THURSDAY, JULY 31, 1879

THE NEW NATURAL HISTORY MUSEUM

NOTWITHSTANDING the delay caused by discussions on the Zulu campaign and the Army Discipline Bill, the Civil Service Estimates must shortly come before the House of Commons, and an opportunity will be given for obtaining from the Government some explanation of the course they propose to adopt with regard to the administration of the New Museum of Natural History. As will be seen by the memorial, of which we gave a copy some weeks ago, the Council of the British Association for the Advancement of Science, in accordance with the resolution adopted by the Association at the Dublin meeting, have strongly urged the pressing importance of this question upon the Government. Naturalists, we believe, are one and all of the same mind on this subject, but owing to the many important political questions of the day, and to the general ignorance of, or we may perhaps say, indifference to, the true wants of science, find it very difficult to get their wishes attended to. Their general opinion upon the British Museum question may, we believe, be shortly stated as follows.

The dominant idea, as we all know, of the Founders of the British Museum was the library. The collections of natural history and antiquities which have been added to it during the past fifty years have always been regarded as entirely subordinate, and not-to-be-too-much-encouraged parts of the general scheme. The executive officer of the whole institution has always been the "principal librarian," trained up in the book department, and having his great aim and object to make that department as perfect as possible. Some years ago, in obedience to pressure from without, an eminent naturalist was made "superintendent" of the four sections of the museum which relate to natural history, namely, zoology, botany, geology, and mineralogy, but care was taken to give him no real power, and his authority, we believe, has remained completely nominal up to the present day. The "superintendent of the natural history departments" has never been allowed to interfere in any way with the important functions of the principal librarian, in whom the administrative power of the whole of the Museum is vested. Now fifty years ago, in the infancy of natural history in this country, such an arrangement as this might have answered very well, but with the gigantic strides that science has made of late years, it is not likely that naturalists will be content to allow the great National Museum of the country to continue to be governed by an individual of no scientific attainments whatever, and to be entirely subordinated to the predominant interests of the Public Library. When the Royal Commission on Science was appointed in 1872, and the question of the British Museum came before it, the grievances of the naturalists found vent, and the systematic injustice with which this department of the Museum had always been treated was fully exposed. After taking full evidence on this subject, the Royal Commission came to the conclusion that the objections raised to the present system of government of the natural history collections were "well founded," and were "unable to discover that the system is attended by any compensating advantages." The Royal Commission recommended consequently that the opportunity should be taken of the proposed removal of these collections into the new building at South Kensington, to separate them at the same time entirely from the control of the trustees and to place them under the rule of a director, who should be responsible to one of the Ministers of State.

It might have been well supposed that such a recommendation, coming as it did from a Royal Commission composed of some of the leading scientific authorities of the country, and backed by the universal opinion of naturalists, would not have been ignored. But such is the apathy displayed by our Government, when questions merely of scientific interest are at stake, that the recommendation appears to have been entirely overlooked. At the fag end of last session the trustees of the British Museum were permitted to pass an act enabling them to move the natural history collections to South Kensington without making any changes whatever in the mode of their administration, and not a single member of the Legislature appears to have raised his voice against this summary mode of dealing with the question.

Unless something can be done to upset the conclusion thus arrived at, it is obvious that the evils so loudly complained of during the stay of the natural history collections in Bloomsbury will accompany them in their migration to South Kensington. The library at Bloomsbury will continue to be regarded as the main business of the fifty trustees, and the natural history at South Kensington will, as of old, be starved in order to feed the wants of the more favoured institution. Besides this, many absurd laws and antiquated regulations exist in the British Museum which it would be highly inexpedient to introduce into a new institution, and which can only be got rid of by a complete change of the ruling powers.

It is said that the trustees of the British Museum, having had the memorial of the British Association pressed upon their attention by the Treasury, are prepared to make certain concessions as regards the management of the New Museum at South Kensington. But in the face of the strong recommendations of the Royal Commission we do not believe that any arrangement of this character will be deemed satisfactory.

In fact, the only hope of good government for the new Museum of Natural History lies in its entire separation from the unnatural foster-sister with which it has been hitherto reared. Every man of science will, we think, agree with the Duke of Devonshire's Commission in considering that natural history has now full claims to a separate maintenance, and will render thanks to the Council of the British Association for their efforts to impress the importance of the recommendations of that Commission upon Her Majesty's Government.

BRAIN AND MIND

The Relations of Brain and Mind. By Henry Calderwood, LL.D., Professor of Moral Philosophy in the University of Edinburgh. (London: Macmillan and Co., 1879.)

THE object of this work, Prof. Calderwood says in his preface, "is to ascertain what theory of mental life is warranted on strictly scientific evidence."

"The order followed is to consider, *first*, the latest results of anatomical and physiological research as to the structure and functions of the brain; *second*, the facts in human life unaccounted for by anatomical and physiological science, and requiring to be assigned to a higher nature."

In these words our author indicates not merely his method, but the conclusions as to the relations of mind and brain to which his investigations have led him.

The first six chapters deal with the anatomy and physiology of the brain, both human and comparative. In these and also in other parts of the work Prof. Calderwood exhibits an extensive acquaintance with the facts of cerebral anatomy, physiology, and pathology, worthy of any technical neurologist, and which reflects especial credit on an author hitherto identified with purely speculative philosophy.

As the result of his study of the comparative anatomy and physiology of the brain, he reaches the position that the brains most elaborate in convolution are indicative mainly of the most highly developed muscular system. The development of the brain is, however, no test of "intelligence." This, he contends, is most strikingly brought out by a comparison of the brain of man and the ape. "The ape, with a brain modelled like man's, and weighing 15 to 20 oz., shows himself active, powerful, and able to assail any adversary; man, with a brain better developed, and 10 to 15 oz. heavier, is tottering, feeble, and idiotic, unable to defend himself from even a weak assailant. If configuration and structure of brain afford a measure of intelligence, our poor idiotic fellow-man should be so much clearer in intellect and decided in action than the highest specimens of apes. But it is not so" (p. 161).

The comparison here instituted is a very fallacious one. The exact formula for the relationship between brain development and intelligence in different animals has yet to be found. That it is not a mere matter of size is generally admitted. But that the relationship is thorough-going is proved by the very fact here alluded to by Prof. Calderwood, that below a certain standard of development idiocy is the invariable result. The comparison should not be between a microcephalic idiot and a normal ape, but between a normal ape and a microcephalic one. The microcephalic ape would certainly be idiotic.

At the close of his review of the facts of cerebral anatomy and physiology Prof. Calderwood says: "At this stage it seems our only possible conclusion that anatomical and physiological investigation as to brain and nerve, so far as they have yet been carried, afford no explanation of our most ordinary intellectual exercises" (p. 216).

He quotes with approval Prof. Tyndall's words that "the passage from the physics of the brain to the corresponding facts of consciousness is unthinkable," &c. (p. 212); but not content to accept the two as correlated facts insusceptible of further simplification, he endeavours to prove by "personal experience" that mind is altogether distinct from brain, and of a higher and immaterial nature.

"That we discriminate between sensations and perceptions, and consequently form conceptions of things, are facts towards the explanation of which all that is known concerning the action of nerve-fibres and cellular substance contributes nothing" (p. 224). "The known laws of brain action do not provide for this; they imply that the nerve system is not equal to such work" (p. 221).

In his chapter on "Experience as connected with Motor Activity" (Chapter VIII.) we find the following account of the nature of volition:—"What we mean by volition or exercise of will-power is best shown, in the first instance, by marking its contrast with nerve-action. It is not that which moves the muscles, but that which moves the nerve-cells to act upon the muscles. It is not that which moves the limbs, but that which determines that they shall be moved. In its lower and simpler aspect this may be illustrated by reference to sensory activity. A falling stick touches the hand, or a neighbour jostles the elbow. By contact with some external body, an impulse is given to the sensory nerve which is transmitted to the sensory cells. Let us now turn to motor activity. In so far as the originating power *acts upon* the motor apparatus, its action is, in a sense, analogous to that which produces a tactile impression—it operates as an external power, that is, *external* to the apparatus. Or, to take a form of expression more familiar, there comes from an inner sphere, from the region of personal experience, an impulse which acts upon the motor cell, and throws it into activity. That which acts upon the motor cells is as certainly external to the system as is the object which comes into contact with the sensory system. But in the case of voluntary muscular activity, that which operates acts directly on the cell. And what is not reflex, as not being the product of movement of the sensory nerve, must be accounted for by energy from some other quarter, that is, from a sphere external to the nerve system, though within the nature of the person" (p. 247).

Such being the standpoint assumed by Prof. Calderwood in reference to the simplest forms of mental manifestation, it is unnecessary to follow him in his analysis of the higher mental operations.

He advocates *essentially* the so-called "clavier" theory, that the mind is something of a higher nature and distinct from brain, which plays on brain as on a musical instrument. If the brain is diseased, mental manifestations will be limited or inharmonious, but the defect is purely in the instrument, and not in the performer.

Prof. Calderwood admits that the brain is the organ of the mind; that "a pure independence of mind is not known in our history" (p. 314); that defective development of the brain and idiocy invariably go together; that diseases of the brain are associated with mental derangement (Chapter XIII., "Brain Disorders") and deficiencies (Chapter X., "Use of Speech"); that mind has a powerful influence on body, and that mental work implies physiological waste (Chapter XI., "Action and Reaction of Body and Mind"); and yet, notwithstanding the thoroughgoing correlation between mind and brain which these and similar facts demonstrate, he professes by the aid of personal experience to prove the existence of something distinct from and independent of the conditions of its manifestation. This process reminds one very much of an attempt to lift oneself by the hair of the head, or raise the chair on which one sits. Prof. Calderwood cannot divest himself of his brain by personal experience, nor can he give us any evidence of personal experience without brain.

He acknowledges that "however carefully we study consciousness, we do not thereby attain to any knowledge of the nerve system," and that "only by the slow and laborious methods of anatomical and physiological research has the human race become aware of the physical conditions of sensory impressions and motor activity" (p. 212).

So far, therefore, as personal experience is concerned, Prof. Calderwood might have equally well relied on it for asserting that sensation and voluntary motion are independent of sensory and motor nerve structures, as for his assertion that mental operations are distinct from the action of brain.

That we are in ignorance of the physical processes underlying many special psychical manifestations may be admitted without invalidating the general fact of their correlation, otherwise clearly established. But to make ignorance on the one side the basis of very positive statements on the other, is to say the least extremely rash. We may not know how or under what collocations of nerve cells and nerve structures subjectivity becomes apparent; but for Prof. Calderwood to exclude it in his definition of the properties and modes of activity of nerves and nerve-centres, and then to argue that personal experience demonstrates it to be something beyond and above, is to beg the whole question.

He very ingenuously estimates the true value of such an argument in a passage, in which he says, "the insufficiency of brain and nerve to perform such work *is really involved in the statement of the laws of brain action and the functions identified as belonging to fibres and cells*" (p. 1. 122, ital. ours). It would be more logical to reconsider and amend the definition.

Prof. Calderwood's endeavour to prove by scientific evidence the distinct nature and independence of mind, is to attempt the impossible. The utmost that scientific evidence is able to accomplish is to show that cerebral activity and the facts of consciousness are correlated facts insusceptible of further simplification and incapable of being expressed in terms of the other.

Whether we adopt the hypothesis of a duality or a dual unity, is a question of faith, not of scientific demonstration. Science can only deal with the knowable.

Considering the very decided stand Prof. Calderwood has taken on the dual theory in the light of the latest researches in cerebral physiology and pathology, it was not unreasonable to expect some new contribution towards the elucidation of the vexed question as to how the immaterial mind can act and be reacted on by the material body. As to whether they are attuned on the pre-established harmony principle or otherwise, Prof. Calderwood gives us no information. On the whole, perhaps, he has in this exercised a wise discretion. But whatever theory as to the intimate nature of mind and brain may be adopted, the correlationship between the psychical and the physical must be accepted, not merely in a general sense, but as regards each individual manifestation. Any work will be gladly welcomed, and will do great service, which serves to throw further light on the relations between psychical phenomena and their anatomical and physiological substrata. Prof. Calderwood's work does not help us in this respect:—rather the reverse. While, as regards the facts of brain on the one hand, and the facts of mind on the other, it contains much that is worthy of praise, as regards their relations it is eminently unsatisfactory.

D. FERRIER

SOUTH-INDIAN PALÆOGRAPHY

Elements of South-Indian Palæography from the Fourth to the Seventeenth Century, A.D. By A. C. Burnell. Second Enlarged and Improved Edition. (London: Trübner and Co., 1878.)

A WORK like that before us is one of those which make us feel proud of our Indian civil servants. Dr. Burnell has made a name for himself in a field of research peculiarly his own, and the appearance of a second edition of his important work on South-Indian Palæography is a matter of congratulation for science. Apart from the historical and linguistic value of the numerous inscriptions here copied and explained, the light thrown by their decipherment upon an obscure chapter in the history of writing is so important that I shall make no excuse for confining myself to this side of Dr. Burnell's labours, the more especially as this is the side to which he has himself devoted the larger part of his book.

Two questions are brought before us at its outset—the date of the introduction of writing into India and the origin of the South-Indian alphabets. The two questions, indeed, hang very closely together, and the one cannot be completely decided without the help of the other. The earliest examples of writing yet discovered in India are the edicts of Aṣoka, the Constantine of Buddhism, about 250 B.C. They are written in two different alphabets, and the irregularities they present have been supposed to show that writing was still a recent art. The alphabet of the northern inscriptions, which may be termed the North Aṣoka alphabet, has been proved by Mr. Thomas to have been derived from an Aramaic original, and consequently to have been introduced by Semitic traders from the Persian Gulf. Dr. Burnell claims a similar origin for the South Aṣoka alphabet, as well as for a third alphabet used only in Southern India, and known as the Vaṯṯeḻuttu or Old Tamil. Of this Dr. Burnell holds that it "is apparently not derived from nor the source of the Southern Aṣoka alphabet, though in some respects very near to it."

These opinions of Dr. Burnell have met with a vigorous opponent in Mr. Thomas, who maintains that both the southern alphabets were of Dravidian origin, the Sanskrit alphabet itself being an adaptation of some pre-existing Dravidian one. But it will be difficult to resist the force of Dr. Burnell's arguments based upon the earliest forms of the South Indian characters and their likeness to corresponding characters in the Aramaic alphabets of the fourth and third centuries B.C. As he justly observes: "perhaps the most important proof of the Semitic origin of the two South Indian alphabets is the imperfect system of marking the vowels which is common to them both. They have, like the Semitic alphabets, initial characters for them, but in the middle of words these letters are marked by mere additions to the preceding consonant."

If we once admit with Dr. Burnell that the South Indian alphabets have the same Phœnician origin as most of the other alphabets of the world, we must go further with him and derive them "from an Aramaic character used in Persia or rather in Babylonia." The progress of palæography has made it impossible to derive them directly from Phœnicia, as Benfey wished to do, or from the Himyaritic characters of Yemen as Lenormant alleges. The traditional belief of the Hindus that their ancient literature was handed down by oral recitation alone is thus confirmed, and a remarkable illustration afforded of the powers of a trained memory. The famous maxim that a literature cannot exist without writing must be given up, and the rigid sceptics who refuse to admit that any historical truth can be extracted from oral tradition lose their most formidable argument.

The earliest material used for writing purposes in India seems to have been the bark of the *bhûrja*, which is usually identified with the birch. It is worth notice that our own word *book* has the same origin as *beech*, and testifies to a similar employment of the bark of the beech-tree among our Teutonic ancestors. It is probable, however, that the characters of our first "books" were cut upon the soft wood or bark of the beech in the form of runes, and not painted as in the case of the birch books of ancient India. Nevertheless we must not forget that Venantius Fortunatus when alluding to the runes in the seventh century speaks of them as being "painted" on tablets of ash.

A. H. SAYCE

OUR BOOK SHELF

Parasites; a Treatise on the Entozoa of Man and Animals, including some Account of the Ectozoa. By T. Spencer Cobbold, M.D., F.R.S., F.L.S. (London: J. and A. Churchill, 1879.)

THERE are several groups of animals which from time immemorial have been more or less generally neglected by zoologists, and which have induced but very few amongst the latter to make a specialty of their investigation. As an instance of comparatively highly developed animals to which this remark applies, we need only point to the whole class of cephalopoda, and among the lower animals the entozoa are certainly a good case in point. Apart from the comparative scantiness of the literature treating of these animals, it has the additional disadvantage, in common with much other zoological literature, of being scattered in the numerous volumes of several hundred different scientific serials. Dr. Cobbold has for a long time been held an eminent authority on helminthology, and, as he states in his preface, many hundreds of correspondents, not having ready access to the works of Rudolphi, Diesing, and Dujardin (three great foreign authorities on the subject), have applied to him for identification of parasites they met with in dissections or otherwise. He therefore pronounces the most justified hope that by the work now published a reasonable limit may be placed upon the number of future applicants. What particularly characterises Dr. Cobbold's work is the thoroughly scientific enthusiasm with which it is written, and which in itself is admirable. We cannot help reproducing the closing sentences of the preface, which will give to our readers a true notion of the spirit to which, according to our view, a scientific work ought to owe its origin:—

"The study of the structure and economy of a humble parasite brings to the investigator no slight insight into the workings of nature. If these workings cannot at all times be pronounced to be 'good and beautiful,' they must at least be characterised as 'true.' The knowledge of the true—especially if that knowledge by its practical applications be calculated to confer substantial benefits upon man and his inferior fellow-creatures—ought to be held in high esteem; but apart from this purely utilitarian view, there remains for the investigator the delight occasioned by the inrush of new scientific ideas. The average mind, being either essentially commercial or ridiculously sentimental, as the case may be, is totally incapable of comprehending the motive power that animates and guides the votary of science. The late Prof. Faraday, a man wholly untinged by the ambitions of wealth and power, once remarked to me that there were no people so difficult to instruct as those who were ignorant of their own ignorance. It is just these very persons who, when placed in high positions of social, political, or professional trust, most powerfully contribute to check a nation's progress. There are too few genuine workers at science in this country. As one of the rank and file I claim only to have honestly contributed my mite. I should like to see a small army of helminthologists rise up and lay siege to the fortresses at present securely held by thousands of death-dealing parasites." None but an honest and true worker will write such sentences as these; every well-meaning man of science must concur with Dr. Cobbold in the ideas he thus forcibly expresses. Upon an array of workers of Dr. Cobbold's stamp a nation may justly look with pride.

Turning now to the book itself we need hardly say that the author has executed the task he set himself in a most praiseworthy manner. Apart from a voluminous contribution of original work, he has consulted an almost interminable list of bibliographies on the various kinds of parasites, a work which in itself involved stupendous labour.

