THURSDAY, FEBRUARY 24, 1876

REPORT OF THE VIVISECTION COMMISSION

NO one who examines this voluminous Report of the Commission appointed some months ago to inquire into "the practice of subjecting live animals to experiments for scientific purposes," can deny that the Commissioners have done their work thoroughly, as they have done it without delay. The Commissioners evidently entered upon their important inquiry with the determination of discovering the whole truth as to the practice of vivisection, and of eliciting the opinions and reasons not only of its advocates but of its most determined opponents. A mere glance at the long list of names of the witnesses will serve to assure anyone that the evidence which has been obtained is the expression of the most weighty and trustworthy opinion on both sides, and both the advocates and opponents of the practice must feel relieved that the Commission was appointed and has done its work: the former will be glad that the true condition of things is now before the public, that the worst, so to speak, is known, and the latter that they now know definitely what they have to contend against.

The bulk of the blue-book is of course occupied by the evidence. Prefixed to this is the Report and recommendations of the Commissioners, and in the Appendices are contained various documents which throw light upon the inquiry and enable the reader to learn exactly what has been done in the matter up to the present time, and what will be the starting-point of Parliament in discussing the Report. We need do little more here than lay the Commissioners' recommendations before our readers. In their Report they candidly discuss the varied methods and uses of experimenting on living animals, the bearings of the evidence elicited during the inquiry, as well as every possible suggestion as to what legislative action ought to be taken in reference to the matter. As might be expected from the character of the men who compose the Commission, every point of importance is brought prominently out, the subject is looked at all round, and from every point of view. Their recommendations, therefore, which they give after briefly referring to the two Bills of last session, will without doubt have great weight with all who take an interest in the matter. They are as follows:—

"What we should humbly recommend to Your Majesty would be the enactment of a law by which experiments upon living animals, whether for original research or for demonstration, should be placed under the control of the Secretary of State, who should have power to grant licences to persons, and, when satisfied of the propriety of doing so, to withdraw them. No other persons should be permitted to perform experiments. The holders of licences should be bound by conditions, and breach of the conditions should entail the liability to forfeiture of the licence; the object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided. This should be the general scope of the conditions; but their detailed application should be left to be modified from time to time by the minister responsible according to the dictates of experience. In the administration of the system generally, the responsible minister would of course be guided by the opinion of advisers of competent knowledge and experience. Dr. Playfair's bill provided a machinery for the purpose, and some arrangements of the kind proposed in that measure would be necessary. But we think it is inexpedient to divide the responsibility of the Secretary of State with that of any other persons by statutory enactment, and we recommend that his advisers should be from time to time selected and nominated by himself. Their names should be made known to the profession and the public. It may be found desirable that one of the conditions to be attached to a licence should be that the experiments should be performed in some particular place; but this is a detail which may vary with circumstances, and we think it ought not to be stereotyped by statute.

"The Secretary of State must have the most complete power of efficient inspection and of obtaining full returns and accurate records of all experiments made. Any place in which experiments are performed must be registered and open to efficient inspection. The appointment of an inspector or inspectors will be necessary, and we have seen that the analogy of the Anatomy Act has been appealed to by many high authorities. It is to be observed that the duties under that Act are of a nature much more mechanical than those which will be required in the present instance. The inspectors must be persons of such character and position as to command the confidence of the public no less than that of men of science.

"Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal; but this will involve great disgrace; and the withdrawal of the licence of an eminent man without real cause might be a serious public mischief. We have felt it necessary, therefore, to consider what steps should be taken when the question of such withdrawal may arise. We think that the holder of a licence, when he shall receive notice that the Secretary of State intends to withdraw it during the period for which it has been granted, should be at liberty to demand a public inquiry; that this inquiry should be held before one of the Judges of the Supreme Court, with two competent assessors to be appointed by the Secretary of State, the Court having the full power of conducting it as a legal investigation by summoning and swearing witnesses, issuing commissions, and so forth:—that on the result of this inquiry, the Secretary of State should determine whether the licence ought to be withdrawn, and when he decides in the negative, should have the power of giving the holder of the licence the reasonable costs of his defence.

"Magistrates ought to be empowered, on cause shown, to authorise the police to enter and search the premises of persons suspected of performing experiments without a licence, and the performance of such experiments without a licence should be penal.

"It has been suggested that cases may occur in which an urgent necessity may have occasioned an experiment when there has been no licensed person within reach, and it has not been possible to apply for a licence; such as a sudden case of suspected poisoning, arising, perhaps, in a remote place, when the experiment has been reasonably considered indispensable, for the purpose either of cure or of medico-legal investigation. *Bonâ fide* cases of this kind ought evidently to be free from the risk of vexatious prosecution, and this can be secured by vesting in the Secretary of State the power of putting a veto on a prosecution.

"We believe that by such a measure as we have now proposed the progress of medical knowledge may be made compatible with the just requirements of humanity. In zeal for physiology, the country of Harvey, Hunter, Bell, and Darwin may well endure the test of comparison. We trust that Your Majesty's Government and the Parliament of this kingdom will recognise the claim of the lower animals to be treated with humane consideration—and will establish the right of the community to be assured that this claim shall not be forgotten amid the triumphs of advancing science."

The recommendations, we are confident, will meet with the approval of all moderate persons on both sides. Indeed, some may be inclined to think that Science has made too great concessions to popular feeling; that she has made concessions all who take the trouble to read the Report and evidence will allow. The reasonable opponents of vivisection will no doubt also be prepared to make concessions, as they must admit that, after the evidence adduced in this inquiry, its uncompromising suppression would be a calamity to humanity; and they must also admit that the outcry of "cruelty to animals" has had a very slender justification. We hope the Report will speedily be brought before Parliament, and the recommendations essentially adopted, so that both for the credit of science and for the satisfaction of popular feeling the practice may be carried on under well-defined and universally understood regulations.

"*THE GEOLOGICAL RECORD*"

The Geological Record for 1874. An Account of Works on Geology, Mineralogy, and Palæontology, published during the year. Edited by William Whitaker B.A., F.G.S., of the Geological Survey of England. (London: Taylor and Francis, 1875.)

THE late Sir Charles Lyell used to relate how, on the occasion of a visit which he paid to M. Deslongchamps at Caen, the eminent French palæontologist rose from the piles of books amid which he was working, and exclaimed, with a sigh of relief, "Let us devoutly thank Heaven that our lot is not cast with the next generation of geological workers!—for how they will manage to grapple with the ever-increasing literature of the science I am at a loss to conceive." The difficulty which Deslongchamps thus playfully anticipated is now a present and pressing one, which, it is not too much to assert, is almost painfully felt by every scientific student and worker. While, on the one hand, it is absolutely impossible that any man can read everything that issues from the press relating even to his own department of science, yet, on the other, no one can afford to neglect the results which are being obtained by his contemporaries. It is sad to remember that a large part of the energy of the illustrious Dalton was wasted—owing to his not being able to make himself acquainted with what other chemists of his day were accomplishing—in solving problems which had been already completely disposed of. And we are persuaded that the painful questions of priority in discovery which frequently arise between the workers in the same branch of science ought to be referred, not to the existence of petty jealousies or of a disposition to take unworthy advantages, but to the difficulty which each investigator finds in consulting the latest published results of his fellow-workers in the same paths of inquiry.

So far as relates to the scientific memoirs of past years, the Royal Society has conferred an inestimable boon on the labourers in every department of science by the publication of its admirable "Catalogue," for the appearance of the first supplement to which we are now anxiously looking forward. Aided by a grant from the British Association, too, the "Zoological Record" gives a yearly summary of the work which is being accomplished in that department of science. It has long been felt as a serious and yearly increasing want—though one which has been already to some extent met by publications in France, Germany, and Switzerland—that no similar work of reference for the geological sciences has hitherto appeared in this country. We are now happy to inform the readers of NATURE that this want has been very admirably supplied by the volume, of which the title appears at the head of the present article.

In the preface to this work the editor gracefully notices the important services rendered by his fellow-workers, but he has not referred to the great difficulties which attended the first establishment of this important year-book of reference; for the overcoming of which difficulties we are mainly indebted to his own energy and perseverance. When the proposal for this work was first drawn up by Mr. Whitaker—whose well-known works on Tertiary Geology, and especially those relating to the vicinity of the metropolis, gave him such claims on the confidence of geologists—the Council of the British Association did not find itself in a position to accord to it immediately the same assistance as it annually gives to the "Zoological Record." Undeterred by this preliminary difficulty, however, Mr. Whitaker determined to proceed with his task unaided. A list of guarantors was formed, who agreed to indemnify the editor against pecuniary loss; and among those who thus signified their sense of the importance of the work, we find the names of Lyell, Poulett-Scrope, and Logan, who have not lived to witness its publication, together with those of almost all the leaders of geological science in this country. Happily, the sale of the work has sufficed, even during this its first year of publication, to cover all expenses; and a grant from the British Association will serve to remove any anxieties which the editor might have felt as to its future.

In the plan of the work we think that Mr. Whitaker has exercised a very wise discretion. He has not attempted anything like reviews or critical notices of the various books and memoirs which he catalogues. In the publications in which this has been done, like the "Die Fortschritte auf dem Gebiete der Geologie, 1872," edited by Dr. Hermann J. Klein, or the "Revue Géologique Suisse pour l'Année 1874" of Ernest Favre, we have nothing like the complete work of reference supplied by the publication of the "Geological Record." In the latter, the notices of the various contributions to geological science are confined to terse statements of the subjects treated in them, with an enumeration of the plates and maps by which they are illustrated. Where, however, a short account of recent discovery or a summary of a new classification could be given in a few lines, or the bearing of a memoir on the progress of science briefly indicated, this has been often well done in the work before us.

The difficult task of classifying the memoirs according to the various subjects of which they treat has been, on the whole, very successfully accomplished; and for the general superintendence of the work, Mr. Whitaker has secured the aid of a number of well-known cultivators of different departments of the science to act as sub-editors. Mr. Topley takes the departments of British and Economic Geology; Mr. Labour deals with the works relating to Europe, the Arctic Regions, and America; Mr. Drew with those on Asia; and Mr. Robert

Etheridge, jun., with those referring to Australasia. The important department of Physical Geology has been undertaken by Prof. Green, and those of Mineralogy and Petrology by Prof. Rudler; while the science of Palæontology has been equally well cared for—Mr. Miall taking the papers referring to the Vertebrata, Prof. Nicholson those relating to the Invertebrata, and Mr. Carruthers those on Fossil Plants. Besides the sub-editors, a number of other contributors have given their assistance in connection with this important work.

When we reflect on the immense body of literature on the different branches of the natural sciences which is yearly published, we shall find good reason to be satisfied with the approximately complete character already attained by this, the first volume of the "Geological Record." It is only necessary to refer to the yearly increasing activity of our great scientific societies, the continual formation of new local associations and field-clubs (whether connected with particular districts or with our Universities and public schools), most of which publish their own transactions, to show the difficulty of making a complete catalogue even of the scientific publications which appear yearly in the British Islands alone. But when we add to these the prolific publications of the different State surveys and the numerous scientific institutions of the United States and of our own colonies and dependencies; when we bear in mind the scientific activity exhibited by the French, German, and Italian speaking populations of Europe, and the books and journals written in languages, which of course few scientific men are able to read, such as the Russian, Danish, Dutch, Scandinavian, Hungarian, Bohemian, Serbian, &c.; and when we recollect that geological memoirs are published even in Japan and Tahiti!—we may have some idea of the magnitude and difficulty of the task with which the conductors of the "Geological Record" have to grapple.

In illustration of the energy which has been brought to bear upon this task, we may mention that the first volume of the "Geological Record" extends to nearly 400 pages; that the journals of which the contents, so far as they relate to geology, have been given in abstract, number nearly 200; and that the separate entries of books, memoirs, and maps exceed 2,000.

Henceforward, the yearly volumes of the "Geological Record" must find a place on the shelves of every scientific library; and in congratulating the editor on the manner in which he has surmounted the first and greatest difficulties of his arduous undertaking, we find only one cause for complaint. So far as the title-page shows, no arrangements have been made with agents residing abroad for the circulation of the work in America, the colonies, and on the Continent. We are persuaded, so very general is the use of the English language among the scientific men of all parts of the world, that so soon as this omission is remedied, the foreign circulation of the "Geological Record" will equal or even exceed that which it already has at home; while most valuable aid will be given in the preparation of the future volumes of the work by the secretaries of foreign societies and the editors of Continental and American journals sending copies of their publications, immediately that they appear, to the conductors of this important work of reference.

J. W. J.

OUR BOOK SHELF

Lessons on Rigid Dynamics. By the Rev. G. Pirie, M.A. (London: Macmillan and Co., 1875.)

THIS work treats of the geometry of motion, D'Alembert's principle, reduction of the expressions for the effective forces, moments and products of inertia, energy, precessional motion, and certain differential equations which occur in treating of the subject of Rigid Dynamics. There is an excellent selection of exercises, many of which are worked out, and the answers are in many cases accompanied by useful hints. The book appears to us to be in every respect an admirable one, and to be a good introduction to the study of this difficult branch of natural philosophy. We agree with Mr. Pirie in thinking that much of the difficulty students find in this subject arises from the explanations which are given in the ordinary text-books being for the most part brief and often, in consequence, obscure. We believe the author's hope that his book may be useful not only to students of natural philosophy, but also to engineers, is likely to be realised. We cordially recommend the book.

The Secret of the Circle, its Area Ascertained. By Alick Carrick. (London: H. Sotheran and Co. Chiswick Press, 1876.)

ONE more contribution to the long list of works on the Circle, put forth with the usual assurance that now the question must be set at rest. "Dedicated with great deference to the different schools of learning and to the intelligence of the public generally in this and other countries, in the confident hope and full belief that the truth pointed out in these pages will soon be acknowledged." There is a prefatory notice taking us down to page 16 (there are 48 pages in the pamphlet), from which we learn that the author's name is an assumed one, and that he is now dead. "Introductory" takes us to page 39. "The Secret of the Circle, its Area Ascertained," occupies the rest of the book. The Rule given is, "Diameter $\times$ radius $+$ four-sevenths" (*sic*), hence our friend π is equated to $\frac{22}{7}$. There are ten figures, some pretty to look at, but there is a dearth of letters, and it is often hard to make out what parts are intended in the demonstration. There is much that is true and not new; for instance, that the inscribed dodecagon is equal to the inscribed square and half that square; what is new is not proved to be true. Thus to get the result, the circular segment bounded by the side of the dodecagon ought to be for his purpose $\frac{1}{84}$ (radius)^2, and this is not shown on pp. 44, 45, for it is not proved there that Q contains the nine segments which it is said to contain. Hence we are led to say that the truth about the Circle is *not* to be found *here*.

Australian Heroes. By Charles H. Eden. (Society for Promoting Christian Knowledge).

MR. EDEN has written a very interesting book. As might be surmised from the title, he has brought into prominence the adventures of the explorers of Australia rather than the results of their explorations. Australia is unlike almost any other country which has been the field of exploration; its sameness, the dreary tameness of the bulk of the continent, the comparative paucity and low state of the aborigines, deprive an explorer's narrative of many of the points of interest to be met with in the case of other countries—Africa, for example, South America, or even the Arctic regions. Still this little book shows that during the comparatively brief period that Australia has been a field for exploration, there have been plenty of deeds of daring and determination and self-sacrifice in the cause of scientific knowledge, to render any skilfully written narrative of Australian discovery interesting. Mr. Eden has told the story attractively, and the reader will not only be greatly interested, but will have a fair idea of what has been done to extend our knowledge of the "fifth continent" from its first discovery down to the trans-continental journeys of Warburton and Forrest—the latter, however, being referred to in a sentence or two.

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.*]

Dr. Bastian and Prof. Tyndall on Spontaneous Generation

I BEG you to allow me a few lines to protest, as Prof. Tyndall has done elsewhere, against Dr. Bastian's proceeding, in citing a number of observers *in support* of his views (NATURE, vol. xiii. p. 284), whose researches taken in each case—as a whole—furnish conclusive arguments *against* his views.

