THURSDAY, JULY 5, 1883

WILLIAM SPOTTISWOODE

ENGLISH science is still staggering under the blow it received last week in the death of the universally respected President of its leading scientific society. The world is always the poorer for the sudden withdrawal from its many activities of a man *sans peur et sans reproche*, but there is always an inner world where the loss is more keenly felt, and in this case it is the turn of the world of science to mourn one who has made her name so honoured while he has made his own so loved. It is not too much to say that the death of William Spottiswoode is felt as a personal loss by every real student of any department of natural knowledge who ever came within his influence or had the opportunity of knowing anything of the pure and earnest nature of the man. As is but natural, those who have been working along those lines of thought—and they are many—which he had made or almost made his own, will feel the loss most keenly, not merely because the so precious sympathy is gone, but because of the swift insight, valuable criticisms, and happy suggestions as to future work always so freely at the disposal of any one who would consult him either in difficulties or success.

We should however entirely fail in our duty and in our estimate of what he has done for science did we lay too great stress either upon the special work which he did himself or that which he in a greater or less degree influenced in the manner we have just indicated. How much he has personally done we stated some little time ago, little thinking, alas! that what we gave as the results he had achieved and the honours which had followed upon them was anything more than an earnest of what was to follow. It has proved to be the full tale, but it is still one which places him high in the ranks of scientific workers. But, as we have said, high as his place in science would be from this point of view, we doubt whether it is on that that the greatest stress must be laid.

Some men of science of first class working power are so constituted that the less interest they take in the general conduct of affairs connected with science or scientific bodies the better. A man of this kind helps the affairs on very little and he loses his own time. Spottiswoode was exactly the opposite of such a man. In council every word he uttered was pure gold, and when we remember that it is now twenty-two years since he began his council work as Treasurer of the British Association, and that it has never been interrupted till the time of his death, we get an idea of his influence on our national scientific activity. No effort was too great for him, no time spent too long, no margin of time too short, if anything worth doing had to be done; the personal force and the personal example were both there; dullards became enthusiasts if doing was in question, while enthusiasts were checked at times when action was impolitic or premature.

It can easily be imagined that so cultured a man with such qualities as those to which we have referred was a large figure in other than scientific activities; and that both on the ground of his own personal merit, and as representing the Royal Society as its President, he was a marked figure in our English society.

Hence it is that the movement in consequence of which his remains are being buried in Westminster Abbey to-day was one not at all confined to the scientific world, nor was the claim embodied in the memorial to the Dean of Westminster made simply on scientific grounds. As remarked in the *Times*, "no more distinguished body of men, none more thoroughly representative of the community, ever united for a similar object." When we consider that their names were obtained within two days, the quickness of the sympathy and the unanimity of the feeling indicated among the most prominent and gifted sections of our society were certainly remarkable.

The Dean's letter granting the prayer of the memorialists is one again which does such honour to Spottiswoode that we give it in this place:—

"I am deeply sensible of the loss which the country has sustained in the death of the President of the Royal Society. The names appended to the weighty memorial which you have just laid before me are sufficient evidence of the widespread desire that the highest public honours should be paid to the memory of one whose peculiar claims have been urged so forcibly. In addition to that memorial, I have this morning received one expressing the same desire, and bearing the signatures of many hundreds of working men, with whom he was brought in daily intercourse. Although in consideration of the limited space yet remaining for interment within the Abbey I should have myself suggested a monument rather than a grave, yet I cannot but assent, after much anxious consideration, to the wish that your memorial expresses. I recognise in the late Mr. Spottiswoode, not merely a man of special scientific attainments, but one who from his interest in and sympathy with all the many branches and departments of scientific knowledge was peculiarly fitted to represent English science in its widest aspect, and who was at the moment of his death the chosen and the honoured President of the Royal Society. I recognise in him also a man of the very highest and most stainless character—one whose great gifts were only equalled by the purity and attractiveness, and, I may be allowed to add, the devoutness and humility, of his daily life. And, not least of all, I feel that in honouring him we are not only honouring one whose name is dear to men of science and of literature, and of eminence in every sphere of public and of social life, but one whose memory will long be treasured by the working classes, to whose highest interests and welfare he was so deeply devoted."

William Spottiswoode then is buried in Westminster Abbey to-day, by the side of his ancestor, an Archbishop of St. Andrews'; and his remains will be followed to the grave by representatives of the scientific bodies and other interests with which he was connected; nor will sympathy for the widow be wanting to fill up the cup of sadness. English science sorrows, and will long sorrow for the heavy loss, but still she is the richer for Spottiswoode's life and work, not least because his life was so good and so pure, and because, as President of the Royal Society, he has set an example which whoever succeeds him will be proud to follow.

It must not be forgotten that the Presidency of the Royal Society is the highest honour which it is in the power of the Fellows of that Society to bestow. How worthily and how well it was bestowed in the case of Spottiswoode is patent to all. A great responsibility,

therefore, now rests upon them, for he upon whom their choice falls will not be merely the representative of English science in London, he will represent it on the Continent and in America; the choice must bear the criticism of scientific men in other lands. EDITOR

SIR EDWARD SABINE

SPOTTISWOODE, round whose grave in Westminster Abbey so many men, great in so many ways, have stood to-day, is not the only President of the Royal Society, and not the only man of science whose loss we have to deplore. While one, however, was cut off in the full tide of his life, and while there seemed to be a rich promise of many years of valuable work in store, the other had far outlived his working powers, and by many years exceeded those of his activity.

A reference to the life-work of Sabine will clearly show how justly his high position and reputation were accorded to him, how nobly he has worked in the cause of science, and how imperishable a record of his life remains in the existence of a whole branch of scientific research, the foundation of which was mainly due to his untiring industry.

Coming of an old family said to be of Italian origin, which early settled in Normandy, and removed thence to our own country, Edward Sabine was born in Dublin on October 14, 1788, being the son of Mr. Joseph Sabine of Tewin. He received his early education at the Royal Military Colleges of Marlow and Woolwich, obtaining a commission as second lieutenant when but fifteen years of age, and receiving his captaincy eleven years later.

Very early in life indeed, his interest became centred in physical science, and especially in magnetism, the study of which he pursued with indefatigable zeal and marked success. The result of his work in this and other fields is to be found in the many papers which issued from his pen. In 1818, six years before Spottiswoode was born, he was elected a member of the Royal Society, and in the same year was appointed astronomer to the expedition under the command of Sir John Ross which left England in search of the North-west Passage. The careful observations which he made whilst with the expedition were of great value. His published papers begin from this date, commencing with a contribution to the *Transactions of the Linnean Society*, on the birds of Greenland, the result of observations made during the voyage; they range from that date down to the year 1872, thus extending over a period of no less than fifty-four years.

During this long period of active work he contributed to the *Transactions* and *Proceedings* of various societies and contemporary magazines upwards of one hundred papers, some of great length and many of considerable value and importance. Although a large number of these deals with the subject of terrestrial magnetism, many other branches of science are included in them, the voluminous nature of his published works being not less remarkable than the wide fields of study over which they range.

A considerable number are to be found in the *Philosophical Transactions*, to which he contributed upwards of forty. To the *Proceedings of the Royal Society* he made numerous contributions during his long association with it; in the *Quarterly Journal of Science* he published twelve papers, in the *Reports of the British Association* we find ten, to the *Philosophical Magazine* he made eight contributions, the remainder of his published works being scattered among the *Edinburgh Journal of Science*, *Journal of the Geographical Society*, the *Proceedings* of one or two foreign societies, and the pages of foreign scientific magazines.

As we have already said, his scientific contributions date from his voyage to the Arctic regions with Sir John Ross in 1818. Next year he again went to the Arctic regions, this time with an expedition under the command of Sir Edward Parry. As the result of his observations there, he made two communications to the Royal Society, published in the *Philosophical Transactions*, dealing, the one with the irregularities observed in the direction of the compass needle consequent upon the attraction of the iron of the ships, the other with the variations of the magnetic needle, and the intensity of the magnetic force during the voyage, and calling attention for the first time to the extreme importance of founding a widely extended series of observations of those strange magnetical disturbances, the origin of which is still mysterious. With this object in view he left England two years later on a long voyage in H.M.S. *Pheasant*, making numerous observations and bringing many new facts to light. At the same time at several equatorial stations on the coasts of Africa and America he made observations with regard to the swinging of the pendulum, with the object of determining the true figure of the earth, publishing the results in the *Philosophical Transactions*. When on the American coast during this voyage he took up amongst other subjects the question of deep-sea temperatures, and in the *Philosophical Transactions* for 1823, he at that early period published a paper on the temperature at great depths in the Caribbean Sea, whilst in the same year his busy pen was giving an account of the barometrical measurement of the height of the Sugarloaf Mountain at Sierra Leone, and the Pico Ruivo in the Island of Madeira. Three years later he published in the *Quarterly Journal of Science* an account of the ocean currents met by H.M.S. *Pheasant* during the voyage from Sierra Leone to Bahia, and thence to New York, in which he records that the Amazon stream was crossed at a distance of 300 miles from the mouth of the river. In this year (1823) he proceeded on another voyage, going this time in H.M.S. *Griper* to Norway, Greenland, and Spitzbergen, to continue his magnetical observations, and to extend the series of pendulum experiments. Whilst at the latter place he again took up the question of barometrical measurement of heights, publishing in the *Philosophical Transactions* for 1824 a comparison of that method of measurement with the trigonometrical determinations. Then in the *Edinburgh Journal of Science* in 1825 he dealt with the presence of the Gulf Stream on the coasts of Europe as determined by his observations in the year 1822, and proceeded to discuss the question of depression over the region occupied by the Stream.

In 1826 an account of his magnetical observations at Spitzbergen appeared in *Poggendorff's Annalen*.

Continuing his pendulum swingings in 1827, he set about determining by direct observation the difference in

he lengths of the seconds pendulum at Paris and London. The results of these experiments were published in a paper of some length which appeared in the *Philosophical Transactions* for 1828. At the same time he also experimented with the object of ascertaining the ratio of the magnetic forces acting on a needle horizontally suspended in London and in Paris. In 1829, in the *Philosophical Transactions*, he wrote on the reduction to a vacuum of the vibrations of an invariable pendulum; and in the *Quarterly Journal of Science* for the same year he gave an account of experiments concerning the force of the earth's magnetism, and on the then recent magnetical observations in Siberia of M. Hanstein. In the *Philosophical Transactions* for 1831 he describes some experiments made with the object of determining the length of the seconds pendulum at Greenwich.

