a circular ring or rim of any uniform section will fly, unless radially sustained, is given by $T = v^2\rho$, where T is the tenacity, and ρ the density of its material. Thus a band of steel just able to bear a load of 30 tons to the square inch will fly to pieces at a peripheral speed of about 800 feet a second; and this without reference to its angular velocity, or radius of curvature. It may be objected that no such accident could occur with purely rectilinear motion, but such motion at the critical speed would be very unstable—the slightest shiver of a vibration running along it would precipitate a catastrophe.

Hence a steel girdle round the earth's equator would burst, however thick it might be, were it not for its weight. Again, an Atlantic cable is only held together by its weight. In the early days of cable-laying, it was suggested to ease matters by attaching floating matter to the cable till it was of the same average density as sea-water; but we now see that such a cable, if lying parallel to the equator, could not hold together, unless it were made of 30-ton steel and laid north of latitude 60°.

OLIVER J. LODGE.

Cutting a Millimetre Thread with an Inch Leading Screw.

IT is possible that many who possess a screw-cutting lathe with a leading screw of so many threads to the inch may wish to use it for cutting millimetre screws. While, of course, it is too much to expect that the absolute value of the millimetre, as given in terms of the inch, can be obtained by ordinary change wheels—and this is not of great importance, since, among other reasons, the two determinations of the value of the millimetre in inches differ by one part in a hundred thousand—yet it may not be well known that a most remarkable degree of accuracy may be obtained with wheels in ordinary use. After some trouble I lighted upon the following numbers, which, with a leading screw of eight threads to the inch, give as a result 25·3968, whereas the inch is 25·3995 millimetres. The wheels are 28 on mandril, 100 and 36 on stud, and 32 on screw. The error would therefore, with a perfect lathe, be less than one part in nine thousand, so that a screw cut in this way would for almost all purposes be correct; in fact, it is doubtful if in the case of short screws many lathes could be trusted to cut inch threads more accurately. For leading screws of other pitches, such as 4, 5, 6, or 10 threads to the inch, the wheels can easily be altered so as to give the same result.

Of course it may be the case that this or an equally good arrangement is known to some; but as I had to start working out the combinations of thirteen wheels taken four together, in which each combination contained six sub-combinations, in order to obtain the result, it is possible that it may be appreciated by those to whom it may be of use, but who would rather be saved so much trouble.

C. V. BOYS.

Royal College of Science, London.

P.S.—It may be worth while to add that the wheels taken in order—

	28 ...	*100* ...	36 ...	*32*	with	*8*	threads to the inch
are the same as	28 ...	*32* ...	36 ...	*100*	,,	*8*	,, ,, ,,
or as	7 ...	*8* ...	9 ...	*25*	,,	*8*	,, ,, ,,
or as	7 ...	*8* ...	9 ...	*10*	,,	*20*	,, ,, ,,

where the followers or multipliers are printed in italics. The last sequence of figures is sufficiently curious, and is one that can easily be remembered.—C. V. B.

A Green Sun.

I RECOLLECT reading some years ago in NATURE an observation of Mr. Norman Lockyer, to the effect that he had seen the sun green through the steam escaping from the funnel of a boat on Lake Windermere. In May 1888, I spent a considerable time one day viewing the sun through steam escaping at various pressures from the boilers of a colliery in Monmouthshire. In no case could I, or the friends with me, succeed in seeing the sun of a green colour through the steam, although we viewed it in a very great variety of ways. All we saw was the usual orange or red coloration.

But, this month, I have been watching the sun through steam puffed out from locomotives, and have, on five or six occasions, seen a bluish-green coloration extending over the whole disk. But sometimes the sun appeared simply white, and sometimes it was coloured orange-red. I cannot exactly determine the circumstances which produce the bluish-green; but I have seen it best with freshly puffed steam which had not risen very far above the funnel.

If the vapour particles are assumed of fairly uniform size, the following may be a possible explanation. The rays, coming through particles of vapour (really water particles in suspension) may be retarded as compared with the rays passing between the particles; and, if this retardation is such as to delay the red light passing through the particles one half wave-length, as compared with the red light not so passing, the result would be the destruction of the red element in the white light, and the light left would then appear bluish-green. This suggestion I owe to the courtesy of Sir G. G. Stokes, who also communicated to me the following very interesting observation. When a jet of transparent steam is escaping from a tube, we know, from Mr. Shelford Bidwell's experiments, that the steam becomes visible vapour, if an electrified point is brought near the jet. Sir G. Stokes noticed that the permanent shadow of the vapour on a screen was orange; but that, for a fraction of a second after the commencement of the electrification, a faint greenish shadow, preceding the orange one, was frequently seen. Water globules, about one ten-thousandth of a centimetre in diameter, might produce the requisite retardation.

I shall be glad to hear of other observations of this bluish-green coloration. After the great eruption of Krakatão we know that the sun was seen coloured green and blue.

CHAS. T. WHITMELL.

18 Park Place, Cardiff, February 26.

Frozen Fish.

THAT fish suffer, when imprisoned under a covering of ice in comparatively shallow ponds or lakes, goes without saying. But do they necessarily die when inclosed for lengthened periods in solid ice? My own opinion is that the latter condition is far less injurious to them than the former. It is a question of importance, for it concerns the conditions under which fish probably exist in comparatively shallow waters in high latitudes.

In one of the "Arctic Voyages" (I am not able, at this moment, to give the reference, and rely upon memory) it is distinctly stated that fish (carp, I think), frozen so hard as to necessitate the use of an axe in order to divide them, revived when thawed before the cabin fire, and "jumped about," as is usual with fish out of water.

There are fish and fish. Has the severe winter of 1890–91 caused any important mortality; if so, to which in particular?

R. MCLACHLAN.

Lewisham, March 6.

Zittel's "Palæontology"—Reptiles.

IT has been pointed out to me that in my review of Prof. v. Zittel's "Palæontologie" (March 5, p. 420) I have omitted to mention that, although other writers have placed the Dolichosauria next to the Pythonomorpha, it is only in a paper recently read by Mr. G. A. Boulenger before the Zoological Society, but not yet published, that the one group was considered to be the ancestor of the other. R. L.

March 9.

THE CHEMICAL SOCIETY'S JUBILEE.

AT the meeting in celebration of the Jubilee of the Chemical Society, held in the theatre of the London University on Tuesday, February 24, 1891, the proceedings were opened by the following address from the President, Dr. W. J. Russell:—

We meet to-day to celebrate the fifty years' existence of our Society, a time, if measured by the progress which our science has made, equal to centuries of former ages, but which in years is so brief a space that we have, I am happy to say, with us to-day some of those who were present and who took an active part in the foundation of the Society, and I need hardly say with how much interest we shall listen to their reminiscences of the time and circumstances connected with the birth of our Society.

I would, by way of introduction, say a few words, first, with regard to our Society, and afterwards with regard to the state of chemistry in England when our Society was founded. We boast, and I believe rightly, that our Society holds the distinguished position of being the first which was formed solely for the study of chemistry. Chemistry and physics, twin sisters, had hitherto always dwelt together, and many were the societies, both in this country and abroad, devoted to their joint study and development.

In London there was the Royal Society, which had hitherto received the most important chemical papers; there was also the Society of Arts, which is 110 years, and the British Association, which is ten years, senior of our Society. In Manchester the Literary and Philosophical Society had been founded and actively at work since 1781; and we admit that our neighbours at Burlington House, the Astronomical, Antiquarian, Linnean, and Geological Societies, are all our seniors: they had a distinct individuality and literature of their own, which called them into existence some forty to eighty years before the commencement of our Society. Small private chemical societies, no doubt, existed: they are the natural forerunners of a large society, and become merged into it. The Chemical Section of the British Association, which is an ephemeral and peripatetic Chemical Society, had existed from the founding of that body. If we turn to other countries, we find that, much as our science had been cultivated on the Continent, it did not until later times engross a whole society to itself, the French Chemical Society not having been formed until 1857, and the now great Berlin Chemical Society not until 1868. Our interest, however, at the moment is rather in the growth of chemistry in this country than in what occurred elsewhere.

To-day we may learn how it came about that the first Chemical Society was established in England. I may, however, state that the reason for our meeting depends on the official record that on February 23, 1841, twenty-five gentlemen "interested in the prosecution of chemistry" met together at the Society of Arts to consider whether it be expedient to form a Chemical Society. Of the twenty-five who then met I am happy to say three are present—Sir W. Grove, Sir L. Playfair, and Mr. Heisch; and Mr. J. Cock is another of this band who is still alive but is not present.

These twenty-five gentlemen appear without dissent to have come to the conclusion that it *was* expedient to form a Chemical Society, and appointed a committee of fourteen to carry this resolution into effect. So expeditious were they in their work, that in little more than a month the first general meeting was held, and the provisional committee brought forward a report embodying a plan for the constitution and government of the Society, and this plan remains essentially the same, save in one point, to the present day. I refer to the formation of a museum of chemical specimens; this project was abandoned some years ago. It is worth recording that at this first general meeting Thomas Graham was elected President; Messrs. W. T. Brande, J. T. Cooper, J. F. Daniell, R. Phillips, Vice-Presidents; Mr. Arthur Aikin, Treasurer; Messrs. Robert Warington, E. F. Teschemacher, Secretaries; Council—Dr. T. Clarke, Rev. J. Cumming, Dr. C. Daubeny, Messrs. T. Everitt, T. Griffiths, W. R. Grove, H. Hennell, G. Lowe, W. H. Miller, W. H. Pepys, R. Porrett, Dr. G. O. Rees. Also that the Society then numbered seventy-seven members. We hail Sir W. Grove as being the most active member who is still among us in founding our Society, for he was a member of the first Council, was present at the first meeting, and was a member of the provisional committee. I must here add to the official record, for it does not tell us how these twenty-five gentlemen "interested in the prosecution of chemistry" were collected together at one time and place. Obviously some special force was required to build up this complicated molecule; that special force was embodied in and exercised by Robert Warington. By his activity and energy he brought about this meeting, and we can imagine how difficult and troublesome a work it probably was, how some of these gentlemen had to be instigated to action, others repressed, some convinced that the aim was desirable, others that it was feasible. But whatever the difficulties were, Mr. Warington succeeded, and to him we are indebted for the formation of our Society. Although he has passed away, he is ably represented here to-day by his son. The love for the Chemical Society has proved to be hereditary: Mr. Warington of to-day is a most active and valued member; is one of our Vice-Presidents: and, as our programme shows, is about to present to us records connected with the early history of our Society which are of great interest now and will become of increasing value as time goes on.

I turn now at once from these matters immediately connected with our Society to the consideration of what was being done in chemistry in this country fifty years ago. At that time public laboratories for the systematic teaching of chemistry did not exist in London. The number of real students of chemistry in this country was very small. They were looked upon by their friends as being eccentric young men, who probably would never do any good for themselves, and these few students found practical instruction in the private laboratories of some of the London teachers.

The practical teaching of chemistry appears to have been undertaken in Scotland much earlier than in England, for Dr. D. B. Reid held practical classes at the University of Edinburgh as early as 1832. Graham came to London from Glasgow in 1837, and until the opening of the Birkbeck Laboratory, in 1846, he had from time to time private students working in his laboratory. And so with the other teachers, who all had private or articled pupils. I doubt whether the pupils received much systematic instruction, but they gained an insight into laboratory work, saw how apparatus was put together, and how analyses were made. We have indeed to wait some years before public laboratories are established, for not till 1845 is the College of Chemistry opened, and this appears to have been really the first public laboratory in London, and its object, as stated by its founders, is "to establish a practical School of Chemistry in England." About the same time both University and King's College established laboratories. The Council of our Society recognized the importance of these occurrences, for in the Annual Report in 1847, they say, "although an event not immediately connected with the Society, the Council has much pleasure in commemorating the late successful establishment in London of chemical laboratories expressly designed to further the prosecution of original research. The new laboratories of the College of Chemistry, and of the two older Colleges of the London University, now offer facilities for practical instruction and research not surpassed we believe in any foreign school."

While speaking of laboratories in London, I should however mention that the Pharmaceutical Society established a laboratory especially if not exclusively for its own students as early as 1843.

It was not till several years later, till 1850 and 1851, that the medical schools in London established classes of practical chemistry.

If we consult the scientific journals of the time immediately preceding the formation of our Society, we find it was by no means a period of chemical activity in this country, but rather a dull time, given more to the study and slow development of the science than to discovery. Methods of analysis, both organic and inorganic, had been much improved, and the dominant idea was the determination of the empirical composition of bodies, and the preparation of new compounds, whose existence was predicted by a study of Dalton's "Atomic Theory." Graham, Kane, and Johnson of Durham were the leaders in scientific chemistry, and the authors of the most important chemical papers of the time. Graham had very lately published his notable paper on the constitution of salts, a paper which gained for him, some years after its publication, a Royal medal. Kane was an active worker and a bold theorist, and at this time his reputation was much increased by a paper on the chemical history of archil and litmus. Johnson was also a most active chemist. His contributions relate to many branches of the science, but especially to the chemical composition of minerals. In 1841, however, he is engaged on a long series of papers on the constitution of resins. He will probably be best known and remembered as an agricultural chemist. Faraday we can hardly claim as a chemist at this time, for he was then rapidly publishing his long series of experimental researches in electricity. While speaking of electricity I should state that it was in 1840 that Smee described his battery, and the Society of Arts awarded him a gold medal for it. An important branch of our science was, however, coming into existence, a branch which has found many and sucessful investigators in this country. I mean photography. It was in 1840 that Herschel published in the Philosophical Transactions his elaborate paper on the chemical action of the rays of the solar spectrum, a paper in which he recognizes a new prismatic colour beyond the violet, and chemical activity in the spectrum beyond the red, and besides discussing many other matters, establishes his previously discovered hyposulphite of soda as the best agent for the fixing of sun pictures. Fox-Talbot had previously given an account of photogenic drawing, and claims that as far back as 1835 he took pictures of his house by means of a camera and chloride of silver paper, but it is not till 1838 that the Secretary of the Royal Society extracts from him a clear account of the details of his process, and it is in 1841 that he is granted a patent for improvements in obtaining pictures or representations of objects. Again, in the following year Herschel publishes another paper of much importance. I can here only mention how actively this line of research was prosecuted by Robert Hunt, how many, ingenious, and interesting were the experiments he made, and how valuable was the account he afterwards gave of this subject in his "Researches on Light." Thus the work done in this branch of chemistry at the time of which I am speaking is certainly noteworthy, probably more so than in other branches of chemistry. In fact, of other advances in chemistry there is little to record, but I may mention that Clarke's process for determining the hardness of water also holds its jubilee this year, for it was in 1841 that a patent was granted to Dr. T. Clarke for a new mode of rendering certain waters less impure and less hard.