It is only by an inadequate statement that the observations of Dr. Pode and myself—which appear in Dr. Bastian's list—can have this signification attached to them. Where we obtained the result which Dr. Bastian obtained, we were able to trace it to a vitiation of the experimental conditions. Our results conclusively and categorically contradicted the particular assertions contained in Dr. Bastian's book, the "Beginnings of Life," into the truth of which we set ourselves to inquire.

Feb. 16 E. RAY LANKESTER

Radiometers and Radiometers

I HAVE recently been trying some experiments with a radiometer, obtained from Mr. Browning, and as some of my results are different from what I was led to anticipate, I should like to know whether there is anything special in my particular instrument, or whether other people have noticed the same things.

In Mr. Crookes' paper on "The Mechanical Action of Light," *Quarterly Journal of Science*, No. xlvii. p. 348, he states that "when only dark heat is allowed to fall on the arms [of the radiometer], as from a vessel of boiling water, *no rotation whatever is produced*." (The italics are mine.)

Now I find that my radiometer is particularly sensitive to dark heat, the presence of a heated copper wire, or still more that of an iron poker when only slightly warmed, instantly accelerating the number of revolutions.

But more than this: when exposed in a room to diffused daylight, the velocity of rotation is greatly influenced by the temperature of the room, and is by no means an indicator of the amount of light only.

One morning this week during the frost, upon looking at my radiometer, it appeared to be motionless, although standing not far from my study window. When placed nearer to the light it revolved, but so slowly that I thought the instrument must have received an injury. The room at the time was very cold, as the fire had not been lighted. After the fire had been lighted and the temperature of the room raised, the velocity of rotation increased, and upon observing the instrument just before dark, when the room was very warm, the rotation was considerably greater than it had been in the window in the middle of the day, although at the time there was only just enough light in the room to enable me to see the instrument at all. When I brought the radiometer near to the fire, which consisted only of dull hardly glowing coals, the rotation of the arms became so rapid as to render them almost invisible.

Upon taking the instrument out of doors between five and six o'clock in the afternoon, the thermometer a few degrees below freezing-point, the arms revolved slowly from right to left as usual, but upon bringing it near to a mass of snow, and shading the light off by some pieces of wood, I could see that the arms revolved slowly in the opposite direction, that is, in the same direction as the hands of a watch. Later in the evening I held the instrument in the open air in bright moonlight, the thermometer being at 24° F., and the rotation was again in the same direction as the hands of a watch. The next morning, when the temperature was nearly the same, but the air foggy with only feeble light, the arms revolved at about the same rate but in the usual direction, from right to left. In the evening I again held the radiometer in the moonlight in the cold frosty air; the rotation was as before, from left to right. Carrying the instrument in my hand I approached the house, the hall door of which stood open. As I came within reach of the light and heat the rotation diminished, and at length ceased, but upon entering the hall it commenced again, only in the opposite direction. In fact, I could stand in such a position that upon moving a few feet either way, I reversed the direction of rotation, while between the two there was no motion at all.

I afterwards repeated the experiment in a different form. I placed the instrument in a cupboard in a very cold room, with a considerable quantity of ice. Upon just opening the cupboard door and peeping in, I could see that the arms were revolving very slowly, but distinctly, from left to right. Upon opening the door a little wider the motion ceased, and when still more light was admitted the motion was reversed. I then removed the ice and nearly closed the door—the rotation ceased entirely; but upon introducing a piece of heated iron the arms spun round as fast as they usually do in full sunlight, and this, be it remembered, when the cupboard was almost dark, the door being only just sufficiently open for me to see the instrument, certainly not more than a quarter of an inch. T. N. HUTCHINSON

Rugby, Feb. 12

Since writing the above, I have been favoured with a note from Mr. Crookes, in which he points out to me that his results have been obtained by means of radiometers constructed with *pith* discs, and having no metal at all in the moving parts. In the little instrument that I have used the discs are of mica, blackened, of course, on the alternate faces, but mounted upon four metallic arms, apparently aluminium foil. Mr. Crookes observes: "I long ago gave up metallic instruments owing to their erratic movements while radiating or absorbing heat. I have mentioned this peculiarity of metallic radiometers in my papers for the Royal Society."

As this difference between the instruments used accounted, to some extent, for my obtaining results so different from those described by Mr. Crookes in the paper referred to, I felt at first that there was no further need to trouble you with these remarks, and that they had better be consigned to the waste paper basket. Upon second thoughts, however, it seems to me that there is still something that requires explanation, or, at all events, that I do not understand, in the different action of dark heat on pith only, and on mica mounted on thin metallic arms. The four arms are very fine, equally bright, and similar in all respects, hence it is difficult to see how rotation should be produced by the action of heat on the metallic parts of the apparatus. The vacuum, no doubt, is not so perfect as that obtained by Mr. Crookes with his exquisite Sprengel pump, but even this would hardly account for the "erratic movements" that I have observed.

I may add that since performing my experiments I have learnt that one of my pupils in Rugby School, Mr. H. F. Newall, has observed very similar results with a radiometer in his possession. T. N. H.

The Sailing Flight of Birds

HAVING had during several long voyages in the Pacific considerable opportunities for observing closely the flight of sailing birds, and especially of *Diomedea Melanophrys*, or "Mutton Bird," as I believe it is called by the Australians, a few suggestions on the subject may perhaps not be uninteresting to your readers.

This bird differs considerably in size from the albatross of the Cape, but as the principles of its flight are the same, the few suggestions I wish to make will apply with equal force to both species, and indeed to all sailing birds.

The *Diomedea* of the Cape it is well known can support itself in the air for a very long time without flapping its wings, and in "The Reign of Law" it is stated that "sometimes for a whole hour together this splendid bird will sail or wheel round a ship in every possible variety of direction, without requiring a single stroke of its pinions." This *may* be accurately true, but in the case of the smaller albatross I refer to, between one and two minutes, or perhaps 1,000 or 2,000 yards in space, is more approximately the limit to which the bird's power of sailing is exercised. When the flight begins after rest the bird appears to feel very considerable difficulty in rising from the sea. It runs along the surface for some distance, flaps its wings very vigorously, and continues to do this after it has left the water, until it acquires a satisfactory velocity. Its subsequent sailing flight until it again increases its rate of speed by flapping, I would suggest to be merely a utilisation of this original *vis viva* to the utmost possible advantage, the ascending and descending movements of the bird being nothing more than a change from actual to potential energy, and *vice versâ*. Suppose, for the sake of simplicity, that the wind is dead ahead, and that the bird commences sailing horizontally with a certain *vis viva*. With this, by fixing its wings so as to present inclined planes to the direction of the wind, it is able to rise to a certain height, the velocity decreasing in some ratio to the ascent, and if the highest point capable of being reached is attained, the bird for the instant comes to rest; up to this moment the actual energy has been gradually changing into potential, and the bird gaining thereby a position of advantage. It is, however, extremely rare that this position is attained—most frequently the horizontal velocity is only partially destroyed. The planes of the wings being now changed with reference to the direction of the wind, the bird begins to descend; the potential energy is transformed into actual, and velocity is acquired, to be again changed into potential, and so on until it becomes necessary to renew it. The line of flight, therefore, of an albatross going directly against the wind consists of a series of undulations, the summits of which correspond to the instants of least relative velocities, or positions of greatest potential advantage; whilst the lowest points correspond to the instants of greatest relative velocity and least potential advantage.

During all this time *vis viva* is of course being extracted by the resistance of the wind, and the velocity after a while is so diminished that the bird loses its power of rising to a satisfactory position of advantage. It is then that flapping recommences and new power of flight is acquired. When it is remembered that the weight of a Cape albatross varies from 16 lb. to 20 lb., and the stretch of wings from 10 to 12 feet, it will be evident how great is the potential energy of such a bird at the height say of 100 feet, and also how complete is its power of utilising that energy. The question may be asked, how long will it be before 2,000 foot-pounds of work have been extracted by air moving at the rate of sixty miles an hour? for until it has been extracted, or nearly so, the sailing flight of the albatross need not cease. By means of a suitable mechanism for changing the inclination of the wing planes every few seconds, the sailing of the albatross, I believe, might be simulated without great difficulty. It is generally supposed that the stronger the wind the greater is the power of sailing-flight. In the special instance referred to, viz., that of sailing directly in the teeth of the wind, this is not the case. A good breeze is evidently better than either a very strong wind or a calm. In the one case, a too great resistance destroys the *vis viva* too rapidly; in the other, the bird suffers from a want of sufficient resistance, very much as a kite does during a calm.

In sailing in any other direction a violent wind may more or less aid the flight, and the velocity attained in some instances be enormous and very deceptive. It is this element, viz., the velocity acquired by sailing obliquely with the wind, that is so difficult for the eye to eliminate in estimating the actual power of the bird to sail against the wind. In flying with the wind, the resistance to the stroke being greater, the necessary speed may be more rapidly acquired and with fewer strokes, provided the bird has the requisite strength. But, as might naturally be supposed, sailing directly with the wind for any considerable distance is rarely or never seen, the bird not finding sufficient resistance in the air for its support.

From what has been said it will appear that the superior sailing power of the albatross, in comparison with other birds, is due—

(1) To its ability to acquire readily very great *vis viva* by means of its extremely powerful wings.

(2) To its almost perfect power of utilising this *vis viva* for the purpose of ascending or descending, *i.e.* of changing from a position of greatest actual to greatest potential energy and *vice versâ*, with least loss of power through resistance of the air.

The above implies an extraordinary rigidity as well as absence of concavity of the wings, by which the bird is enabled to hold them in their place like two rigid planes, and thereby present their surface to the wind under the most favourable circumstances possible. The tremulous movement seen at the tips appears to be nothing more than vibrations due to the want of absolute rigidity in the pinions. The above suggestions, if tenable, furnish an explanation also of the flight of the flying fish—the undulatory motion, or rising over the crest of a wave, which has puzzled so many casual observers, being merely a change of some of the *vis viva* of its flight into potential energy. This means necessarily a loss of velocity depending on the amount of rise, and implies the power of the fish to change its wing planes so as to ascend or descend. The original *vis viva* has of course been created by a preliminary rush through the water before emerging.

It will be seen from what has been said that the principle suggested, rightly or wrongly, as fully explaining the flight of the albatross, is that of a body—gifted with the most perfect power of placing itself in a position of advantage—sliding up and down inclined planes under the most perfect conditions possible.

R. A.

The Use of the Words "Weight" and "Mass"

In the review of Dr. Guthrie's "Electricity and Magnetism" (Nature, vol. xiii. p. 263) the following words occur in reference to Dr. Guthrie's definition of the absolute unit of electric resistance: "Here, irrespective of other considerations, there is the fundamental error of using the term *weight* instead of *mass*."

It is very unfortunate that the word "weight" is ambiguous; and that the ambiguity is actually so great as to lead to all but universal confusion of ideas. It is *not* really improper to use *weight* as synonymous with *mass*, and, had Dr. Guthrie meant to refer to *mass*, his using the term weight would not have constituted any fundamental error. He would only have been using an old ambiguous word in the more authoritatively established of its two common meanings. By an Act of Parliament (18th and 19th of Victoria, Chapter 72, July 30, 1855) for the special purpose of establishing standard weights and measures, it is enacted that a certain piece of platinum referred to as a "weight of platinum" shall be denominated the Imperial Standard Pound Avoirdupois, and shall be deemed to be the only standard of weight from which all other weights and other measures having reference to weight shall be derived, computed, and ascertained. The gravity of a mass, or of a piece of matter, is not once named, or in any way referred to, in the Act as a thing for which a standard is meant to be established by that Act, nor is the word *force* or the notion of force put forward in any way in the Act. Thus the meaning attributed in the Act to the word *weight* is the same as is distinctly expressed in scientific language by *mass*.

However, on turning to Dr. Guthrie's book itself, I found a striking example of the troublesome perplexity which is involved in the ambiguity of the language in common use. A few lines below the passage touched on by the reviewer, the following sentence and appended note occur. (Text), "From the work done by the current in the experimental wire, the resistance in that wire is found, and this resistance is considered unity when the above measures are units, namely, 1 second time, 1 meter space, and 1 gram weight or force." (Note appended), "The force actually taken as unity is $\frac{1}{9 \cdot 81}$ gram, for this force acting on 1 gram for 1 second will give it a velocity of 1 meter a second." The text and the note are utterly irreconcilable. The confusion is complete.

I do not say that no one can possibly understand the subject with the common nomenclature; but I do say, from considerable experience in Glasgow University, where we are in the habit of using the absolute or kinetic system of force-measurement in *all* our calculations with the students of the Natural Philosophy Class, that it is extremely difficult to explain, with the old nomenclature, the beautiful, and in itself simple, kinetic system of Gauss, together with its connection with the gravitation system of force-measurement.

This session, however, I have found a very great simplification in adopting a suggestion of Prof. James Thomson to do away with the word *weight* altogether in cases in which its employment would involve ambiguity. He would still readily use the name, a pound weight, for the standard piece of iron or brass used in weighing; and would continue, so long as our present non-decimal system is maintained, to use the commercial term, a hundredweight of iron, meaning a certain quantity or mass of iron. But he has proposed that when we mean *mass* we should avoid the word weight as far as possible and use the word mass, and that where we mean downward force due to gravitation, called by Dr. Guthrie and his reviewer, *weight*, we should use the word *gravity*. Thus we may speak of a one pound force, or we may say "the gravity of a pound," but never "the weight of a pound." We can scarcely get rid altogether of connecting the idea of heaviness with the word weight, nor would our dictionaries at present allow us to do so; but it is quite proper to feel that, in speaking of a certain weight as being too great to be resisted by a certain chain, we are using a colloquial and inaccurate expression, like calling a door *heavy* when we are not attempting the feat of Samson, but merely opening or shutting it, turning it on its well-oiled hinges.

During the present session we have aided ourselves in Glasgow with four very important helps to the teaching of the kinetic system of force-measurement. One is the improvement in nomenclature just referred to. The second is the use of names for the kinetic units of force. The British Association has sanctioned the use of the name *Dyne* for the kinetic unit of force founded on the centimetre, gramme, and second, as units of length, mass, and time respectively. Prof. James Thomson has given the name *Poundal* for the British kinetic unit of force founded on the foot, pound, and second. The third help is the construction by Prof. Thomson, for the first time, so far as I know, of spring balances for indicating poundals and kilodynes. The fourth aid is Dr. Everett's admirable book on the C. G. S. system of units.

J. T. BOTTOMLEY

University of Glasgow, Feb. 7

Seasonal Order of Colour in Flowers

I AM very much obliged to Mr. Buchan for his elaborate paper in NATURE, vol. xiii. p. 249, on the Flowering of Spring Plants (see my query, NATURE, vol. xiii. p. 129). Although agreeing with Mr. Pryor that the blue is anticipated by various other colours, yet I think that the method of inquiry by averages is the only basis we can go upon; and that is the plan I have adopted for some time. I have now a carefully-assorted collection of hyacinths, and I see that the blue and white are coming out nearly together, the red showing as yet no colour whatever. What would be the action of light upon blue or red flowers, if the blue or red ray was carefully excluded, if this could be done? Would the flower thrive, and if so, would its colour be much altered?

C. E. HERON ROGERS

Retford, Notts, Feb. 7

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—Herr Julius Schmidt publishes (in *Astron. Nach.*, No. 2,074) the results of observations of variable stars made at Athens in 1875, amongst which the following may be noted:—

1. 34 Bootis, a star to which he had directed attention some years since, as certainly variable though observed with difficulty on account of proximity to ϵ, was found to be at a maximum on April 26—a good determination. In 1872 he assigned a period of 369 days from six observed maxima, commencing 1867, July 31, and as many minima, the first, 1867, November 18. Between the maxima of 1867 and 1875, we should have eight periods of about 353 days. The mean place of this star for the beginning of the present year is in R.A. 14h. 37m. 32s., N.P.D., 62° 54′·1.