The contents are divided into two large groups, the parasites of man occupying the first division, and those of animals the second. Each division is subdivided into several sections, and thus in the first we have descriptions in one section of Trematoda or flukes, in the other those of Cestoda or tapeworms, in the third those of Nematoda or round- and thread-worms, and in the last those of Acantocephala (thorn-headed worms), Suctoria (leeches), and the parasitic forms of Arachnida, Crustacea, Insecta, and Protozoa. In the second division the parasites of animals are arranged according to the respective places of their hosts in systematic zoology. The parasites of Mammalia are subdivided according to the orders in this class, beginning with Quadrumana and ending with Marsupialia and Cetacea. After this the parasites of birds, reptiles, fishes, and invertebrate animals are considered in due course. In relation to the parasites of man, the author gives a great deal of valuable statistical information which must be of special interest to the physician.

Altogether we cannot speak too highly of Dr. Cobbold's book, and congratulate the author warmly upon having so efficiently filled a gap in zoological literature, the existence of which had long been felt by all working naturalists and many medical men.

A Contribution to Agricultural Botany. By A. S. Wilson. (Aberdeen: J. Rae Smith, 1879.)

THE author of the small volume before us is already favourably known as an investigator of more than one obscure yet important problem connected with field-botany. The text of his present discourse is "turnip-singling." He approaches this subject in a characteristically careful manner, taking into account, as he does, a number of considerations which might easily escape the attention of an ordinary observer or experimenter. The object and manner of his experiments present no novelty; indeed, it seems to us that Mr. Wilson can hardly be fully aware of the immense number of trials which have been made, both in this country and on the Continent, in order to ascertain the best distance apart for swedes and turnips. However, experiments of this order certainly require frequent repetition in order that the influence of season, climate, soil, and manure, may be duly measured. Any one of these conditions may so affect the result as to invalidate a hasty conclusion drawn from one or two years' trials, even when such trials have been conducted, not in one locality, but in several. Mr. Wilson is quite right in saying that "the theory of no farm plant has been worked out," and that "our turnip shows are conducted on no useful principle." But he is not equally correct in affirming that "the chemist of the Aberdeenshire Agricultural Association initiated a most important mode of teaching one aspect of cultivation," or that questions in agricultural botany "have usually been altogether subordinated to questions on the comparative efficacy of manures. Had Mr. Wilson known the range of work and style of teaching until lately prevailing at Cirencester, and for long and now in vogue in many agricultural colleges in America and on the Continent, he would have hesitated before making such statements.

While Mr. Wilson shows us how, under certain conditions, a larger weight per acre of roots was produced when the plants (both turnips and swedes) were singled so as to leave but 6 inches between them in the drills, although the drills themselves were 27 inches apart, he gives us no information as to the relative feeding values of the larger roots grown at 8 and 9-inch intervals, and the smaller but more numerous roots from the 6-inch intervals. Had the average weight of any of these sets of roots been exceptionally high or exceptionally low, this point would have been of much greater importance. For our object in growing such a crop as turnips or swedes is to obtain the most economical production of the greatest amount of wholesome and keeping food per acre. Very large roots are, we know, very watery, do not keep well, and contain certain nitrogenous and saline matters in excess, so as to become in this way also less desirable as food for farm stock. And it frequently happens that *all* the increase per acre obtained in the form of large roots is water or useless mineral matters. Thus, in all experiments, such as these of Mr. Wilson, fair samples of the crop from different plots should be reserved for analysis—water, ash, and albuminoid nitrogen, at all events, being determined in the produce of each plot.

Arithmetic in Theory and Practice for Higher and Middle Class Schools, &c. By Henry Evers, LL.D. (London and Glasgow: W. Collins, Sons, and Co., 1878.)

HAD Dr. Evers been entirely unknown to us, we should have had no hesitation whatever in saying that this is the work of a practical teacher; of one who has fully realised the difficulties of "teaching arithmetic," and by long experience and patient observation learnt to cope with those difficulties successfully. The arrangement is unquestionably good and in some respects original; the definitions and explanations are short and to the point, indeed we could wish in some cases the author had made them fuller; the problems are numerous and interesting and more of the ordinary daily business type than fanciful improbabilities; and the solutions as far as we have examined them remarkable for accuracy.

The author might have given another method for the extraction of the cube root applicable to all roots; those he gives are certainly the best we remember seeing in any text-book. The examination questions at the end will be found of very great value to those preparing for similar ordeals. The publishers have as usual given a good book a good dress as regards paper, type, and binding.

E. H.

LETTERS TO THE EDITOR

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

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Recent Weather

IT is not necessary to appeal to statistics to demonstrate the cold and sunless character of the weather nearly constantly experienced during the last few months, this being matter of the most ordinary observation; nevertheless some more exact information on the subject may not at the present time be unacceptable to the readers of NATURE.

The year 1878 until October had been generally warm, the temperature on the whole having been above the average in every month, excepting September, and it was very little below in this month. Then a period of cold set in. Beginning with 1878, November, the temperature has been in every month below the average, the deviation in some months being very large. The amount of sunshine, as recorded by Campbell's instrument, has also, since April, been in each month remarkably small. A few particulars, extracted from the Royal Observatory Records, by permission of the Astronomer-Royal, are given in the following table:—

Month, 1878–1879.	Deviation of mean temperature from average of twenty years.	Number of days on which the mean temperature was		Hours of bright sunshine.	Deviation of amount of sunshine from average of the two preceding years.
		Below the average.	Above the average.		
	°				
November ...	−3·0	25	5	40·5	− 5·8
December ...	−7·1	25	6	16·3	− 0·4
January	−6·9	26	5	14·8	−12·1
February ...	−1·5	16	12	31·7	− 2·9
March	−0·3	14	17	91·0	+ 4·8
April	−4·3	25	5	74·6	−36·5
May	−4·7	27	4	135·6	−21·0
June	−2·9	26	4	141·9	−83·3
July (to 19th)..	−5·8	19	0	52·9	−51·6

The sign − indicates *below* the average.

In every month the temperature has been below the average. The generally severe character of all the months, excepting February and March, is well shown in the column giving the number of days on which the temperature was below the average. From April 1 to July 19, a period of 110 days, 97 were below the average. And during the same period the hours of bright sunshine numbered only 405·0, which is 192·4 hours less than the average of the same period in the two preceding years, or 0·68 only of the amount registered in those years.

Royal Observatory, Greenwich, July 23 WILLIAM ELLIS

Some Remarks on the Rev. J. G. Wood's Explanatory Index to "Waterton's Wanderings"

1. THE name of the Indian tribe mentioned ought to be *Tamanacos* and not *Jamunacs*.

2. The botanical name of the arrow-reed is *Gynerium* (not Gynœcium, p. 372).

3. (P. 378).—The Balata gum has reached the English market long ago, though it may have disappeared again. If I do not mistake there existed about 1864 even a Balata Gum Company, in which Messrs. Silver and Co. took the lead. We have in Venezuela (State of Maturin) the same tree, where it is called Purvio. Mr. d'Azevedo, of Maturin, sent several times quantities of the gum to Hamburg, but I am informed it did not pay.

4. (P. 380).—The *Camoudi* is *Eunectes murinus* (not marinus). Mr. William Crookes in his article "Gravitation as a Factor in the Organic World" (*Journal of Science*, January, 1879, p. 42), calls it aquatic, and says that it inhabits the rivers of South America. This is certainly wrong; it is generally found near the water, and swims very well, but is by no means an aquatic animal.

5. (P. 381).—*Copaiva* is an oleo-resin, and should not be called a *gum*. There is on p. 461 a misprint in the name of the tree from which it is obtained. It is *Copaifera pubiflora*, Benth., not *C. publiflora*.

6. (P. 383).—The name of the describer of the birds of Trinidad is *Léotaud* ("Oiseaux de l'Ile de la Trinidad," Port d'Espagne, 1866).

7. (P. 384).—The castor-oil plant belongs to the family of Euphorbiaceæ, but not to the tribe of Euphorbiæ (or better Euphorbieæ).

8. (P. 385).—Read *Anolis* instead of *Anolius*.

9. (P. 394).—The coffee-tree does not belong to the "useful

group of Cinchonaceæ," which is distinguished by numerous seeds in each carpid, though both are included in the same family of Rubiaceæ.

10. (P. 407).—The conical mound of the Flamingo is not at all an error in natural history. I have seen several of these nests with the eggs on the top of the heap on the Roques Islands, north of La Guaira.

11. (P. 412).—*Vulpes cancrivorus.* There is no species of true *Vulpes* in South America, if we follow the distinction established by Burmeister ("Syst. Uebersicht der Thiere Brasil.," i. 92), and the animal in question is undoubtedly the *Canis cancrivorus*, Desm.

12. (P. 431).—"Kurumanni Wax. This is composed of the wax of a wild bee (*Ceroxylon andicola*), mixed with a pitch-like substance obtained from several trees, chiefly the Maam-tree." It is scarcely possible to believe that the accomplished editor of the "Wanderings" should have penned these lines, where a noble palm of the Andes is changed into a wild bee. The latter is most likely a species of *Melipoma.* The *Kurumanni wax* may be identical with a pitch-like substance, called *Caraman*, *Paraman*, or *Peraman* in Venezuelan Guayana, and which is obtained from *Moronobœa coccinea*, Aubl.

13. (P. 434).—The mahogany tree belongs to the family of Meliaceæ, but not to the group of Cedraceæ (or better Cedreleæ).

14. (*Ibid.*).—*Maribunta* is not a Portuguese word signifying a wasp. In Brazil the word *maribondo* is used for a certain species of wasp, but the name is taken from the Tupi language.

15. (P. 440).—*Mosquito.* Even in popular works on natural history authors should call everything by its real name, and it is therefore altogether wrong to speak of the *bites* of mosquitoes; *sting* would have been just as short, and evidently more correct.

16. (P. 447).—*Pataca.* Perhaps we may read *paraca*, one of the names of *Ortalida motmot*, Wagl.

17. (P. 473).—*Sting Ray. Trygon pastinaca* is a maritime species; but there are several other ones in South American rivers, as *Tr. hystrix*, &c.

18. (P. 473).—*Sugar-cane* is *Saccharum* (not *Saccharinum*) *officinarum.*

19. (P. 474).—"The tiger-birds derive their popular name from the peculiar cry which they utter." What powerful lungs they must have, these tiger-birds, in order to be able to roar like a jaguar! This singular blunder might have been avoided by reading carefully what Waterton says (p. 195), that it has no song, its name being due to the black spots on a yellow ground on breast and belly.

20. (P. 475).—The tortoise of Guayana, as far as I know, is *Testudo tabulata*, called *Morrocoi* in Venezuela. *Cistudo carolina* is a North American species.

21. (P. 236).—*Chigoe.* What Waterton says of the hatching of this animal *within* the body of man is certainly not true. The eggs are developed outside, the larvæ leading a free existence.

Some of the foregoing remarks refer to mere clerical errors (a good many less important ones having been passed over in silence); but there are unfortunately not a few inaccurate, and even wholly erroneous statements which we were sorry to find in this *Explanatory Index.* A. ERNST

Caracas, May 15

Swift's Comet.—Williams College Observatory

I HAVE computed a set of elements of the comet lately discovered by Mr. (now Dr.) Swift. They are from observations made by Prof. Lewis Boss, Director of the Dudley Observatory at Albany, on June 24, June 30, July 8. They are these:—

T April 27·1801, M.T. Washington.

log. *q*	9·950918	
ω[1]	3° 28′ 13″·0	M.Eq., &c., 1879·0.
☊	45° 41′ 10″·5	
i	107° 1′ 53″·6	

For the middle observation $c - o$ $\Delta\lambda + 0''\cdot6$
$\Delta\beta + 0''\cdot3$

Or in space, both co-ordinates, about 0″·5.

They were computed by Olbers' method, afterwards varying *M* by the regular rule. A trifling change of *M*, which I have not now time to make, would bring a closer representation of the middle observation; say to about +0″·2 and −0″·1 in longitude and latitude respectively.

I am happy to say that the observatory of this college is to be repaired and put into active operation. It is about forty years old—the oldest I believe in the United States;—and was built by the late Prof. Albert Hopkins. It now contains a 7½ inch equatorial, an early work of Clark; a 3½-inch Simms transit, of the style of forty years ago, with a very poor object-glass; and a sidereal clock by Molyneux and Cope, still in good order. I have been authorised by the Hon. David Dudley Field to procure a meridian circle with telescope of about 5 inches aperture.

The gentleman just named is the founder of my professorship, the "Field Memorial Professorship of Astronomy," and it is probable that in future a portion of the duties of that professorship will consist in making observations, and in their complete discussion. TRUMAN HENRY SAFFORD

Williams College, Williamstown, Mass., U.S.A., July 11

[1] Dist. of perihelion from node.

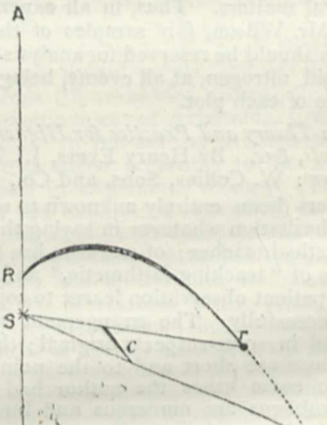
Electric Lighting

IN the evidence given before the Committee on Electric Lighting, some mention was made of the difficulty of equalising the light over any considerable area; but it is worth while to remark that by a simple form of reflector it is possible to make the light very approximately uniform over an area whose radius is twice the height of the lamp above the ground. For imagine a sphere with the lamp as a centre and its height above the ground for radius. Supposing the lamp radiates equally in all directions, the surface of this sphere will be uniformly illuminated, and its surface has an area $4\pi h^2$. If now we take a plane circular area about the foot of the lamp-post with radius R such that—

$$\pi R^2 = 4\pi h^2 \text{ or } R = 2h,$$

it is plain that by a proper distribution by reflection of the light which would pass through the imaginary sphere outside the solid angle subtended at the lamp by the plane circular area, the illumination over that area may be made uniform and equal in intensity to that near the foot of the lamp-post.

To find the proper form for the reflector, which is of course a surface of revolution, it is necessary to find the angle which



each zone of the reflector subtends at the lamp in terms of the angle in which the light is reflected by it. This is given by the equation—

$$\sin\phi\, d\phi = \left(\frac{1}{\cos^3\theta} - 1\right)d\theta,$$

with the condition $\phi = 0$, when $\theta = \tan^{-1}2$, where taking the lamp-post as the polar axis, the upper end being north, $(90 - \phi)$ is N. latitude of that zone of the reflector which sends out its light in S. latitude $(90 - \theta)$. The polar differential equation of the curve for the reflector may then be easily found; it is—

$$\rho\frac{d\phi}{d\rho} = \cot\frac{\theta + \phi}{2}.$$

A figure of the curve is given below at R R′. The surface of the reflector is swept out by the revolution of R R′ round A A′. The dotted portion *r* R′ should be replaced by a separate zone shown at *c*, but the chief value of this will be as a screen to prevent the light from being visible at low altitudes, the small quantity of light reflected by it merely going to reinforce the illumination in the immediate neighbourhood of the lamp. Such reflectors might be applied with great advantage to powerful lights placed at considerable elevations. A. MALLOCK

Brampford Speke, near Exeter, July 14

Did Flowers Exist during the Carboniferous Epoch?

THE systematic position of the genus *Breyeria*, founded for the reception of a fossil insect, having formed the subject of a recent discussion in NATURE (vol. xix. pp. 554, 582), I have just visited Brussels to examine the original type. Through the courtesy of M. de Borre I have been allowed to submit it to a careful microscopical scrutiny, and have sketched, with the aid of a camera lucida, a considerable part of it on a large scale. The facies of the neuration is extremely similar to that of genera allied to *Palingenia* of the Ephemeridæ, resembling theirs not only in the relative abundance of cross-veinlets, but also in the manner of the aberrations of abnormal cross-veinlets. The *Palingenia* group is sufficiently elastic to comprise *Breyeria*, although this differs in detail to some extent from any known genus of recent Ephemeridæ.

May I be allowed to suggest that if photographs of fossil insect-remains be taken for critical purposes, it would be advantageous to execute them on a scale of considerable enlargement. In the present instance it was far easier to distinguish nervures from mechanical depressions in the stone when a 3-inch object-glass was employed, than when a 4-inch glass was in use. These are the lowest powers that I have with me, excepting simple lenses. *Apropos* of fossil insects, there is in Baron E. de Selys Longchamp's collection, a species of recent *Homoptera*, which is likely to be of interest to palæontologists. At first sight it resembles a fragmentary specimen of Ephemeridæ so closely that it was actually reserved for my inspection as such. Viewed through a weak lens, it would appear from its anterior wings to be a representative of the Planipennes, allied more or less to *Panorpa* perhaps. A more careful examination, however, reveals its rostrum and other characters distinctive of its real nature. Its exact affinities have still to be ascertained; but in all probability it is something new and extraordinary. A. E. EATON

Rotterdam, July 24

The Papau

I WAS surprised to read Capt. Oliver's statement in NATURE, vol. xx. p. 241, that the papau (*Carica papaya*) is not eaten by birds in Bourbon and Mauritius. In Samoa it is eaten largely by birds—especially by *Sturnoides atrifusca*, and also by bats of the genus *Pteropus*. The seeds of this fruit appear to have been carried by the agency of birds over at least a great portion of the islands. I have seen many places where the virgin forest has been cleared, and in every instance—as far as my memory serves me—a thick crop of *Carica* appeared as soon as the clearance was made. They do not grow while the forest stands, but spring up in thousands as soon as light is let in to the soil.

S. J. WHITMEE

17, Leinster Square, Rathmines, Dublin

Intellect in Brutes

I HAVE just been watching with care the action of a party of ants, exhibiting an intelligence nearly allied to reason, if indeed it be not "reason."

Clearing the shelf in a hothouse, two large cockroaches were caught, killed, and left lying on the small gravel with which the shelf is covered. This shelf is four feet from the floor, and the nest of the ants is behind a slab at the end of the house. When the cockroaches were killed, very few ants were upon the slab, but they must have communicated to others the discovery of the treasure, for in about twenty minutes a swarm of ants emerged from the nest, climbed the wall, gained the shelf, and there, dividing into two parties, proceeded to take possession of the dead bodies.