For many years from this date he worked mainly at that science on which he had most deeply set his mark, that of terrestrial magnetism. In 1835, in conjunction with Lloyd, Humphrey, and J. C. Ross, he contributed to the *Reports of the British Association* (of which he was an early and active member, filling the post of General Secretary for twenty-one years) an account of the terrestrial magnetic force in Ireland. In the following year he himself published in the *Reports* of that Association an account of the magnetic force in Scotland. As an indication of his range of subjects we may here remark that at this time he published in *Froriep Notizen* a paper concerning the volcanoes of the Sandwich Islands. Then in 1837 we find him again contributing to the *British Association Reports*, this time a paper on magnetic intensity, dealing with the variations it exhibits at different parts of the earth's surface. He also wrote on the same subject two years later in *Froriep Notizen*, *L'Institut*, and *Quetelet's Mathematical Correspondence*. In 1838 a memoir on the magnetic isoclinal and isodynamic lines in the British Isles appeared in the *British Association Reports*, being prepared from observations made by Prof. H. Lloyd, J. Phillips, R. W. Fox, Capt. J. C. Ross, and the indefatigable Sabine. In 1840 he continued his papers on terrestrial magnetism in the *Philosophical Transactions*, now taking for his subject the consideration of lines of equal inclination and intensity in the Atlantic Ocean, and on lines of magnetic intensity between the Cape of Good Hope and Australia. He added to this series in the following year by contributing an account of the observations made by Capt. Belcher on the west coast of America and adjacent islands, and the new determination of magnetic elements at Otaheite. Writing in 1838 Sabine had so conclusively demonstrated the importance of magnetical observations being made in every part of the globe, that Capt. James Ross was sent with the *Erebus* and *Terror* to make a magnetical survey of the Antarctic regions. Sabine of course accompanied the expedition. In extension of the work of the magnetic observatory which he had established in England, and which was carried on entirely by his influence, Sabine had induced the authorities to promote the establishment of observatories in the colonies. On the voyage out, therefore, not only were numerous observations made, but magnetical and meteorological observatories were founded at St. Helena, the Cape, and Van Diemen's Land, thus permitting a great increase in the number of possible observations, and a consequent more rapid advance of the science which Sabine had so much at heart. These observatories —to our disgrace be it said, some have now been abolished —were placed under the superintendence of Sabine, and at this period a general magnetic survey of the globe was commenced by him under the direction of the Admiralty, although from what has gone before it is easy to see that the initiative of such a gigantic task had come from himself.

In 1842 he yet further added to his contributions to terrestrial magnetism, publishing in the *Philosophical Transactions* an account of observations made during the voyage of the *Erebus* and *Terror* from England to the Cape, and from thence to Kerguelen Island. Then in 1843 he wrote concerning the extension of these observations from Kerguelen Island to Van Diemen's Island, giving an account also of the various observations made in the Antarctic circle itself during the summer of 1840 and 1841, adding in the year following (1844) an account of the observations from June, 1841, to August, 1842, in the same region. In 1844 and 1845 he made contributions to the *British Association Reports* concerning the meteorology of Toronto and Bombay. During 1846 he again made contributions to meteorological literature, discussing the winter storms of the United States, and the cause of the mild winters which occur sometimes in our own country.

With reference to the survey of the globe to which we have referred, we find him next giving an account of a magnetic survey of a considerable portion of the North American continent, and of the southern hemisphere between the meridian of 0° and 125° east, and parallels of − 20° and − 70°. In 1849, in another contribution, he gave a map of the magnetic declination for 1840 in the Atlantic Ocean, between the parallels of 60° N. and 60° S. latitude. In this year it was that Humboldt's *Cosmos*, for the author of which Sabine had a profound admiration, began to be issued in England, being translated by Mrs. Sabine, and edited by her husband, it being completed in 1858. In the year following he became vice-president of the Royal Society, with which he had been so long connected.

The colonial observatories were, as we have said, under the control of Sabine, and remained so for many years. In 1851 and 1852, and again in 1856, he continued his papers on the magnetism of the earth.

It had been observed (first by Lamont) that the mean of the larger magnetic disturbances gave signs of being bound by some law, and of having a definite but long-period variation. Previously to this it had been shown by Schwabe that the number of spots on the surface of the sun increased and decreased in obedience to regular law, the cycle occupying nearly eleven years for its completion. The results of the observations at the colonial observatories led Sabine to the discovery that magnetical disturbances were intimately bound up with this solar spot period; that the connection between them was of such a nature, that a year of large declination coincided with a year of maximum sunspots, whereas those years when the range in declination was small corresponded with years when there were but few spots on the sun. In the same year the same fact was independently determined by Dr. Rudolf Wolf and M. Gautier.

In 1853, at the meeting of the British Association at Belfast, Sabine occupied the presidential chair. In this year he turned to a consideration of the moon's influence on terrestrial magnetism, writing concerning the effect of that body on the magnetic declination at Toronto, St. Helena, and Hobarton; and taking up the subject again in 1856, he then discussed the lunar diurnal variation at Toronto. At a later period, in the *Proceedings of the Royal Society*, he contributed a paper on the lunar diurnal magnetic declination obtained from the Kew photograms. In 1857 he made another contribution to the *British Association Reports*, discussing the amount and frequency of the magnetic disturbances and of the aurora at Point Barrow, on the shores of the Polar Sea. In the *Philosophical Transactions* for the same year he discussed the question of the existence of the decennial period in the solar diurnal magnetic variations and its non-existence in the lunar diurnal variation of the declination at Hobarton, as M. Kreil seemed to think was the case. He then stated, as the result of a re-examination of the question by the light thrown upon it by the Hobarton observations, that he was as entirely convinced of the existence of this period in the former case as he was convinced of its non-existence in the latter.

Continuing the investigation of this subject, he contributed to the *Royal Society Proceedings* for 1859-60 a paper on the solar diurnal variation of the declination at Pekin. In the same volume of the *Royal Society Proceedings* he also wrote concerning the laws of the phenomena of the larger disturbances of the magnetical declination at Kew Observatory. In 1861, at the request of the General Committee of the British Association, he prepared a report on the repetition of the magnetic survey of England. In this year he succeeded Sir Benjamin Brodie in the presidency of the Royal Society, which position he occupied for the next ten years. In the *Philosophical Magazine* for 1862 he entered into a discussion concerning the cosmical origin of terrestrial magnetism. Two years later, both in the *Philosophical Magazine* and the *Proceedings of the Royal Society*, he published a comparison of the most notable disturbances of the declination at Kew and Nertschinsk during 1858 and 1859. During the next few years, notably in 1866 and 1871, records of the magnetical observations at Kew were published by him. The chief work, however, of this period of his life consisted in concluding his contributions to the *Philosophical Transactions* by reports and reductions of the work done during the Antarctic expedition. In a lengthy contribution in 1866 he resumed the discussion and co-ordination of the various observations, continuing and concluding this in another paper, which is to be found in the *Transactions* for 1868. His last contribution appeared in 1872, when he gave a magnetical survey of the North Polar regions to serve as a companion to the survey of the South Polar regions which had already appeared. It was his earnest wish that he might be spared to complete this, but the infirmities of age were then stealing over him, and it is doubtful whether it would ever have appeared had it not been for the able assistance of Captain, now Sir Frederic Evans, the Hydrographer of the Admiralty, assistance which the author gracefully acknowledges in a postscript to the memoir.

From this date the work of Sabine may be said to have ceased. He had resigned the presidency of the Royal Society the previous year, and he now sought to spend the evening of his life in that retirement and rest to which his advanced age and great works so fairly gave him a claim. He had received the Copley Medal of the Royal Society in 1821, and the Royal Medal of the same society in 1849. In 1869 he was made K.C.B. He possessed also the Prussian Order *pour le mérite*, and was either an honorary or corresponding member of many foreign societies. We mention these facts to show that he retired from his active life full of well-earned honours. In 1879 he lost his wife, who for more than half a century was the close companion of his labours. In the history of the Royal Society his name will ever be valued as that of one who, both as member and as President, was ever foremost in guarding its honour and maintaining its dignity, whilst the kindness and courtesy which as President he displayed to all, not excluding the younger members, will be always gratefully remembered.

It is chiefly by his pendulum observations and by his magnetic determinations and reductions that, as may be gathered from what has been said, his name is so well known in science. The degree of accordance which some of the early determinations of the former kind exhibited was so much in advance of what was at that time thought likely, that they were received with incredulity in some quarters. The discussion which Sir George Airy made long ago, in his article on the figure of the earth, published in the "Encyclopædia Metropolitana," of the pendulum observations then available for that purpose, shows how large a share belonged to the labours of Sir Edward (then Captain) Sabine.

His own magnetic observations were marked by his wonted accuracy; and his discussion of the results obtained at the colonial magnetic observatories led to new and unexpected results. The most striking, perhaps, of these was the discovery of the relation between magnetic perturbations and the more or less spotted condition of the sun's surface, to which we have already referred. Dissimilar as are these phenomena, and difficult as it then at least was to imagine any possible cause for a connection between them, subsequent observations have fully confirmed the conclusion at which he arrived, that connected they are, though what the precise nature of the connection may be is still a matter of discussion.

Though from the nature of the case the work was one of compilation rather than of original observation, his determination of the magnetic state of the earth at a particular epoch, with its accompanying maps of the isoclinal, isogonal, and isodynamic lines was most noteworthy. The search for the original authorities and the application of the corrections requisite to render the observed results comparable with one another occupied a long time, and the results, as we have pointed out, appeared in instalments, as the various regions into which as a matter of convenience the earth's surface was divided were successively completed.

The establishment of the colonial observatories, too, was the direct result of his exertions; and his name will go down to posterity as that of the man who more than any other laboured for the proper establishment of the science of terrestrial magnetism, interesting and important

in its scientific aspect, and pregnant with so many benefits to mankind at large.

He was buried on Saturday, his remains being placed beside those of his wife in the family vault at Tewin; the funeral, in accordance with his own wish, being of the simplest character. In addition to the members of his family and private friends, the funeral was attended by the Secretary and Treasurer of the Royal Society, the Hydrographer to the Admiralty, and representatives of the other Government services with which he had been so long connected.

A MINISTER OF PUBLIC INSTRUCTION

WE are a longsuffering, patient people. The call of Luther to those around him to educate their children and make men of them, as well as provide them with arms—a call at once answered in Germany—is only just now being answered among ourselves.

One of the most beautiful and one of the most touching sights in London now, and one which in our view is a standing disgrace to the politicians who have held sway during the last hundred years, is the gradual rising above dingy roofs and millions of chimneys of the red brick Board schools. The children in London at all events are now being educated, and our future masters are receiving the first rudiments of their instruction, and this much more on account of the intention of their fathers to have it for them, than on account of any farseeing policy of those who are popularly supposed to look in any and every direction for anything that may conduce to the well-being of our country. We have at last got a public instruction, and it is already in the air that that instruction will in time be as free as it is now compulsory. It is a heartbreaking thing to look back and think what might have been had these all too recently built schools overtopped the squalid dwellings of the poor a century ago. How much less squalid those dwellings would be now. The monumental and extensive prisons would probably be less occupied in their every cell than they are now, but the well-being of the country, the output of the country would have been greater, and the struggle with penury, and dirt, and crime would have been less.

This is only one aspect of education, but yet it seems that in this country at all events it is the mainspring of public opinion with regard to the general question. The cry—on many grounds the mistaken cry—for technical instruction has grown from the work of the Board schools, it has gone along the same line at a higher level, and it will go on still further. The enormous development of the Government Science and Art Classes will also go on, and to the credit of the late Sir Henry Cole be it said here that he was wiser than the politicians, and his clear sight and singlemindedness influenced the head of the department with which he was connected, so that the quiet, slow work in science and art began in 1851, long before the present notions of the importance of education really began to take root in our land.

Now that compulsory education is in our midst, now that the importance of science and of art to the national industries is universally acknowledged, now that it is recognised that the education of our workmen must no longer be so disgracefully neglected as it has been, it is again suggested that there should be a Minister to look after these matters.