2. Mira Ceti. Three curves drawn from comparisons of this star with α and γ Ceti and α Piscuim gave the date of maximum, February 27·5, March 1 and 3 respectively, of which the latter is preferred. Calculating from the formula of sines in Schönfeld's second catalogue, the maximum of 1875 is fixed to February 24·2. Observed minimum, October 30.

3. η Geminorum.—The variability of this star was detected by Schmidt in 1865, and has since been confirmed by Schönfeld, who found for the brighter phase small and not very regular fluctuations, but for the minima a regular diminution and increase, the first continuing about six weeks, and the last perhaps rather longer. This is in near agreement with Schmidt's previous deductions. He had found by comparison with μ Geminorum that η at times remained constant for several months about the maxima, of which, writing in 1869, he states he had been unable to assign the dates. In 1875, however, two maxima were noted, Feb. 25 and Sept. 23; showing an interval of 210 days. The period assigned in the last Manheim catalogue is 229·1 days. This star is of a deep yellow colour. Variation between extremes of 3·2 and 4·2.

4. ϵ Aurigæ.—Schmidt collects the results of his comparisons of the relative brightness of ϵ and η Aurigæ, between the years 1843-1875. The star is irregularly variable within somewhat narrow limits.

5. u Herculis.—The principal period appears to be about 38·7 days, but according to Schmidt (A. N. 2,075) the curve exhibits waves of about twelve hours' duration, which are of the greatest depth at the principal minimum, and comparatively shallow at the maximum, and he has given a figure explanatory of what he considers to have been the law of variation between 1875, July 4, and Aug. 29. So unique a case appears to require further investigation.

6. g Herculis.—This reddish-yellow variable was discovered by Mr. Baxendell in 1857, and has been carefully observed by Schmidt. The period, according to Schönfeld, has varied between 40 and 125 days, the star thus resembling in the great irregularity of period the well-known R Scuti, which was discovered by Pigott in 1795. Last year Schmidt's comparisons showed three maxima and two minima, indicating periods 77, 73, and 77 days. The variation extends through little over one magnitude.

7. α Cassiopeæ.—Of this star Schmidt remarks that the fluctuations of brightness in 1875 were not greater than in the cases of other stars, which are not yet placed upon the variable list.

8. T Coronæ Borealis (Nova 1866).—Mostly ninth magnitude, or rather fainter; exhibiting sensible variation, but to very small amount.

9. R Scuti.—Observed maxima on October 12 and December 8 give the short period of 57 days. The period entered in Schönfeld's second catalogue is 71·1 days. There are great irregularities in the case of this star, not only in the period but in the degree of brightness at both maximum and minimum; the former has been noted between 4·7 and 5·7, and the latter between 6·0 and 8·5.

MINOR PLANETS.—No. 131, *Vala*, discovered by Prof. Peters at Clinton, U.S., 1873, May 24, has so far been unsuccessfully sought at Pola and Berlin between limits of — 30m. and + 15m. in respect to the place of the ephemeris apparently founded on Stockwell's elements; the longitude of perihelion in this orbit differs materially from that given by Knorre's earlier calculation, and possibly a misprint or error of transcription may be the cause of the difficulty.—Prof. Tietjen notifies that the ephemeris of No. 141, *Lumen*, in the *Berliner Jahrbuch* for 1878, is vitiated by an error in *Astr. Nach.*, No. 2,030, where ω is substituted for π; the habit of some computers of giving the orbital angle between perihelion and node, instead of the longitude of perihelion itself, is certainly not without its inconvenience, and this is more particularly the case with early orbits of comets.—No. 156, discovered by Palisa, 1875, Nov. 22, has been named *Xanthippe*.—New elements of No. 158 give a period of 1,889 days, or 5·17 years.

THE SATURNIAN SATELLITE, HYPERION.—Observations of this faint object made with the 26-inch refractor of the U.S. Naval Observatory on forty nights between 1875, June 16 and Nov. 25, appear in No. 2,076 of the *Astron. Nach.* It is stated that the observations were generally made with difficulty. Prof. Asaph Hall acknowledges his obligations to Mr. Marth for his ephemerides of the satellites of Saturn, by which he has endeavoured to facilitate identification of these objects, and which could only have been prepared at an expenditure of much time and trouble.

THE DATE OF EASTER

WE revert to this subject with the view to reproduce the arithmetical rule to find Easter Sunday in the Gregorian Calendar, which was first given by the eminent German mathematician and astronomer Gauss, in Zach's *Monatliche Correspondenz*, 1800.

1. From 1800 to 1899 put $m = 23$, $n = 4$.
   „ 1900 to 2099 „ $m = 24$, $n = 5$.
2. Divide the given year by 19, and call the remainder a.
3. „ „ by 4, „ „ b.
4. „ „ by 7, „ „ c.
5. Add m to 19 times a, divide the sum by 30, and call the remainder ... ... d.
6. Add together n, twice b, four times c, and six times d, divide the sum by 7, and call the remainder e.

Then Easter Sunday is March $22 + d + e$, or
$d + e - 9$ of April.

To apply this rule to the present year, we have—

1. $m = 23 \quad n = 4$
2. For $\frac{1876}{19}$ remainder is 14 . . . . a.
3. For $\frac{1876}{4}$ remainder is 0 . . . . b.
4. For $\frac{1876}{7}$ remainder is 0 . . . . c.
5. For $\frac{23 + 19 \times 14}{30}$ remainder is 19 . . . . . . d.
6. For $\frac{4 + 0 + 0 + 6 \times 19}{7}$ remainder is 6 . . . . e.

And Easter Sunday is March $22 + 19 + 6 =$ March 47 or April 16; or $19 + 6 - 9$ of April = April 16.

NOTE.—The following are the two exceptions to the above rule :—

1. If Easter Sunday is brought out April 26, we must take April 19.
2. If Easter Sunday results on April 25 by the rule, the 18th must be substituted when the given year, increased by one, and then divided by 19, leaves a remainder greater than 11.

PROF. FLOWER'S HUNTERIAN LECTURES ON THE RELATION OF EXTINCT TO EXISTING MAMMALIA [1]

II.

THE extinct *Marsupialia*, *Edentata*, and *Ungulata* were treated of fully in the previous course.[2] It is only proposed now briefly to recapitulate the principal results as bearing on the derivative hypothesis, and to call attention to some recent additions to our knowledge of the past history of these groups.

The *Monotremata*, represented only by the *Ornithorhyncus* and *Echidna* of Australia, the lowest and most reptilian of mammals, seem to be the survivors of a group of animals of intermediate structure through which the passage from the lower to the higher vertebrates was made. But of this palæontology has hitherto afforded not the slightest proof. Except some Pleistocene remains of one of the existing genera, no trace of any extinct animal allied to the Monotremata, or showing any decidedly intermediate characters between Mammals and the Sauropsida, has ever been found. The earliest known remains of mammals—in fact, all those of the Mesozoic period—appear to belong to the Marsupialia, although too little is yet known of their general organisation to pronounce definitely on their affinities. As early as the time of deposition of the Purbeck beds, below the Wealden, two very distinct types of dentition prevailed, bearing some resemblance to those characterising the modern polyprotodont and diprotodont types, the one being mainly carnivorous or insectivorous, and the other frugivorous or herbivorous. True Marsupials, allied to the American Opossums, have been found in the Eocene and Lower Miocene formations of Europe, but not later. Many of the recently discovered insectivorous and carnivorous mammals of the North American Eocenes also appear to have been Marsupials, but the Tertiaries of no part of the world, except the Australian Pleistocenes, have yielded forms allied to the Wombats, Kangaroos, and Phalangers, now so characteristic of the fauna of that land.

Palæontology teaches scarcely anything of the past history of the Edentata, an order which might be supposed from the diversity in structure and geographical distribution of the animals now included in it to have occupied a conspicuous position formerly in the fauna of the world. The American gigantic Pleistocene forms, the *Megatherium*, *Glyptodon*, and their allies, are of great interest, but they are all closely related to the existing species of the same countries, and their progenitors have hitherto been sought in vain in the earlier formations of the country. Apparently they formed no part of the Eocene or Miocene fauna of the northern part of the Continent. The European Miocene Edentates, *Macrotherium* and *Ancylotherium*, appear to be more related to the existing Old World forms, but their structure is as yet very imperfectly known.

The difference between the Perissodactyle and Artiodactyle Ungulates was fully established in the earliest tertiaries, beyond which we are not able to trace their history. Recent researches of Leidy, Cope, and Marsh have enabled us to form a tolerably connected idea of the history of both these groups in North America. Before 1869 not a single Eocene Perissodactyle had been discovered in that country; it is now known that while the *Lophiodons* and *Palæotheriums* flourished in the Old World, similar forms ranged through the sub-tropical forests of the regions in which the stupendous mountain ranges of Western North America have since been elevated. None of these appear to be identical specifically with the European forms, and even the generic indications, being often founded only on very limited portions of the organisation, as a few teeth, must be regarded as provisional. The best known forms, *Hyrachyus* and *Palæosyops*, are respectively Lophiodontoid and Palæotheroid. Some are allied to the *Hyracotherium*, one of which, *Orohippus*, seems to connect that form directly with *Anchitherium*. One form only, *Diceratherium*, is Rhinocerotic; it is found in the uppermost Eocene strata of Utah, and gives the earliest indications of this group yet known. It seems to be connected with the lower Eocene *Hyrachyus* on the one hand, and the Miocene *Hyracodon* on the other. In the Miocene period, the Perissodactyles attained a great development of form and size, and the groups became more differentiated, and some of them highly specialised. True tapirs do not seem yet to have been met with. The Palæotheroid and Lophiodont forms had nearly, if not quite, died out, but the more horse-like *Anchitherium* was abundant, and appears to have continued the line from the Eocene *Orohippus* (which had four toes on the fore foot) to the true horses of the Pliocene period. Rhinocerotic forms now became ascendant, being represented by *Dicerotherium* with a pair of lateral horns on the nasal bones, *Hyracodon*, a very generalised hornless Rhinoceros, with the complete number of incisor and canine teeth, and several species assigned to the European genus *Aceratherium*. But the most remarkable of the Miocene Perissodactyles of North America are a number of species of gigantic size, to the first known of which Leidy, in 1853, gave the name of *Titanotherium*, and of which other, or perhaps the same, forms have been named by Marsh *Brontotherium*, and by Cope *Symborodon*. Their head was large and much elongated, as in the rhinoceros; but they had a pair of stout diverging osseous protuberances like horn cores on the nasals or anterior part of

[1] Abstract of a course of lectures delivered at the Royal College of Surgeons "On the Relation of Extinct to Existing Mammalia, with Special Reference to the Derivative Hypothesis," in conclusion of the course of 1873. (See Reports in NATURE for that year.) Continued from p. 308.

[2] See Abstracts in NATURE, 1873.

the maxillaries in front of the orbits. Their molar teeth were of a simple Palæotheroid type, and the incisors and canines were very much reduced. Their fore feet had four and the hind feet three short stout toes. These animals which, according to the correct rules of nomenclature, should constitute the family *Titanotheridæ*, became extinct, apparently without successors, in the Miocene age.

In the Pliocene period, *Rhinocerotidæ*, in the form of *Aceratherium* and *Aphelops*, were still abundant, though towards the end of the period they became entirely extinct. True rhinoceroses, like those of the Old World with median horns, have never been met with in America. Remains of tapirs are also found but sparingly, and a great development took place in the various forms of three-toed horses, *Protohippus* and *Hipparion*, which replaced the *Anchitherium* of the Miocene. These in their turn gave way to true horses, of which remains of several species have been found in Pleistocene deposits, and scattered throughout almost every region of the Continent from north to south. These also became entirely extinct before the discovery of America by the Spaniards, a most remarkable circumstance when the fitness of the country for their maintenance, proved by the facility with which the descendants of horses introduced by the invaders have multiplied in a feral state, is considered. The tapir, in several modifications, still lingers in some districts of South and Central America, the sole direct representative of the vast and varied perissodactyle fauna of ages long gone by.

The remains of Artiodactyles in the hitherto explored American Eocenes are very scanty and unsatisfactory as affording indications of their characters. Towards the close of the period only do we find evidences of well-defined selenodont (*Agriochærus*) and bunodont (*Elotherium* and *Platygonus*) forms. No species corresponding to the European *Anoplotherium*, *Dichodon*, or *Xipodon*, have been discovered. During the Miocene period, however, Artiodactyles of both types abounded in North America. The selenodonts were chiefly represented by *Oreodon*, and the allied earlier and more generalised *Agriochærus*. These were numerous in species and individuals, but they became nearly extinct by the end of the Miocene, only surviving in the form of *Merychyus*, the most modified form of the group, into the Pliocene. The first indication of the camel-like animals appears in this period in the form of *Poëbrotherium*; also a few of the generalised Pecora are now met with, allied somewhat to *Tragulina*, as *Leptomeryx*, *Hypisodus*, and *Hypertragulus*, the latter a very minute species, but no true deer, *Bovidæ* or even *Traguli*, and no giraffes, *Helladotheria* or *Sivatheria*. The bunodonts were chiefly species of *Elotherium*, and an allied four-toed form, *Pelonax*, remarkable for horn-like lateral processes on its lower jaw, near the symphysis. Peccary-like forms also are now met with. In the Pliocene or Pleistocene periods, except the somewhat problematical *Cosoryx*, founded on some branched horn cores or antlers of a form perhaps allied to *Antilocapra* from the Niobrara Pliocene, all the animals can be assigned to existing families. Of the Suina, all belong to *Dicotyles*, or Peccary (which had formerly a more northern range than at present), or to the allied genus *Platygonus*, no true *Sus*, or *Phacochærus*, *Babirussa*, or *Hippopotamus*, having been found. Thus the American bunodont Artiodactyles, instead of undergoing great and diverse modifications as did the corresponding animals of the Old World, have been gradually dwindling and contracting to the two closely allied species of Peccary, amongst the smallest and most insignificant of all the pigs, and a very old form, having existed (if teeth alone are sufficient evidence) since the Miocene age. The *Camelidæ*, on the other hand, appear to have flourished, the species being numerous, and the individuals attaining very large dimensions. It is probable, in fact, that the family may have originated here, as a tolerably complete series of transitions have been traced from the Miocene *Poëbrotherium*, through *Procamelus* and *Pliauchenia*, to the modern Llamas, which, though now confined to the south, once overran the North American Continent. If this view is correct, the Asiatic camels must have come into that country by immigration. A few traces of *Cervidæ* have been found in American Pliocenes, but their paucity, compared with the Old World, until the Pleistocene epoch, would lead to the belief that they cannot have originated there, but must have been imported from other lands. The same applies to the hollow-horned ruminants, of which no forms different from those now existing have been found in the fossil state.

(*To be continued.*)

THE EFFECTS OF THE SUN'S ROTATION AND THE MOON'S REVOLUTION ON THE EARTH'S MAGNETISM

WHEN the mean horizontal force of the earth's magnetism for each day of the year has been deduced from well-corrected observations of the bifilar magnetometer, and the results have been projected in the usual way, the curves thus obtained show successions of maxima and minima occurring in some instances at nearly equal intervals and in others abruptly and apparently without law. It has been found that these changes are experienced similarly at all stations where observatories have been placed on the earth's surface; they are therefore variations of the magnetic force of the whole earth. The results now considered, though derived from the observations at a single station, may thus be accepted as true generally for all places.

In the projection of the daily mean forces observed at Makerstoun in 1844, the first and last quarters of the year showed large oscillations of the earth's magnetic force, the maxima occurring near the times of new moon and the minima near those of full moon; the ranges of the oscillations were not equally great, and the oscillation disappeared in the months near midsummer. The mean result for the whole year seemed to show that great changes of the earth's magnetic force were due to the moon's position relatively to the earth and sun; but no explanation could be offered for the apparent irregularities in the lunar action. Eleven years later (in 1857), while discussing observations made near the equator, I became persuaded that the variations in question were really due to the sun's rotation on his axis. The result of a re-examination of the Makerstoun observations gave a mean period of nearly twenty-six days for the most probable duration of the magnetic oscillation.