To understand the significance of what I am about to relate, it is necessary to form a distinct conception of the comparative sizes of the ant and the cockroach. The ant was the smallest of its kind; the body of its prey was nearly two inches long and half an inch in width. The proportions were to each other as would be those of a man to the dome of St. Paul's. Their purpose was to carry these two huge carcases to the nest, and to accomplish this it was necessary, first, to draw them for a space of ten inches over rough gravel, then along a smooth board for two feet, then to drop them to the floor beneath, then to drag them over some very rough rubble for one foot four inches, and finally to pass them between two slabs of wood into the nest. This extraordinary feat they performed successfully. It was accomplished thus: They surrounded the corpse of the dead cockroach and seizing it with their mandibles, moved it onward a little way. It was lying inclined on its side. When moved, the projecting edges of the side hitched in the stones and prevented progress.

I observed that, on some larger stones near the spot, half a dozen ants stood looking at the work, but taking no part in it. When the hitch occurred, and always afterwards when there was an obstacle, these "surveyors" left their stations, went to the workers and then returned to their place of observation. They were manifestly directing the operation and instructing the labourers; and they as manifestly made some communication to the labourers, for forthwith these changed their plan. They now turned the cockroach on its back, and in this position they moved it onward triumphantly for three or four inches. How? They stood round the corpse at precisely equal distances apart and by a common effort dragged it forward. They pulled together, apparently in obedience to a signal from the "surveyors," just as men shout when *they* want to pull together.

Another obstacle. Three pieces of gravel bigger than the rest lay in their path. What to do now? They crept under the carcase and lifted it by planting the hind feet on the floor and standing upright, sustaining the load with their heads, while a party mounted the opposing stone, seized the tail, and tried to drag the burden up. But in vain. It was too much for their strength and the load was dropped.

Then the ants that had been directing again moved from their places and ran rapidly about in all directions, as if seeking some more easy passage. Having found one they remounted their post of observation and it may be assumed that they gave some intelligent orders to the labourers, for immediately these resumed their hold upon the carcase and moved it forward in the new direction indicated.

Similar obstacles occurred four times in the course of their journey over the gravel, and on each occasion the same proceedings were observed. Their patience was inexhaustible. At length the body was successfully brought to the smooth edge of the wooden shelf, whence it could be dropped to the floor beneath. But it was necessary to select a fit spot for this purpose, inasmuch as the floor was strewn with bricks and plants. In fact there was but one open space of about four inches square into which the body could be sent so as to be carried securely to its destination. To reach this spot they had to drag the burden along the ledge for a space of 17 inches. In this journey balance was repeatedly lost and the carcase would have fallen, but that it was seized, held, and dragged back by their united efforts. At length, having arrived at the place where its fall would be upon the open floor, it was dropped by all at once losing their hold of it. But previously to their doing so, the "surveyors" ran down the wall to the floor and posted themselves directly under the ledge on which the body lay (4 feet above them). There they waited its fall. I think their business there was to see if all was safe and the place really fit for the purpose, and that they made some communication as to the precise spot to be chosen for the fall, for the ants who bore the corpse shifted it two or three times before they let it down. Then all followed, running down the wall, seized their prey again, and in half an hour carried it a distance of nearly 3 feet to the entrance of the nest.

But here another difficulty occurred. It could not pass between the boards when lying upon its back. They turned on its side and tried again. Again they were baffled; the legs hitched. So they turned it on its back once more, bit off the legs, which were carried into the nest by other ants, and then the body was turned on its side and taken through the narrow way into the nest. To me, looking at it with the eyes of a psychologist, all this seemed to indicate the exercise of a *reasoning* faculty. They devised new methods of meeting new circumstances. It satisfied me, also, that ants at least have means of intercommunication. The fact of the find was obviously communicated by the discoverers. The workers manifestly acted under instructions and obeyed commands.

My object in this communication is merely to place upon record a very remarkable proof that the lower animals have intelligence very like our own. EDWARD W. COX

Moat Mount, Mill Hill, N.W., July 26

As you are publishing notices of intelligence in brutes perhaps the following example of memory in a bird may be interesting. When I returned from the Pacific about two years ago, I brought a sulphur-crested cockatoo (*Cacatua galerita*) from Australia. Soon after my arrival in England I had occasion to cut his wing, and this destroyed all his former friendly feelings towards me. On my removal from Blackheath to Dublin, I placed him for a few weeks in the Zoological Society's Gardens, Regent's Park. Being in London in May I brought him with me on my return to this city. I went to the Gardens for him myself, and was interested and somewhat pleased to find, on speaking to him, that he had apparently forgotten me. On my way, in a cab, to the hotel where I was staying, he was very friendly; but on my arrival there, as soon as I took off my hat, it was evident that he recognised me, for his old manner at once returned. On arrival here he appeared to remember my children, and resumed his former friendliness towards them, but he still regards me as his enemy.

17, Leinster Square, Rathmines, Dublin S. J. WHITMEE

Proceedings of the Aberdeenshire Agricultural Association, 1878

YOUR article (vol. xx. p. 288) touches a subject of vital importance to the farmer in these bad times, and I hope you will follow it up by an appeal to Mr. Lawes and to the Royal Agricultural Society of England to institute a parallel and independent course of experiments, in order to test the conclusion in regard to phosphatic manures announced by the Scotch Association.

The possible presence of humic, carbonic, and other acids in more than usual proportion in Aberdeenshire soil and water has often occurred to me as accounting for the similarity of results between those local experiments of soluble and insoluble phosphates, but it can hardly be denied that a case has been established for further experiments. The fact that acid and manure makers have a direct pecuniary interest in the existence of a prejudice in favour of *soluble* phosphate might alone have led farmers to require some evidence before spending their money under such scientific dictation, but now that they have three years' experiments testifying distinctly against the doctrine, they may surely look to their advisers for something more than bare assertions on authority that a "mineral phosphate is of little or no value as a maure until it has been rendered soluble by acid in course of manufacture." ALFRED S. JONES

Wrexham, July 27

Spicula in Helix

WHILST dissecting, a few days ago, a common garden snail (*Helix aspersa*), I came across two calcareous spicula, lying immediately under the "albuminiparous gland," which I cannot find mentioned in any of the text-books. I at once dissected three other snails of the same species, and in two I found no spicula, while in the third I found one lying in the same place as the two before-mentioned. Can any reader of NATURE tell me whether these are of the same nature as the diffused spicula of Doris, or if not, of what nature they are? They could not have been "spicula amoris" (Huxley) of the dart-sac, as they were not contained in any sac at all, but were lying free in the above position. EDWD. B. PARFITT

3, Waterfield Terrace, Blackheath, S.E.

GENERAL RESULTS OF EXPERIMENTS ON FRICTION AT HIGH VELOCITIES MADE IN ORDER TO ASCERTAIN THE EFFECT OF BRAKES ON RAILWAY TRAINS[1]

II.

SOME special experiments were made with blocks of small area. The brake-blocks generally used in these experiments were 12 inches long, by 3 inches wide, giving a surface of 36 square inches; the small brake-blocks were made so as to afford a surface of pressure against the wheel of only one-third of this amount, or 12 square inches, thus making the pressure per square inch three times as great as before. The diminution of surface was obtained by casting projections upon the face of the block. The author is not prepared to say that any greater coefficient of friction was obtained by the extra pressure per square inch, although in one of the experiments, at a velocity of sixty miles an hour, the rotation of the wheels was arrested by these blocks, whilst this effect had not been produced at that speed in other experiments. The experiments on this form of block were stopped because the blocks were entirely worn down in the course of about twelve experiments.

Mr. Rennie showed[1] that high pressures per square inch produced a greater coefficient of friction between surfaces either moving very slowly or nearly at rest; but it must be borne in mind that the author's experiments were made with high velocities, whereby a serious element of disturbance is introduced, viz., the grinding away of the surface; and it is therefore probable that the increase in the coefficient of friction due to increased pressure, may have been neutralised by the lubricating effect of the fine particles ground off the surfaces.

While no certain opinion can be expressed as to the relation which the coefficient of friction bears to pressure, so far as these experiments are concerned, it is quite clear that in proportion as the pressure is increased or diminished so will the actual friction obtained be increased or diminished. When the friction which exists between the brake-blocks and the wheel reaches a certain point, the wheel ceases to rotate, and becomes fixed. This point is reached when the frictional resistance of the blocks exceeds the adhesion between the wheel and the rail if the speed is kept up; or, if the speed is slackening, when it exceeds the adhesion between the wheel and the rail, plus the effort required to retard the rotation of the wheel equally with the retardation of the train; and the excess of resistance then acts as an unbalanced force, tending to destroy the momentum of the wheel.

Usually there are in a train a certain number of vehicles braked and a certain number unbraked. If the brakes acted on all the wheels, then the rotating momentum of the wheels does not add to the distance in stopping a train, because that momentum can be acted upon by the brakes directly, without in any way affecting the adhesion of the wheels to the rails. It simply requires an additional amount of brake-block pressure.

With the unbraked portion of a train the rotating momentum of the wheels is an addition to the momentum due to the weight of the train (including therein the actual weight of the wheels), which cannot be utilised for retardation; and it therefore seems important that there should be brakes on every wheel of a train.

As it thus appears that it is the adhesion which governs the retardation which the brake-blocks can exert upon wheels, it is manifest that the pressure brought to act on the brake-blocks should never give an amount of friction which exceeds the adhesion. At a high speed, however, the pressure required to produce a degree of friction equal to the adhesion is much greater than what is required at a low speed.

The following table gives approximately the proportion which the pressure to be applied to the brake-blocks should bear to the weight upon the braked wheels, with coefficients of adhesion between wheel and rail varying from ·30 to ·15 of the weight on the wheels:—

Ratio of Brake-Block Pressure to Weight on Wheels

Speed.		Approximate ratio of total pressure on brake-blocks to total weight on braked wheels.			
Feet per second.	Miles per hour.	Coefficient of adhesion 0·30.	Coefficient of adhesion 0·25.	Coefficient of adhesion 0·20.	Coefficient of adhesion 0·15.
11	7½	1·20	1·04	0·83	0·60
22	15	1·41	1·18	0·94	0·70
29	20	1·64	1·37	1·09	0·82
44	30	1·83	1·53	1·22	0·92
59	40	2·07	1·73	1·38	1·04
73	50	2·48	2·07	1·65	1·24
88	60	4·14	3·47	2·77	2·08

[1] Continued from p. 295.

[1] *Phil. Trans.* for 1829, p. 159.

It will be seen that, when the adhesion equals ·30 of the weight, a pressure equal to 1·2 of the weight would skid the wheel at $7\frac{1}{2}$ miles per hour, whilst a pressure equal to 4·14 times the weight would be required to do so at 60 miles per hour.

On the other hand, if the adhesion is only ·15, the pressure requisite to skid the wheel would be only ·60 of the weight at $7\frac{1}{2}$ miles per hour, and 2·08 of the weight at 60 miles per hour.

Thus the efficiency of a brake depends upon the pressure being proportioned to the speed and to the adhesion. If the adhesion were always uniform, the rule would be very simple; but this is not the case.

The adhesion of the wheels to the rails varied according to the materials, that is, whether the train was travelling upon iron or steel rails; and according to the state of the rail, whether dry, wet, or sanded.

On dry rails it was found that the coefficient of adhesion of the wheels was generally over ·20. In some cases it rose to ·25, or even higher. On wet or greasy rails, without sand, it fell as low as ·15 in one experiment, but averaged about ·18. With the use of sand on wet rails it was above ·20 at all times; and when the sand was applied at the moment of starting, so that the wind of the rotating wheels did not blow it away, it rose up to ·35, and even above ·40. Consequently, the retarding effect of the brakes would be greatly increased, were means devised for placing sand under every wheel to which a brake is applied, during the progress of a stop.

The effect in stopping a train is greatest when the friction between the brake-blocks and the wheels amounts to a quantity just short of the resistance caused by the adhesion, because as soon as the brake-block friction exceeds the adhesion, the wheel becomes fixed and begins to slide. In order, however, to secure the best results in stopping, it is obviously necessary that the brake-block pressure should be regulated to give a friction about equal to the adhesion of the wheels at every stage during the progress of a stop.

There is no reason why, in the progress of mechanical science, these conditions should not be regulated by a self-acting arrangement.

Mr. Westinghouse has devised a valve to regulate the pressure between the blocks and the wheels. The principle of the valve is, in the first place, to prevent the actual friction from exceeding the adhesion at any point; and in the next place, whilst allowing the fullest amount of pressure necessary to produce the maximum friction to be applied to the brake blocks when the brakes are first put on, that is to say, when the speed is high at the commencement of a stop, to reduce that pressure gradually during the progress of the stop, so as to maintain as nearly as possible a uniform amount of friction.

As the adhesion varies it is necessary to consider what amount of adhesion for purposes of retardation can be safely calculated upon.

The following table shows the distances required to stop a train on a level line from a speed of fifty miles per hour, with a retarding force of from 5 to 30 per cent. of the total weight of the train:—

Percentage of retardation.	Yards run at fifty miles per hour.	Percentage of retardation.	Yards run at fifty miles per hour.
5	$555\frac{5}{9}$	18	$154\frac{1}{3}$
10	$277\frac{7}{9}$	20	139
12	$231\frac{1}{2}$	25	111
15	185	30	$92\frac{2}{3}$

If the brakes act upon each wheel, then a retardation of 10 per cent. of the load carried by each wheel—counting the rotating momentum as part of the weight—will stop a train in $277\frac{7}{9}$ yards.

If the brakes act upon only half of the weight of a train, a retardation of 20 per cent. would have to be exerted upon the braked half to produce the same result. As pointed out, 20 per cent. adhesion is rather above the average obtainable, while 25 per cent. is the highest result obtained under the most favourable circumstances at any considerable speed, or except when sand was applied to wheels moving slowly.

The above table should be carefully noted, for it will be seen that, even if brakes act upon all wheels, 25 per per cent. retardation will only give twenty-eight yards better result than 20 per cent., or if half of the train only be braked, it will give fifty-nine yards advantage.

A consideration of this feature of the brake problem points out (1) that the advantage to be gained by trying to obtain above 20 per cent. retardation on each wheel is greatly overbalanced by the risk of "skidding;" and (2) that it is far easier and safer to make a stop in 250 yards from fifty miles per hour with the whole train braked, than with brakes upon only half of the train.

The following are some of the results of the earlier experiments, obtained with Mr. Westinghouse's pressure-regulator:—

No. of experiment.	Gradient.	Speed. Miles per hour.	Condition of stops. Time of stop. Seconds.	Distance run in yards.	Distance reduced to fifty miles per hour. Yards.	Total brake block pressure, P, on the four wheels. Maximum.	Middle of experiment.	End or at skid.	Percentage of brake block pressure to weight on wheels. Maximum at commencement of experiment.	Minimum at end of experiment.	Observed coefficient of friction between brake blocks and wheels on one pair of wheels. At moment when maximum pressure was obtained.	At middle of experiment.	At end of experiment.	Mean retardation calculated from stop.
1	$+\frac{1}{264}$	60	$12\frac{3}{4}$	171	118	32370	31200	23010	160	114	·129	·125	·241	23·3
2	$+\frac{1}{311}$	60	$12\frac{1}{4}$	167	116	32760	30810	29250	162	145	·145	·161	skid	23·9
3	$+\frac{1}{330}$	55	$10\frac{1}{2}$	141	116	28470	27690	16380	141	81	·160	·176	·31	23·8
4	$-\frac{1}{406}$	57	$18\frac{1}{2}$	223	171	24570	15990	7800	122	39	·157	·158	·35	16·8
5	Level last thirty yards $-\frac{1}{176}$	55	18	227	194	17550	14430	8190	87	40	·161	·182	·309	14·3

In the second experiment the pressure was not reduced with sufficient rapidity to prevent the skidding, and in the fourth and fifth experiments the pressure was insufficient at the beginning of the experiment. From these and other experiments it was found that the best results were obtained in cases where the pressure applied at first was from about $1\frac{3}{4}$ to twice the weight on the wheels, and where the reduction of the pressure was effected with sufficient rapidity towards the end of the stop to prevent the friction being sufficient to skid the wheels.

The necessity for the instantaneous application of the maximum brake-block pressure throughout the train is evident from the fact that, at a speed which is frequently obtained, namely, 60 miles per hour, a train passes over 88 feet each second; therefore the loss of two or three seconds in applying the brakes means often the difference between safety and danger, and the rapidity of a stop largely depends upon the rapidity with which all the brake-blocks can be brought to act against the wheels of a train.

This points to the advantage of being able to move the brake-blocks with great rapidity from their position of inaction to that of contact with the wheels; because it is essential to provide that the brake-blocks, when out of use, shall be removed to a distance from the wheels sufficient to prevent the possibility of their dragging against the wheels, and thus retard the progress of the train. The question of the rapidity with which brakes can be applied in practice is thus one of much importance.

Some experiments were made in October, 1878, upon the North Eastern Railway, on a train fitted with the vacuum brake, and one fitted with the Westinghouse brake to ascertain the time which was required after moving the brake-handle to set the brakes with various degrees of force in different parts of the train. The following table shows the result arrived at:—

Place of experimental van from engine.	Vacuum brake.				Westinghouse automatic brake.			
	Commencement of movement of blocks.	Half on.	Three-quarters on.	Full on.	Commencement of movement of blocks.	Half on.	Three-quarters on.	Full on.
	secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.
1st vehicle	$\frac{1}{2}$	3	7	11	$\frac{1}{4}$	$\frac{5}{8}$	$\frac{3}{4}$	$1\frac{1}{2}$
7th ,,	2	$6\frac{1}{2}$	$8\frac{1}{3}$	14	1	$1\frac{3}{4}$	2	$2\frac{1}{2}$
13th ,,	$3\frac{1}{3}$	$7\frac{1}{2}$	$9\frac{1}{2}$	14	$1\frac{3}{4}$	$2\frac{3}{4}$	$3\frac{1}{8}$	$3\frac{1}{2}$
21st ,,	$5\frac{1}{2}$	17	30	—	3	$4\frac{1}{2}$	5	$5\frac{1}{2}$

A long interval of time between brakes coming on at the front and rear of a train may become a source of danger; and improvements have been introduced in both the vacuum and Westinghouse apparatus since that date to reduce the interval as shown by the experiments.

In the Westinghouse brake a simplified triple-valve has been adopted, the friction has been reduced by the use of an enlarged pipe and by the removal of bends in the connections between the carriages; by these alterations the interval of time required to put on the brakes, as shown in the above table, has since been reduced by nearly one-half, and an experiment recently made on the application of the brake in rear of a train of twenty-four vehicles on the Western Railway of France showed that the pressure commenced to be brought on in one second, and was fully on in two and a half seconds from the time of first moving the brake lever.