Not a single chemical paper appears in the Phil. Trans. for 1841, but there are two papers which were much discussed at this time, and although they were readily shown to be erroneous, still are interesting as indicating the chemical ideas of the day. One is by Robert Rigg, who is carrying on an experimental inquiry on fermentation, and is termed "Additional Experiments on the Formation of Alkaline and Earthy Bodies by Chemical Action when Carbonic Acid is present"; it is published in the Proceedings of the Royal Society. The other is a paper by Dr. S. M. Brown, entitled "The Conversion of Carbon into Silicon," and is published in the Transactions of the Royal Society of Edinburgh.

With regard to the first paper, Mr. Rigg believes that he has demonstrated that, when fermentation takes place, a great and direct increase in alkaline and earthy salts, viz. of potass, soda, and lime occurs, an increase varying from fifteen to nineteen times the original amount. Denham Smith, who has only very lately passed away, showed that the theory simply rested on inaccurate experiment.

The object of the other paper is to demonstrate that, on heating paracyanogen, nitrogen is given off, and a residue of silicon remains. Dr. Brett and Mr. Denham Smith controverted this, and, in a paper in the *Philosophical Magazine*, proved that the supposed silicon was simply carbon in a very incombustible state. So important an experiment was this alleged conversion of carbon into silicon considered to be at the time of its publication, that it attracted Liebig's attention, and in a letter to Dr. Playfair, which was communicated to the meeting of the British Association at Plymouth, in 1841, Liebig says he has repeated Dr. Brown's experiment on the production of silicon from paracyanogen, but has not been able to confirm one of his results.

As far as pure chemistry is concerned it was rather a time of repose. The beginning of the century had been a brilliant time for chemistry in England. Dalton had published his atomic theory; Davy had decomposed potash and soda, and had demonstrated that chlorine was an element; and Cavendish and Wollaston were then still at work. In fact the most important discoveries of that time were made in this country, but I fancy that during this later period a feeling grew up that the age of brilliant discoveries was over, and that, apart from the preparation of a few new compounds, the essential work of the time was analysis and the determination of the percentage composition of bodies. Still much quiet study of the science was going on, as is indicated by the considerable demand which existed for good text-books. Henry's, Turner's, Kane's, and Graham's "Chemistry"—all these, without mentioning others, went through numerous editions, and played a very important part in the spread of chemical knowledge in our country.

Another text-book, which is interesting as showing how little organic chemistry was studied in this country, is Dr. Thomas Thompson's work on "Vegetable Chemistry." Dr. Thompson states in his preface that the object of the book is to lay before the British public a pretty full view of the present state of the chemistry of vegetable bodies, and further, he says, "that the ultimate analyses he gives have, with very few exceptions, been made upon the Continent, and principally in Germany and France. British chemists have hardly entered on the investigation." Evidently then at this time organic chemistry had been but little studied in this country.

When our Society was founded, Thomas Graham was certainly the most distinguished chemist in England. He came to London in 1837 as professor of chemistry at University College, succeeding Edward Turner. The work he had already accomplished was of a high order, and he was now occupied in writing his book, which appeared in 1842.

The book was an admirable account of the chemistry of the time; it contained a well arranged and clearly written introduction, describing the principles and latest discoveries in those branches of physics which bear most directly on chemistry. There was also an able and succinct account, probably the best which had then appeared in this country, of organic chemistry; and with regard to physiological chemistry, he states in the preface that he gives a "condensed view of the new discoveries in this department, which now enters for the first time into a systematic work on chemistry."

There are, however, indications that a knowledge of the discoveries and discussions going on on the Continent only slowly reached this country. This is strongly insisted on in the *Phil. Mag.* of 1841, by Messrs. Francis and Croft, who state that "but little of what is done abroad, especially in Germany, seems to find its way into England, or at least until the lapse of some years." In proof of this statement they mention results lately published by Dr. Apjohn, Prof. Johnston, and Dr. Golding Bird, all of which had been known on the Continent some time previously. A valuable series of communications described as "Notes of the Labours of Continental Chemists," is afterwards communicated by these chemists to the *Phil. Mag.*, and continued for several years.

The visit of Liebig in 1837, when he attended the meeting of the British Association at Liverpool, must have given some stimulus to the study of organic chemistry in England, and we find that he undertook to report to the British Association on "Isomeric Bodies," and also on organic chemistry, and this great undertaking resulted in his two works, the one "Chemistry, in its application to Agriculture and Physiology," and the other, "Chemistry, in its applications to Physiology and Pathology." Both books were dedicated to the British Association, the first appearing in 1840, the second in 1842. It is very difficult for us now to realize the importance of these works, and properly to appreciate not only the large amount of new knowledge which they contained, but, what is of still greater importance, the novelty of treating such subjects in a truly scientific spirit. Gradually this treatment of the subjects became understood and appreciated, and people took a higher view of chemistry, and regarded it as a true science, and not merely as a study which might lead to useful results.

If then it be true that chemistry at this epoch was not rapidly progressing in this country, we naturally ask how it came about that our Society from its very foundation was so successful. The explanation is not difficult to find, nor doubtful, for we have only to turn from our own country to the Continent and learn what is happening there. Liebig is at Giessen, Wöhler at Göttingen, Bunsen at Marburg, Dumas, Laurent, Gerhardt, and a host of distinguished and active chemists in France, and at this time even Berzelius and Gay Lussac are alive. Liebig, with his wonderful energy and ability, was powerfully advocating the theory of compound radicles, and was extending in every direction our knowledge of organic chemistry, and inspiring all who came within the range of his influence with a love for investigation. Dumas, at the same time, both as a chemist and a finished advocate, was advancing his views on substitution and chemical types. Laurent, and afterwards Gerhardt, were with conspicuous ability showing how these theories were to be extended and modified so as to assume a form which has even with the lapse of time been but little altered. Thus on the Continent it was a time of wonderful activity; chemistry was every day becoming more of a true science, and the constitution as well as the composition of bodies was actively being discussed and investigated. This activity on the Continent took time to reach and really affect us here. The older chemists thought the new theories were visionary and unsound, the simple theories of their younger days were being swept away, and only slowly did they realize the meaning of the newer form of their science; but the wave of progress could not be stopped, and in this country we had been ripening for the change. Clearly the immediate cause of this sudden increase of chemical activity in England was Liebeg. His famous school had now been established for several years at Giessen, and if the older men in this country did not altogether put their trust in him, the younger men, breaking through all restraint, flocked from this country to his laboratory, there to become indoctrinated with his enthusiasm for the study of chemistry, and to learn how scientific investigation was to be carried on. At this epoch our Society was founded, and our Journal shows how successful Liebig's teaching was, how a new spirit was instilled into English chemistry, and how much valuable work his students did. Our Society gave them a ready means of publishing their discoveries, and a meeting-place for discussion and mutual interchange of ideas. Thus do I explain the success which from the first has attended on our Society; and having now led you to this point I stop, for my part was merely to speak the prologue, and I leave the story of the development of our Society in other hands.

INFECTIOUS DISEASES, THEIR NATURE, CAUSE, AND MODE OF SPREAD.[1]

II.

ONE of the earliest and most important discoveries was that made by Pasteur as to the possibility of attenuating in action an otherwise virulent microbe—that is to say, he succeeded in so growing the microbes, that when introduced into a suitable animal they caused only a mild and transitory illness, which attack, though mild, is nevertheless capable of making this animal resist a second virulent attack. Jenner, by inoculating vaccine, inoculated a mild or attenuated small-pox, and by so doing protected the individual against a virulent small-pox Pasteur succeeded in producing such an attenuated virus for two infectious diseases—chicken cholera and splenic apoplexy or anthrax; later on also for a third—swine erysipelas. For the first two he produced cultures grown under certain unfavourable conditions, which owing to these conditions lose their virulence, and when inoculated fail to produce the fatal disease, which they would produce if they were grown under normal conditions. What they produce is a transitory mild attack of the disease, but sufficient to protect the animal against a virulent form; thus in anthrax he showed that by growing the *Bacillus anthracis* at a temperature of 42°·5–43° C. for one week, the bacilli become slightly weakened in action; growing them for a fortnight at that temperature, they become still more weakened, so much so, that if this culture (*première vaccine*) be injected into sheep or cattle (animals very susceptible to anthrax) the effect produced is slight; then injecting the culture which has been growing only eight days at 42°·5, the effect is a little more pronounced, but not sufficient to endanger the life of the animal. Such an animal, however, may be regarded as having passed through a slight attack of anthrax, and as being now protected against a second attack, however virulent the material injected. In the case of swine erysipelas, Pasteur found that the microbe of this disease, transmitted through several rabbits successively, yields a material which is capable of producing in the pig a slight attack of swine erysipelas, sufficient to protect the animal against a second attack of the fatal form. Passing the anthrax virus from however virulent a source through the mouse, it becomes attenuated, and is then capable of producing in sheep only a mild form of disease protective against the fatal disorder. Attenuation of the microbes has been brought about outside the body by growing them under a variety of conditions somewhat unfavourable to the microbe.

Attenuation of the action of the anthrax microbes has been produced by adding to the cultures some slightly obnoxious material (*e.g.* mercuric bichloride 1 : 40,000), by which the growth is somewhat interfered with; or subjecting an otherwise virulent culture for a short time to higher degrees of temperature (anthrax to 56° C. for five minutes; fowl enteritis, twenty minutes, 55° C.); or exposing them for short periods to some obnoxious chemical substance (*e.g.* anthrax to carbolic acid, anthrax to bichloride of mercury 1 : 25,000 for twenty minutes); or the microbes are passed through, *i.e.* are grown in the body of certain species of animals, whereby the microbes become weakened as regards other species (swine erysipelas, anthrax, diphtheria, and tetanus); finally, some microbes become attenuated spontaneously, as it were, by growing them in successive generations outside the animal body, *e.g.* the pneumonia microbe, the erysipelas microbe, and others. However good the nutritive medium, these microbes gradually lose their virulence as cultivation is carried on from subculture to subculture; in diphtheria the culture which was virulent at first loses its virulence as the same culture becomes several weeks old.

All these facts are of considerable importance, inasmuch as they enable us to understand how, in epidemics, the virulence of the microbe gradually wears off and becomes ultimately *nil*, and because they indicate the ways of attenuating microbes for the object of protective inoculations.

Another important step in the study of Bacteria was this: it was shown that they have, besides their special morphological and cultural characters, definite chemical characters. Specific chemical characters (specific ferment actions) of Bacteria have been known for a long time through the earlier researches of Pasteur—*e.g.* the Bacteria causing the acetic acid fermentation of alcohol, the mucoid fermentation, *e.g.* when beer becomes ropy, the lactic acid fermentation of milk-sugar, when milk becomes spontaneously sour, &c.

Similarly, it has been shown that when animal or vegetable matter undergoes the change known as putrefaction or putrid decomposition, substances are produced which resemble alkaloids in many ways, and which, introduced into the circulation of man or animals, act poisonously, the degree of action depending, *cæteris paribus*, on the dose, *i.e.* the amount introduced. These alkaloids—called ptomaines of Selmi—have been carefully investigated and analyzed by Brieger; they are different in nature according to the organism that produces them, and according to the material in which this organism grows: neurin, cadaverin, cholin, &c., are the names given to these substances.

Recent research has shown that pathogenic Bacteria, *i.e.* those associated with, and constituting the cause of specific diseases, are capable of elaborating poisonous substances—toxalbumins or toxins, as they are called—not only in artificial culture media, but also within the human or animal body affected with the particular pathogenic microbe. Thus, in anthrax or splenic apoplexy, Hankin and Sidney Martin have shown this to be the case; in diphtheria (Fraenkel and Brieger), in tetanus (Kitisato), similar toxins have been demonstrated. We can already assert with certainty that a microbe that causes a particular disease causes the whole range of symptoms characterizing the disease by means of a particular poisonous substance or substances it elaborates in and from the tissues of the affected individual.

Another important fact ascertained about some of the toxic substances produced by the different pathogenic Bacteria was this: that if, after they are elaborated in an artificial culture fluid, and, by certain methods of filtration, separated from the Bacteria, and injected into a suitable animal, they are capable of producing the same

[1] Lecture delivered at the Royal Institution on Friday, February 20, 1891, by E. Klein, M.D., F.R.S., Lecturer on Physiology at St. Bartholomew's Hospital Medical School. Continued from p. 419.

disease as their microbes, the rapidity and intensity varying with the amount introduced, so that it became evident that also in the human and animal body the intensity of the particular disease depends, amongst other things, on the amount of poisonous substance elaborated by the Bacteria in the tissues. A further important step made was this: that if the poisonous substance be introduced in such doses that only slight disturbance would follow, and the dose be repeated several times, the body of the animal eventually becomes refractory to the growth and multiplication of the particular Bacteria.

Wooldridge's researches on septicæmia and on anthrax, Roux's researches on septicæmia and diphtheria, Beumer and Peiper, Salmon, and many others, have shown the same for a variety of infectious diseases: in all these instances it has been proved that, when the chemical products of a specific microbe, elaborated in an artificial culture medium or in the animal body, are injected into a healthy animal, this latter is rendered refractory against the specific microbe, so that, if the specific microbe be introduced into such a prepared animal, the microbe cannot grow and multiply, and cannot therefore produce the disease. Pasteur's brilliant researches on protective inoculation against hydrophobia are based on this principle.

The same explanation applies also to those diseases, like scarlet fever, anthrax, fowl cholera, swine fever, certain forms of septicæmia, in which a first, even mild attack is sufficient to protect the animal against a second attack, however large the number and however great the virulence of the particular Bacteria introduced.