Ten years ago, as it was well put, the Kinderpest was the care of the Government side by side with the Rinderpest. Both were practically on the same level, both were acknowledged to be nuisances, both might require a public department to look after them, and then money would have to be spent. This was quite a sufficient argument with "statesmen" to let things go on in the old harum-scarum way; for the policy of a Government is to keep money in its purse, honestly if it can, but in any case to do so, as if England were a miser, acknowledging no responsibilities, spurning all delights, and wishing to live a sordid life like the burghers, caring only for their dykes and pikes, whom Luther shamed out of their indifference centuries ago.

There has again, this week, been a suggestion made that there should be a Minister of Public Instruction, who should be responsible for the preparedness of the country in this respect, just as the Minister of War is responsible for the preparedness of it in another direction. Sir John Lubbock must be congratulated upon the way in which he brought the motion forward last Friday. It was a mild, pleading story. As long ago as 1856, he pointed out, the late Lord Derby said:—

"It appeared to him well worthy of consideration whether it would not be well to have a Minister, or the head of a department, who should have no other duties to perform, and who should be, in fact, responsible for the education of the people. . . . He had a strong feeling that the institution of a Minister of Instruction was desirable, that the subject should be altogether separated from the Privy Council."

But that did no good. In 1862 there was another resolution put to the House calling on it to affirm that for the education estimates and for the expenditure of all moneys for the promotion of education, science, and art a Minister of the Crown should be responsible to the House. That also did no good. In 1865 a Select Committee was moved for to inquire into the constitution of the Committee of the Council on Education. It was then urged that education and science and art were beginning to be considered of such importance that—

"The great duty of superintending the various branches connected with the Department of Education should be intrusted to some one responsible Minister, some Minister who should be regarded as a State officer of high authority who should have the sole conduct of that department, and be solely responsible."

And that was shelved.

Nine years later, in 1874, the same view was urged, and the present Prime Minister then admitted "that there was much to be said in favour of the general principle that the expenditure of money for the promotion of education in science and in art should be placed under the control of a single responsible Minister." It is true he said this, but he supported the previous question, so that again came to nothing.

Now that education and science are the great things of the day, not only in this but in all countries, England enjoys the proud preeminence of being the only country—civilised country, we know nothing of Timbuctoo—in which there is not a Minister of Public Instruction. It is lamentable, terrible, to read the debate of last Friday,

and to see the way in which the question was discussed. Mr. Gladstone was impressed by the condition of the House at nine o'clock, but it does not appear that he was impressed with anything else; the importance of education, the importance of science, the importance of art, the daily, almost hourly, increasing importance of these things does not seem to have entered into the question. To a large extent it was merely a question of Cabinet convenience and Parliamentary tweedledum and tweedledee. How can there be made room in the Cabinet for a Minister of Public Instruction? Are not the affairs of the Duchy of Lancaster of much greater importance, and would not the recognition of the importance of education make the Cabinet unwieldy and give rise to difficulties in Parliamentary procedure? And then there is the Scotch business that must be looked after first, and so on, and so on. Education is evidently not in the region of practical politics.

Heaven knows changes sufficiently great have been made of late years, and it is not absolutely certain that the fundamental bearings of the nature of the changes to be made have in all cases been fully considered; but it seems as though they are to be most carefully considered before any change is made touching the matter of education.

Still it is acknowledged that the question is, after all, one that deserves the attention of Parliament, but Mr. Gladstone had, as usual, three objections to make. In the first place he expressed very great doubt whether, if he had a plan ready to alter the present arrangement, it would be wise to make any declaration on the subject by way of motion. Secondly, he admitted that there was no plan, and he did not think the time had arrived for one; and lastly, he considered that the subject ought to be a great deal more examined before the House committed itself to a final opinion whether there should be a plan or not.

With reference to his first objection he stated that the House knew perfectly well that administrative changes are made piecemeal, and must continue to be so; and he remarked that there was a good deal to be said in favour of what was called a patched house, because most of us found it the most comfortable sort of house to live in. A Minister of Public Instruction would be a new patch, and as there is patching going on elsewhere he objects to this; and so on and so on.

The argument which he used in favour of the second objection was, we imagine, the strongest he could have used against it, namely, that the business of the Council Office in respect to education has been in an almost incessant state of flux and change. Of this there can be no doubt that the flux and change will get more pronounced as time goes on. That is the very reason why everything should be brought to a focus.

We may gather from Mr. Gladstone's speech that the Universities should ever, in his opinion, remain divorced from the general question of education; but if so, what is to become of Prof. Huxley's ladder from the gutter to the university? We think, too, if Mr. Gladstone had been fully informed on the subject he could have urged as an additional objection that a great many questions referring to education are never now touched by the Education Department at all.

Several of the speeches might, if we had more space at our disposal, be noticed at some length. Still, we think it worth while to cull the following from the speech of Mr. Forster, an old Vice-President of the Committee of the Council on Education:—

"The Committee of the Council for Trade, or Agriculture, or Education meant nothing whatever. Persons might imagine that the Privy Council occasionally met for the transaction of business, but they never did so either in England or Ireland. The Minister for Agriculture was the President of the Committee of the Council on Agriculture, but he greatly doubted whether that Committee ever met, or ever would meet. . . . The real objection (to Sir John Lubbock's proposal) probably was that it was undesirable to make too much of education, that if we were to have a Minister of Education he might be pushing things on too quickly. . . . There might be a fear that under one Minister too much money would be spent. . . . What was complained of now was that there was no really defined responsibility. The man who moved the estimates and did the work was not the head of a department, and he ought to be. The work was done by a Minister who was controlled by another, and the latter was scarcely seen by the public. He did not see why we should continue that Japanese mode of managing affairs."

It is satisfactory to see that the House of Commons is gradually getting into a better position to discuss such questions as these, but we have felt that the main point is, that the head of the Government does not yet consider that the question of education is one of an importance sufficient to be discussed side by side with what in his opinion is the much larger questions of Parliamentary procedure, and the saving of so many pounds, shillings, and pence. It is true a Select Committee has been agreed to, but we fear that after Mr. Gladstone's speech very little will come of it, as has happened before.

It would be ungraceful not to state that the debate brought out in the clearest possible way the valuable services rendered under great difficulties by the present Vice-President of the Committee of the Council on Education, Mr. Mundella.

But the result remains that we are not to have a Minister of Education. There is agricultural business, including the Rinderpest, and other matters, and these are larger questions than that of national education! Therefore national education must wait. As we said before, we are a longsuffering and patient people. There is, however, little doubt that in some political programme of the future this question will find a place; equal electoral districts and the payment of members are not the only things to be cared for. F.R.S.

EVOLUTION AND CREATION

A Few Words on Evolution and Creation; A Thesis maintaining that the World was not made of Matter by the Development of one Potency, but by that of Innumerable Specific Powers. By Henry S. Boase, M.D., F.R.S., &c. (London: John Leng and Co., 1882.)

Notes on Evolution and Christianity. By J. F. Yorke. (London: Kegan Paul, Trench, and Co., 1882.)

THE first of these works is, as may be inferred from its title, a most curious production. The chief aim of its author is that of sustaining the Biblical Cosmology against what he regards as the fallacious inroads of the theory of Evolution. In carrying out his design he

devotes the first part of his book to a general criticism of the Evolution theory, and the second part to a consideration of the first chapters of Genesis, which he regards as justifying his view that the world "was made by the development of innumerable specific powers." Our readers must not suppose from this form of expression that Mr. Boase seeks to develop a system of Polytheism; on the contrary he is a Monotheist of the most orthodox type, and by his "innumerable specific powers" means only the properties with which matter has been endowed by its Creator. This, at least, is the only meaning which we have found ourselves able, after a somewhat hasty perusal of his book, to attach to this term, which constitutes the core of his "thesis." But if this is his meaning we fail to appreciate the speculative importance which he somewhat ostentatiously attaches to his opinions. For the great distinction which he draws between these opinions and those which are held by evolutionists consists, as he says, in their making "no assumption of an unknown matter endowed with an imaginary all-becoming potency." But so far as physical causation is concerned the two statements amount to exactly the same thing; the only difference between them is the old and well-worn distinction between theism and non-theism—viz. as to whether the observed "potencies" of matter are or are not God-endowed. We cannot see that Mr. Boase has contributed anything new to this question, and therefore regard his work as lost labour. There is a simplicity about some of his remarks which appeals to us as almost pathetic. For instance:—"From my point of view, the occurrence of some of the same kinds of organisms in the rocks of adjoining formations may arise from the remains of the older rocks being transported into the newer formations, or from the older organisms being created anew as a part of the more recent series. . . . It may here be noticed that such an alternate destruction and reproduction of living creatures is set forth in the civ. Psalm—'Thou takest away their breath, they die, and return to their dust. Thou sendest forth Thy spirit, they are created; and Thou renewest the face of the earth.' This verbal coincidence is curious, but, of course, cannot be adduced to prove that the doctrine of Creation and science are in accord with one another." If the "point of view" in question is to be thus calmly attributed to "science," the concluding sentence of this passage is one of the very few in the book with which we are able cordially to agree.

Again, speaking of the creation of Eve, our author remarks:—"It may also have served as an occasion for the important lesson, that the Lord God was the Creator of these living creatures (animals), for immediately after this, God gave Adam a practical proof of His power to create a living being." Practical proof, no doubt, but we should have thought almost more startling than could have been justified for the purpose suggested. Seriously, however, the absurdity which such passages as these display might be amusing from the mouth of a street-preacher; from a man of cultivation they are, as we have said, pathetic.

The other work which we have to notice stands in every way at the opposite pole of thinking from the one which we have just considered. For the object of Mr. Yorke is to show that the principles of evolution are alone sufficient, without any hypothesis of supernaturalism, to explain the origin and development of Christianity. The book is, therefore, mainly of an historical character, and although its views cannot fail to be obnoxious to orthodox opinion, the temperate manner in which they are stated ought everywhere to commend the approval of good taste. Moreover, whatever his readers may severally be inclined to think of his arguments, they can scarcely fail to agree that Mr. Yorke has written a highly interesting book. His object being, as already stated, to trace in the antecedents of Christianity the natural causes of its rise and progress, he has given a selection of quotations from the Jewish writings about the time or shortly before the commencement of the Christian era, and also of the Buddhistic writings long before it, in order that a just estimate may be formed of the extent to which the world is indebted to Christ as a moral reformer. In our opinion Mr. Yorke has shown a sound critical judgment in making this estimate. On the one hand, he is careful to sift out all the elements of the moral teaching which were, so to speak, in the air at the time when Christ taught; and, on the other hand, he is equally careful to distinguish the points wherein the "originality of Christ" was shown. Here we meet with what appears to us a more full appreciation of this "originality" than is shown by most of the other and some of the more eminent writers of the same school.

We shall conclude this notice by quoting two brief passages, one to show the high development of moral feeling which obtained among the Jews immediately anterior to the teaching of Christ, and the other to show the degradation of moral feeling which now obtains in the Roman Catholic ministry of the Christian Church.

"Wear mourning for the Egyptians, suppress the prayer of glorification on the seventh day of the Passover. It is the anniversary of the day when your enemies perished in the Red Sea, and God desires not to be glorified because his creatures have been drowned beneath the waves."