The importance of simultaneous action of the brakes in every part of a train arises from the fact that the train is not a rigid mass, but is made up of separate vehicles connected by means of spring draw-bars and buffers. The length of the train can thus be modified to a certain extent by the degree of compression of these springs. In a recent experiment on the North-Eastern Railway the train consisted of twenty-four carriages, and the whole extent to which the buffers could be compressed amounted to 35 feet. A train travelling at 60 miles an hour moves at 88 feet in a second. If the brakes act on the front part of the train before they affect the hind part the speed of the front carriages may be diminished by 10 to 18 feet in a second, whilst the hind part moves on with undiminished speed; thus the hind part may press against the front part with a force of from 12 to 20 foot tons for every ton weight of the hind vehicles. The buffer springs would be compressed by this force and remain so till the brakes acted equally on all the wheels, when a reaction of the buffer springs would take place; this reaction creates the violent jerks often felt with continuous brakes, and occasionally results in fractures of couplings and draw-bars. In a perfect brake the application would be instantaneous, and simultaneous on all the wheels of a train.

It is beyond the scope of this paper to enter fully into the merits of different kinds of brakes, but it may be convenient to sum up what seem to be the requirements of a perfect brake.

1. It should be fitted to act upon each wheel of the engine, tender, and every other vehicle in a train of any length. The brake-blocks, when out of action, must be kept a certain distance away from the wheels, in order to prevent any liability to drag against the wheels; and this distance, after being once adjusted, gradually increases by the wear of the blocks, and often exceeds three-quarters of an inch; while the springing of the brake-gear under great strain also adds to the extent of movement required in the brake force before the blocks are fully applied. Hence the brake gear should be so adjusted as to be capable of moving the brake blocks instantaneously through a space of one inch.

2. However brought into action, it should be capable of exerting upon the blocks of each pair of wheels, within two seconds, a force of twice, or at the very least one-and-three-quarter times, the load on those wheels.

3. The brake-block pressure acting on each wheel should be regulated so that the friction between the brake-block and the wheel may always be limited so as not to exceed the adhesion between the wheel and the rail; by which means it will produce the maximum effect at each moment of its application.

4. The brake-block pressure should be capable of being applied by engine-driver or by guards.

5. The engine, tender, and vehicles should each carry its own store of brake-power, which should be independent of the brake-power on any other vehicle.

6. The brake-block pressure should be automatically applied to every vehicle by the separation of the train into two or more parts, and it should also be applied by the act of the wheels of any carriage leaving the rails.

7. The brake-block pressure should be automatically applied by such failure of the connections or appliances as would render it afterwards incapable of application until the failure had been remedied.

8. The brake-block pressure should be capable of application with any degree of force up to the maximum, and it should be capable of continued action on inclines, or, of repeated applications at short intervals at junctions and stations.

In addition to these requirements, the questions of cost, durability, convenience in operation, and other essential points, will of course come under consideration.

The experiments which have been here described were made on trains travelling under conditions which were necessarily continually varying, both in respect of the condition of the rails and other matters; and they therefore contained many elements beyond the reach of calculation. It is hoped that some opportunity may arise, ere long, for resuming expeiiments on friction at high velocities under conditions whence these elements of disturbance may be eliminated. Meanwhile it is evident that a continuous brake, capable of being applied simultaneously to every wheel of a train under the conditions which have been enumerated in this memorandum, is a much more practical and scientific method of bringing a train to rest than the old plan of concentrating the brake-power in two or three heavy brake vans placed in different parts of the train, and leaving the rest of the wheels without brakes.

The advantage which thus evidently ensues from utilising the adhesion of every wheel of a train for the purpose of stopping a train suggests the further consideration as to whether it would not be a more scientific arrangement, as well as more economical in regard to the permanent way of railways, to utilise the adhesion of every wheel of a train for causing the train to move forward, instead of depending for the moving force upon the adhesion of one heavy vehicle alone, viz., the locomotive. Experiments connected with the action of brakes on railway trains require very delicate apparatus; the credit of the design of the apparatus used in these experiments belongs to Mr. Westinghouse. The efficiency of the arrangements for making the experiments is due to the London, Brighton, and South Coast Railway Company, as represented by Mr. Knight, their general manager, who afforded every facility for the use of the line, and by Mr. Stroudley, the locomotive engineer of the Company.

DOUGLAS GALTON

OUR ASTRONOMICAL COLUMN

THE COMET OF 1532.—This comet, the second of the five observed by Apian, as described in his rare work, the "Astronomicum Cæsarium," has been the subject of much computation and discussion in connection with its long-assumed identity with the comet of 1661 observed by Hevelius, to which attention was directed by Halley when he published his "Synopsis of Cometary Astronomy." We read: "Halley was apt to believe that the comet of 1532 was the same with that observed by Hevelius in the beginning of 1661, but Apian's observations, which are the only ones we have, are too inaccurate to determine anything certain from them in so nice an affair." Pingré fully believed in the identity of the comets of 1532 and 1661, and in his "Cometographie" has endeavoured to point out several previous appearances of the same body, as in the year 1402, when he expresses his conviction that the great comet recorded in so many of the European chronicles about Easter was no other than the one in question. Between the perihelion passages of 1532 and 1661 is a period of 128¼ years, and so the return of the comet was long expected about 1789. Shortly before this year, however, the rediscussion of the observations of 1532 and 1661 was made the subject of a prize by the Paris Academy of Sciences, which was gained by Mechain.

His calculations threw much doubt upon the presumed identity of the comets, indeed were pretty generally considered as decisive against it. Olbers also recomputed the orbit from the observations of 1532, and although he found one much closer to that of the comet of 1661 than Mechain had done, seems to have arrived at the conclusion that the comets were not identical. Nevertheless, as the year 1789 approached, sweeping-ephemerides were prepared to facilitate a search, the then Astronomer-Royal, Dr. Maskelyne, taking a part in this work. The search was ineffectual, no one of the comets which appeared about that year presenting any indications of being the expected body.

It is probable that the elements of the comet of 1532 are open to even greater uncertainty than has been usually supposed. Apian's observations are clearly affected with large errors, yet we are under the necessity of relying upon them as the best data available, neither the vague and contradictory observations (if they deserve the name) by Fracastor at Verona, nor those of Vogelin at Vienna, being of service in the determination of a more certain orbit than can be inferred from the observations in the "Astronomicum Cæsarium." Apian appears to have observed at Dresden, and the times of observation are given by altitudes of Regulus and Arcturus; the amplitudes of the comet (S. to E.) and its altitudes are recorded. The positions of the stars for 1532·0 were:—

	Right Ascension.	Declination.
	° ′	° ′
Regulus	145 49·7	+14 12·1
Arcturus	208 35·2	+21 40·1

Assuming Apian's station to have been in longitude 0h. 54m. 56s. E. and latitude 51° 3′·7, his data furnish the following places, which, except for the first day, do not differ more than might have been expected from Pingré's reductions:—

	G.M.T.	Right Ascension.	Declination.
		° ′	° ′
1532, October	1·6635	155 43·8	−4 26·9
	2·6491	160 10·7	−3 20·3
	30·6699	206 3·6	+3 48·6
	31·6939	208 48·3	+4 20·6
November	7·6747	218 51·1	+5 55·8

We subjoin an orbit depending on the observations of October 2, 30, and November 7, and also Olbers' elements from Hindenburg's *Magazin für Mathematik*, 1787:—

	New Orbit.	Olbers.
Perihelion passage	Nov. 3·1355 G.M.T.	Oct. 18·3324
	° ′	° ′
Long. of perihelion	174 51	111 48
,, ascending node	132 32	87 23
Inclination	57 41	32 36
Perihelion distance	0·6736	0·5192
Motion	Direct.	Direct.

The comparison with the above-observed positions is slightly in favour of Olbers' orbit, though this differs from the place for November 7 by − 1° 40′ in longitude and − 4° 36′ in latitude. Still it will appear that Apian's observations may be represented within their evident limits of error, by orbits which differ widely.

The Chinese observed this comet from September 2 to December 25, according to the extracts from their annals which have been given by E. Biot and Williams: on the former date, according to Olbers' elements, the comet was in longitude 98°, latitude 47° south, distant from the earth 0·78, and on the latter date in longitude 249°, latitude 16° north, distant 2·13. The mention of the comet having traversed Cygnus probably applies to that of 1533; at any rate the comet of 1532 could not have passed through that constellation.

THE SUN'S PARALLAX.—Mr. David Gill, writing from Madeira, on his voyage to the Cape of Good Hope, to take the direction of the Royal Observatory, as successor to Mr. Stone, states in a communication to the Royal Astronomical Society, that the reduction of the observations of Mars, made during his expedition to Ascension, in 1877, have been so far completed that he is able to give the resulting solar parallax. He presents values, differing little *inter se*, deduced from various combinations of the observations and, as the definite figure, 8″·78, which being interpreted with the aid of Col. Clarke's last determination of the earth's equatorial semi-diameter, implies that the mean distance of the earth from the sun is 93,101,000 miles. This is a smaller parallax than perhaps was generally looked for, though not differing materially from several values which have been worked out recently.

METEOROLOGICAL REGISTERS[1]

THERE is now scarcely a meteorological observatory which is not provided with registering instruments. The number of these is already considerable, and the methods of their construction are almost as numerous. At the observatory of Montsouris I have preferred continuous registration to registration by points, and the tracings effected by a fine metallic point on glazed paper blackened with smoke to the use of the pencil or to the *gaufrage* by pressure or by shock.

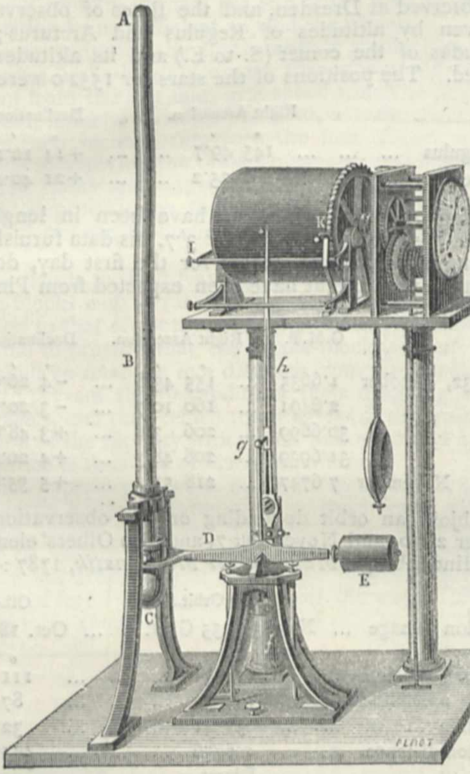


FIG. 1.—Registering Barometer.

The barograph of Montsouris is a barometer-balance devised by P. Cecci, and latterly adopted by P. Secchi. The barometric tube A B (Fig. 1) is of iron, of 3 centimetres internal diameter, and of the same calibre throughout its length. It is fixed, and has, at its lower extremity, a cylindrical plunger, the full section of which is equal to the vacuum section of the barometric tube. The cistern, C, is of steel, and suspended to one end of the beam of a balance, D, the other end of which carries a counterpoise. The centre of gravity of the weighted beam coincides, as nearly as possible, with the central knife-edge, so as to render the equilibrium indifferent. Hence it results that when, in consequence of a rise of the barometer, a part of the mercury rises from the cistern into the tube, the former, thus rendered lighter, tends to rise, with the effect of introducing into the mercury of the cistern a greater length of the cylindrical plunger. Equilibrium is only re-established when (the volume of mercury displaced by the plunger being equal to that which corresponds in the tube to the increase in height of the barometric column) the level of mercury in the cistern returns to its constant point, and when, consequently, the apparent weight of that cistern has resumed its fixed value. The needle, *h*, of the beam of the balance marks its displacements on the surface of the blackened cylinder, K. It only remains to read the indications. To do this as accurately as possible, the support of the cylinder carries an electro-magnet, the armature of which is provided with an arm bearing a metallic point.[1] In ordinary weather this point is fixed, and traces on the cylinder a datum-line from which the ordinates of the barometric curve are measured; but every hour the electro-magnet is acted on by a clock marking the time on all the registers; the point departs momentarily from the line, and makes a clear stroke, which is reproduced at the same instant on the other cylinders. At the end of each week the cylinder is taken away, to be replaced by another kept ready, and is placed on the reading apparatus.

This apparatus (Fig. 2) consists of a horizontal steel bench, *f g*, provided on its upper surface with two columns, *h h'*, intended to support the axis of the cylinder, and on a vertical stand with two microscopes, which may be moved either separately or together in a horizontal direction. By means of an endless screw a movement of rotation on its axis may be communicated to the cylinder for the purpose of presenting successively to the microscopes the various faces of the cylinder. The microscope *b* is always pointed on the datum-line, and it is to follow this line in its accidental displacements, that the two microscopes may be moved together by the action of an adjusting screw. The microscope which points to the traced line carries at its focus two crossed threads, one horizontal and the other vertical. At each hourly reading one of the horizontal tracings on the datum-line is brought under the horizontal thread of the microscope. But, as the barometric curve often presents inflexions of which it is useful to note the exact time, the critical point of this curve being brought under the movable microscope *a*, a second horizontal thread, movable by a micrometric screw, serves to measure the distance of this point from the hourly mark immediately preceding.

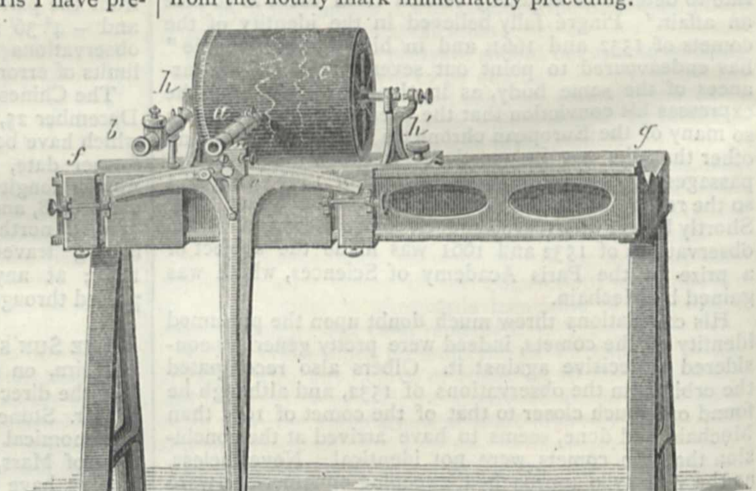


FIG. 2.—Micrometer or reading curves traced by the register.

The microscope *a* is supported at the extremity of an arm of a lever the length of which is equal to that of the

[1] Paper by M. Marié-Davy, *Journal de Physique*, April, 1879.

[1] This electro-magnet, which is seen in the thermograph (Fig. 3), is not shown in Fig. 1.

barometric needle; it describes, consequently, an arc of a circle of the same radius as the point of the register. This second microscope has a cross of threads like the first, but without a movable thread. The first microscope being pointed to an hourly sign, the axis of the second is directed to the corresponding point of the curve. The sine of the angle of position of its lever, multiplied by a

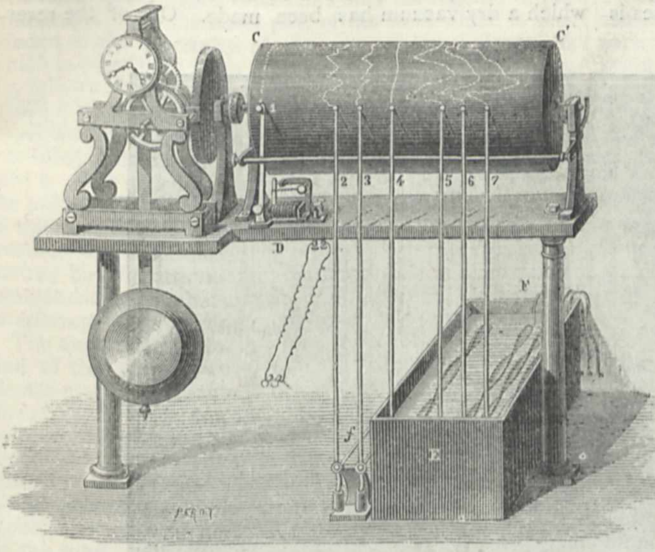


Fig. 3.—Thermograph.

constant factor and increased by a number equally constant, gives the height of the barometer at the moment selected. When the reading of the curve is made and verified, the sheet of blackened paper is taken off the cylinder and soaked in a weak solution of gum-lac or of copal in alcohol. By this means the smoked sheet is "fixed," and thereafter put in a portfolio.

The feeblest barometric variations are thus shown with

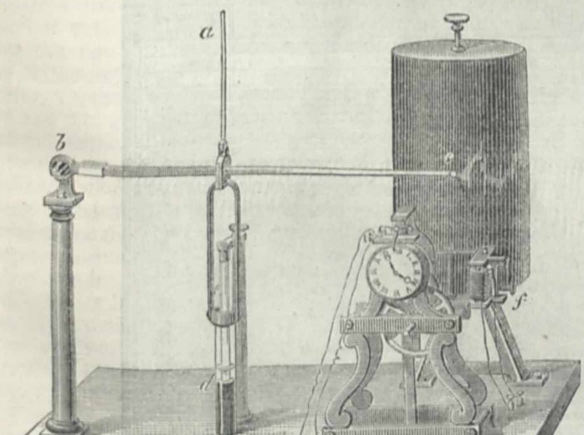


Fig. 4.—Register of the Atmograph.

great fidelity, and by examination of the curves we readily recognise the influence which the dynamical state of the atmosphere exercises in increasing or diminishing the weight of its pressure on the ground. It should be added that the uniformity of the calibre of the barometric tube annuls almost completely the action of temperature on the indications of the instrument.

The thermographs act in an analogous way, except tha the motor is here formed by a Bourdon tube of hammer-hardened copper, with a very elongated elliptical section, and twisted into a sort of spiral, the thread of the spire varying from 2 to 3 centimetres, according to the purpose for which it is intended. A B (Fig. 3) represents one of these tubes, forming a little more than one spiral. This tube is exactly filled with alcohol, and closed at its two ends. The expansion of the alcohol compels it to untwist; but in order that its elasticity may be preserved, the internal pressure, corresponding to the highest temperatures to be reached, should not exceed 8 or 9 atmospheres. This pressure is all the greater in proportion as the thread of the spire is short. The twisted tube is fixed by one of its ends, the other free extremity bears the needle which gives the indications. At Montsouris this needle is 5 centimetres long, as is also that of the barometer. The process of reading the curves is thus exactly the same for all the needle registers. The new thermograph of Montsouris Observatory contains seven needles acting on the same cylinder. The first needle (No. 1, Fig. 3) belongs to the electro-magnet which traces the datum-line, and the hourly signs; needles 2 and 3 belong to the psychrometer; needles 4 and 5 record the temperature of the surface of ground exposed to the air without shelter; needles 6 and 7 correspond to the actinometer.