Astronomers who till then had occupied themselves with the determination of the time of the solar rotation had found for it from 27·3 to 27·7 days. It was difficult, in the face of this result, to suppose that the magnets were better acquainted with the true time of the sun's rotation than the eminent observers, who, with the best telescopes, had watched the movement of the solar spots; and it was suggested that a movement of the sun's magnetic poles might explain the difference of the periods obtained. More recently, however, it has been found that the spots give considerably different times for the sun's rotation, and especially that those nearest the solar equator indicate, as Spoerer has shown, a period of 26·3 days, thus approaching nearly to that obtained previously from the magnetic observations. Dr. Hornstein, director of the Prague Observatory, discovered, independently, nearly the same period from his observations in 1870.

There still remained for explanation the irregularities already noticed in the lengths and ranges of single oscillations. I, on a reconsideration of all the discussions previously made by him, arrived some time ago at the conclusion that the results obtained for the solar and lunar actions did not exclude each other, but that both sun and moon were concerned in the changes of the earth's magnetic intensity; and that possibly the variations in the character of the single oscillations were due to the sun and moon sometimes acting in the same and sometimes in opposite directions; just as in the case of the oceanic tides, for which the differences would be even greater were the solar more nearly equal to the lunar action.

This conclusion is put to the test; the mean variations derived from the observations for each of two successive years are calculated for periods of 26, of 27·3, and of 29·53 days, the two latter being the times of the lunar, tropical, and synodical revolutions respectively. The variations for each of these three periods corresponding to the positions of the moon and of a given solar meridian for each day of the year are then added together; the sums should represent the total actions of the two bodies for each day, and if no other causes are in question, they should agree with the observed variations.

I have shown that when the calculated results are projected so as to form a red curve, on the same mean line as a black curve representing the observations, the two agree very nearly with each other throughout the two years. The different durations and ranges of single oscillations, and the total disappearance of the latter in certain months, are found to be produced, as was supposed, by the greater or lesser agreement or opposition of the three actions.

These results demonstrate, I think, not only that the sun's rotation and the moon's revolutions produce variations of the earth's magnetic force, but that all the marked variations are really due to these causes.

There appears to be one exception to the generality of this conclusion, in sudden great changes, generally diminutions, of the earth's magnetism, which appear of variable magnitude and apparently at irregular intervals. I have examined these cases, and find that if a considerable diminution of intensity happen suddenly when a given solar meridian is in the same plane with the earth, that a similar sudden diminution generally occurs twenty-six days or some multiple of twenty-six days after, when the same solar meridian and the earth are again in the same plane. In one case the sudden loss of force begins five times in succession at the exact interval of twenty-six days.

If we examine these cases of successive disturbance when a given solar meridian arrives opposite the earth, we are induced to conclude either that the solar action exists only for this position, that is to say, that the earth is its cause; or that the action is continuous, but, unlike light and heat, is propagated only in one direction (or plane); or, which seems more probable, that the medium through which these actions are transmitted proceeds from the sun, is not uniformly distributed around it, nor always distributed in the same way. This idea may aid in explaining many facts in terrestrial magnetism for which hitherto no clue has existed.

We arrive then at the conclusions that the variations of the daily mean magnetic force are due to causes external to the earth, depending on the sun's and moon's motions; that all the principal variations of this force can be calculated approximately for each day in twelve months, on the hypothesis that the actions of these bodies are constant throughout the year for the same positions relative to the earth; and that the great magnetic disturbances (accompanied by the aurora borealis) are due to actions proceeding from certain parts of the sun's surface, since so many of them repeat themselves at intervals of twenty-six days, when the same solar point returns opposite the earth. It appears from other investigations that the sun's rotation produces marked effects on our atmosphere.

J. ALLAN BROUN

PHYSICAL SCIENCE IN SCHOOLS

WE have received the following important communications on this subject:—

Dr. Watts has shown in last week's NATURE, p. 311, how the Regulations of the Oxford and Cambridge Schools Examination Board affect injuriously the interests of science in certain schools, viz., those in which boys have "studied science *instead of* the older well-established subjects of classics and mathematics."

But it is fair to say that these are not the schools which are common in the country, even if they are to be found at all; and the business of the Board was rather to examine effectively what schools profess to teach, than to direct their studies into a new line.

I do not think, or mean to say, that the Board and the headmasters, who are believed to have influenced its regulations, have acted in the interests of science altogether; but the way they have damaged its interests in all the principal schools of the country is not by making it "a refuge of fools," as Dr. Watts suggests.

I will, with your permission, point out how the Regulations affect the position of science at Rugby and similar schools.

The main inducement a boy has to go in for a certificate is to be excused the "Little go," if he is going to Cambridge; or "Responsions," if he is going to Oxford. To be excused any part of the Little go he must pass in Latin, Greek, Elementary Mathematics, and Scripture Knowledge; to be excused the whole of it, he must also pass in Additional Mathematics. And since four subjects are all that is required, these four or five are all that he will take up. To be excused Responsions, he must pass in Latin, Greek, and Elementary Mathematics, and in some one other subject. Naturally, he selects the easiest. This will be French, or Scripture Knowledge, or History, or English, according to his tastes. That the three last of these subjects are easier than any Natural Science is certain. All boys know something of them; they are not wholly new to any boy, and it is at any rate commonly believed here that boys have passed and obtained distinctions in them with very great ease. And boys have probably learned French for the last eight years of their lives.

It follows that boys destined for Oxford who know something of science do not take it up, as it would be profitless to do so, unless it is distinctly easier to them than any other subject they could select as a fourth subject; and boys destined for Cambridge do not take it at all.

It should also be observed that though the examination papers last year were very easy, yet the range they cover is rather large. What does "the Elementary Parts of Inorganic Chemistry" mean? This can only be discovered by a study of the examination papers. And certainly the wording is loose enough to enable the examiners to set harder papers, if it is found that the boys can do them.

The only important alteration in the description of the examination in Natural Science that I would make is a very obvious one, and one that might at once be made if any of the headmasters on the Committee would care to ascertain and represent to the University Board the interests of science at schools. It is to divide Group IV. like Group II., making the present part the elementary subjects, and adding to it an additional subject, viz., a practical examination in analysis (inorganic, qualitative), as necessary to be passed by those who wish to gain *honours* in science. To make this change would at any rate be a guide to a sound opinion on teaching science, though, while the grouping remains as it is, few boys will trouble the examiners.

The study of science at schools has now, it seems to me, entered on a fresh and not altogether unhealthy phase. Ten or twelve years ago there was an outcry for it, and it was introduced into public schools, and protected, and nursed, and encouraged by scholarships at the Universities, &c. On the whole it got quite as fair play and more favour than could have been expected. Now it is no longer nursed; it is left to find its level. It is protected by regulations against the extinctive power of headmasters, and that is all. Meantime, the methods of teaching it are improving; the supply of teachers is increasing; the number of scholarships at the Universities is quite adequate to the demand for them; and the examinations for them are very good. With these favourable circumstances, and a slowly maturing opinion in the minds of most people that education in science is valuable as a part of training, I think we can afford not to be very impatient at the Regulations of the Universities Board, or at the strict neutrality of headmasters when the interests of science are concerned.

JAMES M. WILSON

May I be allowed a few words with reference to some criticisms passed in last week's NATURE on the Regulations adopted by the "Oxford and Cambridge Schools Examination Board" in regard to science?

I fully concur with the writer (Mr. N. M. Watts) that these Regulations and the two papers printed point to a low standard of scientific knowledge in our great schools. It must be borne in mind, however, that the Board does not issue these Regulations as an ideal scheme of school work, but merely intend them to answer the existing state of the case. With the curriculum of any school they have nothing to do, their function being to appoint examiners to such schools as apply for them, leaving the schools free, within reasonable limits, to choose their subjects.

Now granting Mr. Watts' premises that certificates can be obtained *very cheaply* by taking up two sciences instead of Latin and Greek, this would give an impetus to the study of those subjects in schools, resulting in a large flock of scientific candidates.

Whether this has been the case, the following abstract from the examiner's report of last July shows:—

Subjects.	Number of Candidates offering the subject.	Number who satisfied Examiners.	Number who obtained distinction.
Latin	438	308	37
Greek	433	253	35
French	51	34	13
Mathematics ... (Elementary)	455	328	—
English	43	26	9
History	305	234	82
Nat. Philosophy (Mechanical)	21	10	3
Nat. Philosophy (Chemical) ...	28	16	3
Botany	6	4	—
Phys. Geography and Elem. Geology ...	15	7	—

These results show that the number of candidates offering any branch of science is comparatively very small, only seventy out of a total of 461, of whom only thirty-seven succeeded in satisfying the examiners out of a total of 232; and of these thirty-seven only six succeeded in gaining distinction. These figures show, I think, that in proportion to the time and attention given to science in the schools examined last July, the papers set were neither unreasonably easy nor difficult. I wish especially to point out that increasing the difficulty of obtaining certificates by help of science would tend as far as possible to exclude science from the school curriculum, while retaining a low standard encourages boys who have gained a certain crude scientific culture in the lower forms, not, as is so often the case, to let it drop entirely on reaching the sixth.

It does seem to me therefore wiser to commend the wisdom than to deplore the ignorance of the compilers of these Regulations, who aimed at testing the soundness of the small modicum of existing knowledge, rather than fixing a standard which would have practically acted as a prohibition of science.

In considering the amount of knowledge to be expected from boys of eighteen, we must remember the time usually devoted to science work. The following will, as far as my experience goes, be not an unfair statement of the case. A boy commences Latin and French at about eight years of age, at the same time imbibing the first ideas of Mathematics in Elementary Arithmetic; Greek (or German when it is substituted) at twelve or thirteen, and probably Euclid about the same age; Science seldom, if ever, before fourteen or fifteen. Thus the candidate offering himself for examination at eighteen has given ten years to Latin, six to Greek, and about three to Science, the number of hours in those three years given to Science being certainly less than that given to Greek. This programme would be true of certainly nine-tenths of our public school-boys who offer themselves for the examination, the remaining tenth consisting of boys who at seventeen show a distinct aptitude for Science or Mathematics, and who then drop a large proportion of their classical work and are enabled to devote one-half of their time, or thereabouts, to their special subjects. The complaint might with more reason be urged by the classical boys proper against these specialised boys, who are allowed to gain their certificates too easily. When the necessity arises, the standard will doubtless be raised, perhaps, by a division (similar to that made in the Mathematical Group) into Elementary and Advanced Science, with a provision that only one elementary science can be taken up.

Mr. Watts, in the article referred to, asks, rather contemptuously, whether "the knowledge of the composition of the air, the reasons for belief in the rotundity of the earth, the meaning of the words watershed, dip, &c., is the utmost that can be demanded of a boy of eighteen who has studied science instead of the older well-established subjects of classics and mathematics." I hope I have shown that the standard of the examination papers was not too low for the candidates who offered themselves. With reference to the desirability of the change in our whole system of education to which Mr. Watts refers, I may be allowed to say that there is by no means at present an agreement even amongst science teachers that such a change is desirable. I refrain from opening up this very wide subject, because I feel that the experiment has not yet had a fair trial.

Rugby LINNÆUS CUMMING

THE ORGANIC IMPURITIES OF DRINKING WATER

ON Thursday last Prof. Frankland delivered a discourse to the Fellows of the Chemical Society at Burlington House on the detection and analytical determination of the organic impurities in potable waters. He said that the more his inquiries into the influence of water upon the public health had extended themselves the more had he become convinced of the great importance of this application of chemical analysis to the community at large, contending that, in the interests of the public health, the bringing to perfection of this branch of analysis was worthy of the greatest efforts of chemists.

The two chief objects to be kept in view in the analysis of potable water are, firstly, the discovery of the evidence of *past* pollution by organic matter; and secondly, the quantitative determination of *present* or *actual* organic impurity.

The past history of a water is made out chiefly through the mineral products of oxidation which the polluting organic matters have yielded, and which are still present in the water. As these products are innocuous, it is obvious that if all kinds of organic matter behaved alike under the influence of oxidising agents, such evidence of previous pollution might be safely disregarded; but it is almost superfluous to point out that there are wide differences between various kinds of organic matter in regard to the rapidity with which they combine with oxygen; and of all kinds, that which is organised and living opposes by far the greatest obstacles to oxidation. Now the researches of Chauveau, Burdon Sanderson, Klein, and others, scarcely leave room for doubt that the specific poisons of the so-called zymotic diseases consist of organised and living organic matter, and it is now certain that water is the medium through which some, at least, of these diseases are propagated. It is evident, therefore, that an amount of exposure to oxidising influences which may resolve the dead organic matters present in water into innocuous mineral compounds, may, and probably will, fail to affect those constituents which are endowed with life, and Dr. Frankland adduced, as a striking instance of the persistency of the typhoid poison when diffused in water, the outbreak of a violent epidemic of typhoid fever in a Swiss village through the use of spring water, which, after contamination with the poison, had filtered through nearly a mile of porous earth, but which had nevertheless lost none of its virulent properties. As the typhoid poison is always liable to be present in sewage, and as there is no test for it, except its effects upon man, the discovery of previous sewage contamination in potable water ought to be one of the chief objects of the analyst.

The *actual*, or present, as distinguished from the *past*, polluting organic matter of potable water can only be ascertained from the amount of carbon and nitrogen found as constituents of the organic matter present in the water at the time when the analysis is made. The method of performing this operation, known to chemists as the "combustion" method, was fully described to the Fellows of the Society by the speaker eight years ago. Improvements since made were mentioned, and the following proofs of the delicacy and accuracy of the analytical method were adduced:—

To 100,000 parts of a sample of water, rendered as nearly chemically pure as possible, 1·5572 parts of sulphate of quinine were added. The water was then submitted to the method for determining organic carbon and nitrogen just mentioned. The following data compare the quantities of organic carbon and organic nitrogen thus actually added to the water, with those afterwards extracted in each of two analyses:—

	Present.	Found. I.	Found. II.
Organic carbon in 100,000 parts of water	0·857 part.	0·912	0·904 part.
Organic nitrogen in ditto	0·100 ,,	0·0996	0·098 ,,

To 100,000 parts of a similar sample of water 0·7786 part of sulphate of quinine was added, and the following results obtained on analysis:—

	Present.	Found. I.	Found. II.	Found. III.
Organic carbon in 100,000 parts of water	0·429 part.	0·435	0·442	0·440 part.
Organic nitrogen in ditto	0·050 ,,	0·047	0·048	0·048 ,,

To 100,000 parts of a third similar sample of pure water 0·07786 part of sulphate of quinine was added. On analysis this water yielded the following numbers:—

	Present.	Found. I.	Found. II.	Found. III.
Organic carbon in 100,000 parts of water	0·043 part.	0·047	0·050	0·055 part.
Organic nitrogen in ditto.	0·005 ,,	0·006	0·005	0·006 ,,

The close approximation of the experimental to the calculated numbers is the more striking when it is remembered that the weight of nitrogen *actually determined* in the litre of water used for analysis was, in the last series of experiments, only $\frac{1}{20000}$th of a gramme.

Applied to actual specimens of potable water, the accuracy of the method was tested by the uniformity of results obtained in the following duplicate analyses of the same samples of water:—

		Results of analyses expressed in parts per 100,000. I.	II.
Thames water as supplied to London ...	Organic carbon	0·280	0·285 part.
	,, nitrogen	0·032	0·035 ,,
River Lea water as supplied to London.	Organic carbon	0·231	0·239 part.
	,, nitrogen	0·042	0·042 ,,
Kent Company's water as delivered in London	Organic carbon	0·054	0·056 part.
	,, nitrogen	0·016	0·017 ,,

But as practical illustrations of the trustworthiness of the process, the speaker relied most upon the results of the monthly analyses of the water delivered by the eight metropolitan companies made for the Registrar-General during the last eight years, and embodied in two large diagrams which exhibited, at a glance, the results of nearly 800 separate analyses. One of these diagrams showed, by means of curves, the mean proportions of organic elements (organic carbon and organic nitrogen) in the waters of the Thames and Lea, and compared them with that found in the deep well-water of the Kent Company. It also showed the rate of flow of the Thames nearly opposite Hampton Court Palace, and consequently near the place where the Thames water companies abstract their supplies. This diagram showed how sharply the distinction between these three waters is drawn by the method of analysis. In no instance did the curve representing the average organic impurity in the Thames approach near to that indicating the like impurity in the deep-well water, whilst the curve of organic contamination in the Lea water intersected the Thames curve but thrice, and the deep-well curve only once in eight years; and even these intersections, when closely studied, were found to be striking illustrations of the delicacy of the analytical method.