For it must be obvious that it is practically the same, whether the protective amount of the toxic substance is produced by the Bacteria in the animal body, as is the case during a mild first attack of the disease, or whether the protective amount of toxin is elaborated outside the body, *i.e.* in an artificial culture, and is then introduced into the animal body. In both instances the effect is the same, viz. the animal body is hereby rendered capable of withstanding the growth and multiplication of the particular Bacteria when a new invasion takes place.

What is the cause of this immunity or refractory condition?

In order to explain this, I wish first to draw your attention to the familiar fact that different species of animals, and even different individuals of the same species, offer a different degree of resistance to the different infectious diseases. Whereas splenic fever or anthrax is communicable to man, rodents, and herbivorous animals, it is only with difficulty communicable to carnivorous animals or birds; cholera and typhoid fever are not communicable to any but man; diphtheria is communicable to the human species, to guinea-pigs, cats, and cows, it is not communicable to some other animals; tubercle or consumption is communicable to man and herbivorous animals, in a less degree to carnivorous animals, though these also take it but in a smaller intensity; certain other diseases are common to animals, but are not communicable to the human species.

If we inquire into the cause of this different susceptibility, we find some very striking facts. Take anthrax: cold-blooded animals, *e.g.* frogs, are unsusceptible as long as they are in their natural conditions of temperature; but if a frog be kept at the temperature of a warm-blooded animal, it is found susceptible to anthrax (Petruschki). Birds are not susceptible to anthrax, but if its temperature be lowered a few degrees it becomes susceptible to anthrax (Pasteur).

Or take another instance: rats are not susceptible to anthrax, but if the animals be kept for some time under severe muscular exercise, they become susceptible to the disease. Tame mice are unsusceptible to glanders, but if phlorizin is administered to them for some days, whereby a deposit of sugar takes place in their tissues, they become susceptible to glanders. The susceptibility and unsusceptibility are expressed by saying that the living tissues in an animal offer in the one case a favourable, in the other an unfavourable, soil for the growth and multiplication of the microbe, and that this unfavourable condition can be altered in a variety of ways, *e.g.* temperature, muscular fatigue, sugar in the tissues, &c. But also a primary favourable condition can be rendered unfavourable: for instance, a human being that has passed through one attack of scarlatina offers tissues unfavourable for the growth of scarlatina microbes; attenuated anthrax protects against virulent anthrax; an animal that has been first treated with repeated small doses of the chemical products of a particular microbe becomes unsusceptible to that microbe.

In order to explain the whole group of phenomena of refractory state, immunity, and protection, a theory has been put forward which is as simple as it is fascinating. There can be truly no greater satisfaction and no greater aim in any branch of science than to express a great number of facts and phenomena by the simplest possible formula; the greatest minds and the most successful philosophers have achieved this. Now, in regard to the numerous and extremely complex phenomena that we have under consideration, a simple formula has been put forward which is supposed to cover all the facts and to explain all the phenomena; this formula is comprised in a single word, "phagocyte." This word is put forth whenever and wherever a difficulty arises in explaining or understanding the complex problems involved in the intimate pathological processes, the refractory condition, the unsusceptibility to and immunity from an infectious disease. To any and every question referring to infectious diseases the answer is simply "phagocyte." By a "phagocyte" is understood one of those elementary microscopic corpuscles abounding in the animal and human body, possessed of spontaneous or amœboid movement, and occurring in the blood as white blood-cells or leucocytes; in the lymph and lymph glands and most tissues, as lymph corpuscles; in all acute and chronic pathological processes, as pus-cells or round cells. The cells, by their protoplasmic or amœboid movement, have the power to take up into their interior all manner of minute particles or granules, living or non-living; it seems as if these granules and particles were being swallowed up, eaten, and destroyed by the cells—hence the name of eating globule, or "phagocyte," given to them.

These cells—white blood-cells, lymph-cells, or round cells—are supposed to have the important function to act as the sanitary police against the invading Bacteria, to be always on the look-out for them, and where they meet them to at once engage in battle with them; that is to say, to do as the giants do—to eat their victims. If the phagocytes are victorious—that is, if they succeed in eating up the Bacteria—no harm is done to the animal body; no disease is produced; but if, for one reason or another, the Bacteria succeed in evading the grasp of the phagocyte police, then the Bacteria grow and multiply, and cause the disease. Sometimes this latter result follows on account of the phagocytes not being capable of moving sufficiently briskly to the places of mischief, or, for some inherent reason, not being able to cope with the Bacteria, or being altogether indifferent to the presence of the enemy; when this is the case in an animal or human body, the phagocytes being powerless to destroy the Bacteria, we are supposed to be dealing with a body that is susceptible to the disease; but when the phagocytes do their duty, then the body is unsusceptible to the disease. Again, when an animal or human being, by a mild first attack, or by protective inoculation of one kind or another, becomes unsusceptible to a second attack, this is explained by saying that, though the phagocytes have not done, or have not been able to do, their duty during that first attack, they have now been rendered capable of doing it.

Now, if you ask what is the evidence on which this theory of phagocytosis is based, you will find that it is of the most slender kind, and you will further find that there is an overwhelming number of observations which directly negatives this theory of *universal* phagocytosis, and, moreover, proves conclusively that if phagocytosis has any share in producing a refractory condition on the part of animals towards a particular infectious disease—be that a primary unsusceptibility or an acquired immunity against a second attack—this share is of a remarkably small degree. The whole theory was started by Metschnikoff by the interesting and fundamental observation that, if anthrax bacilli are introduced into the dorsal lymph-sac of the frog—an animal unsusceptible to anthrax while living under normal conditions—the bacilli become inclosed in the lymph-cells, and are gradually broken up; they do not multiply, and do not therefore set up the disease anthrax. This observation, which is easily verified, was the starting-point for the theory. Metschnikoff and others have described similar appearances in other conditions of refractory states. Now the above observation is explained by Metschnikoff in this way: the lymph-cells are acting the part of guardians, swallowing up the bacilli and preventing them from entering the circulation, and thereby preventing the outbreak of the disease. It must seem very extraordinary that this should be really a true explanation of the refractory state of the frog towards anthrax, considering that the bacilli, like other minute particles, when injected into the lymph-sac, would be absorbed and brought into the circulation in a few minutes, nay, seconds—at any rate some hours before the phagocytes have got into the lymph-sac in sufficient numbers to do battle with the bacilli. That the bacilli really enter the circulation in this and other cases, but are destroyed by the blood, not by the leucocytes but by the fluid part of the blood—the plasma—has been abundantly proved; and it has likewise been proved that the fluid part of the blood and of the lymph in general has a remarkable germicidal action, independent of any cellular elements, leucocytes, or other cells. The observation of Metschnikoff admits of an explanation different from that given by him; it may mean, and probably does mean, that the bacilli cannot exist in the fluid of the lymph and of the blood; they are destroyed here, but they *take refuge in the leucocytes or lymph-cells in which they can live and exist*—for a time, at any rate—these cells offering to them more favourable conditions of existence. In the case of the normal frog, this is also only of a temporary nature, since the substance of the lymph-cells suspended in the lymph or in the blood-plasma becomes likewise permeated with the germicidal influence of the fluid lymph and the fluid plasma, and hence the bacilli soon die, even in the substance of the cells. This explanation is in perfect harmony with the large number of carefully conducted experiments of a host of workers (Fodor, Nutall, Buchner, Petruschki, Lubarsch, and others), according to whom the refractory condition of an animal to a particular infectious disease is due to a chemical germicidal action of the tissue-juices, the lymph, or blood plasma, and independent of any cellular elements. The more pronounced this germicidal action of the juices is, the more refractory the animal. Hence we find that, for instance, when in an animal even a very small number of Bacteria introduced are sufficient to produce disease, the germicidal action of the living blood fluid is very small indeed; but when a considerable number of bacilli are required to produce infection, as in animals possessing only a slight refractory power, this germicidal action of the blood and tissue fluid is greater, and it is greatest of all in those bodies in which not even a large number of bacilli can produce infection, as is the case in animals possessed of immunity. As stated just now, this part of the subject, as to the germicidal action of the fluid of the tissues and the blood, has been worked out very carefully, and it has been shown that, as regards the destruction of Bacteria, the leucocytes of the lymph and blood, or other similar cells, might just as well be absent altogether. Quite recently the whole argument has been clenched by showing[1] that if an animal susceptible to a particular disease be first infected with the bacilli causing this disease, and then into such an infected animal the cell-free serum or plasma of the blood of an animal refractory to that disease be injected, the development of the disease in the former animal is stopped or prevented. Thus mice are very susceptible to anthrax; if they are infected with anthrax bacilli they die of virulent anthrax within thirty-six to forty-eight hours; but if simultaneously with, or soon after, the introduction of the virulent anthrax bacilli, blood of frog or blood of dog (both animals very refractory to anthrax) be injected into these mice, no fatal anthrax follows. Guinea-pigs, very susceptible to diphtheria, when infected with virulent culture of the diphtheria bacilli, die in the course of a day or two, but rats are little or not at all susceptible to the diphtheria bacilli; and therefore if the guinea-pigs, after infection with the diphtheria bacilli, are injected with rat's blood they recover, this blood being a powerful destroyer of the diphtheria bacilli. Tetanus is easily communicable to mice, in which it produces fatal tetanus in one to three days, but it is not communicable to rabbits; mice infected with the tetanus bacilli and then injected with rabbit's blood do not become affected with tetanus and remain alive.[2]

While on the one hand, then, the tissue juices and the blood, independent of the cellular elements, possess this germicidal action—small or *nil* in susceptible, larger in animals less susceptible, and largest in unsusceptible animals—there is, on the other hand, a considerable body of evidence to show that the least germicidal action seems to be possessed by those very cells themselves which figure in the theory as the destroyers of Bacteria, as phagocytes; that is to say, that of all the tissues the so-called phagocytes are the materials offering to the Bacteria the best means of existence. Even in cases in which the lymph and blood fluid have against particular Bacteria the greatest germicidal power, the so-called phagocytes are for a time the last refuges for the Bacteria. I will illustrate this by a number of examples both of acute and chronic infectious diseases, as gonorrhœa, Egyptian ophthalmia, Koch's mouse septicæmia, leprosy, and tuberculosis; this latter is particularly instructive, as it demonstrates the absurdity of the alleged phagocytosis of the cells of the spleen in tuberculosis, for it is the latter cells in which the tubercle bacilli thrive well, and which they choose pre-eminently.

[4. Demonstration: tubercle cells and leprosy cells.]

Nay, more than this: non-pathogenic Bacteria cannot exist in the normal blood and in the tissues, in the wall of the alimentary canal, in the tonsils, in the tongue; they are destroyed and are therefore absent in the living tissue. But they can, for a time at any rate, exist in the cells of those parts, and in these, and these only, they are met with; these cells are therefore just the reverse of phagocytes, being the last refuges of the Bacteria.

These facts seem to show that cells containing in their substance living Bacteria is no evidence whatever of a battle going on between the cells and the Bacteria, but rather the reverse. The assumption of the presence in the so-called phagocytes and similar cells of a "defensive proteid" seems therefore opposed by these facts. The cells seem to possess a particular chemical attraction for the Bacteria, just as is possessed by certain chemical substances; such attraction is spoken of as *positive chemotaxis* in contradistinction to *negative chemotaxis*—that is, the opposite or repulsive interaction between

[1] Ogata and Tasuhara, *Mitth. der Med. Facultät d. Kais. Japan Universität, Tokio.*

[2] Behring and Kitisato, *Deutsche Med. Woch.*, 1890, N. 49.

Bacteria and certain substances. This line of inquiry is of quite recent date, and promises to produce important and interesting results.

From all this we conclude, then, that in some cases the blood and tissues are, or include, a natural antidote; in others the antidote is not present naturally, and is only furnished by the Bacteria themselves, and still in others the tissues, though possessed of this antidote, may lose it owing to altered conditions.

Another point worth considering is the peculiar inimical action exerted by one microbe on the other: this practically means that the products of one microbe either prevent the growth of another microbe or neutralize its toxic action. It is perfectly well established that while the products of one microbe exert an inimical action, when present in sufficient amount, on the growth and life of the same microbe (the protective inoculation by chemical products of the Bacteria cited above), the accumulation of the products of the particular microbe interferes with, and eventually altogether stops the further growth of its microbe. Outside the body this is easily proved in artificial cultivations. Inside the body it is proved by those cases in which recovery takes place.

It has been shown that while some pathogenic microbes can well thrive side by side in the same culture, inside or outside the body, there are others where the growth of one is antagonistic to the action of the other: erysipelas and anthrax (v. Emmerich), swine erysipelas and swine fever, anthrax and *Bacillus pyocyaneus* (Charrin), anthrax and *Staphylococcus aureus*; this is due to the fact that the chemical products of one species inhibit or neutralize the other species. That this antagonism is really of a chemical nature is shown in the case of anthrax and *Bacillus pyocyaneus*; if the chemical products of this latter microbe be injected into the animal simultaneously or soon after inoculation with the anthrax bacilli, no anthrax disease ensues, the anthrax bacilli do not multiply and do not produce the disease. Apart from specific antiseptics, there exist, then, in the battle against the action of pathogenic microbes which have already entered the system of an animal, the following means: (1) the chemical antagonism offered by the healthy tissues themselves—in some cases this is nought, in others very great and powerful, alterable by various conditions; (2) the germicidal action of the blood and tissue juices of unsusceptible animals on the multiplication of pathogenic Bacteria within a susceptible animal; (3) the antagonism existing between the Bacteria and their own chemical products; (4) the antagonism of one species and its chemical products against another species. These principles have, then, to be borne in mind as indicating the ways in which the microbes can be prevented from producing eventually the disease in a body into which they have found access. Pasteur's hydrophobia inoculations, and many of the recently published results of curative inoculations against tetanus, against anthrax, and against diphtheria, are illustrations of these principles.