In painful contrast to the singular beauty of this passage, our other quotation is selected from several pages in the same strain which are republished by Mr. Yorke from two pamphlets written expressly for children by a Reverend Father, whose name is, with a singular appropriateness, Mr. J. Furniss. As Mr. Yorke remarks, the Rev. Father Furniss evidently feels that in these degenerate days "Hell is not pictured vividly enough for purposes of practical terrorism, and has accordingly done his imaginative best to supply this great want. And he deserves every credit for his work, for anything better calculated to drive a sensitive child mad with fright it would be impossible to conceive."

After describing the "*Dress of Fire*," and the "*Red-hot Floor*," in one of which there is represented a girl of eighteen, and in the other a girl of sixteen with the Devil taunting their agonies, "The Sight of Hell" goes on to describe—

"'*The Red-hot Oven.*'—See! it is a pitiful sight. The little child is in this red-hot oven. Hear how it screams to come out. See how it turns and twists itself about in the fire. It beats its head against the roof of the oven. It stamps its little feet upon the floor of the oven. You can see on the face of this little child what you see on the face of all in Hell—despair, desperate and horrible."

The only corrective of immoral publications of this description is to be found in reproducing them before public opinion of another kind from that of the unfortunates whose eyes alone they are intended to meet; and it is partly this consideration that has led us to review Mr. Yorke's essay, which, although excellent in itself, is hardly in close enough contact with natural science to demand notice in these pages.

GEORGE J. ROMANES

OUR BOOK SHELF

Iconographie der schalentragenden europäischen Meeresconchylien. Von Dr. W. Kobelt. 4to. Heft 1. (Cassel: Theodor Fischer, 1883.)

THE object of this work is to supply a want which is continually felt by conchologists, and it deserves the greatest success. Dr. Kobelt is well known to science as the editor of the *Jahrbucher* and *Nachrichtsblatt der deutschen Malakozoologischen Gesellschaft*, which has now been published for between fifteen and sixteen years, and as one of the editors of the new *Conchylien-Cabinet* of Martini and Chemnitz; and he is also the author of several works and papers on conchological subjects. It appears from the prospectus of the present work that its scope will be confined to the coasts of Europe, including the English Isles, the Faroes, and Scotland, and bounded by the north coast of Africa, but excluding not only tropical and subtropical species of Mollusca, but those Arctic species from Spitzbergen and the north of Iceland which are not found on the coasts of Upper Norway. This scope, although extensive, is not very definite; and it scarcely accords with our usual notion of the European seas. We do not know what may be the author's limit of depth, whether it is the line of soundings or 100 fathoms; nor whether he will even take the Mollusca now about to be published from the *Triton* cruise between the Faroes and Scotland. The expeditions of the *Josephine*, *Lightning*, *Porcupine*, *Challenger*, *Vöringen*, *Travailleur*, *Washington*, *Knight Errant*, and several others, have of late years done much to aid in the exploration of the European seas at various depths; and the number of species thereby added to the Mollusca has been very considerable and is still increasing. Some additions have likewise been made from time to time to the Mediterranean Mollusca, especially by myself during the present month. Taking into account all these discoveries, I am inclined to reckon the number of species hitherto described as inhabiting the littoral zone and moderate depths in the European seas as not less than 1000; probably 1200 would be nearer the mark.

The first part of the present work, which has now appeared, gives figures of four species only and their varieties, one of which species (*Murex gibbosus*) is Senegalese, and has never (to the best of my knowledge and belief) been found in any part of the European seas. This reduces the number of figured species to 3. Perhaps the species will not be so profusely illustrated in the next and following parts. The published prospectus does not give any idea of the extent of the work. But assuming even that twenty species (large and small) may on an average be figured in each part, the entire work would take not less than from fifty to sixty parts, and would cost for an uncoloured copy 10*l.* to 12*l.*, and for a coloured copy 15*l.* to 18*l.* If all the species known to inhabit the European seas, including the abyssal and benthal zones, are to be figured—and I think this ought to be done—the extent and cost of the publication must be increased by probably a fourth more.

However, such calculations have doubtless been considered by the author or his publisher. The work will assuredly be far more scientific and valuable than the very irregular but expensive *Conchologia Iconica* of the late Mr. Reeve, and be not merely an "ouvrage de luxe."

The family *Muricidæ*, which is the first selected for publication, does not seem to be placed in the usual order of classification. All the figures are admirable. The descriptions are in Latin, the text in German. The geographical, hydrographical, and geological distribution, as well as the odontophore and synonymy, are carefully worked out.

J. GWYN JEFFREYS

LETTERS TO THE EDITOR

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

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

Sand

I HAVE recently been favoured with a reprint of Mr. J. G. Waller's paper upon sand, read before the Quekett Microscopical Society. The subject is so full of interest that I trust I may be allowed to give it a wider publicity in your columns. To render the study of practical use to geologists and physicists, the first step appears to me to ascertain whether it is possible to distinguish with certainty, by aid of the microscope, sand that has been worn by action of wind from sand that has been for long exposed to surf, and this again from sand brought down by torrents. The degree of rounding and the average size of the grains would be, I presume, among the chief characteristics, and it is to be hoped that naturalists abroad will kindly forward examples of undoubted blown and torrential sand, so that this point at least may be settled.

If it should prove that the origin of sand can be pronounced upon with any degree of certainty, from a microscopical examination, we should come into possession of a most valuable aid to the study of at least Tertiary geology. It is well known that marine and freshwater deposits succeed each other repeatedly throughout our Eocene formations, and where deposits of sand are in juxtaposition, it is at present impossible to draw any line between them. It is only possible to surmise that they are of different origin and therefore age, when pebble or oyster beds on the one hand, or films of clay with plant impressions on the other, are accidentally included in them. So far as our own Eocenes go, it appears from Mr. Waller's results that their sands, when of marine origin, possess a percentage of flint grains, but that purely fluviatile sands do not possess any. Marine and freshwater sands are in direct contact in very many of our Eocene sections, and I hope Mr. Waller's researches will enable us to distinguish them and apportion the proper thickness to each.

With regard to the relative rarity of flint-grains and preponderance of quartz, in all the Tertiary and recent sands hitherto examined, it appears just possible that the concussion the flint grains must undergo when beaten for ages in the surf, might induce a molecular change from the colloid to crystalline state, but in the absence of any fact or argument to support such a theory it cannot be seriously entertained. It is however possible that quartz grains reach a final state of subdivision, and then suffer relatively little by attrition, and are therefore almost indestructible, while flint grains become rapidly degraded into mud. This appears to be very much the opinion Mr. Waller has formed. It does seem at first sight matter for surprise that the grinding of flint should not more largely affect the composition of our sea sand; but we must on the other hand reflect on the indestructible nature of the quartz grains that chiefly compose it, that it may have been accumulating since palæozoic times, and the enormous bulk of the quartzose rocks that must have been ground down to supply it during such vast ages, and then compare the sources with flints which in comparison only appeared yesterday, and then but as scattered segregations in a limited portion of a single formation. Flints and flint beaches, recent and ancient, are at our gates, and are continuously renewed by the wearing away of the chalk of which so much of our part of England is composed, and their aggregate mass therefore astounds us; but they after all occur over only a

limited area, and mostly in unconsolidatad beds, and it is quite probable that they would not outlast the destructive influences to which they are subjected if these were continued throughout a geological period. The coast-line occupied by flint-shingle is almost limited to portions of Western Europe, and is relatively insignificant. J. S. GARDNER

Science Club

The Great Comet *b* 1882

M. RAOUL GAUTIER, of Geneva, has recently published, in *Astronomische Nachrichten*, No. 2519, three sets of elements of this comet, calculated from a few observations before perihelion. He says that, as it is possible to represent with the same curve, either a parabola or an ellipse, the nearest observations before and after perihelion, he believes "que si la comète a subi une perturbation dans son mouvement lorsqu'elle a passé à son périhélie, cette perturbation a dû être insensible."

As I am not so far advanced with my calculations, for I have begun a thorough discussion of the movement of that comet, I do not know whether there has been or not any considerable perturbation during the passage near the sun; but can the simple fact alluded to by M. Gautier give us much information on that point?

In fact, we can easily understand that although the orbit after perihelion might be quite different from the orbit before that point, still the positions of the comet at a short distance from perihelion may be pretty well represented, within the limits of the errors of observations, by a single curve, which of course will be of second order, but which will not certainly give the calculated positions of the comet at a certain distance from perihelion agreeing with the observations. If we could prove that the orbits calculated, for instance, from observations between September 7 and 12 and between the 22nd and the 30th of the same month agree together, and give the positions of the comet immediately before and after perihelion according to the observations, then we could say that the movement of the comet was not perturbated during the passage near the sun. But this fact is not proved at all, and instead it seems that the passage through the corona has had some effect upon the movement of that remarkable comet. E. RISTORI

13, Pembridge Crescent, Bayswater, June 16

THIS comet was visible here with the naked eye up to February 28. I saw it myself on the evening of that day. Owing partly to cloudy weather, partly to moonlight, I had not seen it for ten days or a fortnight previously, but found it on that evening with little difficulty and without any optical help. In my telescope (4-in.) it appeared, roughly, like a long, flat-sided, oval nebula, the central part of the major axis being the brightest of the whole. Two cloudy evenings intervened, and on the following night (March 3) I could not see it with the naked eye, even after finding it with the telescope and knowing exactly where to look, and though the optical condition of the air seemed the same. During April I saw it, with the same telescope, on sixteen evenings, cloudy weather and moonlight interfering on the others. In the present month (May) I saw it five times, that is, up to the 6th certainly, and I believe I saw it on the 9th, but decreasing visibility and increasing moonlight prevented verification. I have just received a somewhat larger instrument (5-in.), with which after the moon has passed I hope to see it again. A. S. ATKINSON

Nelson, N.Z., May 19

Sun Pillar seen in Jamaica

AT sunset on May 15 I saw for the first time in my life the phenomenon called the *Sun Pillar*. A few days later the mail-packet arrived from England, and in NATURE I found much correspondence on its appearance on April 6 at several places in England and Wales between Hull and St. David's.

Major Gibney's admirable description of its general appearance on April 6 (vol. xxvii. p. 605) was so fully confirmed on May 15 in Jamaica that a very brief description may here suffice.

At 6h. 30m. p.m. Kempshot mean time it appeared as a bright ray of light of a faint roseate hue, 2° in width and 30° in height above the horizon, vertical, but not passing through the sun. A rough sketch was made at the time, and the circles of the equatorial were afterwards employed to determine the azimuth of the point where the pillar cut the horizon. This was 70° from the north towards the west; and as the sun's azimuth was 69° at the same time, the pillar passed 1° to the west of the sun. In the sketch the pillar is represented as passing its own breadth to the west of the sun, but as the sun was then just below the horizon the former measure is likely to be more correct.

Now with regard to the nature of the phenomenon, it certainly was not the usual display of the zodiacal light. The light is here seen to perfection; every fine night when there is no moonlight the zodiacal light may be seen following the ecliptic from the one horizon to the other with but little variation, except perhaps as to the *gegenschein* or stronger illumination near the point in the heavens diametrically opposite to the sun. And so clearly is it seen, that some years ago I carefully measured its breadth at different distances from the sun, and so formed the following table:—

Ang. dist. from Sun.	Breadth of Z.L.	Ang. dist. from Sun.	Breadth of Z.L.
°	°	°	°
30	41·4	110	20·3
40	38·7	120	17·8
50	36·1	130	15·3
60	33·4	140	13·0
70	30·7	150	10·8
80	28·1	160	8·9
90	25·5	170	7·6
100	22·9	180	7·0

From various considerations based upon the figure corresponding to these measures I consider the zodiacal light a terrestrial phenomenon—rays of light are swept back from the sun, chiefly from the tropical parts of the earth, and tend to accumulate at the point in the heavens diametrically opposite the sun.