The psychrometer is formed of two twisted tubes placed outside on the north face of the wooden kiosk, which shelters the cylinder, and perpendicular to that face. Their furthest extremity is fixed, the other is prolonged through the wall of the kiosk by a stem of copper carrying the indicating needle. One of the twisted tubes is uncovered, and forms the dry thermometer; the other is covered with cambric, and kept moist by means of cotton-wicks dipped in small glass cups, connected with a Mariotte flask placed in the kiosk by means of a long and fine tube of caoutchouc. An instrument of this kind has been in use for fifteen months at Montsouris; its action is regular, and its sensibility very great.

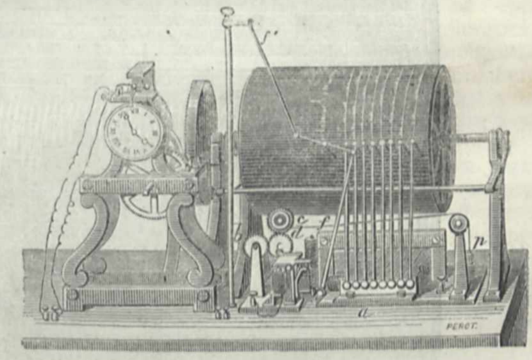


Fig. 5.—Anemograph.

The earth-thermometer is composed of two parts:—A thermometric reservoir of black copper is placed on the surface of a mass of vegetable mould, the top of which is flush with the platform of the roof of the kiosk; this reservoir communicates by a capillary tube of copper with a twisted tube placed in the kiosk under the cylinder of the register. When the temperature of the ground rises, a portion of the alcohol passes from the upper reservoir into the twisted tube, the pressure increases and the tube untwists; but the twisted tube itself and its capillary tube of copper, are themselves acted on by the variable temperatures which complicate the results. A second twisted tube, similar to the first, provided with its capillary tube, serves to give the necessary corrections. The two twisted tubes are arranged parallel to each other in a trough, E F, filled with glycerinated water; the two capillary tubes are soldered to each other throughout their length. The two needles trace two curves, one very slightly sinuous, the other very undulating; the difference of their ordinates is measured. We thus obtain the temperatures of ground exposed to the sun during day, to nocturnal radiation during night, to rain and to evaporation.

The actinometer is also composed of two twisted tubes resting parallel to each other in the above-mentioned trough, E F, and of two capillary tubes. To each of these tubes corresponds a reservoir placed at the top of the roof of the kiosk, and inclosed in an envelope of glass on which a dry vacuum has been made. One of the reservoirs is silvered, the other is blackened. Placed symmetrically at a little distance from each other, and far from any shelter, they give the same indications during the night; but during the day the black thermometer registers higher than the white one. The differences of the ordinates of the curves traced by the two needles serve to measure the actinic degree. Every cloud which passes over the sun gives rise to a rapid and considerable lowering of the temperatures of the white and black thermometers, and of that on the surface of the ground.

The atmograph is a third registering instrument intended to mark the variations of weight of a mass of earth whose surface is flush with the summit of the platform of the roof of the kiosk, and is exposed to the sun, to evaporation, rain, snow, and dew. At a little distance is the registering rain-gauge. The comparison of the

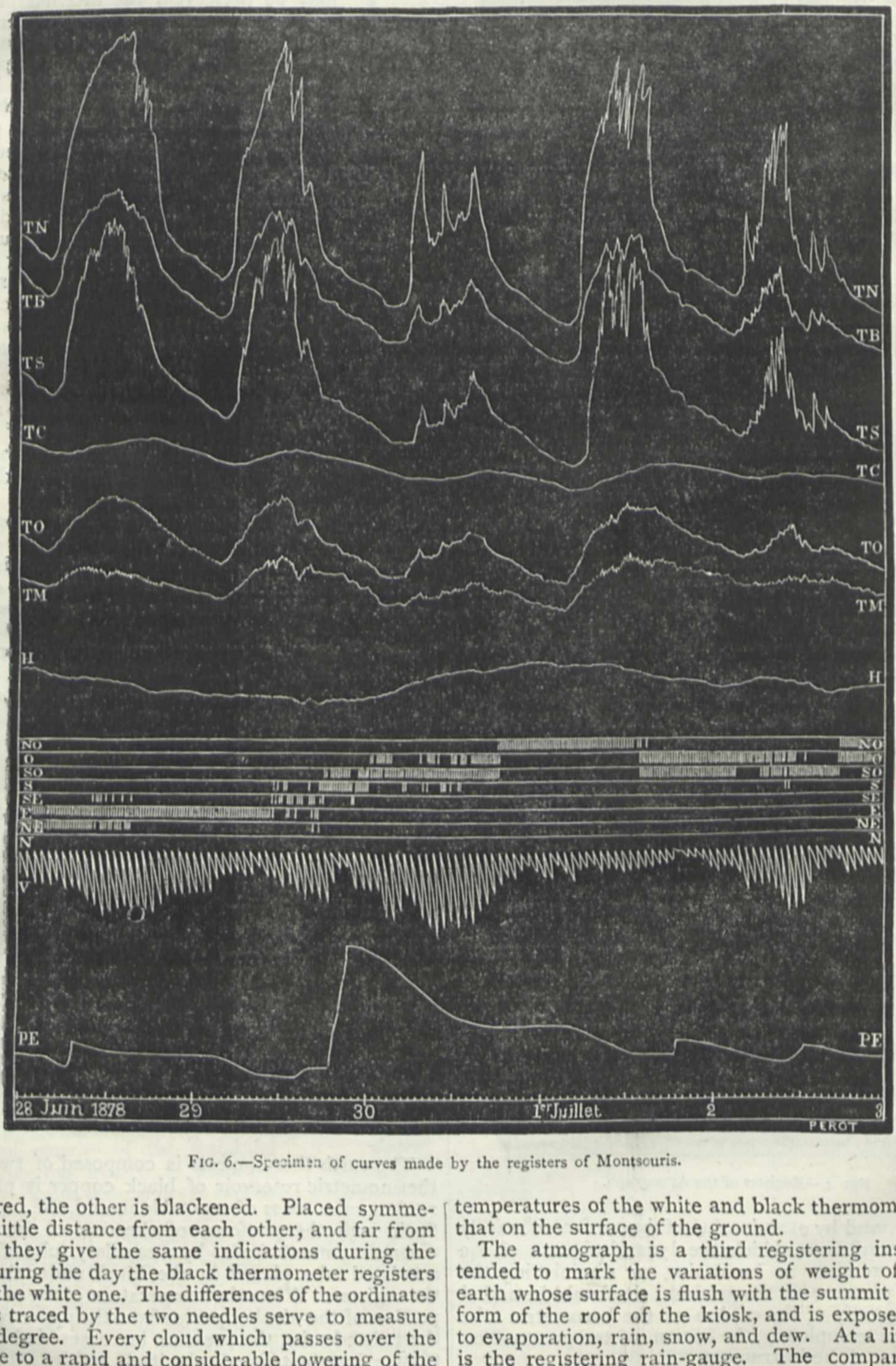


FIG. 6.—Specimen of curves made by the registers of Montsouris.

two curves shows what becomes of the water which falls on the naked earth, without vegetation, distinguishing between what returns to the atmosphere by evaporation, and what penetrates the subsoil which is permeable or drained. Another atmograph gives similar indications for a soil covered with various plants; but the latter being sheltered from the wind, ought to be moistened according as is necessary. Fig. 4 represents only the register of the atmograph. *a* is the lower part of the stem which is suspended to the extremity of the arm of the balance, in which is placed the mass of earth. A second lever arm, *b c*, follows and amplifies the movements of that stem which it inscribes on the vertical cylinder covered with paper blackened with smoke. This same stem bears a glass test-tube, *d*, containing mercury, in which is a fixed glass tube *e*. The diameter of this stem is so calculated that the point *c* traverses 100 millimetres for each millimetre of water gained or lost by the mass of earth. In calm weather we may thus appreciate the $\frac{1}{100}$th of a millimetre; but when the atmosphere is disturbed, the vertical components of the wind cause the needle to oscillate, thus interfering with the precision of the readings.

The anemograph (Fig. 5) gives us, at once, the direction of the wind and its mean rate per hour. Eight electro-magnets communicating electrically with the sectors arranged on the vane according to the eight principal points of the compass, can, by acting singly or two and two, record the winds for sixteen directions, which may be regarded to be sufficient for the wants of meteorology. A ninth electro-magnet is acted on each time that the Robinson drum shows a length of one kilometre traversed by the wind. The toothed wheel *b* then moves one division, and its movement is transmitted by the satellite wheel *d* to a third toothed wheel, *c*, on the axis of which is rolled a thread, *cp*. The point which marks the velocity then advances 1 mm. towards the left. This effect is produced during one hour for each kilometre traversed by the wind; but at the end of each hour the needle of the clock establishes an electric contact; the satellite wheel, *d*, is lowered; the wheel, *c*, becomes free; and the weight, *p*, restores the metallic point to its starting-place.

Fig. 6 presents a specimen of the curves traced by the registers from June 28 to July 3, 1878, reduced to one-third of their natural size. Beginning at the top, we find first the two curves, T N and T B, which together furnish the actinometric data; T N is the curve of the black thermometer, T B that of the silvered. The two following curves, T S and T C, give the temperature of the surface of the ground without shade; T S corresponds to the ground thermometer; T C gives the correction to be made in the ordinates of the first. The two curves, T O and T M, are those of the dry and wet thermometers; besides the temperature of the air in the shade, they give its hygrometric degree and the elastic force of its vapour. H is the curve of barometric pressure.

Underneath are eight straight lines corresponding to the eight principal directions of the wind; the vertical lines which rest on them indicate the directions in which the wind has blown. Further down are shown the velocity, V, of the wind in kilometres per hour.

The last curve, P E, is made by the atmometer; the increase in a vertical direction of this curve marks rain; the decrease marks evaporation. Notwithstanding the frequent and at one time very copious rains, the earth, on July 3, had very nearly resumed its weight of June 28. Finally, the last line is the datum-line on which the hours of the day are marked.

GERMAN PHYSIOLOGICAL CHEMISTRY[1]

As our general knowledge of nature increases, the possibility of individual knowledge decreases; the variety of discovery, the immense number of investigators, and the innumerable details which they accumulate in their respective branches of science, preclude the possibility of a modern "admirable Crichton." Werner's sigh, "True I know much, but yet I would know all," has been long acknowledged as an aspiration incapable of fulfilment, even supposing him to limit his desire of knowledge within the bounds of what is already known. To know "something of everything, and everything of something," is all that can be hoped for; day by day each science advances with such rapid strides, that one brain is incapable of grasping more than the general principles of one science; and any man who aims at enlarging the domain of science by fresh discovery, must content himself with confining his attention to a small corner, and by patient industry and indomitable perseverance seek to elicit some new facts.

Such, expressed in general terms, is the drift of the preface to Hoppe-Seyler's "Zeitschrift für physiologische Chemie." It seldom happens—unfortunately too seldom in this country—that medical men have more than a smattering of chemistry. A very low standard of chemistry is required for a medical degree, comprising a superficial knowledge of inorganic chemistry, chiefly of the non-metals; the merest smattering of organic chemistry, and ability sufficient to detect the acid and base in simple salts—such are the qualifications in chemistry necessary for graduation in medicine. When the student, following out the prescribed course, comes to attend lectures on physiology, and hears—almost always for the first time—the names of the various principles contained in the animal fluids, in the brain, in the liver, and in the muscular tissue, they fail to convey any definite idea to his mind, and he is utterly unable to comprehend, or even to form an idea of the reactions which take place in the animal organism. In Germany this state of things has been recognised in many universities, and special professors of physiological or animal chemistry have been appointed; these professors are not merely teachers, but are engaged in the active extension of their branch of science; and it is to facilitate the interchange of ideas between them, and to afford a medium in which the results of their investigations may be brought together, that Hoppe-Seyler has undertaken the editorship of this journal.

One noticeable feature of the investigations of German physiological chemists at present, is the attention devoted to ascertaining the changes which food undergoes in passing through the system. At least six memoirs on the subject are to be found in the nine published numbers of the journal, comprising the work of a year and a half. The methods and results of these experiments are worthy of a short description.

The food which we eat consists for the most part of carbon, hydrogen, and nitrogen; all food, however, does not contain nitrogen; starch, sugar, and fat are devoid of that element. The carbon and hydrogen, after being absorbed by the tissues, and performing work, are cast off as waste material, partly by the lungs, in the form of carbonic acid and water-vapour, and also, to a much smaller extent, in the urine. An almost inappreciable amount of nitrogen escapes by the lungs; by far the largest portion passes into the urine in combination with hydrogen and carbon, in the form of urea—a white crystalline body. Now this substance, urea, possesses a chemical, as well as a physiological interest. It was formerly supposed that all chemical compounds could be divided into two grand classes: inorganic bodies, such as could be prepared from purely mineral matter; and organic bodies, those existing only in a living organism, or obtained from these compounds by a process of decomposition. It was, therefore, imagined that an insurmountable barrier separated the two classes. Urea was ranked as an organic substance, for it had never been obtained except from the organism, till Prof. Wöhler, of

[1] From Hoppe-Seyler's "Zeitschrift über physiologische Chemie."

Göttingen, succeeded in preparing it from material of purely mineral origin. This process was to heat ammonium cyanate, $NH_4.CNO$, when, without decomposition, a new body was formed, possessing all the properties of, and undistinguishable from, urea. It has since been discovered that urea is very closely connected with the carbonates of ammonium, that, in fact, it is simply carbonate of ammonium minus water. Now it appears probable that urea is formed in the organism either from ammonium cyanate or from ammonium carbonate, and the question which Prof. Salowski, of Berlin, has tried to answer is: By which process is it formed? (vol. i. p. 1). As there is a general resemblance between all nitrogenous food, inasmuch as it contains albumen itself, or principles closely allied to albumen—among others, myosine, vitelline, serum-globuline (which form the subject of an article by Th. Weyl [vol. i. p. 72]); and moreover, as cyanic acid, carbonic acid, and ammonia, are products of decomposition of albumen, the question is an open one.

Every one knows the old plan of detecting the filcher of coin from a till, by placing a secret mark on a number of the coins, and so making their identity unmistakable. The plan adopted by Prof. Salowski is somewhat similar, though the simile is not quite applicable. There is a method of putting a private mark on cyanic acid and on ammonia, by using compounds containing more carbon in the former case, and by employing a substituted ammonia in the latter, that is, a substance possessing in the main the properties of ammonia, but capable of recognition afterwards. But as neither ammonia nor its salts are normal constituents of food, it was necessary to prove that by giving ammonia, a compound of nitrogen and hydrogen, along with food, the amount of urea in the urine is increased. Direct experiments on rabbits proved the point. After feeding them on a diet of potatoes containing a known amount of nitrogen, the amount of urea eliminated was augmented by addition of salts of ammonia to that diet. Now by introducing ammonia to form carbonate of ammonium, two atoms of nitrogen are introduced; and if that ammonia were "marked" the resulting urea would be "marked" also, and would contain two atoms of "marked" nitrogen. But, on the other hand, if the reaction takes place between cyanic acid (a body containing one atom of nitrogen itself) and ammonia, only one atom of nitrogen would be derived from the introduced ammonia, and if that ammonia could be afterwards identified, the urea into which it is resolved should contain only one atom of marked nitrogen.

In chemical language, let ethylamine be substituted for ammonia; in the first instance the equation should be—

$$C_2H_5NH_2 + HCNO = CO\left\{\begin{matrix} NH_2 \\ NH.C_2H_5 \end{matrix}\right. ;$$

and in the second—

$$CO(C_2H_5 . NH_2)_2 - 2H_2O = CO(NHC_2H_5)_2.$$

The former hypothesis was found to be true; only one nitrogen atom in the urea bore the mark, and the reaction is proved to take place by cyanic acid and ammonia combining, and then altering into urea, a body of the same composition but different properties, otherwise called an isomeride.

In spite, however, of this apparently convincing experiment, Prof. Salowski regards it as improbable that urea is actually formed by the reaction between ammonia and cyanic acid, unless, indeed, ammonia be supplied directly to the organism; he takes the view that under normal circumstances the urea is derived solely from cyanic acid assimilating water and evolving ammonia thus:—

$$2CNOH + H_2O = CO(NH_2)_2 + CO_2.$$

This would account for the increase of urea under a diet containing ammonia.

Dr. E. Baumann (vol. i. p. 60) contributes a paper announcing the discovery of phenol, or carbolic acid, in urine, and remarks that it is curious to observe a substance regarded as a preventive of fermentation generated, although in extremely small quantity, by fermentation. He also noticed the appearance of a nearly allied body, indicane, which, on allowing the urine to stand, changed into indigo, the well-known dye.

The colouring matter of the blood forms the subject of a series of contributions from the pen of the editor, Prof. Hoppe-Seyler (vol. i. pp. 121, ii. 149). Every one has noticed the fact that blood from an artery has a brighter red colour than that from a vein; the cause of this is that arterial blood is pumped by the heart from the lungs, where it has received a supply of oxygen from the air, into the arteries, which distribute it through the body. During its progress this oxygen is gradually used in oxidising waste matter and in converting the spent carbonaceous substances, which have fulfilled their purpose, into carbonic acid. The blood takes up this carbonic acid, and in doing so its colour becomes darker. The bright red principle of the blood is named oxyhæmoglobine, and after it has turned dark it has changed to hæmoglobine. Both of these substances can be separated from blood by appropriate processes. When placed in a tube and viewed through a spectroscope, oxyhæmoglobine exhibits two dark bands, whereas hæmoglobine shows only one band, occupying a position nearly between that of the two bands of oxyhæmoglobine, but slightly overlapping the one at the red end of the spectrum. These two substances can thus be easily distinguished from each other, and as the smallest trace of oxygen converts hæmoglobine into oxyhæmoglobine, the spectrum of which is easily recognised, even in presence of the other, a dilute solution of the former is a very delicate test for the presence of oxygen in liquids; so delicate, indeed, that one cubic millimeter of oxygen, or about as much as would occupy the space of a pin-head, can be detected.

It is of course evident that, as decay, and consequently removal of used-up material proceeds throughout the whole body, it is impossible to obtain blood, either wholly charged with oxygen or wholly free from it. Yet to be able to detect oxygen in such minute quantity, it is necessary to procure hæmoglobine absolutely free from oxygen. Hoppe-Seyler effects this by exposing the colouring-matter of blood to a putrefying medium; the small living organisms consume the oxygen that it contains, and reduce it to hæmoglobine. He remarks, *en passant*, that the colouring-matter of the blood withstands putrefaction perfectly, as well as the action of a special ferment found in the pancreas, which in this respect resembles bacteria, the living organisms in ordinary putrefying media.