The second diagram might be regarded as a magnified representation of the first. In it the curve representing the average organic impurity in Thames water was decomposed into five constituent curves showing the organic impurity in the water delivered by each of the five metropolitan water companies which abstract their supplies from the Thames; whilst the corresponding curve of impurity in the River Lea was split into two, one representing the impurity in the New River Company's water, and the other that in the beverage delivered by the East London Water Company. As deep-well water is delivered to London by one company only, the curve representing the minute impurity in this water was the same in both diagrams.

These diagrams demonstrated how faithfully the analytical results recorded, firstly, the well-known superiority of deep well over river water; secondly, the superiority of the water of the Lea to that of the Thames; thirdly, the variations in the three great conditions which govern the intensity of organic contamination in the river waters, viz., heavy floods, small floods when the river is low, and decay of vegetation in autumn; and lastly, the method has shown itself competent to reveal the finer shades of quality in waters drawn simultaneously from the same source, but treated differently by the various companies who manipulate them.

Against these advantages the process of analysis advocated by the speaker involves more trouble and more careful manipulation than are usually bestowed upon what are called "commercial" analyses, and although these drawbacks ought not to be paramount considerations, where such important issues are involved, yet if any other more simple method existed from which trustworthy quantitative information about the organic matter in water could be obtained, the more troublesome process would cease to have a *raison d'être*.

Such a simple alternative method of determining organic nitrogen, but not organic carbon, is now very extensively used by chemists. It is known as the "albuminoid ammonia" method, and depends upon the fact that, by boiling with an alkaline solution of potassic permanganate, most nitrogenous organic bodies are decomposed with evolution of ammonia. From the amount of ammonia so evolved, the proportion of organic nitrogen is calculated. A critical examination of the results obtained by this method conclusively demonstrates that it is incapable of converting into ammonia either the whole, or any definite proportion, of the organic nitrogen of potable waters. Indeed, this is shown not only by the following instances, but also by numerous others in which known quantities of nitrogenous organic matters of known composition were submitted to the process.

Results of analysis expressed in parts per 100,000 :—

Artificial Waters containing Peaty Matter.

	Organic nitrogen by combustion.	Organic nitrogen by albuminoid ammonia process.
Sample No. 1	·068 part	·016 part.
,, No. 2	·042 ,,	·016 ,,
,, No. 3	·076 ,,	·022 ,,
,, No. 4	1·015 ,,	·308 ,,
,, No. 5	1·175 ,,	·422 ,,
,, No. 6	·029 ,,	·011 ,,

Natural Waters.

Chelsea Company's water	·058 ,,	·011 ,,
West Middlesex Company's water	·027 ,,	·012 ,,
Southwark Company's water ...	·061 ,,	·024 ,,
Grand Junction water	·031 ,,	·006 ,,
Lambeth Company's water... ...	·062 ,,	·030 ,,
Artesian well water	·033 ,,	{·003 ,, ·004 ,,
Sea water, No. 1	·217 ,,	·006 ,,
,, No. 2	·134 ,,	·018 ,,

It is almost superfluous to say that any opinion as to the quality of a sample of water, based upon the albuminoid ammonia obtained, must be entirely untrustworthy.

Dr. Frankland summed up with the following conclusions, to which he had been led by the experiments of himself and others :—

1. That the "albuminoid ammonia" process of analysing water affords no evidence whatever of the absolute quantity, either of organic matter, or of organic nitrogen present in potable water.

2. That it does not indicate, even approximately, the relative quantities either of organic matter or of organic nitrogen in different samples of such water.

3. That it affords no indication, either of the presence or of the proportion, of *albuminoid* as distinguished from other nitrogenous organic compounds.

4. That the "combustion" process, though more troublesome, is the only method at present known which affords any trustworthy information respecting the organic matters present in potable waters.

5. That it is the only method which even professes to determine organic carbon in such waters.

6. That the determinations by it of organic carbon and nitrogen are fairly accurate, notwithstanding the very minute quantities of matter dealt with, and that the errors even of a comparatively inexperienced analyst fall far short of the limits which would affect a verdict upon the quality of the water submitted to investigation.

7. That it is the only process which discloses the proportion of nitrogen to carbon in the organic matter of waters, such information being often of prime importance in reference to the origin of the organic matter.

8. That since the improvements which have been made in the mode of evaporating the water to be analysed, the process can now be conducted in any laboratory and with a moderate expenditure of time and labour.

RELATION BETWEEN THE LIMIT OF THE POWERS OF THE MICROSCOPE AND THE ULTIMATE MOLECULES OF MATTER[1]

THE subject which I have selected for my address is the relation between the limit of the powers of the microscope, and the ultimate molecules of organic and inorganic matter. I think I may at all events claim for this question sufficient novelty. Until the last few years the subject could scarcely have been attempted, and even now so many necessary facts are imperfectly known, that nothing more can be done than to fill the gaps with plausible assumptions. This necessarily imparts more or less of a speculative character to some of my remarks; but it appears to me that in his annual address the president of a society cannot do better than endeavour to point out the general bearings of what is already known on some great question, even if for no other object than to prove the need of further inquiry.

Though fully impressed with the imperfect state of our knowledge, yet, even now, the facts are sufficiently definite to indicate, if not to prove, the existence of as wide a world of structure beyond the limit of the power of the microscope, as what has been revealed to us by it is beyond the powers of the unassisted eye.

I propose to divide my subject into three heads—

1. The limits of the power of the microscope.

2. The size of the ultimate molecules of organic and inorganic matter.

3. Conclusions to be drawn from the general facts.

In considering the limits of the power of the microscope, I shall assume that the instrument itself is perfect, and that the invisibility of the objects examined is in no way dependent on the want of the necessary characters. The point to which I particularly wish to direct attention is the limit of visibility depending on the constitution of light, beyond which light itself is of too coarse a nature to allow of our seeing objects distinctly defined. This question has been treated of in an admirable manner by Helmholtz in the jubilee volume of *Poggendorff's Annalen* (1874, p. 573). The conclusion to which he arrives is that the limit depends on the confusion in the image due to the bright interference fringes overlapping the dark outlines of the object. This limit varies directly as the wave-length of the light, and inversely as the sine of half the angle of the aperture of the object-glass when illuminated by means of a condenser of equal aperture. According to this principle the limit for dry object glasses of 60° aperture is, roughly speaking, about equal to the wave-length of the light, and for the largest possible aperture equal to $\frac{1}{2}$ the wave-length. In the case of immersion object glasses of the same aperture, the limit is about $\frac{3}{4}$ of that for dry.

On comparing this theory with the results of observation, the agreement is very striking. It indicates exactly the same law for the increased defining powers of lenses of large aperture, as has been determined by experiment, and gives for the theoretical limit of distinct visibility $\frac{1}{80000}$th part of an inch, which is exactly the same as the mean of the experimental determination of a number of the most skilful microscopists. It also shows why in the case

[1] Anniversary Address of the President of the Royal Microscopical Society, H. C. Sorby, F.R.S., &c. Abstract by the Author.

of lines at equal intervals, like Nobert's bands or the markings on Diatomaceæ, it is possible so to manage the illumination that the dark fringes of interference may coincide with the true lines of structure, and give rise to good definition, even beyond the normal limit, and also agrees with the fact that lines less than $\frac{1}{100000}$th of an inch apart can be *photographed*, though *seen* with extreme difficulty, if indeed truly resolved, except under very peculiar and exceptional conditions; since the waves of light at the blue end of the spectrum, which are concerned in photography, are short enough to give good definition of lines so near together that the interference fringes due to the longer waves at the red end would give an indistinct image. Taking everything into consideration, the agreement between observation and the theory is so close as to make it extremely probable, and to justify the conclusion that the normal limit of distinct visibility with the most perfect microscope is $\frac{1}{2}$ of the wave-length of the light. If so, we must conclude that, even with the very best lenses, except under special conditions, light itself is of too coarse a nature to enable us to define objects less than $\frac{1}{80000}$th or $\frac{1}{100000}$th of an inch apart, according as a dry or an immersion lens is used. We must also conclude that, *as far as this question is concerned*, our microscopes have already reached this limit, whatever improvements may remain to be made in other respects.

2. *The Size of the Ultimate Atoms of Matter.*

After discussing the results obtained by Stoney, Thompson, and Clerk-Maxwell, mainly derived from the properties of gases, I come to the conclusion that in the present state of our knowledge the best approximation that we can make to the size of the ultimate atoms of matter is the mean of their determinations. I adopt for simplicity $\frac{1}{1000}$th of an inch as the unit of length, and $\frac{1}{1000}$th of an inch cube, or $\frac{1}{1000000000}$th of a cubic inch, as the unit of volume. In the case of a true gas the number of atoms in the length of $\frac{1}{1000}$th of an inch at 0° C., and a pressure of one atmosphere, would be 21,770, and hence, in $\frac{1}{1000}$th of an inch cube, about 10,320,000,000,000.

If this gas were a mixture of two volumes of hydrogen and one of oxygen, on combining to form water there would be a contraction to $\frac{2}{3}$, and on condensing into liquid water a contraction to $\frac{1}{770}$ of this; but since the molecules of the resulting liquid would contain three atoms of the gases, the total number of molecules of liquid water in a given volume would be $\frac{2}{3} \times 770 \times \frac{1}{3} =$ 385 times the number of atoms of a gas. This gives for the number of molecules of liquid water in $\frac{1}{1000}$th of an inch cube about 3,900,000,000,000,000.

As an illustration of a far more complex substance I take albumen, and calculating as well as one can from its chemical composition, and from the specific gravity of horn, I come to the conclusion that the diameter of the ultimate molecule of dry albumen is about 3·82 that of the molecules of water, and that $\frac{1}{1000}$th of an inch cube would contain about 71,000,000,000,000.

If such be the case, we must conclude that in the length of $\frac{1}{80000}$th of an inch (the smallest interval that could be distinctly seen with the microscope) there would be about 2,000 molecules of liquid water lying end to end, or about 520 of albumen. Hence, in order to see the ultimate constitution of organic bodies, it would be necessary to use a magnifying power of from 500 to 2,000 times greater than those we now possess. These, however, for reasons already given, would be of no use unless the waves of light were some $\frac{1}{2000}$th part of the length they are, and our eyes and instruments correspondingly perfect. It will thus be seen that, even with our highest and best powers, we are about as far from seeing the ultimate structure of organic bodies as the naked eye is from seeing the smallest objects which our microscopes now reveal to us. As an illustration, I have calculated that with our highest powers we are as far from seeing the ultimate molecules of organic substances as we should be from seeing the contents of a newspaper with the naked eye at the distance of a third of a mile; the larger and smaller types corresponding to the larger and smaller molecules of the organic and inorganic constituents.

3. *General Conclusions to be deduced from the above facts.*

When we come to the application of these principles to the study of living matter we are immediately led to feel how very little we know respecting some of the most important questions that could occupy our attention. As illustrations I do not think I can select better than the facts bearing on the size and character of minute germs, and on Darwin's theory of ultimate organised gemmules.

For the sake of simplicity I will take into consideration only the albuminous constituent of animals, using the term *albumen* in a sort of generic sense. Whatever be the special variety of this substance, it is so associated with water in living tissues, that in most, if not all, cases they would cease to live, if thoroughly dried. Much of the water is no doubt present simply as water mechanically mixed with the living particles; but it appears to me that we ought to look upon some portion as being in a state of *molecular combination*. The existence of such a state of union is clearly proved by the optical characters of various solutions of the same coloured substance. These are by no means such as would result from the mere mechanical diffusion of very minute particles of the solid substance in the liquid, and cannot be explained unless we suppose that the coloured substance is to some extent in the state of molecular combination with the solvent. This molecular affinity is also in some cases manifested by a swelling up of a solid substance when placed in certain liquids, even when perfect solution occurs to a very limited extent. Such a condition appears to be very characteristic of the living tissues of animals, and makes it very probable that the ultimate living particles are molecular compounds with water, and not molecules of free dry albuminous substances. So little is known of the true proportion of water thus combined, that the only course now open is to suppose, for illustration, that living albuminous matter contains half its volume of water mechanically mixed, and one-fourth of free albumen, united with one-fourth of molecularly combined water. On this supposition a sphere of such living matter $\frac{1}{1000}$th of an inch in diameter would contain the following number of molecules:—

Albumen	10,000,000,000,000
Water in molecular combination .	520,000,000,000,000
	530,000,000,000,000

The very small relative amount of dry matter in some living animals makes it probable that the molecularly combined water plays such an important part in their ultimate structure, that we may base our provisional conclusions on this total number of molecules.

Theory of Invisible Germs.

Calculating then from the various data given above, we may conclude that a spherical particle one-tenth the diameter of the smallest speck that could be clearly defined with our best and highest powers, might nevertheless contain no less than one million structural molecules. Variations in number, chemical characters, and arrangement would in such a case admit of an almost boundless variety of structural characters. The final velocity with which such particles would subside in air must be so slow that they could penetrate into almost every place to which the atmosphere has access.

Darwin's Theory of Pangenesis.

Full particulars of this theory will be found at p. 374 of vol. ii. of his work on the variation of animals. He nowhere gives any opinion as to the actual size of gemmules, or the number present in particular cases, but it appears to me interesting to consider how far the theory will hold good when examined from this more physical point of view.

For the sake of argument, I assume that gemmules on an average contain one million structural molecules of albumen and molecularly combined water. Variations in number, composition, and arrangement would then admit of an almost infinite variety of characters. On this supposition it would require a thousand gemmules to be massed together into a sphere, in order to form a speck just distinctly visible with our highest and best magnifying powers. By calculation I find that a single mammalian spermatozoa might contain so many of such gemmules, that, if one were lost, destroyed, or fully developed in each second, they would not be completely exhausted until after the period of one month. Hence, since probably a number are concerned in producing perfect fertilisation, we can readily understand why the influence of the male parent may be very marked, even after having been, as regards particular characters, apparently dormant for many years.

In a similar manner I calculate that the germinal vesicle of a mammalian ovum might contain enough gemmules for one to be destroyed, lost, or fully developed in each second, and yet the entire number not be exhausted until after a period of seventeen years, and the entire ovum might contain enough to last at the same rate for no less than 5,600 years.

These calculations are made on the supposition that the entire mass is composed of gemmules. Of this there is little probability; but still, even if a considerable portion of the ovum consists of completely formed material and of mere nutritive matter, it might yet contain a sufficient number of gemmules to explain all the facts contemplated by the theory of pangenesis. The presence of any considerable amount of such passive matter in spermatozoa would, however, be a serious difficulty in the way of the theory, unless indeed very many spermatozoa are invariably concerned in producing fertilisation.

Taking everything into consideration, it does not appear to me that any serious objection can be raised against pangenesis when examined from a purely physical point of view, as far as relates to the inheritance of a very complex variety of characters by the first generation, though there would have been may serious difficulties to contend with, if the ultimate atoms of matter had been very much larger than is indicated by the properties of gases.

When we come to apply similar reasonings to the second or following generations, we are compelled, along with Darwin, to conclude that gemmules have the power of producing other gemmules more or less closely resembling themselves, and of being collected together in the sexual elements, since otherwise the number that could be transmitted in a dormant state for several generations would be far too small to meet the requirements of the case.

Conclusion.