If there be any truth in this theory, the sun pillar may be a strong and comparatively local development of the same light; this is the only explanation I can give; the explanation given by Mr. G. J. Symons, the well known meteorologist, "that it is merely a portion of a halo passing through the sun" (vol. xxviii. p. 7), will not apply to the Jamaica observation at all; the sky was far too pure and transparent at the time, and there was not the least trace of *cirrus* cloud. MAXWELL HALL

Kempshot Observatory, Jamaica, June 7

Error in Hutton's Tables of Logarithms

AT the end of Hutton's "Mathematical Tables" (new edition, 1858, Longmans and Co., London) there is a very useful table containing the logarithms of certain constants frequently used in calculation. The tropical revolution of the earth in days is there given as 365·24226, and the logarithm of this most important constant is given as 2·5625910 instead of 2·5625810.

I would be glad to know from any of your readers whether there are any other important errors in this edition, especially among those tables of logarithms in frequent use.

Jamaica, June 4 MAXWELL HALL

Palæozoic Sclerotic Plates

IN the course of my researches among the coal shales of Northumberland I discovered two specimens of osicular rings known as sclerotic plates. The external diameter of one ring is five-eighths of an inch, and the orbital orifice is one-quarter of an inch; this ring of sclerotic plates consists of nine bones arranged as are the eye bones of Ichthyosaurus, Plesiosaurus, and eagles, viz. in tolerably uniform segments. The second specimen is a quadrant of a ring, and consists of six plates of larger size than the other specimen. I shall be glad to learn if any of your readers have discovered similar sclerotic plates in the Palæozoic rocks of the British Isles, as specimens are not exhibited in the British Museum, Jermyn Street Museum, or Edinburgh Museum. T. P. BARKAS

Newcastle-on-Tyne, June 25

Graft-Hybridisation

ST. PAUL, in his Epistle to the Romans, says (ch. xi. v. 17), in illustration of the admission of the Gentiles to the religious privileges of the children of Israel, "If thou, being a wild olive, wert grafted in among them, and didst become partaker with them of the root of the fatness of the olive tree," &c. Olshausen, in his commentary on this epistle, says (English translation, p. 369),

"Whereas, according to the image in this place, the wild branches are ingrafted into the generous tree, reversing the usual process by which good branches are grafted into wild trees, we are informed by both ancient and modern writers that such a process is practicable in this very tree, the olive, and is often practised in the East. Compare Columella 'De Rebus Rusticis,' *v. g.*" Can this be confirmed? It seems scarcely credible. The question bears on the subject of graft-hybridisation, about which many curious facts are collected in Darwin's work on "Variation under Domestication." JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 29

Wild Duck and Railways

LAST autumn I visited Canada and made a journey to the extreme west point (then reached) of the Canada Pacific Railway, on which three or four thousand men were at the time employed laying down the rails on the prairie, at the very rapid rate of three or four miles a day, or more than twenty miles a week. There are many ponds and lakelets along the track, which abounded at that season with a variety of ducks—mallard, teal, widgeon, &c.—usually very shy, and not easily approached by the sportsman. Yet these ducks had in the short space of from two to five days become so accustomed to the noisy and (to most of the birds) novel engine moving along, that they remained sitting quietly in the water within easy shot when the train was passing. On my return journey I was sorry to find that this confidence on the part of the ducks was taken advantage of by the conductor and other wretched sportsmen (?) who shot at the poor birds from the platform of the cars whilst in motion, although when they did *kill*—I am glad to say there were more misses than hits—they could not stop to pick up the game. A sportsman, to get equally near to the ducks as they permitted the train to approach them, had to use the cover of the long grass and some artful dodging to attain his object. This quick intelligence on the part of the ducks seemed to me something remarkable, as the senses both of sight and hearing must have been, one would suppose, at first alarmingly affected by the great noisy, smoking monster rushing along their favourite and hitherto usually silent haunts. JOHN RAE

4, Addison Gardens, June 30

Large Hailstones

A SEVERE thunderstorm passed over Woodlesford, six miles south-east of Leeds, between 3.10 and 4 p.m. this afternoon, proceeding from south-west to north-east. Flashes of lightning during that time were almost continuous. At 3.15 heavy rain began to fall, becoming so thick at 3.25 as to render objects a short distance away almost indistinct; at 3.30 this changed to hail, the stones during the worst period being generally irregular parallelopipeds of ice, with two edges of about one inch each, and the third of one-quarter of an inch. These blocks consisted of hard, colourless, transparent ice, surrounding a central, irregularly-shaped mass of opaque white, small air-bubbles of roughly ellipsoidal shape being ranged round this. The white nucleus was not quite so hard as the exterior transparent coating. The force of collision on the railway line was sufficient to make the masses bound to a vertical height of two or three feet. At 3.45 the hail had moderated, when a few light loose clouds were observed quickly passing from north-east to south-west, and thus directly opposite to the direction of the storm, and at a much lower level. R. WEBB

June 30

Extinction of Flatfish

I HAVE been advised by Mr. Murray of the *Challenger* expedition to inquire, through your columns, whether the experience of any of your correspondents coincides with mine as to the gradual failure—in some places almost the extinction—of flatfish where whelk-gathering is prosecuted. MALCOLM MCNEILL

The New Club, Edinburgh, June 30

Garfish

IN March last I was being pulled off from the shore to H.M.S. *Himalaya* in the harbour at Aden, when a fish jumped out of the water over the boat, and in doing so struck the hat of another officer and knocked it into the water. When the hat was recovered we found in the *hard* felt a slit about four inches in length. Unfortunately the fish escaped, but the impression of those who saw it was that it was some kind of garfish, and that the damage done was inflicted by the beak. It appeared to me to be about ten inches long. It is obvious that had the fish struck my friend in the face or neck, or even in the chest, it might have resulted in a fatal injury. S. ARCHER

Sheerness, June 29

The "Spirogyra quinina"

CAN any of your readers inform me of any practical method of exterminating, in a lodge or reservoir, confervoid algæ, more especially the fine filamentous variety *Spirogyra quinina*?

Hanley, Staffordshire, June 19 FREDK. HAIGH

ACTION OF LIGHT ON INDIARUBBER

IN continuation of the experiments described in NATURE, vol. xxvii. p. 312, two pieces of caoutchouc tube, about 48 mm. long and 7 mm. wide, were introduced on January 23, 1883, into test tubes containing oxygen confined over mercury. One of these tubes was surrounded by a case of black paper, and both tubes were placed side by side in a north window. On June 27 the tubes were examined: in that exposed to light about 17 cc. of oxygen (about three-quarters of the gas the test tube at first contained) had been absorbed, and the indiarubber had become altered, so that on pressing the tube between the fingers superficial cracks were produced. In the other test tube no appreciable diminution of gas had taken place, and the caoutchouc was unchanged, thus fully confirming the results of the former experiments. We may therefore conclude that caoutchouc alters under the combined influence of light and oxygen, but that neither alone produces any effect.

Cooper's Hill, June 29 HERBERT MCLEOD

ON WHALES, PAST AND PRESENT, AND THEIR PROBABLE ORIGIN[1]

II.

THOUGH the early stages by which whalebone has been modified from more simple palate structures are entirely lost to our sight, probably for ever, the conditions in which it now exists in different species of whales, show very marked varieties of progress, from a simple comparatively rudimental and imperfect condition, to what is perhaps the most wonderful example of mechanical adaptation to purpose known in any organic structure. These variations are worth dwelling upon for a few minutes, as they illustrate in an excellent manner the gradual modifications that may take place in an organ, evidently in adaptation to particular requirements, the causation of which can be perfectly explained upon Darwin's principle of natural selection.

In the Rorquals or fin-whales (genus *Balænoptera*), found in almost all seas, and so well known off our own coasts, the largest blades in an animal of 70 feet in length do not exceed 2 feet in length, including their hairy terminations; they are in most species of a pale horn colour, and their structure is coarse and inelastic, separating into thick, stiff fibres, so that they are of no value for the ordinary purposes to which whalebone is applied in the arts. These animals feed on fish of considerable size, from herrings up to cod, and for foraging among shoals of these creatures the construction of their mouth and the structure of their baleen is evidently sufficient. This is the type of the earliest known extinct forms of whales, and it has continued to exist, with several slight modifications, to this day, because it has fulfilled one purpose in the economy of nature. Other purposes for which it was not

[1] Lecture delivered at the Royal Institution on the evening of Friday, May 25, 1883, by Prof. Flower, LL.D., F.R.S., P.Z.S., &c. Concluded from p. 202.

sufficient have been supplied by gradual changes taking place, some of the stages of which are seen in the intermediate conditions still exhibited in the Megaptera, and the Atlantic and Southern Right Whales. Before describing the extreme modifications in the direction of complexity, I may mention, to show the range at present presented in the development of baleen, that there has lately been discovered in the North Pacific a species called by the whalers the Californian Gray Whale (*Rachianectes glaucus*), which shows the opposite extreme of simplicity. The animal is from 30 to 40 feet in length; the baleen blades are only 182 on each side (according to Scammon) and far apart, very short (the longest being from 14 to 16 inches in length), light brown or nearly white in colour, and still more coarse in grain and inelastic than that of the Rorquals. The food of these whales is not yet known with certainty. They have been seen apparently seeking for it along soft bottoms of the sea, and fuci and mussels have been found in their stomachs.

In the Greenland Right Whale of the circumpolar seas, the Bow-head of the American whalers (*Balæna mysticetus*), all the peculiarities which distinguish the head and mouth of the whales from other mammals have attained their greatest development. The head is of enormous size, exceeding one-third of the whole length of the creature. The cavity of the mouth is actually larger than that of the body, thorax and abdomen together. The upper jaw is very narrow, but greatly arched from before backwards, to increase the height of the cavity and allow for the great length of the baleen, the enormous rami of the mandibles are widely separated posteriorly, and have a still further outward sweep before they meet at the symphysis in front, giving the floor of the mouth the shape of an immense spoon. The baleen blades attain the number of 350 or more on each side, and those in the middle of the series have a length of ten or even twelve feet. They are black in colour, fine and highly elastic in texture, and fray out at the inner edge and ends into long delicate, soft, almost silky, but very tough hairs.