The principle on which Koch's antituberculous lymph acts is apparently of a different nature. Koch has found by experiment on guinea-pigs that if the chemical products or an extract of the substance of the tubercle bacilli be injected into a body affected with tuberculosis, the tubercular tissue becomes necrotic, while the tubercle bacilli themselves remain unaffected; at the same time a remarkable reactive inflammation sets in, by which the necrotic tissue becomes eliminated, either spontaneously, *e.g.* where the tubercular process is superficial, as in lupus of the skin, or it may be removed by surgical aid, as in tuberculosis of the bones and joints. All who have followed the numerous cases treated in this manner must agree that it is an immense advance on all previous methods of treatment of some forms of lupus and of bone tuberculosis.

After having shown you what an enormous amount of accurate knowledge about the nature and causation, about the prevention and treatment of infectious diseases has been gained by the experimental method and by this alone, it will hardly be credited that a number of persons, as well-meaning as they are ill-instructed, are still maintaining the contrary; it is they who have succeeded in inducing Parliament to pass a law restricting, if not in some cases altogether prohibiting, the use of that method. This law is interfering with research in this country to a large extent, and has even put a stigma on those who are engaged in elucidating truths that are for the benefit of mankind, and of the animals themselves: what can be of greater benefit in the battle against diseases than the knowledge of their causes, and the devising of means for their prevention and treatment?

Fortunately for progress in general, this country is the only one in which such restrictions disfigure the statute-book; other countries, more enlightened and able to recognize the value of researches of this kind, have wisely resisted the clamour for restrictions.

In connection with all this knowledge, of which I have only been able to give you an outline, I have heard it stated that "ignorance" (meaning the ignorance of former times) "is bliss" as compared with the present knowledge of the dangers surrounding us; but I have also heard it stated that the wise man, knowing and recognizing the nature of these dangers, has the possibility given him of avoiding and preventing them, and no truer words have been spoken than these, that "he who cures a disease may be the skilfullest, but he that prevents it is the safest, physician."

My best thanks are due to my friend Mr. Andrew Pringle for the admirable photographs prepared by him of the microscopic slides illustrating the different pathogenetic microbes exhibited, and to my friend and pupil Mr. Bousfield for the fine lantern slides of tube cultivations.

THE ROYAL METEOROLOGICAL SOCIETY'S EXHIBITION.

THE twelfth annual Exhibition of the Royal Meteorological Society was opened on Tuesday evening, March 3, in the rooms of the Institution of Civil Engineers, 24 and 25 Great George Street, Westminster. This year's Exhibition is devoted to rain-gauges and evaporation-gauges, and also such new instruments as have been constructed since the last Exhibition. It might at first be thought that an exhibition of rain-gauges would be a very small and insignificant affair, and would not be of any interest to the general public. Anyone, however, visiting the Exhibition will at once see that a very large collection of various forms of rain-gauges has been got together by the Society, and that the information obtained from the records of these simple instruments is of the highest importance. There are altogether fifty-six different forms of rain-gauges shown in the Exhibition, and it is interesting to compare the old with the new patterns.

Side-tube rain-gauges of various diameters are exhibited. In this instrument the water passes into the body of the gauge, and also into a glass tube in the front, and stands at the same level in each. As the combined area of these tubes is very much less than that of the receiving surface, the natural depth of the rain is proportionally increased, and thus the scale is lengthened in proportion—usually about eight or ten times—so that the quantity can be read off to hundredths of an inch. The objection to this form of instrument is that the glass tube is liable to be burst by frost, and the record lost.

Messrs. Negretti and Zambra exhibit a contracted float-gauge, the receiver of which contains a copper float, to the upper side of which a rod is attached. When rain falls, the rod is lifted, and, owing to the small area of the body of the gauge as compared with that of the rim, the float rises about eight times the natural depth of the rain.

Mr. Symons shows Fleming's rain-gauge, which is a very small float-gauge. This pattern was formerly much used in Scotland, but is now nearly abandoned, because when the quantity of rain collected exceeds 2 inches, rain which ought to pass over the gauge is caught by the measuring-rod, and runs down it into the gauge. It was also usually placed so nearly level with the ground that surface water occasionally entered.

Various modifications of Howard's rain-gauge are exhibited. This pattern was designed by Luke Howard, F.R.S., and engraved in the first edition of his "Climate of London," published in 1818. It simply consists of a funnel 5 inches in diameter, with a long tube at the bottom, which fits into the neck of a glass bottle. The area of the funnel is about eleven times that of the measuring-jar, so that minute measurements can easily be made. In 1850 a stone-ware bottle was substituted for that of glass, with the view of reducing the frequency of breakage. Mr. H. H. Treby modified this form of gauge somewhat by having rough divisions put upon the glass bottle, so that an approximate idea of the amount of fall might be obtained without the gauge being interfered with. Mr. Symons further modified this gauge by attempting to protect the glass bottle with an outer cylinder having two slits in it, so as to allow of inspection, as in Mr. Treby's gauge. This gauge, however, had two faults, viz. (1) the bottle did not hold enough; and (2) if it burst, the can, being pierced, could not save the water. This gauge was subsequently so modified as to remove the above-mentioned evils, the bottle being larger, and the can water-tight.

Glaisher's rain-gauge is 8 inches in diameter, and has a bevelled rim and curved pipe. The rim round the gauge, about two-thirds of the way up, was designed to make a water-tight joint, so as to prevent any of the rain inside escaping by evaporation. The same object was aimed at by the curved tube or inverted siphon, in which the last few drops of rain remained, and (until they dried up) formed a water-seal. This gauge has subsequently been modified by the substitution of a vertical rim (to cut the rain-drops) for the original bevelled one on which they would break, and by the substitution of a long straight pipe for the curved one, which was found to be frequently choked with leaves, &c.

In the autumn of 1864 the late Major Mathew undertook to provide a number of gauges for the district round Snowdon; for that district Mr. Symons provided gauges 5 inches in diameter, with cylinders rising 4 inches vertically from the edge of the cone of the funnel. These are called "Snowdon rims," and funnels so provided are gradually displacing all others, because they are so much better in time of snow. A gauge of this kind in copper is nearly indestructible and independent of frost, because two vessels (one of glass and one of copper) must burst before the water can be lost. Specimens of this form of gauge, Symons's Snowdon, are shown both in copper and in galvanized iron.

The Meteorological Office gauge, which is 8 inches in diameter, is generally regarded as the best gauge for ordinary observers to whom cost is not a primary object; it has all the good features of the Glaisher and of the Snowdon patterns, and being of copper is very durable.

Several mountain rain-gauges are exhibited. As these are for use in places where the rainfall is heavy, and where they can only be periodically examined, they are made to contain 40 or 50 inches of rain. The rod is detached from the float (to avoid error from its intercepting the rain), and is only dropped into the cup when an observation has to be made. The instrument has an outer cylinder to guard against frost, and to facilitate emptying.

The Manchester, Sheffield, and Lincolnshire Railway company, who for many years past have had regular rainfall observations made at about fifty of their stations, exhibit a specimen of the gauge they employ, which is 8½ inches in diameter, also a map showing the sites of their rain-gauge stations, and specimens of their forms and publication.

Mr. Symons shows a number of the original gauges which were employed about twenty-five years ago in the experiments carried out by Colonel Ward and others to determine (1) the effects of placing gauges at different heights above the ground, not (as had been done previously) on buildings, but on posts; (2) to ascertain whether there is any difference in the indications of gauges ranging in diameter from 1 to 24 inches, and including square ones of 25 and 100 inches area; and (3) to test the effect of various receiving surfaces, such as tin, copper, glass, porcelain, and ebonite.

Among the other old pattern gauges may be seen:—Stevenson's, which has the rim of the gauge brought to the level of the ground, and is surrounded by a brush to avoid in-splashing; FitzRoy's, in which the amount of rain is ascertained by a graduated dipping tube which has a hole at each end; the old copper gauge, with square funnel, used at the Kew Observatory from 1850 to 1890; and the coffee-pot rain-gauge—so called from its shape.

Mr. Symons shows both the first and second pattern of his storm gauges. These are not intended for general use, but to enable observers to watch the most minute details of heavy rain during thunderstorms. Carefully attended to, they yield information of the very highest importance, both for architects and for engineers, as to the rate at which rain falls. With one of these instruments in London on June 23, 1878, rain was ascertained to be falling for 30 seconds at the rate of 12 inches an hour.

The earliest registering rain-gauge is probably that known as Crosley's. The area of this gauge is 100 inches, and beneath the tube leading from the funnel there is a vibrating divided bucket; when one compartment has received a cubic inch of water, *i.e.* 0·01 inch of rain has fallen, the bucket tips, the index advances on the first dial, and the other bucket begins to fill, and so on indefinitely. Messrs. Yeates and Son's electrical self-registering rain-gauge is simply a Crosley gauge, in which electrical contact is made at each turn of the bucket, and the index hand moved one division. The advantage of this instrument is that the funnel may be placed in any exposed position out of doors, while the registering part can be fixed indoors.

The Kew Committee exhibit Stutter's registering rain-gauge, which has twenty-four collecting jars, one for each hour, and also Beckley's self-recording rain-gauge, which is the pattern in use at the Observatories in connection with the Meteorological Office; its funnel has a receiving area of 100 square inches, and is provided with a lip 1¼ inch deep, to retain the splashes. The rain flows down into a copper receiving vessel, which, floating in a cistern of mercury, sinks and draws down with it a pencil which marks on a cylinder moved by a clock. When the receiving vessel is full, a siphon comes into action and rapidly draws off the whole of the water, the vessel rising almost at a bound, the action being recorded by a vertical line on the cylinder. Mr. Casella shows the recording portion of his self-recording gauge, which is another mode of effecting the same object.

MM. Richard Frères, of Paris, exhibit two of their self-recording rain-gauges, which are very ingenious instruments and promise to yield good results. They are already at work at many stations on the Continent, and to our own knowledge at four in this country.

Several rain-gauges as used in foreign countries are included in the Exhibition. Prof. Mascart, of the Bureau Central Météorologique de France, has sent four specimens of gauges as used in France; and Dr. Hellmann, of the Kon. Preussisches Meteorologisches Institut, Berlin, has forwarded the first and second patterns of his rain and snow gauge (the latter of which is now used in the Prussian Meteorological Service), and also his gauge for measuring the density of snow. Wild's rain-gauge, as used in Russia, and Nipher's protected snow-gauge, as used in the United States, are also exhibited.

Among the miscellaneous gauges may be mentioned the marine rain-gauge, mounted on gimbals for use on board ship; Livingstone's rain-gauge, with funnel 3 inches in diameter, as made for the late Dr. Livingstone; tropical rain-gauge, with funnel 4½ inches in diameter, and receiver large enough to hold 40 inches of rain; Colladon's gauge for determining the temperature of hail; Sidebottom's snow-melting rain-gauge; and Mawley's snow-gauge.

Perhaps the largest rain-gauge that has ever been made is that employed by Sir J. B. Lawes and Dr. J. H. Gilbert on their experimental farm at Rothamsted; this has an area of one-thousandth of an acre. The funnel portion of this large gauge is constructed of wood lined with lead, the upper edge consisting of a vertical rim of plate glass, bevelled outwards. The rain is conducted by a a tube into a galvanized iron cylinder underneath, and when this is full it overflows into a second cylinder, and so on into a third and fourth, and finally into an iron tank. Each of the four cylinders holds rain corresponding to half an inch of depth, and the tank an amount equal to 2 inches. Each cylinder has a gauge-tube attached, graduated to 0·002 inch. Of course, this gauge itself could not be exhibited, but two of the collecting gauge cylinders are shown, as well as a coloured view of the gauge *in situ*, drawn by Lady Lawes. A coloured view of the Rothamsted drain or percolation gauges, drawn by Lady Lawes, is also shown. There are three drain-gauges, each one-thousandth of an acre area, which are used for the determination of the quantity and the composition of the water, percolating respectively through 20, 40, and 60 inches depth of soil (with the subsoil in its natural state of consolidation). Sir J. B. Lawes and Dr. Gilbert exhibit a table giving the results of rainfall and drainage at Rothamsted for the twenty harvest years ending August 31, 1890. The annual means are as follow:—

Rainfall.	Drainage through soil (uncropped).			Difference approximately = evaporation.		
	20 in.	40 in.	60 in.	20 in.	40 in.	60 in.
in.	in.	in.	in.	in.	in.	in.
30·29	14·38	15·16	13·61	15·91	15·13	16·68

The Exhibition also includes a number of evaporation gauges for determining the amount of evaporation from a free surface of water or from plants. The Meteorological Council exhibit von Lamont's atmometer, and Wild's, De la Rue's, and Piche's evaporimeters; Mr. Casella shows Babington's atmidometer, and an 8-inch pedestal evaporator. Dr. W. G. Black shows his floating rain-gauge and evaporating cup for use on ponds; and Mr. W. H. Dines exhibits the apparatus used by the late Mr. G. Dines for measuring evaporation. Mr. Symons shows the following from the series of evaporators constructed under the supervision of Mr. Rogers Field, and used in experiments at Strathfield Turgiss about twenty years ago, viz. Fletcher's, Watson's, Miller's wet-sand, tin, tin with overflow, Casella's can and bottle; also Field's hook gauge, used for determining the depth of water evaporated from the large tank, 6 feet square and 2 feet deep, which was used as the standard wherewith the foregoing and some other forms of instrument were compared. The Cambridge Scientific Instrument Company exhibit a self-recording evaporimeter, designed for use with growing plants in a botanical laboratory; and MM. Richard Frères show their self-recording evaporation gauge for use with either water or plants.

Several new instruments are also shown in the Exhibition, among which may be mentioned the following:—Latham's self-recording apparatus for wells, rivers, and reservoirs; Dines's and Munro's helicoid and Robinson's anemometers, and helicoid air-meter; and Richard Frère's statoscope (which is a very sensitive atmospheric barometer) and anemo-cinemograph. Mr. Clayden exhibits a small and large camera for meteorological photography, showing a simple method of attaching a mirror of black glass for photographing meteorological phenomena. The Kew Committee exhibit the frames designed by General Strachey and Mr. Whipple for measuring cloud pictures for determining the height and drift of clouds.

The Exhibition also includes a large number of photographs illustrating meteorological phenomena, &c., as well as a number of maps and diagrams showing the distribution of rainfall over various parts of the world.

The Exhibition will remain open till Thursday, the 19th instant.

William Marriott.

THE PTOLEMAIC GEOGRAPHY OF AFRICA.