In my remarks I have made no endeavour to conceal our present ignorance of many very important questions connected with my subject. Want of the requisite data necessarily imparts a speculative character to many of my conclusions; but perhaps there is no more fruitful source of knowledge than to see and feel how little is accurately known, and how much remains to be learned.

THE TUFTED DEER OF CHINA

AMONG the many most valuable additions which Mr. R. Swinhoe has made to our knowledge of Chinese zoology, there are none more important than his discoveries in the deer-tribe. The Water Deer of Shanghai (*Hydropotes inermis*), first described in 1870, is one of the most interesting of these. It is of small size, without horns of any kind, and with long canine teeth present in the males only. In outward appearance it in these respects closely resembles the Musk Deer. Its colour is light chestnut, and the hairy coat is harsh. It is called the *Ke* and the *Chang* by the Chinese. Sir Victor Brooke has demonstrated that its skull differs in important points from that of *Moschus*.

Still more recently, in 1874, Mr. Swinhoe has described another small deer from the mountains near Ningpo, of much the same size as *Hydropotes*; it also resembles that genus in being hornless and possessing large canine tusks in the males. Mr. Swinhoe, in the "Proceedings" of the Zoological Society (1874, p. 452), writes as follows:—"My friend and correspondent, Mr. A. Michie, wrote me a letter dated Shanghai, December 19, 1873, as follows:—'I send another note to overtake the mail, to tell you I have just found a new deer from the Ningpo country. It is a dark iron-grey or pepper-and-salt colour, like some Scotch terriers, with white tips to its ears, square-built (that is, straight back and pointed hip), with very short tail. On its forehead is a thick black mane, like the bristles of a boar. . . . It has the lachrymal sinus, but not so large as the Muntjac; in size the beast about equals the Muntjac.'" An excellent figure accompanies this description. It was drawn from a skin sent by Mr. Michie to Mr. Swinhoe, who has named the animal *Lophotragus michianus*.

A living example of this species, the first ever brought to this country, has just reached the Zoological Gardens in Regent's Park. From this male specimen it can be seen that the drawing above referred to, made from the flat skin, excellently represents the figure of the animal, and is truthful in that it shows the canine teeth and the absence of horns. In the living specimen there is a pair of hair-covered tuberosities on the frontal regions, at the postero-lateral angles of the hairy head-tuft, but, as in the Giraffes, these have no horns upon them. Comparing this condition with that of *Elaphodus cephalophus*, also from China, described by M. A. Milne-Edwards, the intimate relation of *Lophotragus* to the Muntjacs (*Cervulus*) is evident; the series of gradual antler-reduction being in the following order:—*Cervulus*, *Elaphodus*, *Lophotragus*. Whether *Hydropotes*, or *Moschus*, or both are extremes of this series, remains to be proved; and it must be mentioned that it is not perfectly certain, though highly probable, that the above-described individual specimen of *Lophotragus* exhibits its highest degree of antler-development.

NOTES

SOME weeks since it was stated that the collection of fishes made by Mr. Francis Day, Inspector-General of Indian Fisheries, would be deposited in the New Indian Museum at South Kensington. It was offered to and accepted by the Secretary of State for India, but it was subsequently considered that neither the expense of bottles in which to exhibit them, nor of spirit for their preservation, could be rightly debited to the resources of India. Mr. Wood, the well-known artist, very liberally proposed, in exchange for the type collection, numbering about 1,200 species, to increase Mr. Day's plates in his work, the "Fishes of India," from 160 to 190, or to 1,140 figures. The Director of the Indian Museum in Calcutta hearing of this arrangement, proposed to the trustees that he should secure it at once on these terms, and we understand that he has been instructed to do so. It will doubtless render the Museum in Calcutta the most complete in Indian fishes in the world; but whether this collection finding a place in the British Museum might not have proved more beneficial to science we leave for the decision of our readers.

MR. WILLETT has just issued a report on the Sub-Wealden boring, stating that the bore-hole has been widened and lined to a depth of 1,760 feet, and that boring was recommenced on the 8th inst. at a depth of 1,825 feet, when the hard limestone passed into soft shales, and mentions the occurrence of imperfect specimens of ammonites, at 1,849 feet. Geologists will probably not be disposed to agree with Mr. Willett, "that the theory of the presence of a ridge of old rocks north of the English Channel and south of the Thames is no longer tenable," for we believe that no one ever denied that the Wealden axis was "a true anticlinal elevation." In stating that the Cretaceous rocks are the same thickness on both sides of the axis, Mr. Willett appears to forget that the old Palæozoic rocks were contorted and their upturned edges denuded, before the Secondary rocks, much less the Tertiary, were deposited, and in stating that the 1,800 feet of strata, explored by the boring, as well as the overlying beds, were "deposited during a prolonged and continuous subsidence of this part of the earth's surface," we believe few men of science will coincide with him. In the conclusion of his report, he offers personally to bear the cost of any boring in Kent or Sussex, above the Wealden horizon, which may reach the Palæozoic rocks, within a depth of 2,000 feet.

In answer to Dr. Playfair on Monday, Mr. Cross said the recommendations of the Royal Commission for Scientific Instruction and the Advancement of Science had been for some time under the consideration of the Government. With reference to what steps the Government proposed to take in the matter, he would rather not anticipate, he said, the statement which it would be the duty of his noble friend the Vice-President of Council to make on the subject. We look forward with considerable interest to the statement which Lord Sandon has to make, and hope we shall not have long to wait for it.

Mr. Ward Hunt stated last Thursday in the House of Commons that Capt. Nares will send a sledge party down to the entrance of Smith's Sound in the spring of this year, if possible, with despatches, for the chance of a ship from England calling there. The Admiralty have arranged with Mr. Allen Young, who is contemplating a voyage to the Arctic regions this year in his yacht, to look for cairns in which such despatches might be deposited, and he has, with great public spirit, consented to make this the primary object of his voyage, undertaking to bring home any such despatches, unless he can find means for sending them to England otherwise.

A large and influential meeting of the citizens of Glasgow was held on Wednesday, Feb. 16, to make arrangements for the meeting of the British Association on Sept. 6. The Lord Provost presided. The University buildings were granted for the Association meetings. Most of the guarantee fund of 4,000*l.* was subscribed, and resolutions were adopted to extend the hospitality of the city to strangers attending the Association.

The death of the well-known French engineer, M. Thomé de Gamond, is just announced. M. de Gamond is probably best known as the originator of the Channel Tunnel, and he died on the very day on which the Commissioners took the final steps for the completion of that great work. He was born in 1798 at Paris, but was educated in the Netherlands, where he gave great attention to hydrography. His great scheme was remodelled by him many times before it took its final form. It was brought before several International Exhibitions and Commissions, and he published a great number of pamphlets, documents, and books before the scheme gained the favour of those who were able to help him in carrying it out. He was not destined to see the boring actually commenced, but he saw all obstructions and objections removed. M. Thomé de Gamond had also conceived a vast scheme for the improvement of the streams and rivers of France. He proposed to enable the country to utilise the whole of its hydraulic resources, and was anxious to put an end to the immense loss of water which might be used for irrigation and as a motive power. He was modest, energetic, and benevolent.

The death of M. Adolph Brogniart, Professor of Botany and Member of the French Academy of Sciences, was announced in Paris on Saturday.

Prof. Nils Peter Angelin, Intendent of the Palæontological Department of the Riks Museum, Stockholm, and author of "Palæontologia Scandinavica," died at Stockholm on the 13th inst., aged upwards of seventy years.

The death is announced of M. d'Orbigny, Assistant Naturalis at the Natural History Museum, Paris.

Herr von Loeher has recently read a paper before the Munich Academy of Sciences in which he argues that the Guanch or Wandsch population of the Canary Isles, who for more than a century repulsed all invaders, are the descendants of the Vandals. Most of the names of places are barbarian, but some Germanic. Many common expressions are a mixture of both, and names of persons are almost Germanic, as also religious phrases and the titles of public functionaries. Herr Loeher believes that the Vandals or Goths settled in the Isles in the eighth century, finding there a weak barbarian population whom they subjugated, that they gradually lost the use of iron and shipbuilding, and mostly relapsed from Christianity into German heathenism, but, though degenerating in their complete isolation, retained the features and customs of their race in all essential points until their discovery by Europeans. Fair-haired mummies have been found in their tombs, and the dimensions of the skulls agree with those of Germanic races.

In a letter to the Vice-Chancellor of Oxford, Prof. Max Müller expresses his great gratification with the decree passed in Convocation on Tuesday week. "It was solely," he says, "in order to secure the leisure necessary for the completion of my labours connected with the ancient literature of India that I came to the decision to resign my professorship. Now that you have in so generous a way granted me that leisure, I look forward with great satisfaction to spending the remaining years of my life at Oxford, and, if my health be spared, I still hope to be able to prove to the members of the University that they have bestowed this privilege on one not quite unworthy of their confidence."

A magnificent new map of France has recently been completed by the French Engineering Department, and is now being published in sheets. It is on the scale of 1·500000, and must take the place of all previous maps, most of which are, we believe, inaccurate in many important particulars. By means of various easily understood devices, the new map shows all the main natural and artificial features of the country, and by means of its metrical divisions, the distances of places from each other may be observed at a glance.

A very interesting museum will be opened in a few weeks at the village of Castleton, in the Peak of Derbyshire, a place much visited by geologists, &c. It will contain: (*a*) a series of articles of the Bronze and Neolithic periods from Switzerland, Denmark, Cissbury, Yorkshire, with a large number of the prehistoric remains from the tumuli, &c., near Castleton, the result of the explorations of Mr. Rooke Pennington, F.G.S., and Mr. John Tym during the last few years. (*b*) Palæolithic implements. (*c*) A magnificent series of the Pleistocene animals of the Derbyshire district—bison, grizzly bear, reindeer, rhinoceros, hyæna, &c. (*d*) A good geological series, of about 1,500 species of fossils numbering about 3,500 specimens, from Crag to Laurentian, and including a good Ichthyosaurus, Plesiosaurus, Crag and Eocene mammals, some good fish, &c. Most of the formations are well represented, but particularly those prevailing near Castleton (mountain limestone, Yoredale series, and coal measures). (*e*) A series of minerals and rocks, the Derbyshire minerals being specially good; and some educational sets of fossils and minerals. All the above are properly arranged with explanatory notes, so as to be useful to the uninitiated and to teach geological rudiments, whilst affording advanced students opportunity of comparing their "finds" and naming them. (*f*) A series of the fauna and flora of North Derbyshire, including mammals (stuffed), birds, and their nests and eggs, ferns and mosses, &c. (*g*) Collection of old china, entirely obtained from the older houses in the neighbourhood, with old books, ornaments, coins, &c. (*h*) Set of archaic mining tools from the old lead mines of Castleton. (*j*) The natural and commercial productions of the neighbourhood. (*k*) Geological maps and sections, guide-books, and a small scientific reference library. Mr. Pennington's collections are all included in the museum.

THE investigation into the cause of the explosion at the Jabin pit, near Lyons, in France, seems to show that the workmen were not to blame for any imprudence in the use of their lamps, but that the catastrophe was probably produced by the inflammable air escaping from the coal beds by a great diminution of barometric pressure, which reached 10 millimetres in a few hours. This connection of explosions in mines with a diminution of barometric pressure has been frequently referred to recently in connection with explosions in England. The question has been asked whether it is not desirable to extend the system of storm warnings to coal-mining districts; if the miners could only be induced to attend to them there seems no doubt that a great saving of life would be thus effected.

A VALUABLE and in many respects exhaustive memoir on the temperature of the air at Brussels, by Prof. E. Quetelet, based on forty years' observations ending with 1872, appears in Vol. XLI. of the Memoirs of the Royal Academy of Belgium. The paper presents in a more extended and permanent form the leading features of the most important element of the climate of Brussels, which appeared about a year ago in the form of a small tract, briefly reviewed in NATURE at the time (vol. xi., p. 444).

MRS. MARSHALL HALL, sen., writes that the lady who made a successful ascent of Mont Blanc on the 31st ult., mentioned in our last number, was Miss Stratton, a Welsh lady, not an American.

AN apparatus of great delicacy has lately been devised by Dr. Mosso of Turin, for measuring the movements of the blood-vessels in man. A description of it, with figures, appears in *Comptes Rendus* of Jan. 24. The arrangement of the *plethysmograph* (as it is called) consists in enclosing a part of the body, the fore-arm, *e.g.*, in a glass cylinder with caoutchouc ring, filling the cylinder with tepid water, and measuring, by a special apparatus, the quantity of water which flows out or in through a tube connected with the cylinder, as the arm expands or contracts. An opening in the cylinder is connected by a piece of caoutchouc tubing with a glass tube opening downwards into a test tube suspended from a double pulley with counterpoise to which the recording lever is attached, in a vessel containing a mixture of alcohol and water. When the vessels of the arm dilate water passes from the cylinder into the test tube, which is thereby immersed further, so that the counterpoise rises; in the opposite case water flows back from the test-tube into the cylinder, the test-tube rises, and the counterpoise descends. Among other applications of the apparatus, Dr. Mosso employs it in studying the physiology of thought and cerebral activity. The slightest emotions are revealed by the instrument by a change in the state of the blood-vessels. The entrance of a person during the experiment, in whom one is interested, has the effect of diminishing the volume of the fore-arm four to fifteen cubic centimetres. The work of the brain during solution of an arithmetical or other problem, or the reading of a passage difficult to understand, is always accompanied by contraction of the vessels proportional to the effort of thought.

THE Perthshire Society of Natural Science has recently conferred a great benefit on the City of Perth by drawing attention through one of its members, Dr. Lauder Lindsay, to the many imperfections of its water-supply. Perth, as our readers know, stands on the banks of the finest river in the kingdom, and yet its water-supply is lamentably deficient in quantity and quality. The present system of supply was organised about fifty years ago, and Dr. Lindsay brought it to the test of the universally recognised principles of sanitary science, with the result stated. Unfortunately Perth lies very low, and on that very account unusual care must be taken to keep the supply of water pure. After the lesson which Dr. Lindsay has read the inhabitants, it will be their own blame if they do not exercise what would be genuine economy, and remedy a state of matters which must undoubtedly exercise a deleterious influence on the health and prosperity of the fair city. We think this practice of bringing science to bear on matters of local importance is one quite within the sphere of local scientific societies.

THE Meteorological Commission of Allier have now twenty regular meteorological stations at different heights, varying from 686 to 3,773 feet. These stations, together with eighty others for the observation of thunderstorms, have been established for the investigation of the local climates of the department. It is resolved by the Commission, in the interests of general meteorology, to connect its observations as much as possible with those which are collected at Paris.

THE additions to the Zoological Society's Gardens during the past week include a Bay Bamboo Rat (*Rhizomys badius*) from India, presented by Mr. Jas. Wood Mason; an Anderson's Kaleege (*Euplocamus andersoni*) from Burmah, two Hill Francolins (*Arboricola torqueola*) from India, presented by Mr. W. Jamrach; a Sociable Vulture (*Vultur auricularis*), two Cape Francolins (*Francolinus capensis*) from Africa, presented by Mr. J. C. Hobbs; two White-necked Storks (*Ciconia episcopus*) from India, received in exchange; two White-backed Pigeons (*Columba leuconota*) from the Himalayas, a Tiger Bittern (*Tigrisoma brasiliense*), five Geoffroy's Doves (*Peristera geoffroii*) from South America, purchased.

THE INDUSTRIAL APPLICATIONS OF OXYGEN[1]

II.

WE must now direct our attention to a small group of proposals for extracting oxygen from the air by purely mechanical means, without the aid of any chemical action. They are founded on one or other of two physical principles, diffusion or absorption.

T. Graham, whose "inquiries into the laws of the diffusion of gases" will always be remembered as one of the most perfect of his numerous and great researches, observed in 1866[2] that air drawn through a small fissure of a thin india-rubber leaf contains the constant proportion of 41·6 parts of oxygen to 58·4 parts of nitrogen, so that half of the nitrogen of the atmospheric air is kept back, and that this mixture makes red-hot coals burn with a flame. Deville,[3] however, tested this method with regard to its industrial merits, and found that it required too much time to be considered practical.