How these immensely long blades depending vertically from the palate were packed into a mouth the height of which was scarcely more than half their length, was a mystery not solved until a few years ago. Capt. David Gray of Peterhead, at my request, first gave us a clear idea of the arrangement of the baleen in the Greenland whale, and showed that the purpose of its wonderful elasticity was not primarily at least the benefit of the corset and umbrella makers, but that it was essential for the correct performance of its functions. It may here be mentioned that the modification of the mouth structure of the Right Whale is entirely in relation to its food. It is by this apparatus that it is enabled to avail itself of the minute but highly nutritious crustaceans and pteropods which swarm in immense shoals in the seas it frequents. The large mouth enables it to take in at one time a sufficient quantity of water filled with these small organisms, and the length and delicate structure of the baleen provides an efficient strainer or hair sieve by which the water can be drained off. If the baleen were, as in the Rorquals, short and rigid, and only of the length of the aperture between the upper and lower jaws when the mouth was shut, when the jaws were separated a space would be left beneath it through which the water and the minute particles of food would escape together. But instead of this, the long, slender, brush-like ends of the whalebone blades, when the mouth is closed, fold back, the front ones passing below the hinder ones in a channel lying between the tongue and the bone of the lower jaw. When the mouth is opened their elasticity causes them to straighten out like a bow that is unbent, so that at whatever distance the jaws are separated, the strainer remains in perfect action, filling the whole of the interval. The mechanical perfection of the arrangement is completed by the great development of the lower lip, which rises stiffly above the jaw-bone, and prevents the long, slender, flexible ends of the baleen being carried outwards by the rush of water from the mouth, when its cavity is being diminished by the closure of the jaws and raising of the tongue. The interest and admiration excited by the contemplation of such a beautifully adjusted piece of mechanism is certainly heightened by the knowledge that it has been brought about by the gradual adaptation and perfection of structures common to the whole class of animals to which the whale belongs.

Few points of the structure of whales offer so great a departure from the ordinary mammalian type as the limbs. The fore-limbs are reduced to the condition of simple paddles or oars, variously shaped, but always flattened and more or less oval in outline. They are freely movable at the shoulder-joint, where the humerus or upper-arm bone articulates with the shoulder-blade in the usual manner, but beyond this point, except a slight flexibility and elasticity, there is no motion between the different segments. The bones are all there, corresponding in number and general relations with those of the human or any other mammalian arm, but they are flattened out, and their contiguous ends, instead of presenting hinge-like joints, come in contact by flat surfaces, united together by strong ligamentous bands, and all wrapped up in an undivided covering of skin, which allows externally of no sign of the separate and many-jointed fingers seen in the skeleton.

Up to the year 1865 it was generally thought that there was nothing to be found between this bony framework and the covering skin, with its inner layer of blubber, except dense fibrous tissue, with blood-vessels and nerves sufficient to maintain its vitality. Dissecting a large Rorqual, 67 feet in length, upon the beach of Pevensey Bay in that year, I was surprised to find lying upon the bones of the fore-arm well-developed muscles, the red fibres of which reached nearly to the lower end of these bones, ending in strong tendons, passing to, and radiating out on, the palmar surface of the hand. Circumstances then prevented me following out the details of their arrangement and distribution, but not long afterwards Prof. Struthers of Aberdeen had an opportunity of carefully dissecting the fore-limb of another whale of the same species, and he has recorded and figured his observations in the *Journal of Anatomy* for November, 1871. He found on the internal or palmar aspect of the limb three distinct muscles corresponding in attachments to the flexor carpi ulnaris, the flexor profundus digitorum, and the flexor longus pollicis of man, and on the opposite side but one, the extensor communis digitorum.[1] Large as these muscles actually are, yet, compared with the size of the animal, they cannot but be regarded as rudimentary and being attached to bones without regular joints and firmly held together by unyielding tissues, their functions must be reduced almost to nothing. But rudimentary as the muscles of the Fin-whales are, lower stages of degradation of the same structures are found in other members of the group. In some they are indeed present in form, but their muscular structure is gone and they are reduced in most of the toothed whales to mere fibrous bands, scarcely distinguishable from the surrounding tissue which connects the inner surface of the skin with the bone. It is impossible to contemplate these structures without having the conviction forced home that here are the remains of parts once of use to their possessor, now, owing to the complete change of purpose and mode of action of the limb, reduced to a condition of atrophy verging on complete disappearance.

The changes that have taken place in the hind-limbs are even more remarkable. In all known Cetacea (unless

[1] The muscles of the forearm of an allied species, *Balænoptera rostrata*, were described by Macalister in 1868, and Perrin in 1870.

Platanista be really an exception) a pair of slender bones are found suspended a short distance below the vertebral column, but not attached to it, about the part where the body and the tail join. In museum skeletons these bones are often not seen, as, unless special care has been taken in the preparation, they are apt to get lost. They are, however, of much importance and interest, as their relations to surrounding parts show that they are the rudimentary representatives of the pelvic or hip bones, which in other mammals play such an important part in connecting the hind-limbs with the rest of the skeleton. The pelvic arch is thus almost universally present, but of the limb proper there is, as far as is yet known, not a vestige in any of the large group of toothed whales, not even in the great Cachalot or Sperm Whale, although it should be mentioned that it has never been looked for in that animal with any sort of care. With regard to the Whalebone Whales, at least to some of the species, the case is different. In these animals there are found, attached to the outer and lower side of the pelvic bone, other elements, bony or only cartilaginous as the case may be, clearly representing rudiments of the first and in some cases the second segment of the limb, the thigh or femur, and the leg or tibia. In the small *Balænoptera rostrata* a few thin fragments of cartilage, embedded in fibrous tissue attached to the side of the pelvic bone, constitute the most rudimentary possible condition of a hind-limb, and could not be recognised as such but for their analogy with other allied cases. In the large Rorqual, *Balænoptera musculus*, 67 feet long, previously spoken of, I was fortunate enough in 1865 to find attached by fibrous tissue to the side of the pelvic bone (which was sixteen inches in length) a distinct femur, consisting of a nodule of cartilage of a slightly compressed, irregularly oval form, and not quite one inch and a half in length. Other specimens of the same animal dissected by Van Beneden and Prof. Struthers have shown the same; in one case, partial ossification had taken place. In the genus *Megaptera* a similar femur has been described by Eschricht; and the observations of Reinhardt have shown that the Greenland Right Whale (*Balæna mysticetus*) has not only a representative of the femur developed far more completely than in the Rorqual, being from six to eight inches in length and completely ossified, but also a second smaller and more irregularly formed bone, representing the tibia. Our knowledge of these parts in this species has recently been greatly extended by the researches of Dr. Struthers of Aberdeen, who has published in the *Journal of Anatomy* for 1881 a most careful and detailed account of the dissection of several specimens, showing the amount of variation to which these bones (as with most rudimentary structures) are liable in different individuals, and describing for the first time their distinct articulation one with the other by synovial joints and capsular ligaments, and also the most remarkable and unlooked-for presence of muscles passing from one bone to the other, representing the adductors and flexors of mammals with completely developed limbs, but so situated that it is almost impossible to conceive that they can be of any use; the whole limb, such as it is, being buried deep below the surface, where any movement, except of the most limited kind, must be impossible. Indeed, that the movement is very limited and of no particular importance to the animal was shown by the fact that in two out of eleven whales dissected the hip-joint was firmly anchylosed (or fixed by bony union) though without any trace of disease. In the words of Dr. Struthers, "Nothing can be imagined more useless to the animal than rudiments of hind-legs entirely buried beneath the skin of a whale, so that one is inclined to suspect that these structures must admit of some other interpretation. Yet, approaching the inquiry with the most sceptical determination, one cannot help being convinced, as the dissection goes on, that these rudiments really are femur and tibia. The functional point of view fails to account for their presence. Altogether they present for contemplation a most interesting instance of those significant parts, rudimentary structures."

We have here a case in which it is not difficult to answer the question before alluded to, often asked with regard to rudimentary parts, Are they disappearing or are they incipient organs? We can have no hesitation in saying that they are the former. All we know of the origin of limbs shows that they commence as outgrowths upon the surface of the body, and that the first-formed portions are the most distal segments. The limb, as proved by its permanent state in the lowest Vertebrates, and by its embryological condition in higher forms, is at first a mere projection or outward fold of the skin, which, in the course of development, as it becomes of use in moving or supporting the animal, acquires the internal framework which strengthens it and perfects its functions. It would be impossible, on any theory of causation yet known, to conceive of a limb gradually developed from within outwards. On the other hand, its disappearance would naturally take place in the opposite direction; projecting parts which had become useless, being in the way, would, like all the other prominences on the surface of the whales, hair, ears, &c., be removed, while the most internal, offering far less interference with successful carrying on the purposes of life, would be the last to disappear, lingering, as in the case of the Greenland Whale, long enough to reveal their wonderful history to the anatomist who has been fortunate enough to possess the skill and the insight to interpret it.

Time will not allow of more illustrations drawn from the structure of existing Cetacea; we turn next to what the researches of palæontology teach of the past history of the order. Unfortunately this does not at present amount to very much. As is the case with nearly all other orders of mammals, we know nothing of their condition, if they existed, in the mesozoic age. Even in the cretaceous seas, the deposits at the bottom of which are so well adapted to preserve the remains of the creatures which swam in them, not a fragment of any whale or whale-like animal has been found. The earliest Cetaceans of whose organisation we have any good evidence, are the Zeuglodons of the Eocene formations of North America. These were creatures whose structure, as far as we know it, was intermediate between that of the existing suborders of whales, having the elongated nasal bones and anterior position of the nostrils of the Mystacocetes, with the teeth of the Odontocetes, and with some characters more like those of the generalised mammalian type, than of any of the existing forms. In fact *Zeuglodon* is precisely what we might have expected *a priori* an ancestral form of whale to have been. The remarkable smallness of its cerebral cavity, compared with the jaws and the rest of the skull, so different from that of modern Cetaceans, is exactly paralleled in the primitive types of other groups of mammals. The teeth are markedly differentiated in different parts of the series. In the anterior part of both jaws they are simple, conical, or slightly compressed and sharp pointed. The first three of the upper jaw are distinctly implanted in the premaxillary bone, and so may be reckoned as incisors. The tooth which succeeds, or the canine, is also simple and conical, but it does not greatly exceed the others in size. This is followed by five teeth with two distinct roots and compressed pointed crowns, with denticulated cutting edges. It has been thought that there was evidence of a vertical succession of the molar teeth, as in diphyodont mammals, but the proof of this is not quite satisfactory. Unfortunately the structure of the limbs is most imperfectly known. A mutilated humerus has given rise to many conjectures; to some anatomists it appears to indicate freedom of motion at the elbow-joint, while to others its characters seem to be those of the ordinary Cetacea.

Of the structure of the pelvis and hind limb we are at present in ignorance.

From the middle Miocene period fossil Cetacea are abundant, and distinctly divided into the two groups now existing. The Mystacocetes, or Whalebone Whales, of the Miocene seas were, as far as we know now, only *Balænopteræ*, some of which (as the genus *Cetotherium*) were, in the elongated flattened form of the nasal bones, the greater distance between the occipital and frontal bone at the top of the head, and the greater length of the cervical vertebræ, more generalised than any now existing. In the shape of the mandible also, Van Beneden, to whose researches we are chiefly indebted for a knowledge of these forms, discerns some approximation to the Odontocetes. Right Whales (*Balæna*) have not been found earlier than the Pliocene period, and it is interesting to note that instead of the individuals diminishing in bulk as we approach the times we live in, as with many other groups of animals, the contrary has been the case, no known extinct species of whales equalling in size those that are now to be met with in the ocean. The size of whales, as of all other things whose most striking attribute is magnitude, has been greatly exaggerated; but when reduced to the limits of sober fact, the Greenland Right Whale of 50 feet long, the Sperm Whale of 60, and the Great Northern Rorqual (*Balænoptera Sibbaldii*) of 80, exceed all other organic structures known, past or present. Instead of living in an age of degeneracy of physical growth, we are in an age of giants, but it may be at the end of that age. For countless ages impulses from within and the forces of circumstances from without have been gradually shaping the whales into their present wonderful form and gigantic size, but the very perfection of their structure and their magnitude combined, the rich supply of oil protecting their internal parts from cold, the beautiful apparatus of whalebone by which their nutrition is provided for, have been fatal gifts, which, under the sudden revolution produced on the surface of the globe by the development of the wants and arts of civilised man, cannot but lead in a few years to their extinction.