At the meeting on Monday of the Royal Geographical Society, Dr. H. Schlichter read a paper on "Ptolemy's Geography of Eastern Equatorial Africa." Ptolemy, as a geographer, has received very different treatment at different times at the hands of his critics. At one time it was the fashion to sneer at the industrious Alexandrian geographer as entirely untrustworthy, as a mere imaginative arm-chair geographer, without critical discrimination. That Ptolemy was an arm-chair geographer no one denies, but in geography, at least, it should be remembered that the looker-on often sees most of the game. Basing his system on that of his predecessor, Marinas of Tyre, Ptolemy seems diligently to have collected the itineraries of all travellers that came within his reach, and his position at the great port of Alexandria was highly favourable for work of that kind. Of course his methods were faulty, his fundamental data erroneous, and the observations with which he had to deal often of the vaguest kind. Still, when all due allowance is made for these drawbacks, there is no denying that Ptolemy's map of North-Eastern Africa bears a wonderful resemblance to reality—just the resemblance that might be expected in the infancy of cartography, before the invention of instruments of precision, and ere travellers had learned to make good use of their eyes. Recent discoveries in Central Africa have attracted increased attention to the geography of Ptolemy, and make one wonder how he came so near the truth. It has been recently attempted by Dr. Meyer (who in this case is merely the mouthpiece of Mr. E. G. Ravenstein) to show that Ptolemy's knowledge of East Africa did not extend beyond Abyssinia; that his Nile is simply the Abyssinian River, and his lakes the lakes of that country, projected downwards, to suit later knowledge, into the heart of Africa. However that may be, Dr. Schlichter, in his paper, gives the result of an ingenious method adopted by him to test Ptolemy's accuracy, and to prove that he must have somehow obtained information about the lakes which we now know give origin to the Nile, and about the snow-clad mountains that cluster round them, and which are all that remain to us of the once famous Mountains of the Moon that extended like a barrier across the continent. After discussing Ptolemy's cartographical methods, and making allowances for his error as to the length of the degree (600 instead of 500 *stadia*), Dr. Schlichter's *modus operandi* is as follows:—

1. To look for the basis on the coast which Ptolemy used in order to fix the position of this part of Africa; and to eliminate his error of geographical latitude.

2. To reduce the positions of his points to modern graduation.

3. But in all other respects to leave the distances from the basis of the map intact with the exception of the itineraries round the Victoria Nyanza.

Dr. Schlichter rightly takes the Rhapta of Ptolemy and his Periplus as the central point of his calculations. Besides Rhapta, Ptolemy mentions a promontory called Rhaptum, and a river called Rhaptus. The "metropolis of Rhapta" must have been somewhat inland, but Dr. Schlichter has no difficulty in identifying the Pangani River with the Rhaptus, and Ras Mamba Mku, a cape to the south of Zanzibar as Ptolemy's Rhaptum. Taking this as his starting-point, and making due allowance for Ptolemy's mistakes as to the length of the degree, Dr. Schlichter measures off with his compasses the distances given by Ptolemy, and in this way identifies most of the places in East Central Africa mentioned by Ptolemy with well-known places of the present day. He measures off, for the sake of minute accuracy, his distances in millimetres. He has constructed two maps—one based merely on Ptolemaic data and another showing the latest knowledge: the coincidences are striking. In this way Dr. Schlichter identified the coast places marked by Ptolemy with such well-known places as Melinda, the mouth of the Tana, the towns of Brava, Marka, Magdishu, Warsheikh, and other places. Applying the same method to the positions in the interior given by Ptolemy, Dr. Schlichter identifies Ptolemy's Eastern Nile Lake with the Victoria Nyanza; the circle, with Rhaptum as the centre and the position given by Ptolemy in the interior as the other end of the radius, cuts the south-east shore of Victoria Nyanza. Following the same method, Dr. Schlichter finds that the position given by Ptolemy for the eastern end of the Mountains of the Moon coincides with a point a little to the south of Mount Kenia. Again, in a similar manner he identifies the Western Nile Lake with Lake Albert or Lake Albert Edward, the western end of the Mountains of the Moon with Ruwenzori, and the confluence of the two rivers which form the Nile with the place where the Somerset Nile flows into Lake Albert.

These instances are sufficient to indicate the method followed by Dr. Schlichter, and its success in identifying the positions given by Ptolemy with features which we know now really do exist. In the subsequent discussion, Mr. Ravenstein endeavoured to prove that Dr. Schlichter's method was entirely misleading, even although he admitted that the position adopted for Rhaptum was approximately correct. Mr. Ravenstein's arguments cannot, however, be regarded as convincing; and although we are not interested in upholding Dr. Schlichter's position, still we think that, in justice to Ptolemy, and in the interests of historical truth, his methods and results deserve serious consideration.

CARL JOHANN MAXIMOWICZ.

CARL JOHANN MAXIMOWICZ, who died at St. Petersburg on February 16, after a few days' illness, was born at Tula in 1827. He went early to St. Petersburg, where he was brought up at the St. Annenschule, a renowned German Lutheran College. In 1844 he left the Russian capital for the University of Dorpat. After completing his studies, he was appointed director's assistant at the botanical garden of Dorpat, a post he held until 1852, when he was made Conservator of the Imperial Botanical Garden at St. Petersburg. The following year he set out on a voyage around the world on board the frigate *Diana*, his chief task being to make acquisitions of living plants for the botanical garden at St. Petersburg. The *Diana* visited Rio de Janeiro, Valparaiso, and Honolulu. But when war was declared by the Western Powers against Russia, she was compelled to call at the nearest Russian harbour, De Castries, on the coast of Mantchuria, at that time the youngest, and scarcely an organized, Russian colony. Maximowicz had to leave the frigate, and decided at once to go up the River Amur, and to explore its banks and the adjoining country, which was then little known. Though furnished with only limited means, he carried out his task under great difficulties and severe privations in a very successful manner. He returned to St. Petersburg by way of Siberia in 1857. The next two years he devoted entirely to the working out of his "Primitiæ Floræ Amurensis: Versuch einer Flora des Amurlandes," a thick quarto volume, which appeared in 1859, and contained a full enumeration of his botanical collections, and a most clear exposition of the general physical features of the country visited by him, and particularly of its phytogeographical character. Immediately after, the full Demidoff Prize was awarded to him in acknowledgment of the excellence of his work. At the same time he was directed to proceed again to the far East. In 1859 and 1860 he travelled in Mantchuria; in 1861 he visited the island of Yesso; 1862, Nipon; 1863, Kiu-siu. He returned to Europe by the sea-route in 1864. It was then that he first visited England. He was at that time in a bad state of health, in consequence of an obstinate fever he caught in Japan, and from the effects of which he suffered from time to time throughout his life. In 1869 he was appointed Botanicus Primarius at the Imperial Botanical Garden at St. Petersburg, and he was a Fellow of the Imperial Academy of Science from 1864. Consequently he was also entrusted with the direction of the Herbarium of the Academy. After 1866 he published many contributions to the flora of Eastern Asia in the *Mémoires* and the *Bulletins* of the Academy, the most important being a monograph of the rhododendrons of Eastern Asia, the "Diagnoses breves Plantarum Novarum Japoniæ et Mandshuriæ, Dec. i.-xx."; the "Diagnoses Plantarum Novarum Asiaticarum, i.-vii.," &c. It was in the latter that he began to work out the large and exceedingly important collections made by Prjevalsky, Potanin, &c., in Central Asia. In consequence, however, of the extreme thoroughness of his work, and his highly critical method, combined with overwhelming official duties, the first parts of these important works did not appear before the end of 1889. These are the "Flora Tangutica" and the "Enumeratio Plantarum hucusque in Mongolia, &c., Lectarum," each comprising only the Thalamifloræ and the Disciflora of the collections. A general review of the phytogeography of Central Asia, founded on the collections of Prjevalsky and other Russian explorers, however, was submitted by him to the Botanical and Horticultural Congress at St. Petersburg, 1884; it is a model of lucidity of style and arrangement. Now, we fear, these two works, so comprehensively planned, will proceed no further, although Maximowicz's preparations for the remaining parts were considerably advanced and a large number of most beautiful plates are ready for press. But we look in vain for the man in Russia who could take up the work. Russia was so unfortunate as to lose her great explorer by sudden death at the very moment when he was setting out to gather new laurels, and now his most famous interpreter has breathed his last not less unexpectedly. Deeply as we must regret that he was not permitted to finish his work himself, one thing is certain—that whatever he completed will last. He was of a noble, high-minded nature, a highly cultivated scholar in almost every branch of learning, and a gentleman in the truest sense of the word.

OTTO STAPF.

NOTES.

THE next ordinary general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, the 19th, and Friday evening, the 20th, at 25 Great George Street, Westminster. The chair will be taken by the President, Mr. Joseph Tomlinson, at half-past seven p.m. on each evening. The following papers will be read and discussed, as far as time permits :—Fourth report of the Research Committee on Friction : experiments on the friction of a pivot-bearing. On recent trials of rock drills, by Mr. Edward H. Carbutt, past-President, and Mr. Henry Davey, of London.

The annual general meeting of the Linnean Society of New South Wales was held on January 28. Dr. J. C. Cox, Vice-President, delivered the annual address. Pointing out that Australia offered an unrivalled opportunity of working up completely, and under the most favourable circumstances, the flora and fauna, especially interesting in itself, of one of the great tracts of the globe, he inquired how it was that with such a splendid harvest still waiting to be gathered in spite of all that had been accomplished, the number of workers was relatively so few. He attributed the slow increase in the number of enthusiastic naturalists partly to defective educational methods which left children blind to the beauties and attractions of Nature ; partly to the want of descriptive catalogues, and well-illustrated hand-books, written from the Australian stand-point ; and partly to the very slender inducement to young men in Australia to qualify themselves for the serious pursuit of science.

At Oxford, on Tuesday, Convocation voted the sum of £150 per annum for three years for a scholarship to be held by a student in the Marine Biological Institute at Naples.

The General Board of Studies at Cambridge has recommended the appointment of a Demonstrator in Palæobotany in the department of geology.

Last week Mr. Leon Warnerke delivered before the Photographic Society of Great Britain a lecture on a simplified photo-collographic process. Instead of a glass or metal plate, as is usual in the collotype process, Mr. Warnerke makes use of a gelatine film supported upon vegetable parchment. He demonstrated the method of sensitizing and drying the film, explained the process of printing, and passed round specimens for inspection before and after printing and after washing with water. He then demonstrated the method of printing, and showed its application to wood engraving, to canvas, and to the decoration of china and porcelain. The walls of the room were hung with between two and three hundred examples of collotype printing sent for exhibition by the Autotype Company, the London Stereoscopic Company, and Messrs. Bemrose and Sons, of Derby, Waterlow and Sons, Römmler and Jonas, of Dresden, L. Rouillé, of Paris, F. Thevoy and Co., of Geneva. It was announced that the exhibition would remain open for a month.

The first electrical launch ever built for the English Government was sent off the stocks into the Thames at Messrs. Woodhouse and Rawson's yard, near Kew Bridge Station, on Tuesday afternoon. The *Electric*, as the pinnace is called, is to be used for the conveyance of troops between the dockyards of Chatham and Sheerness. The *Daily News* describes the motion of the boat as "delightfully smooth—the very poetry of motion." With her forty fully-equipped soldiers, she can run at a speed of eight knots an hour. A single charge of electricity enables her to run for ten hours.

As various erroneous statements have been made with regard to Dr. Nansen's Arctic expedition, the *Times* gives the following account of what has actually been arranged. Dr. Nansen's desire is to leave Norway in February 1892, but it is doubtful whether the special vessel which is being built will be ready by that time. Outside of Norway not a farthing has been contributed by anyone. The expedition is purely Norwegian, and will remain so. The Norwegian Government contribute 200,000 kroner ; King Oscar, 20,000 ; twelve private individuals (all Norwegians except one Englishman who has lived in Christiania for many years), 90,000 : in all, 310,000 kroner, equal to £17,200. That, Dr. Nansen believes, will be sufficient. The ship, of course, is being specially constructed for the peculiar conditions which exist between the New Siberian Islands and the Pole. Dr. Nansen will be accompanied by probably not more than eight young men, all as stalwart and strong in physique as himself, and all equally confident of success.

At a meeting of the Royal Botanic Society of London on Saturday, a report by Mr. Lecky was read, giving a summary and digest of the sun record in the Gardens during the year 1890, showing the percentage of each month. As compared with the returns for the previous year, this shows an increase of 156 hours of bright sunshine, a result due to the latter half of the year (the earlier months being comparatively sunless). The total recorded for the year amounts to 1092 hours, as against a possible total of 4455 hours. A noticeable feature was the predominance of afternoon sunshine, due, it seems, to the position of the Gardens in the north-west of London, and the difficulty the sun has in piercing the smoke and mists from the eastern districts as it rises. Not the slightest trace of sunshine is recorded as having occurred during December.

A propos of the recent severe weather, *La Nature* has a representation of an ice colonnade, made near Rheims in January, which is about 14 feet high, and bends in half circle round an ice figure of Father Christmas. M. Caron describes a peculiar kind of ground ice found in rivers in the Jura region. In clear cold nights crystals form at the bottom, and rise in groups to the surface. They consist of small needles and lamellæ of ice held by mutual attraction, but not adherent to each other. They offer no resistance to boats or oars, but may sometimes cause much inconvenience : thus they accumulate and stop water-wheels ; or, rapidly collecting on a dam, they may cause flooding. M. Franger (an Alsatian engineer) points out that all circumstances unfavourable to radiation from a river bed, are also against formation of ground ice, which is rarely, if ever, met with in a stream that is muddy or too deep, so as not to transmit the heat-rays, or where the motion is sluggish enough for surface ice to form, and for gravel at the bottom to be covered with sediment. At the sluices near Maigrange, according to M. Cuony, ground ice forms about the iron work largely used in the sluices, and is got rid of by heating the upper part of the structure with wood fires. M. Cuony produced ground ice experimentally, by cooling an iron bar 10° to 15° below zero C., and plunging it in cold water ; thus illustrating the part played by the piles of bridges.