This very delicate test for oxygen has been applied to various animal secretions. Saliva from the parotid and submaxillary glands, as might be expected from its proximity to external air, contains oxygen, but neither gall nor urine contain any, owing to the presence of easily oxidised substances named bilirubine and hydrobilirubine, by which any free oxygen is consumed.

It is not uncommon to hear of deaths caused by sleeping in an apartment in which there is a charcoal-stove. This is owing to the poisonous qualities of carbonic oxide gas produced by the combustion of the charcoal, and the effect is not improbably due to the formation of a compound between the hæmoglobine of the blood and the gas. Blood, when charged with carbonic oxide, acquires an almost vermilion-red colour. It also gives a characteristic spectrum, and its examination shows with certainty the cause of death. Hoppe-Seyler has noticed that, like oxyhæmoglobine, the compound of carbonic oxide with hæmoglobine is not destroyed by putrefaction, and hence blood taken from the veins long after death reveals the carbonic oxide spectrum, if death has resulted from charcoal-poisoning.

M. Dionys Szabó has made the character of the acid in gastric juice a subject of research (vol. i. p. 140), and has ascertained that it consists of both hydrochloric acid and lactic acid, analogous to that produced by the souring of milk. As hydrochloric acid is the more powerful corrosive and solvent agent, it is natural to expect it to be present in larger quantity than lactic acid in the stomach of the dog, which requires it to bring fragments of bone, &c., into solution. In certain dyspeptic cases hydrochloric acid is wanting, hence, probably, the dyspepsia, owing to lactic acid alone not being sufficient to bring the food into solution. It is probable that the lactic acid is produced from the albuminous constituents of food by oxidation, and that it acts on the salt which we take with our food, forming lactate of sodium, and liberating hydrochloric acid. Chemical decompositions of this nature, the converse of what happens in laboratory experiments, appear to be greatly favoured by dialysis through colloidal membranes, such as the walls of the ducts of the mucous membrane of the stomach.

Prof. Richard Maly (vol. i. p. 174) has made an attempt to explain the phenomenon of inverse chemical reactions, occurring under the influence of diffusion, with regard to the formation of hydrochloric acid. Prof. Graham, in his well-known researches on diffusion, showed that hydrochloric acid is the most diffusible of all liquids; that if a jar be filled with it, and carefully immersed in water, taking care not to mix the acid with the water, more hydrochloric acid escapes in a given time than is the case, under similar circumstances, with any other liquid. Now it is an ascertained fact that a weak acid can replace a strong one to a small extent, provided the weak acid is present in large quantity compared with the strong one. This replacement proceeds to a given point, when balance sets in, and the reaction goes no further, owing to the strong acid being liberated in such amount as to check any further decomposition, provided no disturbance takes place. But in the case of lactic and hydrochloric acids in the stomach, disturbance does take place, owing to the more rapid diffusibility of hydrochloric acid through the walls of the ducts. The hydrochloric acid is constantly being removed as it is formed, and the sodium chloride, or common salt, is continually in process of decomposition by the lactic acid. Hence the presence of hydrochloric acid in gastric juice. This decomposition is also effected by what is generally called "neutral sodium phosphate," which, although it has a faint alkaline reaction on litmus paper, yet, in a chemical point of view, is an acid substance, for it still contains hydrogen replaceable by a metal.

Dr. O. Lassar (vol. i. p. 165) contributes a paper on irrespirable gases. Every one who has visited a vitriol work knows the insufferable feeling of choking produced by the fumes of the evaporation-chamber, and even those who have not had that opportunity must occasionally have experienced the disagreeable sensation of breathing the fumes of burning sulphur from a sulphur match. This choking sensation seems not to be felt by animals; it is due to spasm in the glottis and involuntary contraction of the vocal chords. The object of Dr. Lassar's experiments was to ascertain whether such acid fumes are absorbed by the lungs, conveyed into the blood, and passed out by the urine. For this purpose he exposed rabbits and dogs to the dense fumes of sulphuric acid for more than an hour at a time, and examined the urine carefully for that acid. It was invariably absent, showing that the acid is not absorbed by the lungs. It was curious to remark that on exposure of an animal to nitric acid vapour of such strength that the hair, and even the membrane of the lungs, turned yellow, the animal did not suffer in health, and that the only effect of acid fumes in air is to diminish the proportion of oxygen. This explains what has often been wondered at—that workmen in the chlorine chambers and sulphuric acid evaporating-chambers are not injuriously affected by the acid fumes.

WILLIAM RAMSAY

GEOGRAPHICAL NOTES

THE special committee appointed by the International Meteorological Congress at Rome for the promotion of expeditions to the Arctic seas charged with making synchronous meteorological and magnetic observations, will meet at Hamburg on October 1 next, in order to arrange details and to discuss the means of arriving at the object aimed at. Preliminary steps in this direction have, as our readers are doubtless aware, been taken by Count Wilczek and Lieut. Weyprecht.

ON August 7 next a century will have elapsed since Karl Ritter, unquestionably the greatest geographer of his time, was born at Quedlinburg.

IN the last issue of the *Colonies and India* the attention of members of the Alpine Club is directed to the mountain peaks of the West Indies. In the Blue Mountains of Jamaica, for instance, views can be obtained which cannot be surpassed in the world. Many of these mountains have been, as yet, untrodden by the foot of man, and they offer a wide field to the student of natural history as well as the practical explorer. In the Island of Dominica, again, there are opportunities for exploring mountains which are hardly, if at all, known. An expedition under two Englishmen has lately scaled for the first time one of the peaks, known as Morne Trois Pitons, situated to the north of Roseau. The heights of these peaks are 4,528, 4,552, and 2,672 feet respectively. The foot of the centre *piton* was found to be at an elevation of about 1,800 feet. For a considerable distance the party were able to follow a wild-pig track, but they had to leave this and cut their way through dense vegetation and scrub. On reaching the summit they found it to be nearly flat, and covered with impenetrable vegetation. This curious plateau was estimated to be about ten acres in extent.

THE new number of the Belgian Geographical Society's *Bulletin* publishes reports from M. Cambier and Dr. Dutrieux on the march of the first Belgian African expedition from Mpwapwa to Tabora, in Unyanyembe. These are accompanied by a sketch-map of the country between the East Coast and Lake Tanganyika, on which the route of the expedition is laid down.

A GOOD harbour is stated to have been discovered near Point Parker, in the Gulf of Carpentaria, which will probably be of service in the development of that part of Australia.

THE Coreans are as little given to leaving their own country as the Japanese used to be, but we learn from a Shanghai contemporary that there appears to be a little colony of them forming in the neighbourhood of Chinkiang, for, in addition to the usual *ginseng* traders, there are now there several well-dressed Coreans having the appearance of the better class of officials. They wear slate-coloured garments as a sign of mourning for the Queen.

THE new number of *Le Globe* contains M. Veniukof's account of geographical work in Asiatic Russia during 1878, and a paper on the Sahara.

THE last *Bulletin* of the Société de Géographie Commerciale of Bordeaux has a long note on French establishments in India, which will be found useful in supplying information on a subject respecting which the world at large knows but little.

THE just published number of *Les Annales de l'Extrême Orient* contains the continuation of Dr. Harmand's notes on Khmer monuments, and of the Marquis de Croizier's essays on Indo-China, based on Dr. Bastian's investigations.

NOTES

PROF. TACCHINI has been appointed Director of the Central Office of Meteorology and of the Observatory of the Collegio Romano.

WE are requested to announce that during the months of August and September, the apartments of the Geological Society will be closed on Saturdays at 2 o'clock in the afternoon.

WE are glad to hear that there is a likelihood of the late Prof. Rankine's papers being republished in a collected form. Mr. W. J. Millar, of Glasgow, has been asked to act as editor, and is at present drawing up a list of papers which it might be advisable to reprint.

THE French Association for the Advancement of Science will hold its next session at Montpellier, as we announced some time ago. One of the two lectures will be on the "Electric Light," of which there will be a splendid display. The meeting of the Association will take place on August 28, so that the great heat will be no obstacle to the excursions, which will be attractive, as every effort will be made to take advantage of the exceptional position of the city. Montpellier being one of the oldest universities in the world, a number of historical relics will be exhibited for the first time to strangers, of high interest in the development of science.

THE meeting of the Swiss Association of Naturalists will take place at St. Gallen on August 10-12 next. The committee invites foreign naturalists to attend the meeting. The detailed programme may be had upon application to the secretary, Herr Stein, at St. Gallen.

DURING the last days of June shocks of earthquake of moderate intensity were felt on the south-east side of Mount Etna, particularly in the vicinity of the little town of Zafferana. The great crater at the time was sending forth a mighty thick black column of smoke. On the southern slope of the mountain the earthworks for the erection of the great astronomical observatory, for which Prof. Tacchini drew the plans, have been begun. The observatory will stand at an elevation of 3,000 metres above the sea; the largest telescope will measure 33 centimetres in diameter. The Commission appointed by the Italian Government to study the phenomena accompanying the recent eruption, hav proposed the establishment of a Cabinet for Vulcanology, of which Prof. O. Silvestri is to be the director.

THREE violent shocks of earthquake occurred at Athens on July 3 at 4.15 P.M. The centre of the oscillations seems to have been at Xylokastron, near Corinth. On Mount Parnassus and at Thebes the phenomenon was also noticed. At Athens another shock was felt on the 6th shortly after midnight.

MR. W. LANT CARPENTER describes in the *Bristol Mercury* a visit he recently paid to Mr. Edison's laboratory at Menlo Park. The laboratory, workshops, &c., he states, as well as some isolated buildings for delicate electrical measurements, are spread over an acre of ground, railed in, admission to which is only given to privileged visitors. While waiting for Mr. Edison, Mr. Carpenter was conducted by a messenger through all the physical, chemical, and private experimenting laboratories, and then through the workshops, the machinery of which is driven by a beautiful, high-pressure, eighty-horse engine, also used to drive the electric-light machinery, most of which is in the same shop. About a dozen workmen were engaged, some in electrical test measurements, &c., but chiefly in manufacturing Mr. Edison's latest form of telephone, constructed for the electric and hygrometric conditions of our English atmosphere. Mr. Edison admitted that he was not doing very much at present at the problem of domestic electric lighting. He appeared to consider the question of its economical subdivision a solved problem (he had sixteen lamps in the workshop, each with its small coil of platinum wire, in a glass globe, three to four inches diameter), and was now giving attention to the details of lamp construction. This new form of lamp is to be a minute cylinder of compressed pure zircon, a rare earth allied to magnesia, which is to be heated to whiteness by the surrounding coil of platinoiridium wire. A chemist was engaged in purifying zircon for this purpose. The newest thing in the shop was a dynamometer, the last and best of several invented by Mr. Edison, with the result of which he was perfectly satisfied, and he stated that with this instrument he had been able to show that, after deducting the necessary amount for friction in the machinery, more than 95 per cent. of the mechanical force employed was obtained in the form of light. Mr. Carpenter informs us that in consequence of a circular addressed by Mr. Edison to miners in Colorado (whence Mr. Carpenter has just returned) and elsewhere, platinum is being widely discovered in these districts, now attention is directed to it.

THE summer meeting of the Institution of Mechanical Engineers will be held in Glasgow, by the invitation of the Institution of Engineers and Shipbuilders in Scotland, commencing Tuesday, August 5. The following papers will be read and discussed:—"On the Pneumatic Marine Governor," by Mr. W. J. Dunlop, of Port Glasgow.—"On the 'Velometer' Governor," by Mr. F. W. Durham, of London.—"On the Maintenance of Constant Pressure in Water Service Pipes," by Mr. Stephen Alley, of Glasgow.—"On the Barton Water Regulator, or Reducing Valve," by Mr. W. H. Thomas, of London.—"On the Forging of Crank Shafts," by Mr. W. L. E. McLean, of Glasgow.—"On Water-pressure Engines with Variable Stroke," by Mr. John Hastie, of Greenock.—"On the Working of Traction Engines in India," by Mr. R. E. B. Crompton, of London.—"On the Construction and Working of a Vertical Action Steam Dredger," by Mr. R. B. Buckley, of the Public Works Department, Bengal.—"On Plate-rolling Machinery," by Mr. Edward Hutchinson, of Darlington.—"On Barton and West's Water Meter," by Mr. W. H. Thomas, of London. A number of interesting visits and excursions have been arranged.

WE are requested by the Committee of the Sunday Society to publish the following:—"Through the kindness of Sir Coutts Lindsay, the proprietor and director of the Grosvenor Gallery, the members of the Sunday Society have to-day (Sunday, July 27) been favoured with admission to the Summer Exhibition, between the hours of 6 and 8.30 P.M., and nearly 500 have availed themselves of the privilege. Next Sunday the Gallery will be opened to the public, free by tickets, which will be issued by the Society to all persons making written application and sending stamped and addressed envelope, for reply, to the Honorary Secretary, 19, Charing Cross, S.W. A special catalogue has been published by the Society, price 4*d.*; this must be obtained before Sunday, as no catalogues will then be on sale."

AN exhibition somewhat similar to the one we described at the Royal Aquarium, but on a larger scale, of plans, diagrams, models, apparatus, and all appliances illustrative of the various subjects that have to be considered in planning water-supplies, and the domestic arrangements for making them in the best way available, is to be held at the Alexandra Palace. The Exhibition will be arranged in fifteen section as follows:—Section I. Physics and Chemistry; Section II. Rainfall, &c.; Section III. Rivers; Section IV. Geology and Hydro-Geology; Section V. Waterworks; Section VI. Distribution of Water; Section VII. Statistics; Section VIII. Water Examination; Section IX. Filtration; Section X. Hardness; Section XI. Disease; Section XII. Antiquarian; Section XIII. Pollution; Section XIV.

Literature; Section XV. Few people really know anything of the various considerations that are involved in selecting and maintaining a "pure" and sufficient water-supply, and the spread of some of the knowledge that has been obtained may help to remove many of the prejudices against improvements, and may show that low water-rates are perhaps not the only points that ratepayers should regard. Sections X. and XI. especially refer to matters of general interest too often overlooked. It is hoped that a permanent museum, to be located somewhere in London, may result from this exhibition. All communications for the Committee should be made to Mr. A. T. Atchison, 34, Great George Street, S.W., and applications for space and for information respecting the exhibition should be made to Mr. W. H. Jones, Manager, Alexandra Palace, N.

THE new (German) Geological Society of Metz has just published its first *Jahresbericht* (for 1878). Apart from the usual report of proceedings at the meetings, the publication contain- an interesting paper by Capt. Schultzen on the old Roman aqueduct from Gorze to Metz.

THE death from apoplexy is announced of M. Louis Favre, the contractor for the St. Gothard Tunnel. He was 53 years of age, and had engaged in nine years to construct a tunnel nearly 15 kilomètres long through beds of granite, gneiss, and serpentine. Notwithstanding difficulties of all kinds, the work had been carried on interruptedly for six years, the estimate being so careful that there was no excess of outlay, and M. Favre expected in two years to complete the work.

WE have received from Mr. E. Paterson, of Bedford Court, Covent Garden, parts i. and ii. of an unusually well got up illustrated Catalogue of Electrical Apparatus manufactured by him. Not only is the Catalogue itself exceedingly full and drawn up with much intelligence, but in the second part some really useful practical information is given on the principles and fixing of electric signals, which, we daresay, will be welcome to many. We are sure many of our readers will find Mr. Paterson's Catalogue of considerable service.

THE Americans have stolen a march upon us in sanitary matters, as they have done in so many other things. In March of this year an Act passed the U.S. Congress organising a Board of Health to look after all matters relating to the public health, and to advise Congress what steps should be taken for the promotion of so all-important a matter. We have just received the first number of the weekly *National Board of Health Bulletin*, published by this body, who have 50,000 dollars appropriated to them for salaries and expenses. Besides mortality statistics the *Bulletin* contains a number of rules and regulations with respect to the sanitary condition of ships.

M. COULON, conservator of the Industrial Museum of Rouen, has discovered a new system for transforming sound into light, which phenomenon has been the subject of a lecture at the Salle des Capucines by M. Frank Geraldy with the Gower telephone. Two Geissler tubes are put in quick rotation on an axis. The Ruhmkorf coil of the first is worked by an ordinary interruptor and gives the deviation of a luminous cross. The interruptor of the second is replaced by a telephone. The figure presented by the second tube projects on the first one, which is coloured by uranoxide glass and exhibits the most rapid changes according to the height of the note delivered in the telephone-trumpet. The sensibility of the changes are startling and most interesting. An exhaustive lecture by M. Coulon will be given at Rouen in a few days, and the apparatus exhibited at the Palais de l'Industrie.

DR. MOESTA has discovered the remains of an oak forest at a depth of 7 feet or 8 feet, near Rotenburg. The number of well-preserved trunks is enormous; one of them is 18 mètres long and 1 mètre and a half in diameter. This bog oak will be us for carving.

THE twenty-fourth meeting of German and Austrian Apiculturists will be held at Prague on September 7-11, and will be accompanied by an international exhibition of living bees, and all products and apparatus connected with bee-culture.

THE second Congress of Austrian wine-growers will be held at Vienna on September 22-25, and will be accompanied by an exhibition of all products, apparatus, and implements connected with viticulture.

AT Roveredo a statue has been erected to the eminent philosopher and statesman, Antonio di Rosmini-Sarbati (born 1797, died 1855). The work was executed by a Florentine sculptor, and the material is Carrara marble. The statue is said to be an excellent likeness.

PHYLLOXERA is unfortunately making rapid progress in Savoy. Since July 1 no less than forty-two different vineyards in the cantons of Chambéry, Montmelard, Yenne, and Rochette have been attacked by the pernicious insect; the head-centre of the infection seems to be at Chambéry and at Montmelard.

HERR KARL RUSS, the eminent German ornithologist, is writing a new work on foreign domestic birds, in three volumes. Two are completed and part of the third has recently appeared. Herr Karl Rümpler, of Hanover, is the publisher.

THE Central School Depot publishes a clearly written and instructive penny tract—"A Few Interesting Facts about Light, simply Told, for Young People," by the Rev. F. J. C. Fenton. Mr. Fenton discards all technical terms, and conveys a wonderful amount of accurate elementary information in a few pages.

A JAPAN paper says that there is a prospect of a new coal mine being worked in Ishigari, in the island of Yezo, the resources of which are only now beginning to be gradually developed. Some foreign experts in the employ of the Kaitakushi, or Colonisation Department, which is doing good work in Yezo, have tested specimens of coal and found them to be of good quality and suitable for purposes of steam navigation. As the locality where the coal exists is at some distance from the coast, it is proposed to construct a railway from it to the sea-board, and a survey of the country for that purpose is now being proceeded with.