It does not need much foresight to divine the future history of whales, but let us return to the question with which we started, What was their probable origin?

In the first place, the evidence is absolutely conclusive that they were not originally aquatic in habit, but are derived from terrestrial mammals of fairly high organisation, belonging to the placental division of the class,—animals in which a hairy covering was developed, and with sense organs, especially that of smell, adapted for living on land; animals, moreover, with four completely-developed pairs of limbs on the type of the higher vertebrata, and not of that of fishes. Although their teeth are now of the simple homodont and diphyodont type, there is much evidence to show that this has taken place by the process of degradation from a more perfect type, even the fœtal teeth of Whalebone Whales showing signs of differentiation into molars and incisors, and many extinct forms, not only the Zeuglodons, but also true dolphins, as the Squalodons, having a distinct heterodont dentition, the loss of which, though technically called a "degradation," has been a change in conformity to the habits and needs of the individuals. So much may be considered very nearly if not quite within the range of demonstrated facts, but it is in determining the particular group of mammals from which the Cetacea arose that greater difficulties are met with.

One of the methods by which a land mammal may have been changed into an aquatic one is clearly shown in the stages which still survive among the Carnivora. The seals are obviously modifications of the land Carnivora, the Otaria, or Sea-Lions and Sea-Bears, being curiously intermediate. Many naturalists have been tempted to think that the whales represent a still further stage of the same kind of modification. So firmly has this idea taken root that in most popular works on zoology in which an attempt is made to trace the pedigree of existing mammals, the Cetacea are definitely placed as offshoots of the Pinnipedia, which in their turn are derived from the Carnivora. But there is to my mind a fatal objection to this view. The seal of course has much in common with the whale, inasmuch as it is a mammal adapted for an aquatic life, but it has been converted to its general fishlike form by the peculiar development of its hind-limbs into instruments of propulsion through the water; for though the thighs and legs are small, the feet are large and are the special organs of locomotion in the water, the tail being quite rudimentary. The two feet applied together form an organ very like the tail of a fish or whale, and functionlly representing it, but only functionally, for the time has I trust quite gone by when the Cetacea were defined as animals with the "hinder limbs united, forming a forked horizontal tail." In the whales, as we have seen, the hind-limbs are aborted and the tail developed into a powerful swimming organ. Now it is very difficult to suppose that, when the hind-limbs had once become so well adapted to a function so essential to the welfare of the animal as that of swimming, they could ever have become reduced and their action transferred to the tail;—the animal must have been in a too helpless condition to maintain its existence during the transference, if it took place, as we must suppose, gradually. It is far more reasonable to suppose that whales were derived from animals with large tails, which were used in swimming, eventually with such effect that the hind-limbs became no longer necessary, and so gradually disappeared. The powerful tail, with lateral cutaneous flanges, of an American species of Otter (*Pteronura sandbachii*) or the still more familiar tail of the beaver, may give some idea of this member in the primitive *Cetacea*. I think that this consideration disposes of the principal argument that the whales are related to the seals, as most of the other resemblances, such as those in the characters of their teeth, are evidently analogous resemblances related to similarity of habit.

As pointed out long ago by Hunter, there are numerous points in the structure of the visceral organs of the Cetacea far more resembling those of the Ungulata than the Carnivora. These are the complex stomach, simple liver, respiratory organs, and especially the reproductive organs and structures relating to the development of the young. Even the skull of Zeuglodon, which has been cited as presenting a great resemblance to that of a seal, has quite as much likeness to one of the primitive pig-like Ungulates, except in the purely-adaptive character of the form of the teeth.

Though there is, perhaps, generally more error than truth in popular ideas on natural history, I cannot help thinking that some insight has been shown in the common names attached to one of the most familiar of Cetaceans by those whose opportunities of knowing its nature have been greatest—"Sea-Hog," "Sea-Pig," or "Herring-Hog" of our fishermen, *Meerschwein* of the Germans, corrupted into the French "Marsouin," and also "Porc-poisson," shortened into "Porpoise."

A difficulty that might be suggested in the derivation of the Cetacea from the Ungulata, arising from the latter being at the present day mainly vegetable feeders, is not great, as the primitive Ungulates were probably omnivorous, as their least modified descendants, the pigs, are still; and the aquatic branch might easily have gradually become more and more piscivorous, as we know from the structure of their bones and teeth, the purely terrestrial members have become by degrees more exclusively graminivorous.

One other consideration may remove some of the difficulties that may arise in contemplating the transition of land mammals into whales. The Gangetic Dolphin (*Platanista*) and the somewhat related *Inia* of South

America, which retain several rather generalised mammalian characters, and are related to some of the earliest known European Miocene forms, are both to the present day exclusively fluviatile, being found in the rivers they inhabit almost up to their very sources, more than a thousand miles from the sea. May this not point to the freshwater origin of the whole group, and thus account for their otherwise inexplicable absence from the Cretaceous seas?

We may conclude by picturing to ourselves some primitive generalised, marsh-haunting animals with scanty covering of hair like the modern hippopotamus, but with broad, swimming tails and short limbs, omnivorous in their mode of feeding, probably combining water plants with mussels, worms, and freshwater crustaceans, gradually becoming more and more adapted to fill the void place ready for them on the aquatic side of the borderland on which they dwelt, and so by degrees being modified into dolphin-like creatures inhabiting lakes and rivers, and ultimately finding their way into the ocean. Here the disappearance of the huge Enaliosaurians, the *Ichthyosauri* and *Plesiosauri*, which formerly played the part the Cetacea do now, had left them ample scope. Favoured by various conditions of temperature and climate, wealth of food supply, almost complete immunity from deadly enemies, and illimitable expanses in which to roam, they have undergone the various modifications to which the Cetacean type has now arrived, and gradually attained that colossal magnitude which we have seen was not always an attribute of the animals of this group.

Please to recollect, however, that this is a mere speculation, which may or may not be confirmed by subsequent palæontological discovery. Such speculations are, I trust, not without their use and interest, especially when it is distinctly understood that they are offered only as speculations and not as demonstrated facts.

THE AMERICAN OBSERVATIONS OF THE ECLIPSE

NEWS of the American observations of the last eclipse has now arrived, and although details are yet wanting, enough information has been sent to show us that, as was to be expected, the American observers have left their mark upon the work. The telegram given below has been forwarded to me by the editors of *Science*, and is one transmitted by Prof. Holden to Prof. Young on the arrival of the former at San Francisco :—

"*San Francisco, Cal., June* 11

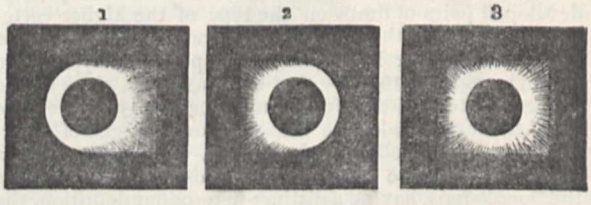
"American Eclipse Expedition arrived at St. Francisco June 11. Holden reports no Vulcan as bright as 5½ magnitude. Hasting's observations prove the corona to be largely a phenomenon of diffraction by the great change in lengh of 1474 line on east and west sides of sun. No black lines in corona spectrum but D. Full observations with grating spectroscopes, prismatic telescope, and integrating spectroscope, by Rockwell, Upton, and Brown. Contacts by Preston. English and French parties successful. (Signed) E. S. HOLDEN"

It will be seen from the above that the spectroscopic attack was a very strong one, and although the telegram gives only the results of the work of Prof. Hastings, these are of unusual interest. I propose, therefore, to devote attention to them in the present notice. It will, however, be well to anticipate my remarks by a prefatory notice of the eclipse work on which it throws light. For this purpose I can scarcely do better than give the following extract from an article which appeared in the *Times* on Monday last :—

"It was only really in the eclipse of 1869 that we began to know anything about the corona, and it was only in the eclipse of 1870 that we began to appreciate what a very difficult problem was presented to us by that phenomenon. The then Astronomer-Royal and Prof. Maedler, to cite some among the eminent authorities writing after the eclipse of 1860, had come to the conclusion that the corona was mainly a non solar phenomenon. That *part* of it, however, was undoubtedly solar was admitted by all, for the reason that it was seen before and after totality. In the eclipse of 1870 the idea that part of it was really non-solar was enormously strengthened by a comparison of observations made by different astronomers. Its shape seemed to change as the moon swept over it, and this obviously, if it were true, implied some action of the moon's edge and reflection by something between the observer and the moon. In 1871, when the Government of India and the British Association took steps to have the corona photographed at the same time that it was carefully observed by the naked eye, the strange fact was first clearly indicated that the corona seen by the eye was a perfectly different thing to that recorded on the photographic plates. The explanation given at the time was that the coronal light was much more actinic than ordinary solar light of the same visible intensity, so that in the eye and on the photographic plate two different images were built up by different qualities of light proceeding from different sources. Hence the view was distinctly enunciated that the corona seen during eclipses was a dual phenomenon, partly solar, partly non-solar in its origin, the true solar corona being filamentous with variously-curved streamers, the visible corona being non-filamentous and consisting mainly of radial lines and rifts, extending to different distances from the edge of the moon"

This slight sketch may now be expanded by the following details. Thus, for instance, in March, 1870, Prof. Young, discussing the then current views of the corona, wrote :—"It is not impossible that the so-called corona may be complex, some portion of its radiance may perhaps originate in our own atmosphere, though I do not yet find myself able to agree with the conclusion of Dr. Gould and Mr. Lockyer in this respect, and am strongly disposed to believe that the whole phenomenon is purely solar."

With reference to the eclipse of 1870 I wrote :—"At the commencement and end of totality, when the moon unequally covered the sun, the photographs have recorded an excess of light on the corona on the side where the limbs occur nearest in contact. I am told that this effect in one of Lord Lindsay's photographs is very striking; it is certainly so in one of Mr. Brothers'. In the drawings we have a slightly different effect. At the commencement



of totality, when the western or right-hand limbs were in contact, we get (see figure) 1; at the end of totality the appearance recorded was like 2; the picture at the middle of totality compounding both these appearances, and being roughly represented by 3, in which the rectangular appearance comes out in its full strength."

Let us pass on to the eclipse of 1871. This was my description, written at the time, of what I saw :—"There, rigid in the heavens, was what struck everybody as a decoration, one that Emperors might fight for; a thousand times more brilliant even than the star of India,—where we then were,—a picture of surpassing loveliness, and giving one the idea of serenity among all the activity that was going on below, shining with a sheen as of silver essence, built up of rays almost symmetrically arranged round a bright ring above and below, with a marked absence of

them right and left, *the rays being composed of sharp radial lines*, separated by furrows of markedly less brilliancy."