The Report of the Smithsonian Institution for 1888 (Washington, 1890) contains *inter alia* a review by Prof. Cleveland Abbe on "Recent Progress in Dynamic Meteorology," giving a brief, but careful, summary of the most important works which have appeared up to December 1888, on the movements of storms and the general motions of the atmosphere. The works summarized are classified under the following heads : (1) laboratory experiments on fluid motion ; (2) statistics of actual storms ; (3) theoretical hydrodynamics applied to the motion of the air ; (4) thermodynamics of atmospheric phenomenon ; (5) prediction of storms and weather. The author considers that it is generally conceded that meteorological phenomena, at least those which depend on the motions of the atmosphere, are too difficult to be unravelled at present, although much progress has been made during the last few years. His summary will be a good guide for persons wishing to study the science.

The Royal Meteorological Institute of the Netherlands has published a work upon the tides off the Dutch coast, based on the observations of the light-keepers on the various banks, showing graphically the average times of the turning of the tides, and also the means of sea temperature at and below the surface, deduced from hourly observations. The mean annual temperature of the air is slightly lower than the sea-surface temperature, but in some months the reverse occurs, and the temperature at depths of 5 and 10 fathoms differs very slightly from the sea-surface temperature. Tide rips are most frequent in April, during the first four days of full and new moon, while the least number occur in January. With a strong south-west wind, the tide runs eight miles an hour.

WE learn from the *Oesterreichische botanische Zeitschrift* that Herr H. Leder has undertaken a three years' zoological and botanical expedition to Siberia. During the first year he proposes to visit the Tunka and Sajan mountains to the south of Irkutsk; in the second year he will follow the course of the On-on into Transbaikal; and during the third year he has planned a visit to the upper course of the Amur and Argun in the Katunsk Altais.

THE editors of the *Botanisches Centralblatt*, finding it impossible to keep pace with the ever-increasing mass of botanical literature, have arranged for the publication of *Beihefte*, of which it is intended that seven, of eighty pages each, shall appear annually, with only a moderate increase in the annual subscription. By this means it is hoped that the delay which has hitherto occurred in the *résumés* of important botanical papers may in the future be avoided. The first of these supplements has appeared, and contains notices of several English papers which have appeared in the *Annals of Botany* and elsewhere.

TEA at £10 12*s*. 6*d*. per pound ought to have very special qualities. A small parcel of Ceylon tea, from the Gartmore Estate, was sold at this price at the London Commercial Tea Sale Rooms on Tuesday. The tea is composed almost entirely of small "golden tips," which are the extreme ends of the small succulent shoots of the plant.

THE new number of the Journal of the Institution of Electrical Engineers (No. 91) contains the Presidential address on "Electricity in Transitu," delivered by Prof. Crookes on January 15. There are also papers on "Electric Lighting from Central Stations," by General Webber, and on the "Early History of the Telegraph in India," by Mr. P. V. Luke.

THE use of oil at sea continues to be regarded by experienced masters of vessels as an excellent means of protection in stormy weather. A number of recent reports on the subject, printed in America, afford striking testimony to the efficacy of the method. Captain Rogers, Br. s.s. *Congo*, for instance, writes thus:—"Left Liverpool, January 18, 1891, for New York; arrived February 8. At 4 a.m., February 4, it was blowing a hurricane, ship taking some very heavy seas aboard. Put her before the wind, stopped the engines, and put an oil-bag out on each quarter. Ship rode out the hurricane splendidly, and without shipping any water whatever." Chief Officer Miller, Am. brig. *Marena*, writes:—"During the gale which began on January 10, lat. 32° 56′ N., long. 75° 20′ W., and ended on January 12, lat. 34° 40′ N., long. 74° 45′ W., wind from south to west-north-west, the vessel was hove-to and a hemp-canvas bag was partly filled with oakum, saturated with fish oil. The bag was replenished every two hours, and was allowed to just dip into the sea from the lee quarter. The oil was used for twenty-four hours, and as the vessel lay quite easy and shipped no heavy seas it proved a great success. The captain is always provided with a full supply of fish oil (which he considers the best), and all appliances for instant use." Captain Leseman, Br. s.s. *Miranda*, reports:—"Used oil with most excellent effect in the gale encountered on December 1, 1890, between St. John's and Halifax. The oil was a combination of kerosene and linseed, and was used from the closets forward and from bags on each side amidships. Waves would come bearing down in the direction of the steamer as though to crush her, but they no sooner reached the oil than they rolled harmlessly past. To its use we owe our lives and the safety of the ship."

SOME interesting remarks on squirrels are made by various writers in the current number of the *Zoologist*. It is often said that squirrels are torpid during winter, but there is no really sound evidence for this view. Mr. Masefield, writing from Cheadle, Stafford, says:—"I have seen squirrels abroad on fine days in, I think I may say, every one of the winter months; and while pheasant-shooting near here on a sunny day (January 6 last), which was about the middle of the most severe frost we have had for many years, with several inches of snow on the ground, I saw a squirrel jumping from tree to tree, before the beaters, in the most lively condition." Mr. Blagg, also writing from Cheadle, has "frequently seen squirrels abroad in the middle of the winter, when there has been deep snow on the ground and a keen frost in the air." "I remember," he adds, "once seeing a squirrel abroad during a severe storm of sleet and rain in winter-time, and he appeared to be not at all inconvenienced by the rough weather." Mr. Blagg's idea is that the squirrel probably does sleep a good deal more in winter-time than in summer, as do many other wild animals, but that he has to be continually waking up and taking nourishment. The period of reproduction is unfavourable to the notion of an almost complete state of torpidity. The editor of the *Zoologist* records that he has notes of "finding newly-born squirrels on March 21 (three young), April 9 (three young), April 26 (four young), and April 29 (two young). Those found at the end of March and beginning of April were naked and blind; those taken at the end of April were about three parts grown." According to the editor, "the old squirrels, in case of danger, remove the young from the nest, or 'drey,' to some hole in a tree, whither they carry them one by one in the mouth, just as a cat carries her kitten. One of the prettiest sights in the world is to see an old squirrel teaching a young one to jump."

IT has been recently shown by Dr. Marcet (*Arch. des Sciences*) that different persons respire different volumes of air to furnish to the body the oxygen required, and to yield a given weight of carbonic acid. Thus, to produce one gramme of carbonic acid, three persons were found to need, on an average, 9·29, 10·51, and 11·30 litres of air respectively. The first was 23 years of age, the third 60; and no doubt the less the air required for a given combustion, the better the conditions of respiration. The influence of food on formation of carbonic acid in the body begins in the first hour after a meal, and increases for two or three hours, the period of maximum respiration of CO_2 varying in this time. After a certain time, the weight of CO_2 expired decreases more rapidly than the required volumes of air decrease. The influence of local variations of air-pressure appears in less air being needed, for a given amount of CO_2, with low pressures than with high; but the degree of the influence varies in individuals.

AS it is possible that circumstances may arise during the next few years which may make it desirable to lay down a cable to the Andamans, the Meteorological Reporter to the Government of India has, according to the Indian press, recorded his opinion that it is almost absolutely necessary for the warning of the Bengal and Madras coasts that such a cable should be laid, and that he believes it to be almost essential to the proper and complete protection of the Burmah coast. Observations from Port Blair would rarely give earlier information of the commencement of disturbed weather in the Bay than is shown by the present battery of stations round the Bay. They would, however, in the majority of cases, give earlier definite information as to the storm in process of formation, whether it was a feeble or powerful one, or of small or large extent. They would also enable the probable path of the storm to be determined with greater certainty than can be done under the present arrangements. Telegraphic communication to Port Blair would hence tend to make the work of storm-warning more exact and trustworthy, and enable the Meteorological Department to state with greater certainty than at present the probable path or intensity of storms approaching the Bengal and Madras coasts. It would also give certain and strong indications of the formation of storms in the Andaman Sea, and enable the Department to warn the Burmah coast more efficiently than at present.

Prof. Dubois, of Berne, has been lately studying the physiological action of electric currents and discharges (*Arch. des Sciences*); and he has some interesting observations on the human eye, which, it is known, has luminous sensations under the action of galvanic currents. Sudden variations of intensity, especially at making and breaking the circuit, produce such flashes. With a moistened plate at the nape of the neck, and a pad on the eye, a slight flash was distinctly perceived even with a Leclanché cell of about 1·20 volt, and measuring in the galvanometer 4/100 of a milliampere. Raising the intensity to five-tenths, the observer could tell which pole was applied to the eye. On the other hand, the retina responds much less readily to discharges from condensers or induction coils. Not till a capacity of 0·037 microfarad and a tension of 21 volts was reached, was a true retinal flash perceived; and not even with 10 microfarads were the durable sensations characteristic of the two poles produced. The retina reacts to quantity.

An important new mineral, titanate of manganese, $MnTiO_3$, isomorphous with the well-known titanate of iron or titanic iron ore, $FeTiO_3$, has been discovered in the neighbourhood of Harstigen by Dr. Hamberg, and is described by him in the current number of the *Geol. Foeren. i. Stockholm* (12 Band, 598). The crystals were found embedded in calcite, and were readily isolated by removing the latter by means of dilute hydrochloric acid, the titanate of manganese being but very slightly attacked by acids. They were of tabular habitus, and possessed an exceptionally brilliant metallic lustre of a deep red tint. In thin sections the colour is seen to be orange-red, and is not pleochroic. When ground to powder a yellow ochre is produced, possessing a slight tinge of green. The crystals are as hard as apatite, and have a specific gravity of 4·537. They consist of almost pure manganous titanate, containing 50·49 per cent. of titanium dioxide and 46·92 per cent. of manganous oxide. Like titanic iron ore, the crystals belong to the rhombohedral-hexagonal system, the basal plane (0001) being the principal face, upon which the crystals are tabular. The edges of the tables exhibit faces of the negative rhombohedron ($02\bar{2}1$), and the deuteroprism ($11\bar{2}0$). The angle of the rhombohedron is very near that of titanic iron ore, and if the ratios of the axes of the two compounds are compared the similarity is very striking indeed. In the case of titanate of iron $a : c = 1 : 1·385$, and in the new mineral $a : c = 1 : 1·369$. Indeed, the isomorphism is even more complete, for titanic iron ore is not only rhombohedral, but is also tetartohedral; and a study of the corrosion figures produced by boiling hydrochloric acid upon the basal plane of titanate of manganese shows that this mineral is likewise tetartohedral. The optical properties of the new mineral are also rather remarkable. The refractive index of the ordinary ray for sodium light is no less than 2·4810, almost as high as the refractive index of the diamond, for the extraordinary ray somewhat less, 2·21. The dispersion is likewise large, the respective indices for the lithium red and thallium green rays being 2·44 and 2·54 respectively. The crystals cleave readily parallel to the rhombohedral and prism faces. Dr. Hamberg gives the name pyrophanite to the mineral. He is of opinion that specular iron ore, hæmatite, Fe_2O_3, which he writes $FeFeO_3$, is also truly isomorphous with pyrophanite, and this assumption is certainly supported by the similarity of the axial ratios, those of hæmatite being $a : c = 1 : 1·359$. The ruby and sapphire, Al_2O_3, may perhaps also be included in the series, for their axial ratios are almost identical with those of pyrophanite, $a : c = 1 : 1·363$. Hæmatite, crystallized alumina, pyrophanite, and titanic iron ore, would thus form an isomorphous series with gradually ascending axial ratios.

The additions to the Zoological Society's Gardens during the past week include a Serval (*Felis serval* ♀) from East Africa, presented by Mr. D. Wilson; two Red-backed Weaver Birds (*Quelea sanguinirostris*) from West Africa, presented by Mrs. Hastings; a White Frog (*Rana temporaria*, var.), British, presented by Mr. W. Hannaford; a Rhesus Monkey (*Macacus rhesus* ♂) from India, a West African Python (*Python sebæ*) from West Africa, deposited; a Snow Leopard (*Felis uncia*) from the Himalayas, a Collared Peccary (*Dicotyles tajaçu* ♀) from South America, two North American Turkeys (*Meleagris gallo-pavo* ♂ ♀) from North America, six Shore Larks (*Otocorys alpestris*), British, purchased; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Photographic Spectrum of the Sun and Elements.—The *Johns Hopkins University Circular*, No. 85, issued last month, contains Prof. Rowland's report of progress in spectrum work. The spectra of all known elements, with the exception of a few gaseous ones, or those too rare to be yet obtained, have been photographed in connection with the solar spectrum, from the extreme ultra-violet down to the D line, and eye-observations have been made on many to the limit of the solar spectrum. A table of standard wave-lengths of the impurities in the carbon poles extending to wave-length 2000, has been constructed to measure wave-lengths beyond the limits of the solar spectrum. In addition to this, maps of the spectra of some of the elements have been drawn up on a large scale, ready for publication, and the greater part of the lines in the map of the solar spectrum have been identified. The following rough table of the solar elements has been constructed entirely according to Prof. Rowland's own observations, although, of course, most of them have been given by others:—

Elements in the Sun, arranged according to Intensity and the Number of Lines in the Solar Spectrum.

According to intensity.		According to number.	
Calcium	Zirconium	Iron (2000 or more)	Magnesium (20 or more)
Iron	Molybdenum	Nickel	Sodium (11)
Hydrogen	Lanthanum	Titanium	Silicon
Sodium	Niobium	Manganese	Strontium
Nickel	Palladium	Chromium	Barium
Magnesium	Neodymium	Cobalt	Aluminium (4)
Cobalt	Copper	Carbon (200 or more)	Cadmium
Silicon	Zinc	Vanadium	Rhodium
Aluminium	Cadmium	Zirconium	Erbium
Titanium	Cerium	Cerium	Zinc
Chromium	Glucinum	Calcium (75 or more)	Copper (2)
Manganese	Germanium	Scandium	Silver (2)
Strontium	Rhodium	Neodymium	Glucinum (2)
Vanadium	Silver	Lanthanum	Germanium
Barium	Tin	Yttrium	Tin
Carbon	Lead	Niobium	Lead (1)
Scandium	Erbium	Molybdenum	Potassium (1)
Yttrium	Potassium	Palladium	

Doubtful Elements.

Iridium, Osmium, Platinum, Ruthenium, Tantalum, Thorium, Tungsten, Uranium.

Not in Solar Spectrum.