FROM a recent investigation of the electric arc by Professors Thomson and Houston (*Journ. of Frank. Inst.*) it appears that the relations between arc resistance and current strength and between current strength and illuminating power, are expressed by the following laws:—(1) In arcs of equal lengths the resistances are inversely proportional to the current strengths; (2) The illuminating power of an arc is approximately proportional to the current traversing it; (3) In arcs of equal length the total energy given out is proportional to the current strength.

INTELLIGENCE has been received from Ceylon that the examination of supposed pearl-oyster banks on the east coast has not resulted in the discovery of any deposits worth fishing. The oysters found, though of the pearl kind, contained no pearls. It is supposed that they had been washed by currents from deep-sea banks in the Bay of Bengal.

UNDER date of April 30, a correspondent of the *North China Herald* at Wladivostock, in Russian Manchuria, writes that, although the mountain tops were still covered with snow, the winter had at length come to an end, the ice in the harbour having on that day broken up and floated out to sea. The winter began in October and has been the severest known for many years. The bay was entirely covered with ice from two to three feet thick. Wladivostock is said to be improving, though slowly, and there are signs of an attempt at road-making.

THE *Japan Gazette* learns that as an encouragement to the producers of tea, silk, and cocoons, it has been resolved by the Government to hold an exhibition of these articles in the Machigwai-sho at Yokohama. Foreigners and native dealers are not to be allowed to exhibit, as the scheme is devised entirely for the benefit of producers and manufacturers.

GOLD-BEARING quartz has lately been discovered at Sarugoye in the Yamato district of Japan.

THE *Golos* reports the discovery in the district of Perejaslavl, Pultowa Government, of 370 flint arrow-heads, a number of bones of men and animals, fragments of earthenware, and bronze objects.

THE *Annual Report* of the Goole Scientific Society for 1878–9 records satisfactory progress. As the result of a rule admitting ladies, twenty-one have joined the society.

THE Hull Literary and Philosophical Society, to judge from the Report for 1878-9, seems to be doing a great variety of good work in their district by means of lectures, papers, classes, &c. The Society is in a flourishing condition as to members and funds, and has added, during the session, a Microscopical and a Geological Section.

THE additions to the Zoological Society's Gardens during the past week include two Red and Yellow Macaws (*Ara chloroptera*), a Red and Blue Macaw (*Ara macao*), a Blue and Yellow Macaw (*Ara ararauna*) from South America, a Common Trumpeter (*Psophia crepitans*) from Demerara, presented by Mr. Chas. Fricker; two Golden Eagles (*Aquila chrysaëtos*) from Scotland, presented by Mrs. A. H. Browne; a Geoffroy's Marmoset (*Midas geoffroii*) from Panama, purchased; a Stanley Crane (*Tetrapteryx paradisea*) from South Africa, received in exchange; a Bay Antelope (*Cephalophus dorsalis*) from West Africa, deposited; a Peacock Pheasant (*Polyplectron chinquis*), two Black-crested Cardinals (*Gubernatrix cristatella*), two Geoffroy's Doves (*Peristera geoffroii*), bred in the Gardens.

THE EOZÖON CANADENSE

WE have received the following communications on this subject:—

I shall be glad to be allowed to ask your readers to suspend their judgment in the matter of *Eozöon*, until the appearance of the *full and complete memoir*, based (I venture to say) upon investigations far more comprehensive than those of Prof. Moebius—upon which I am now engaged, in conjunction with my friend Prof. Dawson. But as the production of this memoir will necessarily be a work of considerable time, on account of the elaborate illustrations it will require, I would now offer the following brief remarks on that part of Prof. Moebius's discussion which relates to the so-called "canal-system."

Among the numerous beautiful figures which Prof. Moebius has given of *sections* of the "canal-system," there is not one which represents what I described and figured, when I last wrote on the subject (*Ann. of Nat. Hist.*, June, 1874, plate xix.), as "what appears to be the typical mode of its distribution." Nor is this brought out in any of the small number of figures which Prof. Moebius gives of the *internal casts* obtained by decalcification. Now to any one who will picture to himself how imperfect would be any conception he could form of the ramification of a *tree*, by taking a number of sections of its stem and system of branches through different planes, it must be obvious that "internal casts," in relief, when well preserved, will give a representation of the canal-system, which must be much truer than any conception can be that is based on a comparison of sections only; and that, in fact, it is only when the sections are interpreted by such solid models, that the real forms and relations of these "canal-systems" can be made out.

Having been kindly furnished by Prof. Dawson some months ago, with a large amount of new material, consisting of numerous specimens of *Eozöon*, obtained from different localities and in different states of mineralisation, I am now able to assert with confidence that the peculiar distribution described and figured by me from the actual specimens (one of which has been in Prof. Moebius's own possession) five years ago,[1] is the *regular and characteristic "canal-system" of Eozöon*. For my cabinet now contains *hundreds* of examples of it, both in transparent sections and in the solid models obtained by decalcification; and these last, in partially "dolomitised" specimens of *Eozöon*, show the following singular peculiarities, which do not seem to have fallen under Prof. Moebius's observation. When a band of dolomite runs through the calcite layers, (1) the "canal systems" in its neighbourhood are very commonly filled with *dolomite*, instead of with serpentine; (2) in one and the same canal-system, some of the branches are often filled with dolomite, and others with serpentine; while (3) individual branches are often partly filled with one mineral and partly with the other.

How these facts can be explained, except by the *pre-existence of a system of canals in the calcareous layers* into which these minerals have penetrated, I must confess myself unable to conceive; and that they thus afford demonstrative evidence of a structure which cannot be otherwise than *organic* is not merely my own opinion, but that of such accomplished petrologists as Prof. Geikie, who has been for some years engaged in the microscopic study of the metamorphic rocks of Scotland, and Prof. Bonney, who has been similarly studying the Cornish serpentines.

Whether, when taken in connection with the general structure of the organism, these "canal-systems" indicate its *Foraminiferal* affinities is, of course, an altogether different matter. To Prof. Moebius the difference seems greater than the resemblance; but it is noteworthy that his comparisons are limited to types examined by himself, and do not extend to *Calcarina*, in whose "canal-system" Dr. Dawson and I recognise the nearest approach to that of *Eozoön*. To myself, as to the late Prof. Max Schultze,[2] the resemblance seems greater than the difference. And as the several "canal-systems" of *Nummulina*, *Polystomella*, *Calcarina*, *Tinoporus*, and *Cycloclypeus* (all originally worked out by myself) *differ from each other* in plan, I cannot regard it as a valid argument *against* the foraminiferal affinities of *Eozöon* that its canal-system has a plan of its own. Surely Prof. Moebius would not deny the foraminiferal characters to any new *recent type* that the *Challenger* or any other collection may yield, if it should show a plan of canal-system different from any yet known, and approximating to that of *Eozöon*.

That in its *general plan of growth* (to which the distribution of the canal-system is intimately related) *Eozöon* differs from all recent *Foraminifera* at present known cannot be regarded as a proof of its *non*-foraminiferal character by any who have fully studied the very wide range of forms which that group comprehends, including the numerous indefinite "arenaceous" types, whose import is only now beginning to be understood by those who have the best opportunities of studying them.

I would suggest this further consideration: If we are to relegate to the mineral kingdom every supposed fossil that does not conform to any known existing type, we must expunge not only *Eozöon*, but *Stromatopora*—to say nothing of many other fossils whose place no one has yet been able to assign with certainty. Now, is Prof. Moebius prepared to say that *Stromatopora* is a "pseudomorph," because one zoologist thinks it a coral, another a sponge, and another a foraminifer? On his method of "differences" it is clearly neither one of these; and must, as he says of *Eozöon*, be either shut out of the animal kingdom altogether, or be made to constitute a sub-kingdom in itself. To myself it appears more philosophical to suppose that such "archaic" types combined in themselves characters which were afterwards specialised as those of distinct groups. And following this clue, I find in the chambered structure of *Eozöon*, and in its general relations to the canal-system traversing its calcareous layers, points of essential conformity to the group of *Foraminifera*, which seem to me far to outweigh the differences of detail by which Prof. Moebius has been led to the opposite conclusion.

I limit myself to this special point, because an excellent general criticism of Prof. Moebius's memoir, from the pen of Dr. Dawson, has already appeared in the *American Journal of Science*; and I hope that as you have given so much prominence to the views put forth by Prof. Moebius, you will do Dr. Dawson

[1] I think it rather hard that an *early diagram* of mine should be cited, and made the subject of adverse criticism, while those more recent *representations of actual structures* are ignored.

[2] I have been informed on good authority that Prof. Max Schultze left behind him for publication an elaborate and beautifully illustrated memoir on *Eozöon*, arguing for its foraminiferal character on account of the *resemblance* of its "canal-system" to that of existing types.

and myself the justice of placing before your readers his statement of objections to them, in which I fully concur.

Hereafter I think I shall be able to show that the "cumulative argument in favour of the organic character of *Eozöon* is as strong as that of the human origin of the "flint implements." Any one of the fractures that has given to these their characteristic forms, *might* have been accidental; and yet it is impossible to conceive that any number of such flints can have been so shaped "by accident." WILLIAM B. CARPENTER

London, July 28

The following is the communication from Principal Dawson referred to by Dr. Carpenter:—

Eozöon canadense has, since the first announcement of its discovery by Logan in 1859, attracted much attention, and has been very thoroughly investigated and discussed, and at present its organic character is generally admitted. Still its claims are ever and anon disputed, and as fast as one opponent is disposed of another appears. This is in great part due to the fact that so few scientific men are in a position fully to appreciate the evidence respecting it. Geologists and mineralogists look upon it with suspicion, partly on account of the great age and crystalline structure of the rocks in which it occurs, partly because it is associated with the protean and disputed mineral serpentine, which some regard as eruptive, some as metamorphic, some as pseudomorphic, while few have had enough experience to enable them to understand the difference between those serpentines which occur in limestones, and in such relations as to prove their contemporaneous deposition, and those which may have resulted from the hydration of olivine or similar changes. Only a few also have learned that *Eozöon* is only sometimes associated with serpentine, but that it occurs also mineralised with loganite, pyroxene, dolomite, or even earthy limestone, though the serpentinous specimens have attracted the most attention, owing to their beauty and abundance in certain localities. The biologists on the other hand, even those who are somewhat familiar with foraminiferal organisms, are little acquainted with the appearance of these when mineralised with silicates, traversed with minute mineral veins, faulted, crushed, and partly defaced, as is the case with most specimens of *Eozöon*. Nor are they willing to admit the possibility that these ancient organisms may have presented a much more generalised and less definite structure than their modern successors. Worse, perhaps, than all these, is the circumstance that dealers and injudicious amateurs have intervened, and have circulated specimens of *Eozöon*, in which the structure is too imperfectly preserved to admit of its recognition, or even mere fragments of serpentinous limestone, without any structure whatever. I have seen in the collections of dealers, and even in public museums, specimens labelled "*Eozöon canadense*" which have as little claim to that designation as a chip of limestone has to be called a coral or a crinoid.

The memoir of Prof. Moebius affords illustrations of some of these difficulties in the study of *Eozöon*. Prof. Moebius is a zoologist, a good microscopist, fairly acquainted with modern foraminifera, and a conscientious observer; but he has had no means of knowing the geological relations and mode of occurrence of *Eozöon*, and he has had access merely to a limited number of specimens mineralised with serpentine. These he has elaborately studied, and has made careful drawings of portions of their structures, and has described these with some degree of accuracy; and his memoir has been profusely illustrated with figures on a large scale. This, and the fact of the memoir appearing where it does, convey the impression of an exhaustive study of the subject, and since the conclusion is adverse to the organic character of *Eozöon*, this paper may be expected, in the opinion of many not fully acquainted with the evidence, to be regarded as a final decision against its animal nature. Yet, however commendable the researches of Moebius may be, when viewed as the studies of a naturalist desirous of satisfying himself on the evidence of the material he may have at command, they furnish only another illustration of partial and imperfect investigation, quite unreliable as a verdict on the questions in hand. The following considerations will serve to indicate the weak points of the memoir:—

1. A number of errors and omissions arise from want of study of the fossil *in situ*, and from want of acquaintance with its various states of preservation. Trivial errors of this kind are his referring to my photograph in Plate III. of the "Dawn of Life," as if it were natural size, and his stating that the larger specimens have fifty laminæ, whereas they often have more than a hundred. More important is his failing to appreciate aright the occurrence of *Eozöon* in certain layers of regularly bedded limestones, the rounded or club-shaped forms of the more perfect specimens, the manner in which the layers become confluent at the edges of the forms, as described by Sir W. E. Logan and myself, or the amount of crushing and fracture which most of the specimens exhibit. Thus he fails to convey any adequate idea of the Stromatoporoid forms and mode of occurrence of the organism, or indeed of its general character and probable mode of growth. Further, he treats it from the first as a mere laminated aggregate of calcite and serpentine, without reference to its occurrence in any other state, and also without reference to the fragmental limestones in part made up of its remains. He objects strongly to the want of definiteness of form and distribution in the chambers and connecting passages, without making allowance for defects of preservation, or mentioning the similar want of defined form in some *Stromatoporæ*. He admits, however, that the modern *Carpenteria* and its allies are in some respects equally indefinite. He further objects to the impossibility of detecting regular primary chambers like those in modern foraminifera, but seems not to be aware that, as I have recently shown, some *Stromatoporæ* originate in a vesicular, irregular mass of cells, and that in *Loftusia*, both the eocene *L. Persica* and the carboniferous *L. Columbiana*, the primary chamber is represented by a merely cancellated nucleus.[1]

2. With reference to the finely tubulated proper wall of *Eozöon*, he has fallen into an error scarcely excusable in an observer of his experience, except on the plea of insufficient access to specimens. He confounds the proper wall with the chrysotile veins traversing many of the specimens, and obviously more recent than the bodies whose fissures they fill. That he does so is apparent from his stating that the proper wall structure sometimes crosses the bands of serpentine and calcite, and also that it presents a series of parallel four-sided prisms, whereas, when at all perfectly preserved, it shows a series of cylindrical threads penetrating a calcite wall. That some of his specimens have contained the proper wall fairly preserved is obvious from his own figures, in which it is possible to recognise both this structure and chrysotile veins, though confounded by him under the same designation. He objects, somewhat naïvely, that many of the chambers fail to exhibit this nummuline wall, and that it sometimes presents a ragged appearance or is altogether opaque. In point of fact it can appear distinctly, either in decalcified specimens or in slices, only when the minute tubes are filled with some substance optically distinguishable from calcite, or not acted on by dilute acid. When the proper wall is merely calcareous (and I have specimens showing that it is often in this state, and without any serpentine in its pores), its structure is ordinarily invisible, and it is the same when the calcareous skeleton has from any cause lost its transparency or has been replaced by some other mineral substance. Even in thickish slices, the tubes, though filled with serpentine, may be so piled on one another as to be indistinct. All this may be seen in tertiary *Nummulites*. When wholly calcareous their tubulation is often quite invisible, and when imperfectly injected with glauconite or other silicates, they often present a very irregular appearance. If Prof. Moebius will study the Nummulites injected with glauconite from Kempten,[2] Bavaria, in addition to the casts of *Polystomella* from the Ægean, to which he refers, he will be better able to appreciate these points. It may be worth repeating here that, in examining the original specimens of *Eozöon*, I did not recognise the proper wall. I did not doubt that it must have existed in some form, since I could easily detect the canals in the supplemental skeleton; but I did not wonder at its non-appearance, knowing the chances against its preservation in a recognisable form. Its discovery was due to the subsequent investigations of Dr. Carpenter.[3]

3. To the canal system, Prof. Moebius does more justice and admits its great resemblance to the forms of this structure, in modern *Foraminifera*. This indeed appears from his own figures, which well show how wonderfully this structure has

[1] See *Journal* of London Geol. Soc., January, 1878.

[2] I am indebted to Mr. Otto Hahn for specimens of these most interesting fossils.

[3] It may deserve mention here that the carboniferous *Fusulina* very rarely shows it tubulated wall, and that Dr. Carpenter had maintained its Nummuline affinities before he obtained specimens showing this particular structure. Structures so delicate as these are indeed only preserved exceptionally in fossil specimens.

been preserved, and how nearly it resembles the similar parts of modern *Foraminifera*. He thinks, however, that these round and regularly branching forms are rather exceptional, which is a mistake; though it is true that the sections of the larger canals are often somewhat flattened, and that they become flat where they branch. They are also sometimes altered by the vicinity of veinlets or fractures, or by minute mineral segregations in the surrounding calcite, accidents to which all similar structures in fossils are liable. Another objection, not original with him, is derived from their unequal dimensions. It is true that they are very unequal in size, but there is some definiteness about this. They are larger in the thicker and earlier formed layers, smaller or even wanting in the thinner and more superficial. In some slices the thicker trunks only are preserved, the slender branches having been filled with dolomite or calcite. It is difficult, also, to obtain, in any slice or any surface, the whole of a group of canals.[1] Further, as I have shown, the thick canals sometimes give off groups of very minute tubes from their sides, so that the coarser and finer canals appear intermixed. These appearances are by no means at variance with what we know in other organic structures. Another objection is taken to the direction of the canals, as not being transverse to the laminæ but oblique. This, however, may be dismissed, since Moebius has of course to admit that it is not unusual in modern *Foraminifera*. It may be added that some of the appearances which puzzled Moebius, and which are represented in his figures, evidently arise from fractures displacing parts of groups of canals, and from the apparently sudden truncation of these at points where the serpentine filling gives place to calcite. It would also have been well if he had studied the canal systems of those *Stromatoporæ* which have a secondary or supplemental skeleton, as *Cænostroma* and *Caunopora*.

4. A fatal defect in the mode of treatment pursued by Moebius is that he regards each of the structures separately, and does not sufficiently consider their cumulative force when taken together. In this aspect, the case of *Eozöon* may be presented thus: (1) It occurs in certain layers of widely distributed limestones, evidently of aqueous origin, and on other grounds presumably organic. (2) Its general form, lamination, and chambers, resemble those of the silurian *Stromatopora* and its allies, and of such modern sessile foraminifera as *Carpenteria* and *Polytrema*. (3) It shows under the microscope a tubulated proper wall similar to that of the Nummulites, though of even finer texture. (4) It shows also in the thicker layers a secondary or supplemental skeleton with canals. (5) These forms appear more or less perfectly in specimens mineralised with very different substances. (6) The structures of *Eozöon* are of such generalised character as might be expected in a very early Protozoan. (7) It has been found in various parts of the world under very similar forms, and in beds approximately of the same geological horizon. (8) It may be added, though perhaps not as an argument, that the discovery of *Eozöon* affords a rational mode of explaining the immense development of limestones in the Laurentian age; and on the other hand that the various attempts which have been made to account for the structures of *Eozöon* on other hypotheses than that of organic origin have not been satisfactory to chemists or mineralogists, as Dr. Hunt has very well shown.