Endeavours to utilise absorption have been made in two different ways. Messrs. Montmagnon and De Laire obtained a patent in France in 1868 for a process, founded on the observations of Angus Smith,[4] according to which charcoal takes up more oxygen

[1] Translated, by permission of the editor, from the "Report on the Development of Chemical Industry, in conjunction with friends and fellow-workers, by A. W. Hofmann." The present article, as well as the previous one, it should be understood, are by Dr. A. Oppenheim. Continued from p. 295.

[2] Graham, Compt. Rend. lxiii, 471.

[3] Deville, Wagn. Jahresber., 1867, 216.

[4] Bull. Soc. Chim. [2], xi., 261.

than nitrogen from the air. They have found that 1,000 litres of charcoal absorb 925 lit. of oxygen and only 750 lit. of nitrogen. When moistened with water, 350 lit. of oxygen and 650 lit. of nitrogen are given off; so that 575 lit. of oxygen and 55 lit. of nitrogen remain, which may be extracted by means of the air pump. By repeating the same process with this gaseous mixture, they succeeded in obtaining oxygen in almost a pure state. Whether this method was ever employed on an extensive scale is unknown. But this has been the case with Mallet's[1] method, founded on the higher coefficients of absorption of water for oxygen, as compared with nitrogen. The coefficients of absorption of these gases in water are 0·025 for nitrogen, and 0·046 for oxygen. Multiplied by their volumetric proportion in the atmosphere 0·79 and 0·21, these figures yield the proportion in which these gases occur in water = 0·0197 N and 0·0097 O; or, the air absorbed in the water contains in one volume 0·67 N, and 0·33 O. If the non-absorbed nitrogen is now allowed to escape, and the absorbed mixture of the two gases is extracted from the water and submitted a second time to absorption, we shall find by multiplying their coefficients with the numbers just obtained, 0·67 (N) and 0·33 (O), that the mixture now absorbed will contain 0·525 N and 0·475 O. A third absorption will raise this proportion to 0·375 N : 0·625 O; a fourth to 0·25 N : 0·75 O; a fifth to 0·15 N : 0·85 O; that is the same relation in which the gases occur in the mixture ordinarily produced by Tessié du Motay's process. After the eighth absorption, the gas evolved is almost pure oxygen (0·973 O and 0·027 N).

Mallet's apparatus consists of a greater or smaller number of strong iron water-reservoirs, connected by forcing and sucking pumps. Into the first air is pumped through fine openings, at a pressure of about five atmospheres. After this the non-absorbed nitrogen is removed by opening a valve, and then by means of the second forcing and sucking pump the absorbed gas is drawn out of the first vessel and forced into the second. With four vessels a complete operation is performed in five minutes. If the vessels vary in size, decreasing from the first of 10 cb.m. to the fourth of 5 cb.m. in capacity, uninterrupted working will produce a result of 7,760 litres of a mixture containing 75 per cent. of oxygen per hour; or, 168 cb.m. in twenty-four hours. The cost of working and keeping this system in order is said to be trifling, and a small amount of superintendence will suffice if the machine is made automatic. Where working power is cheap, such as water power, or the lost heat of metallurgical processes, these methods might possibly be of use, especially for metallurgical processes themselves, which could be effectually assisted by mixtures containing a smaller proportion of oxygen.

Summing up the practical results of this long list of inventions, we find in the foremost rank the well-established method of Tessié du Motay. The next place is taken by the mechanical method of Mallet, just described; which, however, has not yet met with a practical verification.

We arrive at last at the question, What uses has pure oxygen hitherto served? As the supporter of combustion, we are indebted to it for warmth and light; as a means of respiration, it is the foundation of our lives.

Let us look at it then, from these three points of view. Its metallurgical uses claim our first attention. The important part it has performed in the history of platinum has been already described. We have learned to do without it in lead soldering; hydrogen or coal gas, burnt in air, supplying a sufficient quantity of heat. The example of this industry encourages us to cherish the greatest hopes for its further and wider employment. "Just as gold," says an esteemed metallurgical chemist,[2] "while it was still used in soldering platinum, destroyed its appearance by yellow marks, in the same way white soft solder offends the eye when applied to coloured metals. This unsightliness induced the Society for the Promotion of Industry in Prussia to offer a prize for the discovery of a yellow solder. It would be difficult to solve this question, unless a new easily fusible metal of a red or yellow[3] colour could be discovered. A better chance of success offers in the self-soldering of metals by means of the oxyhydrogen blow-pipe, which has already gained triumphs in the manipulation of two metals of different natures. Is it not possible with this powerful agent, which has succeeded in soldering lead with lead, and platinum with platinum, to solder every other metal and every alloy, just in the same manner; as tin with tin, copper with copper, brass with brass, silver with silver, gold with gold, and even iron with iron?

"The probability of such an innovation exists, and there is no question of the tangible benefits to be derived from it.

"We need only picture to ourselves the neatness of a workshop in which soldering is effected by means of a light, elegant gas burner, instead of the soldering iron or forge; the workman remaining uninjured by the glowing heat, smoke, and vapours, able at any moment, by the turning of a cock, to regulate the supply of heat with the greatest nicety. We need but look at the solidity of a soldering which no longer depends on a foreign substance, but is the actual blending together of the two parts, thus saving material and work, as no filing of the soldered part would be required. Such palpable advantages must silence every prejudice, and give a strong impulse to the setting on foot of thorough and persevering researches on this subject."

Since oxygen has become cheap, however, its use has likewise been recommended in that largest branch of metallurgy, the production of iron and steel.

Cameron[1] advises the use of oxygen or enriched air, as produced by Mallet's absorption cylinders, instead of common air from the bellows for high furnaces; and here it will be well to remember that the absorption of oxygen in water has already accidentally contributed to this result, in a manner which leaves room for improvement. Br. Kerl[2] calls attention to the fact that air from water blowing machines is richer in oxygen than common air. Besides, it has already been observed that charcoal, when stored up, burns with increased vigour, because it has absorbed oxygen from the air, and that this forms a valuable assistance in refining iron.[3]

Kuppelwieser[4] recommends oxygenised air for the Bessemer process, and he is of opinion that the price of Tessié du Motay's method need not be greatly reduced to allow the use of oxygen for this purpose. A great future seems here to dawn on the application of oxygen! Nevertheless, we must not omit Le Blanc's[5] objection, that the necessity of using fire-proof materials would render the economical advantages very questionable.

But turn from the metallurgical application of oxygen to its use for illuminating purposes. The discovery of the oxyhydrogen light by Drummond[6] in 1826, and its employment in surveying and for lighthouses, has destroyed every doubt as to the value of oxygen for these purposes. The reduction of the price of oxygen brought it into wider use. This time America led the way.

H. Vogel[7] found oxygen successfully employed in New York in 1870, not only for lighthouses, signals, and ordinary buildings, but also to illuminate the beds of rivers, for the building of bridges, and for various appliances of the magic lantern. The building of the great Brooklyn Bridge over the East River, then in an early stage of construction, was facilitated by twelve oxyhydrogen lamps, which consumed about 2,000 cubic feet[8] of oxygen daily. Instead of chalk cylinders, the more durable zirconium-cones were employed with great advantage, and in the same way the Théâtre de la Gaîté and the Alcazar, in Paris, were lit up with fairy-like brilliancy. In the opera house in New York, a diagram of about ten square metres was thrown on a screen of damp muslin, the lamp placed behind the stage, producing wonderful effects. The magic lantern has with the help of this light become very popular in lecture rooms, for the projection of apparatus, glass-photographs, and drawings, especially since Outerbridge[9] taught us to draw pictures with pen and ink on thin gelatine plates. The effects are easily explained when we remember that the oxyhydrogen gas yields a light sixteen-and-a-half times stronger than the same quantity of ordinary gas would yield.

The daily amount produced in 1870 by the New York Oxygen Company was 30,000 cubic feet (850 cb.m.) The oxygen is sold in iron cylinders (patent of Robert Grant, New York), nine inches in diameter and thirty inches in length, which are filled with oxygen under a pressure of twenty to thirty atmospheres.

[1] Mallet, Dingl. pol. J. cic., 112, and Philipps on Oxygen, Berlin, 1874, 24 ff.
[2] Clemens Winkler, Deutsche Industrieblätter, 1871, S. 182, and Zeitschr. d. Vereins deutsch. Ingen. xvi. 714.
[3] For which reason the offer of reward has since that time been withdrawn.

[1] Cameron, Berg-u. Hütten Zeitung, 1871, 132.
[2] Br. Kerl, Grundriss der Hüttenkunde, i., 217.
[3] J. pr. Chem. ci. 397; Bergwerks Freund, iii. 513.
[4] Kuppelwieser, Berg-u. Hütten Zeitung 1873, 354.
[5] Le Blanc, Journal f. Gasbeleucthung, 1872, 641.
[6] Drummond, On the means of facilitating the observation of distant stations in geodetical operations. Phil. Trans. 1826.
[7] H. Vogel, Ber. Chem. Society, iii. 901.
[8] In Vogel's Report, cubic metres is printed by mistake.
[9] Morton, Journal of the Franklin Institute, liii. liv. lv. See also Vogel in the passage quoted before

The cylinder is sold at one dollar per cubic foot (35 dols. per cb.m.), including the oxygen it contains under ordinary pressure; the refilling with oxygen costs five cents per cubic foot under the pressure of one atmosphere,[1] a very high price, exceeding the calculation of Kuppelwieser more than twenty-two times. Tessié du Motay tried to apply oxygen to the lighting of streets and public places. The "Places" before the Tuileries and Hôtel de Ville were at that time brilliant with light given off from zirconium-cones under the influence of coal gas and oxygen. The unsteadiness of the flame and its great cost led him to prefer the carburation of hydrogen and of coal gas, by passing the gases through a vessel of heavy hydrocarbons fixed to every lamp before it entered the burner. In this way the Boulevards were illuminated from the Rue Drouot to the Rue Scribe with seventy oxygen burners. But this method was given up, and the preparation of a very heavy gas instead of the usual coal gas was at last resorted to, to be burnt by means of oxygen. In this new form the visitor to the Vienna Exhibition met with it at the railway station, Kaiserin-Elizabeth-Westbahnhof. We are permitted to extract the following description from an unprinted report of Herr Karl Haase, director of the Berlin Gas Company, given before the Berlin Town Council:—

"The appearance of the grounds surrounding the Elizabeth railway station, and of the hall itself, lighted up by a mixture of coal gas and oxygen, is in the highest degree surprising. The effects caused by the little bluish flames are quite peculiar, and cannot be compared with any other light. The green of the trees seems more vivid, the colours of the dresses more brilliant, and, above all, the faces of the people appear clearer, every shade and colour showing almost as distinctly as in full daylight, notwithstanding which the light did not tire the eyes.

"The favourable impression made on entering the grounds is heightened on entering the large second-class waiting-room, where everything, down to the minutest detail of ornamentation, is most distinctly seen by the light of the little flames of only two moderate-sized gaseliers.

"However, the best conception of the new method of lighting is produced in the up-train station-hall. Here, in order to make the comparison more striking, the ordinary platform used by up-train passengers was lighted with heavy gas and oxygen, only half the number of jets being lit as were used on the opposite platform, where the old gas was burning with the aid of oxygen. Notwithstanding the double number of lamps and the good quality of the gas, the space lighted by the new method was incomparably more brilliant. The shadows of the candelabra, and even of the smoky flames, were perceptible on the white walls."

In spite of this favourable impression, Herr Haase comes to the conclusion that the new double gas, conducted in two pipes, is not adapted for general private use, particularly for the following reasons:—"The advantage of its brilliancy is more than counterbalanced by its cost, which in Berlin, taking the usual lighting power as the standard of comparison, would amount to double the price of the ordinary gas: the consumer would not understand the working of the cocks: the oxygen would deteriorate in the long conducting pipes, and the repairs would be expensive, &c. Although for public buildings, for shops, and some other purposes, the new method might answer, it would be impossible to lay down three gaspipes for these limited ends." This opinion stands diametrically opposed to that of Schiele,[2] who warmly recommends the new method of lighting, but it is in close accordance with the opinion stated by Le Blanc,[3] about a year before, in a report to the Town Council of Paris. This report is the result of extensive researches by Messrs. Péligot, Lamy, Troost, De Mondésir, and Le Blanc, appointed a commission for the purpose by the Prefect of the Seine in 1869. They undertook to test the method on the Place de l'Opéra, as well as in the laboratory, by burning with half its volume of oxygen, in separate burners, ordinary coal gas, Boghead gas, and gas saturated with fluid hydrocarbons according to various systems. They came to the conclusion that at equal illuminating powers Tessié du Motay's method is almost always more expensive, mostly twice as expensive, as the usual illumination. Only in one case, where the fluid hydrocarbons of Boghead coal were used for carburation, according to Levêque's method, by saturating wicks with the oil and letting the gas pass over them, the price of the new light appeared twice as cheap as the ordinary method, and that only when large burners were used, and consequently a greater quantity of light was produced.

The calculations were of course founded on the data furnished by the Tessié du Motay Gas Company respecting the prices of oxygen, of the carburation, &c. In truth, however, it appeared that in the last-named experiment 1 cb.m. of gas, instead of taking up 50 gr. of fluid hydrocarbons, as the company pretended, actually absorbed 266 gr.; and thus the economy, to say the least, became very questionable. As to the lighting power, it was possible to increase it so as to form three to seven times the power of ordinary street-burners. But Boghead gas can also produce three times the quantity of light, in suitable burners, without having recourse to pure oxygen, and for general purposes such an intensity of light is not desired; on the contrary, the light is lessened 30 per cent. by globes or shades. The Commission came to the decision, therefore, of advising the Corporation of Paris not to authorise the laying down of oxygen pipes, but rather to leave it to the company to supply oxygen and carburetted coal-gas in portable vessels to the comparatively few who stand in need of such an increased intensity of light.

The results arrived at in Brussels were no more favourable. During the last year lighting by oxygen was tried for a short time in some cafés, as well as in the Passage St. Hubert, and then discontinued[1] on account of the aforesaid objections. In Vienna, in April 1874, the Westbahnhof was still lighted with oxygen, but the system had spread no further; for in spite of its intensity and acknowledged beauty, the bluish moonlike light did not produce anything like a general satisfaction.[2]

The jury of the Vienna Exhibition inspected the oxygen lighting at the Westbahnhof. In the Exhibition building itself, oxygen industry was not represented.

If further experiments confirm the above opinions, the industry of oxygen will have lost the root from which it commenced to grow, because wherever it has sprung up it was fostered by the hope of being employed for illuminating purposes.

Many of the alleged disadvantages, especially the cost of the laying down of pipes, are avoided in the arrangement which Philipps[3] proposed for oxygen lighting. According to this proposition lamps (manufactured by George Berghausen, of Cologne) were to be fed with very heavy tar oil containing naphthalin, whilst oxygen was introduced through the middle of the wick. It is, however, very doubtful if any larger city would renounce the advantages of gas-light in favour of this arrangement, and consequently, if there is any chance of its application on a large scale.

Let us be all the more hopeful that oxygen industry will find its saving ally in metallurgy.

In medicine it has won no friend. Up to the present time there is nothing to contradict Pereira's opinion[4] in spite of many more recent praises of the medicinal powers of oxygen,[5] and we can therefore do no better than quote it anew:—

"Soon after the discovery of oxygen, its therapeutical application was in great favour. The want of a proper supply of oxygen to the body was considered to be the cause of many diseases, such for example as scorbute, and it was asserted to have been used in many cases with brilliant success. Beddoes[6] and Hill employed it in England. The latter declares that he found it useful in cases of asthma, weakness, ulcer, humor albus, and scrofulous bone diseases.