Antimony, Arsenic, Bismuth, Boron, Nitrogen, Cæsium, Gold, Indium, Mercury, Phosphorus, Rubidium, Selenium, Sulphur, Thallium, Praseodymium.

With respect to these tables Prof. Rowland adds:—"The substances under the head of 'Not in the Solar Spectrum,' are often placed there because the elements have few strong lines or none at all in the limit of the solar spectrum when the arc spectrum, which I have used, is employed. Thus boron has only two strong lines at 2497. Again, the lines of bismuth are all compound, and so too diffuse to appear in the solar spectrum. Indeed, some good reason generally appears for their absence from the solar spectrum. Of course, there is little evidence of their absence from the sun itself; were the whole earth heated to the temperature of the sun, its spectrum would probably resemble that of the sun very closely."

The powerful instrument used at Baltimore for photographing spectra, and the measuring engine constructed to fit the photographs so that its readings give the wave-lengths of lines directly within 1/100 of a division on Angström's scale, give the foregoing results a weight superior to many others published.

A VARIABLE NEBULA.—Mr. Roberts has recently shown that the nebula in Andromeda is variable (*Monthly Notices R.A.S.*, January 1891). Previous to this, the only nebula whose variability could be accepted as proved was N.G.C. 1555, in the constellation Taurus, discovered by Dr. Hind on October 11, 1852, and observed by D'Arrest four times in 1855–56, but which has since been looked for in vain by a number of astronomers.

In *Comptes rendus* for March 2, M. Bigourdan has a communication on the variability of a nebula, N.G.C. 1186, situated near Algol. Sir William Herschel discovered this nebula in 1785 (Phil. Trans., 1789, p. 247). Sir John Herschel observed it in 1831 (Phil. Trans., 1833, p. 376); but Lord Rosse looked for it without success in 1854 and 1864 (Phil. Trans., 1861, p. 745, and Trans. Roy. Dub. Soc., vol. ii. p. 34). On November 8, 1863, D'Arrest could not see the nebula, although he looked for it assiduously and the atmospheric conditions were most favourable. He therefore concluded that the object did not exist ("Siderum Neb.," p. 56). M. Bigourdan finds that the nebula is again visible in the position indicated by the two Herschels, viz. R.A. 2h. 54m. 20s., Decl. + 42° 10', he having observed it on January 31 and February 26 of the present year. It is difficult to believe that this object could have escaped the scrutiny of Lord Rosse and D'Arrest in 1854, 1863, and 1864; hence the variation is probably real, and merits the attention of astronomers. The nebula may be easily found, as it is very near the binary 694 B.D + 42° (1123 G.C.), the position of which for 1891 is R.A. 2h. 58m. 6s., Decl. + 42° 29'. Photographic evidence of the variability would be most interesting.

FEBRUARY SUNSHINE.

AS everybody is aware, February 1891 was remarkable for its excessive dryness and for the absence of anything approaching stormy weather. Many will also be disposed to remember it as a month in which we had more than our ordinary share of fog, particularly during the second half, when fog seemed to be very general over the eastern and south-eastern parts of England. It will be a surprise, therefore, to learn that, in spite of the exceptionally foggy character of the month, the amount of bright sunshine which was registered, over England especially, was altogether abnormally large. And what is still more surprising is that the second half, which included the days when the fogs were reported as most dense and widespread, was very much more sunny than the first half. According to the statistics published weekly by the Meteorological Office, the average duration of bright sunshine in the twelve forecasting districts for the month of February is 89 hours in the Channel Islands, 72 hours in the south of Ireland, 46 hours in the extreme north of Scotland, and in the other nine districts it varies between 60 and 69 hours. In the period now under review, however, the recorders registered 167 hours in the Channel Islands; 126 hours in England, S.W.; 108 hours in England, S.; 102 hours in England, E., and Midland Counties; 97 hours in England, N.W.; 90 hours in Scotland, E.; 88 hours in England, N.E.; 80 hours in Ireland, S.; 73 hours in Ireland, N.; 59 hours in Scotland, W.; and 54 hours in Scotland, N. With the exception, therefore, of the West of Scotland, where there was a deficiency of one hour, every district showed a considerable excess of sunshine, England and Wales taken as a whole having 104 hours against an average of 65 hours, the increase being 60 per cent., the south-western counties showing an excess of 83 per cent.; while the Channel Islands had 88 per cent. more than the average. The records for the individual stations in the several districts are even more interesting. Out of forty-one stations the following twenty had at least 100 hours of bright sunshine for the month, the excess above the average being shown in the second column of figures. The corresponding figures for the second half of the month are given in the third and fourth columns:—

Station.	Feb. 1–28. Hours.	Excess. Hours.	Feb. 15–28. Hours.	Excess. Hours.
Jersey	167	78	120	72
Hastings ...	135	53	105	59
Plymouth ...	134	58	97	53
Torquay ...	128	?	82	?
Falmouth ...	126	50	80	37
Pembroke ...	125	48	94	51
Cirencester ...	124	55	81	42
Southamptom	123	53	88	49
Cullompton	120	54	81	43
Llandudno ...	120	56	93	58
Eastbourne ...	118	?	89	?
Stowell ...	118	?	86	?
Churchstoke	115	54	88	54
Aberdeen ...	113	?	72	?
Hillington ...	104	40	71	35
Cambridge ...	103	37	68	31
Newton Reigny	102	45	79	47
Marchmont...	101	?	73	?
Geldeston ...	100	31	67	28
Rothamsted	100	?	71	?

A glance at the above table shows that this wonderful outburst of sunshine was not confined to the south coast stations. Llandudno had a better record than Eastbourne, Aberdeen came very near, while Newton Reigny and Marchmont, both northern stations, fall into the list of high totals. It will be seen from the second column that the average daily excess ranged from more than 1 hour at Geldeston to nearly 3 hours at Jersey. The only station in the kingdom which had a deficiency of sunshine was Glasgow, the total duration being 34 hours, or 12 less than the normal. Fort Augustus had 40 hours, but the average is not known for this station; London comes next with 42 hours, and, small as was the total, it was 5 hours above the average. Ireland did not have a large excess: Dublin, with 91 hours, was 22 hours to the good, and Armagh, with 73 hours, was 12 hours above the average; but elsewhere the normal was exceeded by from 2 to 10 hours only. The figures quoted for the month as a whole are quite exceptional for so early a period in the year; but the third and fourth columns of the table show that about three-fourths of the total sunshine was registered in the last fourteen days. Indeed, in the first week the amount recorded fell below the average in eight out of the twelve districts, and in the following week four districts were still deficient; and this fact accounts for the excess for the last fortnight at Hastings, Pembroke, and other places being actually larger than that for the entire month. During the fourteen days, 15–28, there were several stations other than those included in the above table which had from 50 to 80 hours of sunshine. At Dublin, Durham, Geldeston, and Oxford the excess was as much as 2 hours per day above the average; Llandudno and Hastings were favoured with an extra 4 hours per day, and Jersey rather more than 5 hours. For London the Meteorological Office gives 18·9 hours in the first half, and 23 hours in the second half of the month; total, 41·9. The Royal Observatory had respectively 24·2 hours and 46·5 hours; total, 70·7. The influence of the fog on the western districts is seen in the difference for the last fortnight, when the south-eastern quarter had rather more than twice as much sunshine. The Greenwich record is an excess of 29 hours on the average for the month, or a little over 1 hour per day. This is very good under the circumstances, but Londoners cannot help envying the more fortunate districts beyond the limits of metropolitan fogs; even distant Aberdeen, although entitled to less sunshine owing to latitude, having nearly three times as much brightness as western London.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 12.—"On the Organization of the Fossil Plants of the Coal-Measures. Part XVIII." By Prof. W. C. Williamson, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester.

On three preceding occasions the author has directed attention to the existence in the older Carboniferous rocks of a remarkable form of fructification which seemed to belong to the Calamarian family of plants, though presenting features distinct from any that had hitherto been described. In the first instance, in 1871, he placed this fructification in Sternberg's provisional genus *Volkmannia*, under the name of *V. Dawsoni*. Some small fragments of the same type, obtained at a later period by the late Prof. Weiss, of Berlin, led him to identify the plant with Binney's hitherto very obscure genus *Bowmanites*, an identification which is accepted by Prof. Williamson. Still more recently, a number of additional specimens have been obtained from the Ganister Carboniferous beds of Lancashire and Yorkshire, which not only throw further light upon the plant, but have made it possible to re-write its history in an almost complete form.

Like all the other Calamariæ, *Bowmanites* was a plant with a distinctly articulated stem, each node of which bore a verticil of lateral appendages. In the vegetative organs each of these nodal appendages consisted of a verticil of the linear, uninerved leaves characteristic of the old, ill-defined genus *Asterophyllites*. In the fructification these foliar verticils are replaced by a broad circular disk, the margin of which sustained a verticil of leaf-like "disk-rays." These rays can scarcely, at present, be identified with true leaves, since they have not only no midrib, but they seem to contain no traces whatever of a vascular bundle.

The centre of the axis of the strobilus is occupied by a conspicuous bundle of barred and reticulated tracheids of the scalariform type, the transverse section of which bundle is triangular, with concave sides. Each of the three prominent angles is abruptly and broadly truncated. A thin inner cortex seems to have originally surrounded this bundle, but all traces of its tissues have disappeared. The thick outer cortex is composed of a mixture of rather coarse, strongly defined parenchymatous and prosenchymatous cells. At each node this cortex expands into the lenticular disk already referred to. This disk is thickest at its inner border, thinning gradually towards its outer margin, where it subdivides into the verticil of elongated disk-rays already mentioned. Though no vascular bundles can be discovered connecting the central axial one with the surrounding disk, some such must have once existed, since we find them both in the cortex of the internodes and in the nodal disks.

The entire upper surface of each disk has given off numerous very slender sporangiophores, destined to reach three or four concentric circles of sporangia, which were arranged in a single plane in the internodal interval between each two disks. Each sporangiophore, unlike what is usual amongst the Calamariæ, only sustained a single sporangium. In order to reach the more external ranges of the latter organs, the sporangiophores were prolonged outwards in a distinct layer between the upper surface of the disk and the sporangia which rested upon it. Not only was this the case, but when each sporangiophore reached the sporangium with which it was destined to become organically united, it did not at once do so; but it passed under, and even beyond that organ, where it bent back upon itself and became united to the sporangium on its distal side. The outer, or epidermal, layer of the sporangium was merely an extension of that of the sporangiophore.

The numerous spores of *Bowmanites* have also a distinctive form. Each has a rather thin exosporium, but this is thickened along a few reticulate lines, and from each junction of these reticulations a strong radiating spine is projected. It is in the very distinctive features of these reproductive organs that the marked generic individuality of *Bowmanites* chiefly resides.

The second plant described in the memoir, under the name of *Rachiopteris ramosa*, is one of the several fern-like organisms which the author has included in his provisional group of Rachiopterides. Considerable doubt exists respecting the true affinities of at least some of these plants. The one now described may prove to be a less hirsute, more fully developed condition of the *Rachiopteris hirsuta* described by the author in his Memoir XV.

Chemical Society, January 15.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—On magnetic rotation, by W. Ostwald. The magnetic rotation of organic compounds, according to Perkin, is an additive function of their composition, and equal to the sum of the rotations of the components, but this is not the case with the rotation of inorganic compounds, which is usually found greater than that calculated on such an assumption. The author points out that these exceptional values are only obtained in the case of electrolytes, and that they must therefore be referred to a fundamental difference between the constitution of electrolytes and that of non-conductors. The author claims that the facts established with regard to magnetic rotation are in perfect accordance with Arrhenius's theory of electrolytic dissociation, and that any exceptional values in the magnetic rotation of electrolytes are due to the occurrence of electrolytic dissociation.—The vapour density of ammonium chloride, by Frank Pullinger and J. A. Gardner. The authors have made experiments on the vapour density of ammonium chloride at various temperatures. The apparatus used was that of Victor Meyer. The ammonium chloride was vaporized into an atmosphere of ammonia and into air. At a moderate red heat, and at 448° C., complete dissociation took place; at 360° C. in an atmosphere of ammonia it was not wholly dissociated. It was found impossible to vaporize the salt into ammonia at 300° C.—Chlorinated phenylhydrazines, by J. T. Hewitt.—A new modification of phosphorus, by H. M. Vernon. Observations on the rate of rise of temperature of phosphorus and other experiments have led the author to the conclusion that one or other of two different modifications of phosphorus may result when fused phosphorus solidifies.

February 5.—Dr. W. J. Russell, F.R.S., President, in the chair.—It was announced that the following changes in the Council list were proposed by the Council: President, Prof. Crum Brown, *vice* Dr. Russell; Vice-Presidents: Mr. S. Pattinson and Prof. Tilden, *vice* Profs. Crum Brown and Mallet; Foreign Secretary: Prof. Meldola, *vice* Prof. Japp; Members of Council: Dr. Atkinson, Mr. Boverton Redwood, Prof. Perkin, and Dr. J. Voelcker, *vice* Mr. Cross, Prof. Dunstan, Prof. Meldola, and Dr. Plimpton.—The following papers were read:—On the formation of an explosive substance from ether, by Prof. P. T. Cleve. The author describes a remarkable explosion occasioned by impurities in commercial ether. On distilling about 250 c.c. of the ether, it was noticed that a viscid residue remained; after drying on the water-bath, this formed a transparent, amorphous mass. Prof. Cleve states that, having poured a little water on to the substance, he proceeded to stir it gently with a rounded glass rod; this occasioned a most violent explosion. The explosive substance was probably ethyl peroxide, as it gave the well-known perchromic coloration, besides liberating iodine and discharging oxygen from silver oxide; it was at once destroyed by reducing agents.—Does magnesium form compounds with hydrocarbon radicles?, by Prof. Orme Masson and U. T. M. Wilsmore, University of Melbourne. The authors state that they have in vain endeavoured to prepare magnesium ethide (1) from magnesium and ethyl iodide; (2) from magnesium-copper couples and ethyl iodide; (3) from an alloy of magnesium and sodium and ethyl iodide; (4) from magnesium and zinc ethide; (5) from magnesium and mercury ethide; and (6) from anhydrous magnesium iodide and zinc ethide.—Compounds of the oxides of phosphorus with sulphuric anhydride, by R. H. Adie. The author has endeavoured to prepare compounds of phosphorus similar to those which the other elements of the group form with sulphuric anhydride. By the action of sulphuric anhydride on H_3PO_3, he obtained a compound very nearly of the composition

$$H_3PO_4 \cdot 3SO_3 = (SO_3H)_3PO_4.$$

Sulphuric anhydride and phosphorus were found to interact violently to form a compound represented by the formula $3P_2O_4 \cdot 2SO_3$.—The combustion of magnesium in water vapour, by G. T. Moody. The author describes a way in which the combustion of magnesium in water vapour may be performed as a lecture experiment, by carrying out the operation in a piece of hard glass tube, about 10 mm. wide and 250 mm. long, bent at an angle of 120°, so as to leave one arm nearly twice as long as the other. The shorter arm is inserted through a cork closing the mouth of a "tin can" or other convenient vessel in which steam can be generated; and the longer arm, which contains a few strips of magnesium ribbon, is connected by a fairly wide delivery-tube with the pneumatic trough, at which the liberated hydrogen may be collected. The air being displaced by a slow current of steam, the arm of the tube containing the magnesium is heated by means of a Bunsen burner; this is then replaced by a blowpipe flame, which is moved about so that the whole arm becomes very hot; then, on allowing the flame to impinge on a portion of the tube against which the magnesium rests, the metal takes fire and burns with great brilliancy.