Prof. Moebius, in summing up the evidence, hints that Dr. Carpenter and myself have leaned to a subjective treatment of *Eozöon*, representing its structure in a somewhat idealised manner. In answer to this it is necessary only to say that we have given photographs, nature-prints, and camera-tracings of specimens actually in our possession. We have not thought it desirable to figure the most imperfect or badly preserved specimens, though we have taken pains to explain the nature and causes of such defects. Of course, when attempts at restoration have been made, these must be taken as to some extent conjectural; but so far as these have been attempted they have consisted merely in the effort to eliminate the accidental conditions of fossilised bodies, and to present the organism in its original perfection. Such restorations are not to be taken as evidence, but only as illustrations to enable the facts to be more easily understood. It is to be observed, however, that in the study of such fossils as *Eozöon*, the observer must expect that only a small proportion of his specimens will show the structures with any approach to perfection, and that comparison of many specimens prepared in different ways may be necessary in order to understand any particular feature. A single figure or a short description may thus represent the results of days spent in the field in collecting, of careful examination and selection of the specimens, of the cutting of many slices in different directions, and of much study of these with different powers and modes of illumination. My own collection contains hundreds of preparations of *Eozöon*, each of which represents perhaps hours of labour and study, and each of which throws some light more or less important on some feature of structure. The results of labour of this kind are unfortunately very liable to be regarded as subjective rather than objective by those who arrive at conclusions in easier ways.

Taken with the above cautions and explanations, the memoir of Prof. Moebius may be regarded as an interesting and useful illustration of the structures of *Eozöon*, though from a point of view somewhat too limited to be wholly satisfactory.

[1] I have succeeded best in this by etching the surface of broken specimens.

THE COLOURS OF DOUBLE STARS

IN a recent number of the *Bulletin de l'Académie royale de Belgique*, M. Niesten, of the Brussels Observatory, has published some interesting details relating to the colours of double stars, to which subject he has given special attention for a considerable time past. When comparing the periodicity of solar spots with the longitudes of planets in the ecliptic, Messrs. De la Rue, Balfour Stewart, and Loewy had found that a distinct connection exists between solar activity and the relative positions of the different members of our planetary system. A long time ago the attention of astronomers had already been drawn to the fact that Wolf's sun-spot period of eleven years coincides with the period of Jupiter's revolution round the sun. Later on Prof. Balfour Stewart pointed out that the coincidence of the perihelia of Jupiter and Saturn, which occurs about every fifty-nine years, corresponds to another one of Wolf's spot-periods.

If, therefore, the relative positions of the planets with regard to the sun have some influence upon the activity of that luminary, the question is justifiable whether on the other hand the influence of the sun upon the planets might not be apparent through some slight changes in their colour. There is no doubt that the colours of the planets actually do change; their brightness increases and decreases according to their position near the perihelion or aphelion. In the case of Jupiter changes of colour have been repeatedly observed, and they seem to coincide with the sun-spot periods. At the last opposition of Mars, when the planet was near its perihelion, it seemed to be less ruddy than usual; Uranus, which was generally described as shining with pale bluish light, is now, when it is approaching its perihelion, remarkable by its bright white lustre.

These relations between the sun and the planets induced M. Niesten to search for similar relations among the double stars, and specially to try to answer the question whether the changes of colour which have taken place in several double-star systems are in any way connected with the relative position of the components of a double star. For this purpose he collected the observations of astronomers who have given special attention to the colours of stars, and catalogued the colours of the stars visible in our horizon. It was found that many double-stars have not changed in colour since they were first observed, while others in a period of more or less considerable duration have shown a series of changes of colour, which seem to follow a definite law. The changes of colour were particularly remarkable in those double stars which possess great velocity of revolution. M. Niesten gives a table in which the different colours of twenty double stars of known period and periastrium, *i.e.*, the colours of the principal star and of the companion, as observed at different periods, are compiled. From M. Niesten's discussion of the facts revealed by this table, we will give that relating to the first two double-star systems by way of example.

In 70 *p* Ophiuchi, the period of revolution of which is 94·37 years, and for which the periastrium occurred in 1807, the colour of the principal star at Herschel's time (an epoch closely preceding the periastrium) was white; the star then changed in colour, passing from white, through yellow and pale topaz-coloured to golden yellow, reaching this tint about 1854. From this period it showed a tendency to return to white, passing through yellow and pale yellow. In 1877 Mr. Pritchard designated it as pale yellow, and afterwards as white. The companion during its revolution showed similar fluctuations of colour to those of the principal star. In the vicinity of the periastrium Herschel put it down as reddish (we must remember here that Herschel's speculum gave a slight reddish tint to all objects); now the companion is bluish white, and between these two epochs it showed more saturated colours.

The short period of ζ Herculis (34·32 years) allows us to consider the changes of colours during two revolutions. Herschel measured this system about the epoch when the companion was very near to the primary; the latter was white, the former ash-coloured. At the periastrium of 1860 Mr. Knott saw them pale yellow and greenish respectively; at other epochs the colours of the two components are all the more marked the further they are away from the periastrium, the companion always showing warmer tints than the principal star. About the epoch of the apparent periastrium M. Dembowsky designated them as yellow and olive coloured. Similar phenomena are shown by the other double stars given in the table, all of which are systems with closed, *i.e.*, elliptical orbits. In the double star 61 Cygni, where no closed orbit has been observed, but where the small companion moves in a straight line relatively to the larger one, the same yellow tint has been observed from 1828 to 1873.

In the cases of optical double-star systems, *i.e.*, those which only accidentally happen to be in the same line of sight, and which show a rectilinear motion, the principal star is generally yellow and the companion blue. In Mr. Brothers' catalogue, which comprises 105 double-star systems, with closed orbits, there are only thirty-two where the companion is blue, while all the others show the same colour as the principal star. And even these thirty-two systems may possibly be optical ones, in which the companion is almost exclusively blue. The absence of blue in the companions of double stars of short period is most remarkable.

The blue colour of the companions in the optical systems is not an effect of contrast to the yellow colour of the primaries, for the former are blue even if the latter are shut out of the field of vision. It seems possible that, similar to the effect in our atmosphere, where distant objects assume a bluish tint, celestial bodies which send us their rays of light from the most distant regions, may appear blue on account of the thickness of the medium through which the light passes.

M. Niesten has compiled a table of double stars with blue companions, and has arranged them according to their position in declination and right ascension. From this it appears that these double stars are principally situated in a zone extending from decl. 10° S. to 40° N., and further that there are two maxima of occurrence, one in R.A., 4h.–6h. and the other in R.A. 18h.–20h.; the first maximum is near the equator, the other between Decl. 30° and 40° N.; the former is therefore in the constellation of Orion, the latter in Cygnus and Lyra. According to Sestini, the single blue stars occur in the same parts of the heavens.

The conclusions we may draw from M. Niesten's researches are as follows:—

1. In systems with well-established orbits, and particularly in those of short period, the two components generally have the same yellow or white colours.

2. In systems of which we possess sufficiently numerous records of the colours of the components to enable us to perceive a relation between the tints and the relative positions of primary and companion, the former is white or pale yellow when the latter is in its periastrium, while in other positions the primary is yellow, golden yellow, or orange.

3. In these systems the companion follows the colour-fluctuations of the principal star, and frequently surpasses the latter in intensity of tint the further it moves away from the periastrium, at which point in most cases its light is white, like that of the primary.

4. An equality of tints of primary and companion is found in systems with rectilinear motion, as well as in those with closed orbits and long periods.

5. In optical groups the companion is generally blue.

These remarks are, of course, founded upon observations made by different observers, and the records of colour may thus suffer from personal influences; but in many cases one and the same observer recorded the colours of the components of a system as yellow during a series of years, and then he saw them grow paler and turn white. In other instances all astronomers agree that a certain companion is blue.

When more careful attention has been given to the question of colours, both in measuring double stars as well as in investigations of the physical condition of planets, then it will be possible, perhaps, to draw a great many more conclusions, and such to which greater probability attaches, than was in the power of M. Niesten, with the comparatively small number of observations at his command.

At present it is supposed that the fluctuations of colour in stars are caused by changes in the composition of their incandescent gaseous envelopes; these changes must in turn be only effects of another cause producing them; M. Niesten does not think it impossible that in the case of double stars this cause might lie in the relative position of the components.

SCIENTIFIC SERIALS

American Chemical Journal, vol. i., Nos. 2 and 3, present a good array of contributions from different American universities, making in all, with reviews and reports, about 215 pages. Under inorganic chemistry is to be found a description of very slightly modified methods of nitrogen and phosphorus estimation adapted to agricultural products, by Johnson and Jenkins; and a series of analyses of gummite and other uranium minerals from North Carolina, by E. Genth, &c. Among the contributions to organic chemistry is a long paper by Remsden and Iles on the oxidation of substitution products of aromatic hydrocarbons, continued from No. 1. In the first portion the authors describe solid orthokresol from their oxytoluic acid, and they further conclude from their experiments that the presence of a sulphamine group acts protectively towards a methyl group in a substituted aromatic compound submitted to oxidation. A full abstract of this and another interesting paper by Remsden and Morse on oxidation of bromparaethyltoluene, and researches on substituted benzyl compounds, by Jackson, cannot be given in our space. Thorpe on heptane has appeared elsewhere. The remaining communications are of minor interest.

Bulletin de la Académie Royale des Sciences (*de Belgique*), No. 5.—The yellow substance obtained when tetrathionic acid is poured into a solution of mercurous nitrate in water has been proved by M. Spring to be a *trithiobasic sulphate of mercury*. This substance showed some unexpected chemical properties, which M. Spring describes, and he has succeeded in forming some other new bodies similar to it, so as to complete the list of basic sulphates of mercury sufficiently for an attempt at classification of these substances.—The recent terrible catastrophe at the Agrappe coal-pit is the occasion of two communications by M. Cornet and M. Melsens, the former remarking especially on the influence of depth on the instantaneous irruption of fire-damp, and the proportion of that gas met with. (The fire-damp of the Agrappe pit, which ignited at the mouth of the pit, came from 610 metres depth, where a new gallery was being made.)—M. Renait's paper on the distinctive characters of dolomite and calcite in rocks of the carboniferous limestone of Belgium is elsewhere noticed.—There are also here two notes on Belgian minerals.

THE *Rivista Scientifico-Industriale*, Nos. 11-13, contain the following papers of importance:—Researches on the electric conducting power of carbons, by Prof. Rinaldo Ferrini.—On some new applications of the potential energy of liquid surfaces, by G. Van der Mensbrugghe, discussed by Prof. C. Marangoni.—On a telephonic microphone, by Prof. G. Cantoni.—On the endosmose of liquids and on an apparatus for filling endosmometers, by Prof. C. Marangoni.—On the mutual dependence of simple bodies, by P. Provenzali.—On some prehistoric discoveries made at Ostiano, by Dr. Ciro Chistoni.—On a new saccharometer or polarimeter, by M. Laurent.—On the kinzigite of Calabria, by Domenico Lovisato.—On the determining causes of the sexuality of *Cannabis sativa*, by Prof. P. A. Saccardo.—On the constitution of fog and clouds, by Prof. Fernando Palagi.—On a new burner for monochromatic light.—On the phenomena which accompany the electrolysis of metallic compounds, by Prof. Giuseppe Basso.—Crystallographical, optical, and chemical researches on certain minerals, by Prof. Giuseppe Grattarola.—On a new method to determine the melting-point of organic substances, by Prof. Giorgio Roster.

THE *Revue Internationale des Sciences* (June and July).—From these parts we note the following papers:—Analysis of Prof. Ernst Haeckel's treatise, "Monogenetic and Polygenetic Origin of the three Organic Kingdoms and of the Organs," by Jules Soury.—Description of the scientific balloon ascent of October 31, 1878, and remarks on the exploration of great aërial heights, by Louis Tridon.—On the Diatomaceæ of the mouth of the Seine, by M. Manoury.—On the secreting and trophic nerves of glands, by R. Heidenhain.—On the colouring-matter of urine, by M. Masson.—On the beginnings of art, by Dr. Johannes Ranke.—On the circulation of gases and some phenomena of gaseous thermo-diffusion in plants, by J. L. de Lanessan.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xii. fasc. xiii.—We note here the following:—Anatomical and statistical note on cirrhosis of the liver, by Prof. Sangalli.—On albinism in batrachians, by Prof. Pavesi.—On polar systems (continued), by Prof. Jung.—On monodromic functions having a characteristic equation, by S. Pincherle.—Rapid preparation of hydroxylamine, by Dr. Bertoni.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 21.—M. Daubrée in the chair.—The following papers were read:—Theory of the simple pendulum with conical oscillations, regard being had to the rotation of the earth, by M. Villarceau. He concludes that other causes than gravity and the earth's rotation intervene.—Various thermo-chemical data, by M. Berthelot. This relates to formation of diamylene, and heat of fusion and specific heat of glycerine.—Remarks on the last communication of M. Bouquet de la Grye, by M. Ledieu. He doubts the possibility of determining, even approximately, the influence of the sun and the moon on atmospheric pressure.—The last three epidemics of plague of the Caucasus, studied with regard to epidemiology and prophylaxy, by M. Tholozan. These epidemics appeared in 1804-18, 1828-30, and 1840-43. He remarks on the inefficacy of means expected to be effectual for staying the progress of the disease.—M. Schwann was elected correspondent in Medicine and Surgery, in place of M. Rokitanski.—Astronomical observations and measurement of an arc of parallel in Algeria, by M. Perrier. The arc embraces 10°. The triangulation is destined to serve for basis and control of the vast system covering Europe.—Anæsthesia by means of protoxide of nitrogen mixed with oxygen, and employed under pressure, by M. Bert. One operation, by M. Labbé, is described, and sixteen, by M. Pean, referred to. The superiority of the method is chiefly shown in the instantaneousness of the sleep and awaking. There is hardly any nausea. The excess of pressure varied between 0·15 m. and 0·22 m.—Researches on the causes of reinvasion of phylloxerised vineyards, by M. Boiteau.—M. Colladon announced the death of M. Favre, contractor of the Saint Gothard tunnel.—Discovery of a small planet by Mr. Peters at Clinton (N.Y.), on July 17, 1879.—On a generalisation of periodic functions and on certain linear differential equations, by M. Picard.—Hydrodynamic experiments with vibrating bodies, and imitation, in an inverse sense, of the forces of electricity and magnetism, by M. Bjerknes. He describes a modified form of his former apparatus.—On a phenomenon similar to that of Peltier, by M. Bouty. This relates to the experiment with metallised thermometers as electrodes, referred to elsewhere.—On the capacity of voltaic polarisation, by M. Blondlot. The following law is deduced from observations:—For a given electrode and a given electrolyte the initial capacity does not depend on the direction of polarisation.—Action of magnetism in motion on static electricity, by M. Lippmann. This action results rigorously from the existence of the inverse phenomenon, which Mr. Rowland's experiments have demonstrated; and this reversibility is a consequence of the impossibility of perpetual motion. Further, static electricity has a proper mechanical inertia, simply added to that of the electrified body.—On the laws of variations of atmospheric electricity deduced from regular observations made at the Moncalieri Observatory, by M. Denza. *Inter alia*, there are two principal daily maxima, a few hours after sunrise and sunset. The monthly electric tension reaches a maximum in February, a minimum in September. In twelve years negative electricity appeared with rain and snow, at least in 50 per cent. of the cases. The electric tension generally diminishes with the altitude.—Researches on explosive substances, by MM. Nobel and Abel.—Experimental researches on decomposition of gun-cotton in a closed vessel, by MM. Sarrau and Vieille. The pressure, heat, and volume and composition of the gases, are indicated. The latter are simple and few.—On the employment of sulphuretted hydrogen by the dry method in analyses, by M. Carnot. This mode of sulphuration has advantages, in many cases, over that of fusion with sulphur.—On the transformation of hydrocellulose into pulverulent pyroxyles, by M. Girard.—Action of fluoride of boron on acetone, by M. Landolph.—On the determination of urea, by M. Mehn.—On iron reduced by hydrogen, by M. Moissan.—Electric excitation of the point of the heart, by MM. Dastre and Morat. A series of closely successive currents may have on the heart the effect of a continuous current.—Note on the physiological action of bromhydrate of conine, by M. Prevost.—On the biliary secretion, by M. Picard. There are two differences between this and the renal secretion:—(1) An arterial system furnishes urine, a venous system bile; (2) In the biliary secretion certain substances formed in the liver are carried away by the outward movement of the liquid.—Action of the principal poisons on crustacea, by M. Yung. Strychnine and nicotine act with extreme violence; curare is less active than with vertebrates; sulphate of atropine never caused death; digitaline renders the heart's movements slower, &c.

VIENNA

Imperial Academy of Sciences, May 8.—The following among other papers were read:—Old and new methods of solving differential equations by simple determinate integrals, by Prof. Winckler.—Researches on liverworts (Riccieæ), by Prof. Leitgeb.—Determination of path of two fireballs observed on January 12 in Bohemia and neighbouring regions, by Prof. Niessl.—On the employment of quarter-tones in music on the doubly chromatic piano, by Herr Gruss.—New conchylia from Mediterranean strata, by Dr. Hilber.—Diluvial land-snails from Greece, by the same.—On the *rôle* of the *Ligamentumiridis pectinatum*, by Dr. Biggs.—On compounds from animal tar, I. Picolin, by Prof. Barth.—On the internal friction in a mixture of carbonic acid and hydrogen, by Dr. Puluj.

May 15.—On the electro-magnetic rotation of the plane of polarisation of light in air, by Prof. Lippich.—On arsenate of zinc and cadmium, by Herr Demel.—On two peculiar surfaces of the sixth order, and on a certain group of curves of the third and fourth order, by Herr Cantor.—On tertiary fossils brought by Dr. Tietze from Persia, by Herr Fuchs.

May 23.—On the formation of cinchomeronic acid from chinine, and its identity with a pyridindicarbonic acid, by Dr. Weidel and Herr Schmidt.—Action of oxalic acid on carbazol, by Dr. Sinda.—On the decomposition of sulphydantoin by baryta hydrate, and on a peculiar iron-reaction of thioglycolic acid, by Herr Andreasch.—On bromoxyl derivatives of benzol, by Dr. Benedikt.—On the fossil fauna of the Vypustek cave in Moravia, by Prof. Liebe.

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