"These opinions have nevertheless been greatly modified on chemical, no less than on physiological grounds. In cases of asphyxia, caused by want of air, or by inhaling noxious gases, the respiration of oxygen may possibly be useful. For this reason it has been administered in cases of asthma threatening suffocation. But as the patients in such cases are scarcely able to inhale it, if it acts at all, it can only act as a palliative, and is decidedly incapable of preventing fresh attacks. In most cases where oxygen has been inhaled it was therefore powerless to help; and from the physical reasons stated above, very little success can be anticipated from its employment.

[1] Deutsche Gewerbezeitung, 1867, 18; see also Vogel.
[2] Schiele, Journal für Gasbeleuchtung, January 1873.
[3] "Rapport de M. Félix le Blanc sur le nouvel éclairage oxhydrique." Paris, 1872. Short extracts in the Journal f. Gasbel, 1872, 641.

[1] Letter dated April 14, 1874, of M. Melsens, Professor of Chemistry in Brussels, to A. W. Hofmann.
[2] Verbal communication of Professor Hlasiwetz.
[3] Philipps on Oxygen. Berlin, 1871, 46.
[4] Pereira, "Art of Healing," Translated into German by Buchheim, i. 217.
[5] Verbal communication of Prof. Dr. Oscar Liebreich.
[6] "Considerations on the use of factious airs, and on the manner of obtaining them in large quantities." By F. Beddoes and J. Watt. Bristol, 1794-95. There was a Pneumatic Institute established in Bristol in 1798, in which the medical properties of gases were tried, and it was here Humphry Davy discovered the effects of protoxide of nitrogen.

"This did not prevent medical speculators from opening an institution in Berlin for inhaling oxygen, where it is now being sold at 7*d*. the cubic foot, while oxygen water is sold at 2*d*. the bottle. As water of 0° does not absorb 4 per cent. of its volume, a half-litre bottle contains less than 20 cb.m., or 0·0017 grammes of this gas! It seems incredible that such a dose should be expected to produce any effect whatever. Just as travellers are recommended to provide themselves with concentrated food, those who wish to climb the highest mountain tops, or by means of balloons reach great heights, where the thinness of the atmosphere might cause them dangerous inconveniences, are advised to use pure oxygen as a concentrated means of respiration.[1] P. Bert[2] exposed himself and others in proper apparatus, to degrees of rarefaction of air, which far surpassed that of the greatest heights ever reached by man. The want of breath and symptoms of suffocation which ensued, when the barometer stood at from 300 to 250 mm., were, according to his account, at once relieved by one breath of pure oxygen. A mixture of the same with atmospheric air proved even more effectual than the pure gas, and, on an aërial voyage which the late MM. Crocé Spinelli and Sivel undertook from Paris on the 22nd of March, 1874, they provided themselves with mixtures containing 45 and 75 per cent. of oxygen to 55 and 25 per cent. of nitrogen. They were enabled by the help of this gas to make valuable physical observations,[3] at heights of more than 6,000 metres, leisurely and without any bodily inconvenience; and although Glaisher had succeeded in reaching still greater heights without this assistance, oxygen offers a means of gaining strata hitherto inaccessible."

These words, however, were scarcely written when the newspapers announced the death of the courageous navigators on a new aërial voyage; suffocation appears to have set in so suddenly as to incapacitate them at once from using their respiratory apparatus.

The physiological applications of oxygen form the bridge to some considerations on the practical uses of ozone, the discovery of which had been greeted by exaggerated hopes.

A. OPPENHEIM.

[1] Fonvielle (La Science en Ballon Paris, 1869), and elsewhere.
[2] Bert, Compt. Rend. 1874, 911.
[3] Compt. Rend. 1874, 946.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, Feb. 17.—Prof. Abel, F.R.S., president, in the chair.—The president announced that Mr. James Duncan had presented the Society with a most life-like and spirited marble bust of Dr. Hofmann. He then called upon Prof. Frankland to deliver his lecture "On some points in the analysis of potable waters." A report of this we give on another page. —A full discussion of the variation in purity of the water supplied during the past eight years by the various London companies followed, illustrated by most excellent diagrams, and the lecturer concluded by pointing out some of the objections to the other well-known processes employed for water analysis.

Zoological Society, Feb. 15.—Prof. Mivart, F.R.S., in the chair.—Mr. Sclater exhibited the parrot called in Tschudi's "Fauna Peruana" *Conurus illigeri*, and observed that it had been certainly wrongly determined. Mr. Sclater was of opinion that the bird belonged to a species hitherto unrecognised, and proposed to call it *Ara couloni*, after M. Coulon, of Neuchatel, who had sent the specimen for exhibition.—Dr. Cobbold, F.R.S., exhibited and made remarks on a Parasite (*Echinorhynchus*), obtained from the Tamandua Anteater, which had died in the Society's menagerie.—Mr. W. K. Parker, F.R.S., read the second portion of his memoir on Ægithognathous Birds.—A communication was read from the Rev. O. P. Cambridge, in which he described a new order and some new genera and species of Arachnida from Kerguelen Island, from specimens collected by Mr. Eaton during the Transit of Venus Expedition.—Mr. G. French Angas communicated descriptions of four new species of land shells from Australia and the Solomon Islands, which he severally proposed to name *Helix moresbyi*, *Helix ramsdeni*, *Helix beatrix*, and *Helix rhoda*. Mr. Angas also made some remarks on the nomenclature of *Helix angasiana* of Pfeiffer, and *Helix bitæniata* of Cox.—Mr. Sclater read some notes, by himself and Mr. Salvin, on some of the Blue Crows of America, taken from specimens lately examined, and pointed out certain changes which it would be necessary to make in the nomenclature of the group adopted in their "Nomenclator Avium Neotropicalium."

Geological Society, Feb. 2.—Mr. John Evans, F.R.S., president, in the chair.—Edward Richard Alston, David Corse Glen, Thomas Vincent Holmes, William G. M'Murtrie, Charles Bine Renshaw, Robert Drysdale Turner, and George Ferris Whidbourne, were elected Fellows of the Society.—Evidence of a carnivorous reptile (*Cynodrakon major*, Ow.) about the size of a lion, with remarks thereon, by Prof. Owen, F.R.S. The specimens described by the author consist of the fore part of the jaws and the left humerus of a reptile obtained from blocks of Triassic (?) rock from South Africa, forwarded by the late Mr. A. G. Bain. The upper jaw displays a pair of enormous canine teeth much resembling those of *Machairodus*, being of a very compressed form, with the hinder trenchant margin minutely toothed. There is no dentated border to the fore part of the crown. No teeth can be detected in the alveolar border of the right ramus of the lower jaw, which extends about an inch behind the upper canine. In the symphysial parts of the lower jaw the bases of eight incisors and of two canines are visible, the latter rising immediately in front of the upper ones, to which they are very inferior in size, and being separated by a diastema from the incisors. In this character, as in the number of incisors, the fossil resembles *Didelphys;* and in structure both canines and incisors resemble those of carnivorous mammals. The left humerus is 10½ inches long, but is abraded at both extremities. It presents characters in the ridges for muscular attachment, in the provision for the rotation of the forearm, and in the presence of a strong bony bridge for the protection of the main artery and nerve of the forearm during the action of the muscles, which resemble those occurring in carnivorous mammals, and especially in the Felidæ, although these peculiarities are associated with others having no mammalian resemblances. The author discusses these characters in detail, and indicates that there is in the probably Triassic lacustrine deposits of South Africa a whole group of genera (*Galesaurus*, *Cynochampsa*, *Lycosaurus*, *Tigrisuchus*, *Cynosuchus*, *Nythosaurus*, *Scaloposauras*, *Procolophon*, *Gorgonops*, and *Cynodrakon*), many of them represented by more than one species, all carnivorous, and presenting more or less mammalian analogies, for which he proposes to form a distinct order under the name of Theriodontia, having the dentition of carnivorous type; the incisors defined by position, and divided from the molars by a large laniariform canine on each side of both jaws, the lower canine crossing in front of the upper, no ectopterygoids, the humerus with an entepicondylar foramen, and the digital formula of the forefoot, 2, 3, 3, 5; 3 phalanges. The author further discussed in some detail the remarkable resemblances presented by these early reptiles, in some parts of their organisation, to mammals, and referred to the broad questions opened out by their consideration. He inquired whether the transference of structures from the reptilian to the mammalian type has been a seeming one, due to accidental coincidence in species independently created, or whether it was real, consequent on the incoming of species by secondary law. In any case the lost reptilian structures dealt with in the present paper are now manifested by quadrupeds with a higher condition of cerebral, circulatory, respiratory, and tegumentary systems, the acquisition of which, the author thought, is not intelligible on either the Lamarckian or Darwinian hypotheses.—On the occurrence of the genus *Astrocrinites* (Austin) in the Scotch Carboniferous Limestone Series, with the description of a new series (*A.? Benniei*), and remarks on the genus, by Mr. R. M. Etheridge, jun. The author, in his introduction to the paper, commenced with a general history of the genus *Astrocrinites* of Austin, commenting upon the change of name it had received from the several authors who had written upon and noticed the species *A. tetragonus* of Austin. In 1843 Major T. Austin described this aberrant Echinoderm under the name *Astrocrinites*, assigning its geological horizon to be the Carboniferous Limestone, and locality Yorkshire. Dr. H. G. Bronn rejected the name *Astrocrinites* on account of its resemblance to *Asterocrinites* of Münster, and proposed instead that of *Zygocrinus*. Römer, from the four-rayed structure of our *Astrocrinites*, allied it to the Cystoidea rather than to the Blastoidea. Prof. de Koninck, and M. le Hon, however, referred *Zygocrinus* to the Blastoidea, and stated their reasons for so doing. Prof. Morris, in 1854, altered Austin's *Astrocrinites* into *Astrocrinus*, and does not notice Bronn's name, *Zygocrinus*. Prof. Pictet provisionally referred the latter genus structurally to *Codonaster*, noticing, however, its four instead of five pseudambulacra. The author then notices at some length the species he proposes to call *A. Benniei*, which appears to differ much from Austin's *A. tetragonus*. The body or calyx of *A. Benniei* is tetraradiate, having four convex lobes, three of which are alike, the fourth differing considerably from the others, the deep re-entering angles between the lobes are occupied by the pseudambulacra, the dorsal surface is densely covered with closely-set tubercles, but shows no point of attachment, the ventral surface is flattened, having a large central aperture, from which radiate the four pseudambulacra; excentric as compared with the ambulacral system is a second and pyriform aperture of complex structure. The component parts are then minutely described, followed by careful descriptions of the pseudambulacra, apertures, and ornamentation, also a discussion as to the presence of a madreporiform tubercle. The second part of the paper treats upon the affinities of *A. Benniei* (Ether.) with *A. tetragonus* (Austin). Part the third enters fully and critically into the systematic position of *Astocrinites* amongst the Cystoidea and Blastoidea. In the concluding and fourth portion of the paper, the localities and geological horizons are given. Twenty-seven figures, occupying three plates, accompanied the paper.—On the genus *Merycochœrus* (family Oreodontidæ), with descriptions of two new species, by Mr. G. T. Bettany, B.A. Communicated by Prof. T. McKenny Hughes. An account was given of remarkable vertebrate tertiary skulls and other remains brought from Upper Oregon by Lord Walsingham, in 1872, and presented by him to the Woodwardian Museum, Cambridge. The characters of the family of Ungulates (Oreodontidæ), to which they belong, and of the genera of the family, were referred to, and supplemented from examination of these remains. The genus *Merycochœrus*, previously known only from teeth and portions of jaws, was further defined and described from large skulls and portions of skulls. The remarkable size of the temporal fossæ, the form of the zygoma, and especially its great posterior transverse crest, are special points of interest. Finally two new species, *M. temporalis* and *M. Leidyi*, were defined and described.

Meteorological Society, Feb. 16.—Mr. H. S. Eaton, president, in the chair.—Frank C. Capel, Zophar Humphreys, Edward Mawley, Rev. George H. Mullins, William H. Watson, and C. Theodore Williams were elected Fellows of the Society. —The following papers were read:—On an improvement in aneroid barometers, by the Hon. R. Abercromby. The improvement consists in jewelling the ends of the arbor of the index hand like the ordinary pivots of a watch, and making the hand work under the cap instead of in the usual manner. The advantages gained are: (1) increased sensitiveness; (2) increased definiteness of the indications; and (3) diminished influence of weather on the bearings.—Meteorology in India in relation to cholera, by Col. J. Puckle, M.S.C. The author in this paper lays before the Society some facts in connection with several serious outbreaks of cholera in different parts of the Mysore country during the last fifteen years, and draws attention to the similarity of the abnormal meteorological conditions that existed on each occasion. Except in a few of the largest towns in India there are no sewers, and no sewer gas proper. Even in these exceptional towns the drainage is incomplete. The general sanitary arrangements are of the most primitive character. In the rural districts the inhabitants adhere to the Mosaic law, in so far that they go forth to the fields, but they do not carry the "paddle" with them for the purpose that was the exponent of the "dry earth" system; *that* necessary portion of the work is left to the drying action of a powerful sun, to the kites and other carrion birds, and, *horribile dictu*, to the pigs and poultry that afterwards are doubtless turned into food. In this way it is not difficult to conceive that sewage of the direst and most unadulterated kind may possibly be taken into the system through poisoned meat, or during rainfall it may find its way to open reservoirs or wells; from which two sources the inhabitants depend for their water supply. At other times during the hot, dry weather, when no rain falls, malaria may arise and be distributed through the agency of the atmosphere. Notwithstanding all that has been said and done, the clue to the mystery of the origin of the disease remains undiscovered. It is the same with the treatment. Remedies that at one time appeared to be most effectual have, at another, most signally failed. Even during the same attack, the same remedy that cured one person would fail in another, even where the same conditions apparently existed. Failure of the usual rainfall at the proper time, and abnormally high and harsh temperature, have been concurrent with several attacks in Mysore and Southern India. At such times the open reservoirs or lakes and wells are much below the usual spring level, and any contamination received at such a time is obviously much less diluted, and more harmful. The author then gives an account of several attacks that have come under his own personal knowledge, which shows beyond doubt that the disease has been arrested by change of air and surroundings, and that ordinary sanitary practice has prevented a possible outbreak. After referring to the recent outbreaks at Bangalore and Madras the author says that everywhere the same story is told of the occurrence of cholera coincident with long absence of rain and a temperature abnormally high.—On sixteen months' rain at Bristol, by W. F. Denning.

Entomological Society, Feb. 2.—Sir Sidney Smith Saunders, C.M.G., vice-president, in the chair.—Mr. McLachlan directed attention to an article by M. Flaminio Baudi in the *Petites Nouvelles Entomologiques*, respecting the habits of *Cychrus cylindricollis*, which he had taken on Monte-Codeno, feeding on the body of a snail (*Helix frigida*), into the spiral of which the beetle was enabled to enter by means of its long prothorax. Some interesting remarks were made by Mr. Bates and others on the peculiar structure and habits of the insect, which appeared to have been found only on a very sterile portion of the plateau of the mountain, and in no other part.—A valuable paper was communicated by Dr. D. Sharp, entitled "Contributions to an Insect-Fauna of the Amazon Valley (Staphylinidæ)." Of this important group of Coleoptera, 487 species were enumerated as inhabiting the valley, of which 463 were described as new; suggesting forcibly how little is really known of the Staphylinidæ of Tropical America. Dr. Sharp also stated that he had devised a method of covering and hermetically sealing the type specimens which, he believed, would accomplish their almost complete preservation, and that he hoped soon to be able to publish a description of the method. The author concluded with remarking on the great importance of certain sexual characters in distinguishing the species.

Institution of Civil Engineers, Feb. 15.—Mr. Geo. Robt. Stephenson, president, in the chair.—The paper read was on estimating the illuminating power of coal gas, by Mr. William Sugg, Assoc. Inst. C.E.

EDINBURGH

Royal Society, Feb. 21.—Prof. Kelland, vice-president, in the chair.—The following communications were read:—On the structure of the body-wall in the Spionidæ, by Dr. W. C. M'Intosh.—On circular crystals, by E. W. Dallas.—Preliminary note on the flame produced by putting common salt in the fire, by C. M. Smith; communicated by Prof. Tait.

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