Geological Society, February 20.—Annual General Meeting.—Dr. A. Geikie, F.R.S., President, in the chair.—The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1890. In the former the Council once more congratulated the Fellows upon the continued prosperity of the Society, as evinced by its increasing number and by the satisfactory condition of its finances. The Council's Report also referred to the publication of the late Mr. Ormerod's Third Supplement to his Index to the Publications of the Society, to the editing of Nos. 183 and 184 of the Journal by Prof. T. Rupert Jones, to the deaths of the late Foreign Secretary and the late Assistant-Secretary, and in conclusion enumerated the awards of the various Medals and proceeds of Donation Funds in the gift of the Society. The Report of the Library and Museum Committee included a list of the additions made during the past year to the Society's Library, and announced the completion of the glazing of the Inner Museum.—After the presentation of the Medals and the balance of the Wollaston Fund, and the Murchison and Lyell Geological Funds, the President read his anniversary address, in which he first gave obituary notices of several Fellows, Foreign Members, and Foreign Correspondents deceased since the last annual meeting, including the late Foreign Secretary, Sir Warington W. Smyth, the late Assistant Secretary, Mr. W. S. Dallas, M. Edmond Hébert and M. Alphonse Favre (Foreign Members, both elected in 1874), Mr. Wm. Davies, Mr. Robert Wm. Mylne, Mr. Samuel Beckles, Dr. H. B. Brady, Mr. Samuel Adamson, and Prof. Antonio Stoppani (Foreign Correspondent, elected in 1889). He then dealt with the history of volcanic action in Britain during the earlier ages of geological time. He proposed to confine the term "Archæan" to the most ancient gneisses and their accompaniments, and showed that these rocks, so far as we know them in this country, are essentially of eruptive origin, though no trace has yet been found of the original discharge of any portion of them at the surface. Passing to the younger crystalline schists, which he classes under the term "Dalradian," he pointed to the evidence of included volcanic products in them throughout the Central Highlands of Scotland and the north of Ireland. The Uriconian series of Dr. Callaway he regarded as a volcanic group, probably much older than the recognized fossiliferous Cambrian rocks of this country. The Cambrian system he showed to be eminently marked by contemporaneous volcanic materials, and he discusssd at some length the so-called pre-Cambrian rocks of North Wales. He reviewed the successive phases of eruptivity during the Arenig and Bala periods, and described the extraordinary group of volcanoes in Northern Anglesey during the latter time. The volcanoes of the Lake District were next treated of, and reference was made to the recent discovery by the Geological Survey that an important volcanic group underlies most of the visible Lower Silurian rocks in the south of Scotland. The last portion of the address was devoted to an account of the volcanoes of Silurian time in Ireland, and it was shown that during the Bala period a chain of submarine volcanic vents existed along the east of Ireland from County Down to beyond the shores of Waterford; while in Upper Silurian time there were at least two active centres of eruption in the extreme west of Kerry and in Mayo.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—Council: Prof. J. F. Blake; W. T. Blanford, F.R.S.; Prof. T. G. Bonney, F.R.S.; James Carter; James W. Davies; John Evans, F.R.S.; L. Fletcher, F.R.S.; C. Le Neve Foster; A. Geikie, F.R.S.; A. Harker; J. C. Hawkshaw; H. Hicks, F.R.S.; G. J. Hinde; W. H. Hudleston, F.R.S.; Prof. T. McKenny Hughes, F.R.S.; J. W. Hulke, F.R.S.; J. E. Marr; H. W. Monckton; F. W. Rudler; J. J. H. Teall, F.R.S.; W. Topley, F.R.S.; Prof. T. Wiltshire; H. Woodward, F.R.S. Officers:—President: A. Geikie, F.R.S. Vice-Presidents: W. T. Blanford, F.R.S.; Prof. T. G. Bonney, F.R.S.; L. Fletcher, F.R.S.; W. H. Hudleston, F.R.S. Secretaries: H. Hicks, F.R.S.; J. E. Marr. Foreign Secretary: J. W. Hulke, F.R.S. Treasurer: Prof. T. Wiltshire. The thanks of the Fellows were unanimously voted to the retiring Members of Council: Prof. A. H. Green, Rev. Edwin Hill, Major-General C. A. MacMahon, E. T. Newton, and Rev. G. F. Whidborne.

February 25.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—A contribution to the geology of the Southern Transvaal, by W. H. Penning. The following table shows the author's classification of the sedimentary rocks of this region, as compared with those of Messrs. Dunn and Stow and Prof. Rupert Jones:—

DUNN. (Map, 1887.)		STOW.	T. R. JONES.		PENNING.	
Coal-measures: *Upper Karoo* (formerly Stormberg Beds, above Upper Karoo).	SILURIAN. CARBONIFEROUS TO TRIASSIC.	*Upper Karoo.*	*Lower Karoo.*	TRIASSIC FORMATION.	*High Veldt Beds.*	OOLITIC.
Kimberley Shales: *Lower Karoo* (formerly Upper Karoo).		....			*Kimberley Beds.*	
					Klip River Series.	*Megaliesberg Formation.* DEVONIAN.
Lydenburg Beds.					*Witwatersrand Series.*	
Namaqualand Schists.					*De Kaap Valley Beds.*	SILURIAN.

The De Kaap Valley beds consist of schists, shales, cherts, and quartzites, with some conglomerates, chloritic and steatitic beds of great thickness, faulted, according to the author, against the granite. They contain a few obscure corals, and are provisionally referred to the Silurian. The Witwatersrand series consists chiefly of sandstones, shales, cherts, and quartzites, having an estimated thickness of 18,000 feet, possibly formed in a hollow of the granite, and perhaps of marine formation. The Klip River series is formed of shales, flagstones, cherts, and quartzites, with numerous interstratified traps, and is at least 18,000 feet thick. Near its base is the "Black Reef," and a chalcedonite like that described by the author in connection with the Lydenburg district, which confirms his opinion that this area is formed of part of the Megaliesberg formation. The base of the series is generally conformable to the underlying rocks. The whole of the lower half of the Megaliesberg formation is let down against the north side of the granite south of Pretoria. The author divides the formation, which he described in 1884 under the heading of "High-level Coal-fields of South Africa," into the Kimberley beds and the High Veldt beds. The former thin out eastward, and are overlapped by the latter, the estimated thickness of which is 2300 feet. A volcanic rock overlies the coal-formation. Near the base of the formation is a bed of loose, calcareous, sandy clay, inclosing many water-worn pebbles, some of large size, derived from the quartzites and "bankets" of the underlying formation. The author is convinced that the region was under glacial influences at some time during the long period which intervened between the deposition of the Megaliesberg formation and of the coal-bearing rocks of the High Veldt, which latter, he maintains, are certainly Oolitic; the latter contain *Glossopteris* (?) and fishes which he considers to be nearly allied to *Lepidotus valdensis*, the latter being from the Free State. The High Veldt rocks are of fluviatile origin, and there appears to have been continuity of fluviatile denudation on the close of the Oolitic period until now. The reading of this paper was followed by a discussion, in which Mr. Gibson, Mr. Alford, Prof. Rupert Jones, Mr. Smith Woodward, and Dr. Blanford took part.—On the lower limit of the Cambrian series in North-West Caernarvonshire, by Miss Catherine A. Raisin. Communicated by Prof. T. G. Bonney. A discussion followed, in which Prof. Blake, Dr. Hicks, Prof. Hughes, Prof. Bonney, the President, and Mr. Peach took part.—On a Labyrinthodont skull from the Kilkenny Coal-measures, by R. Lydekker.

PARIS.

Academy of Sciences, March 2.—M. Duchartre in the chair.—Observations of asteroids, made with the great meridian instrument of Paris Observatory during the second quarter of 1890, by Admiral Mouchez. The asteroids which have been observed are Pallas, Ceres, Juno, and Melete.—On metallic reflection, by M. H. Poincaré. Further objections are adduced against MM. Cornu and Potier's interpretation of Herr Wiener's experiment on the direction of vibration in a polarized beam of light (*Comptes rendus*, January 6 and February 9).—On an attempt at oyster-culture in the experimental fish-pond of the Roscoff Laboratory, by M. de Lacaze-Duthiers. Oysters measuring from 1·5 to 2 cm. in April 1890 had attained a greatest length of 5 cm. in June, and in September 1890 and March 1891 reached a length of 7 and 8 cm. Of the 8500 young oysters placed in the fish-pond, only 50 died during the severe weather experienced this winter. This is an extremely low rate of mortality when compared with the loss at ordinary oyster-beds. The reason of this success is probably due to the fact that the boxes containing the oysters could be protected from the cold air during low tides by running them down into the sea by means of chains.—On the composition of drainage waters, by M. P. Dehérain.—On a variable nebula, by M. G. Bigourdan. (See Our Astronomical Column.)—History of apparatus for the measurement of base-lines, by M. A. Laussedat.—On the transformation of a geometrical demonstration, by M. A. Mannheim.—On the minima surfaces limited by the four corners of an irregular quadrilateral, by M. Schoenflies.—Results of actinometric observations made at Kief, in Russia, in 1890, by M. Savélief. Observations made from the beginning of June to the end of November give the following results:—(1) In summer and in autumn, the real value of the absolute heat intensity of solar radiation, for an apparently clear sky, reaches a maximum about 10 o'clock; a secondary maximum occurs between 1 p.m. and 2 p.m.; between these two maxima a well-defined minimum may be observed at midday. In autumn, the calorific intensity of solar radiation is greatest between 9 a.m. and 2 p.m., and reaches a higher value than in summer. (2) In summer, the hourly mean of absolute intensities—that is, one-sixtieth of the quantity of heat received normally in one hour by a surface having an area of 1 sq. cm.—reaches a maximum about 10 a.m., and a secondary maximum about 5 p.m. In autumn, the curves are more regular than in summer, and present only a single maximum about 11 p.m.—Remarks on M. Savélief's communication, by M. A. Crova. Variations similar to those described by M. Savélief have been registered on M. Crova's actinometer at Montpellier.—On duplex-beating metallic reeds, by M. A. Imbert.—On some alkaline derivatives from erythrite, by M. de Forcrand. The author has obtained crystallized alkaline erythrates by the action of erythrite on aqueous solutions of potash and soda.—On the dyeing of cotton, by M. Léo Vignon.—On a vegetable hæmatin, aspergillin—a pigment of the spores of *Aspergillus niger*, by M. Georges Linossier. The author shows that the fruits of *Aspergillus niger* contain a pigment having the same characteristics as the hæmatin of the blood of animals.—Idiosyncrasy of certain species of animals for carbolic acid, by M. Zwaardemaker. Small doses of carbolic acid have no effect on dogs or rabbits, but intoxicate and subsequently kill cats and rats, the deaths being always preceded by convulsions lasting several hours.—On the hepatic epithelium of the testacle, by M. J. Chatin.—On the conglomerate of Gourbesville containing fossilized bones, by M. A. de Lapparent.—On the age of the strata cut by the Panama Canal, by M. H. Douvillé. Some fossils that have been collected in Panama Canal cuttings belong to the Miocene and Eocene periods.—On the relation of earth tremors to the seasons, by M. de Montessus. It has been asserted that earth tremors occur more frequently in winter than in summer, and are therefore connected with meteorological phenomena. M. Montessus has investigated 63,555 tremors with respect to their time of occurrence, and finds that the astronomical seasons bear no relation to them.—On the action of running water on some minerals, by M. J. Thoulet.

BRUSSELS.

Academy of Sciences, January 10.—M. Stas in the chair.—M. Folie was elected President for 1891.—Researches on the velocity of evaporation of liquids at the temperature of ebullition, by M. P. de Heen. The author has used specially devised apparatus for the determination of the influence exercised on the velocity of evaporation: (1) by the velocity of a dry current acting on its surface; (2) by temperature; (3) by the nature of the liquid; (4) by the nature of the gaseous current; (5) by the pressure of the gas in motion. He finds that the velocity of evaporation is proportional to the square root of the velocity of the gaseous current, and that for a given velocity of the current the quantity of liquid vaporized is proportional to the vapour tension. Experiments on water, benzine, chloroform, acetic acid, alcohol, ethyl bromide, carbon bisulphide, and ether indicate that, *cæteris paribus*, the amount of liquid vaporized varies as the product of the vapour tension into the molecular weight. The interior friction of hydrogen, carbon dioxide, and air are respectively represented by 95, 163, and 194. Experiments with these gases as currents show that the vaporizing influence is greater when the interior friction of the gas is greater. The amount of liquid vaporized appears to depend on the velocity of the current of gas, but is independent of the pressure.—Preliminary notes on the organization and development of different forms of Anthozoa, by M. Paul Cerfontaine.—Crystallographical notice on the monazite of Vil-Saint-Vincent, by Dr. A. Franck.

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