After there had been time to examine the photographic records of the eclipse in connection with the above description, the enormous difference between the photograph and the eye picture was fully recognised, and in my lecture at the Royal Institution on the eclipse, after referring to the actinic corona, to the striking similarity in the details of the photographs taken at different times and in different places, I said: "The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun."

Passing from the photographs to the drawings, I pointed out that in Mr. Holiday's sketch, for instance, we got an infinite number of dark radial lines extending down to the moon, with a greater extension than in the photographs, though in some places the shape of the actinic corona and some of its details were shown.

Thinking that this difference might be explained by different lights being superposed, so that of two superposed lights the naked eye used one, and the photographic plate the other, I asked the question whether the facts might not be reconciled, and really harmonised with what was actually seen in the telescope, even by supposing that the visual image, this glare let us call it, was sifted in the telescope by using greater or less magnification in the same way as it was separated out on the photographic plates and in our eyes by the different qualities of the light producing the visual and photographic images.

From this point of view, therefore, I regard Mr. Hasting's observation as one of very great interest, and I believe that it throws light upon a good many prior observations. I do not think, however, that any one will go with him when he proposes to abolish a true corona at the sun, for the reason that the observations to which I have drawn attention show that it is really a dual phenomenon as I pointed out in 1870, and although diffraction at the moon's edge may be the cause of one part, it cannot be the cause of the other. It is, perhaps, almost too early yet to speculate upon the changes in our views of the chemical nature of the external boundary of the sun's atmosphere which may be brought about by a complete discussion of the question which these observations again bring to the front. I long ago pointed out that the fact of getting in the spectroscope an indication of a line at so many minutes of arc from the limb of the dark moon, was by no means a proof of the existence of a vapour or gas at that height above the sun. Maclear's observation in 1870 was of course the test, for the reason that if such a *caveat* were not available we must assume the existence of coronal matter between us and the dark moon. But in any case it is not too early to bear this in mind, that if in our spectroscopes we have been dealing with a true glare, from whatever cause produced, there will be an almost complete inversion necessitated, and in this way: the brilliancy of any particular wave-length of the glare may either depend upon the area of the surface at the sun producing light of that wave-length, or upon its inherent intensity. Now if we assume that only the inherent intensity is to be considered, then obviously the region of greatest temperature will cause the brightest light. The brightest light will therefore be produced in the lowest level of the solar atmosphere, but because of the glare it will appear to extend to the greatest distance from the sun. It may therefore have been that the line 1474, instead of indicating, as it has been supposed to do, that a substance which gives a line at the part of the solar atmosphere most removed from the photosphere is really produced by that part of the atmosphere, was produced at that part of the atmosphere nearest the photosphere, and really at first sight—although this is by no means a matter on which one would wish to commit one's self hastily—it does seem as if this view would harmonise a great many facts which are very difficult of explanation in any other way.

I discovered the line 1474 in the chromosphere on June 6, 1869, and up to that time no bright line had been observed beyond those belonging to the spectra of hydrogen, sodium, and magnesium, with the exception of one line of barium, which was first seen in March, 1869. Now we know from the long-extended series of such observations for which we have to thank the industry of the Italian observers, that the line 1474 is now seen more persistently than any line which is not recorded in the spectra of hydrogen, magnesium, and sodium. The eclipse of last year taught us, if it taught us anything, that the lines which are thus persistent are the lines produced at the temperature of the hottest layers, and, if subsequent inquiry strengthens the view that the height to which the line 1474 appears to extend is really due to the depth at which the substance which produces it is restricted, the persistence of 1474 in ordinary chromospheric observations will be at once explained.

J. NORMAN LOCKYER

AGRICULTURE IN JAPAN[1]

DR. LIEBSCHER'S little work is the result, the author tells us, of his investigations during an eight months' sojourn in Japan in 1880. A cursory glance at the contents shows that it bears the physiognomy of a strictly scientific work. The work is divided into five parts:—(1) The condition of the climate and its influence upon the land-products; (2) the condition of soils and its influence upon the land-products; (3) the social condition before the year 1868 (before the reformation); (4) the reformation and reorganisation of the State since the year 1868; (5) foreign commerce. I shall notice shortly each chapter with some remarks. Beginning with the first chapter, Dr. Liebscher commences with the monsoon, within whose sphere Japan is situated. It has, as is well known, a certain determined direction during the whole year. The summer (south-west) monsoon comes from the south-west from April to September, while the winter (north-east) monsoon comes from the north-east during the rest of the year. To the first, according to Dr. Liebscher, Japan owes its tropical flora, such as *Chamærops excelsa*, *Thea viridis*, *Cycas revoluta*, &c., and to the same he attributes the chief land-products, such as cotton, sugar-cane, tobacco, Indian corn, and rice. Why the summer monsoon is so favourable to the growth of the land-products is because, says the author, it causes a warm temperature, and the abundant precipitation of rain (maximum 1794 mm. in a year). He ignores then altogether the geographical position of Japan, that on one side she lies partly in a subtropical and temperate zone, on the other she is surrounded on all sides by a large body of water. The north-east monsoon brings a dry and terribly cold winter, though somewhat modified by the "Kuro-Siwo" current and this monsoon is the sole factor that renders the climate unfavourable, causing the remarkable phenomenon of the "freezing of the soil." Dr. Liebscher says, the regular course of the monsoon assures the people who happen to inhabit those lands which lie within the sphere of that wind, of a never-failing good crop of rice. Thus we are accustomed to depend solely upon rice, and

[1] "Japan's landwirthschaftliche und allgemeinwirthschaftliche Verhältnisse nach eigenen Beobachtungen dargestellt." Von Dr. G. Liebscher. (Jena 1882.)

consequently we become vegetarians. We cannot entirely agree with him, but rather lay more stress upon the influence of the Buddhist religion, which once wielded sway over us. As to the unfavourableness of the climate due to the monsoon, he is unfortunate in selecting as an example the Hakone region. The volcano of Fuji San (3784 m.) is a high peak covered with everlasting snow, and at the foot of this lies the Hakone Pass (804 m.). Here Dr. Liebscher had seen on the western side (toward Fuji) around the Lake Hakone, a dreary sterile slope; while the opposite mountains, lying on the south, are covered with a luxuriant growth of forest. He ascribes the cause of the sterility of the northern to the cold winter monsoon, and the thickly wooded ranges to the summer monsoon. I explain this striking contrast quite in another way. Fuji San is an active volcano, and at the foot of this lies the region referred to. It is natural that no tree will flourish at or near recent volcanoes, which send out an enormous quantity of scoriæ. Moreover the Hakone Pass is situated at a high altitude (804 m.). We find thick forest at the top of the Brocken in the Harz mountains. Could we expect the same at the summit of Vesuvius? The climate of Japan is not so ineffective as Dr. Leibscher has depicted in his work; in reality it is far more conducive to fertility than that of Germany.

The second chapter deals with the soil and its influence upon the agriculture. "A large tract of plain not far from Tokio is left uncultivated," says the author; "while the mountain slopes are turned into useful land. Such an irrational course is not difficult to understand when I consider the other deeds done by the Government and people." This is not so serious as it seems; he did not understand the irrigation of soils, which is of particular importance in the rice-producing countries. He mentions in another page that the total area of the empire is 38,243,640 hectares, of which the cultivated land occupies 4,508,482 hectares, *i.e.* 11·8 per cent.; while the area of Prussia is 34,823,420, with 17,435,605 hectares, *i.e.* 50·7 per cent. of the cultivated land. The balance is evidently against Japan. I must here remark that Prussia is not mountainous. The only notable range in the heart of Prussia is the Harz; the Thuringian forest, the Riesen and Sudeten mountains lie at the southernmost boundaries of that country; all the rest forms what is called the "North German Plain," levelled down uniformly by the Scandinavian glaciers in the Diluvial period. On the contrary, Japan is very mountainous. Moreover, it must be taken into account that we have newly taken possession of the Riū-Kiū Islands. The island Hokkaidô, Chi-Sima (the Kurile Islands) were neglected till thirteen years ago. They are now substantially incorporated into Japan, and the present Government is energetically striving to convert these into utility. From these circumstances the author is not justified in jumping to the conclusion as to the present state of things. We are glad to find that the yield per hectare in Japan is 35·62, while in Germany it is only 6·11 (Dr. E. Naumann). As to the geology, the bearing of which is of great importance to the soil and subsoil, Dr. Liebscher closely follows Rein's "Japan," without contributing his own observations. The chief rock-groups are: (1) the crystalline massive rocks (granite, diorite, diabase, porphyries); (2) the palæozoic schists; (3) the more recent volcanic rocks (trachyte, rhyolite, andesites, dolerite, basalt); (4) the alluvium and diluvium. Among the first group are phosphates, salts of potash and soda in the form of felspar, and apatite; and the same minerals are richly contained in the third group. In Rein's palæozoic schists, recent trias and cretaceous formations are ascertained by geologists of the Geological Survey, and must be separated from the second group of Rein's geological category. The author lays great stress upon the sterility of soils, to the extensive development of the talc and chlorite schists and so-called "tuff soils"—a fine volcanic ejectamenta poured from the vent, and sediment under water. Indeed, I saw, myself, in the provinces of Musasi, Sanuki, and Rü, the phyllite system, in which the talc and chlorite schists form an essential member; still they sink into insignificance when compared with the other rock groups. Moreover, the so-called talc schist is in reality micaceous clay slate, and the pseudochlorite schist is chloritic epidote hornblende schist. These facts will somewhat modify the author's conclusion. As to the "tuff soil," he discusses and repudiates the uncertain analyses of Prof. E. Kinch and Herr von Korschelt. Neither of these gentlemen, I think, are correct, supposing that their analyses have been carefully prosecuted. They select as samples the "tuff soils" from the neighbourhood of Tokio. This city lies in the plain, surrounded by lofty volcanic chains—Fuji, Asama, Sirane, and many other ranges of volcanic nature, bounded on the south-east by sandstone mountains of the Awa province. Tuff and sandstones, *a priori*, could not produce fertile soils, and indeed "tuff soils" are the poorest in Japan. It is not found everywhere in that country, and appears exclusively confined to the neighbourhood of Tokio. I doubt very much the nature of the so-called "tuff soil." It may perhaps be an accumulation of diluvial sand and gravels. If samples for chemical analyses were obtained, the soils from the Mino province among others, we should be able to get a true insight into the Japanese soils. The author's conclusions, based upon these unfortunately ill-chosen samples, could, of course, not be correct, because the premises are already wrong. It is remarkable that Dr. Liebscher, as a professional agriculturist, after travelling through the greater part of Japan, should not be able to throw some new light on this point.

On the third chapter I have little to say, for the description relates to the bygone world prior to the year 1868. At present, our social condition assumes quite a new phase. Moreover the facts are compiled from the *Transactions of the Asiatic Society of Japan* and from the *Mittheilungen der deutschen Gesellschaft für Natur und Völkerkunde Ostasiens*. Most of the *Transactions* are translations from old obscure Japanese documents under the guise of new titles. One thing cannot however be passed unnoticed, that is the footnote on p. 72, which runs as follows:—"The Mikado may have, according to the Land Statute, 12 wives; the nobles, 8; the samurai, 2; the commons, 1." No such law ever existed in Japan. We are neither Mormons nor Mohammedans!

The fourth chapter deals with the political and social changes since the year 1868. It presents nothing new, except some odd remarks of a fanciful nature. The full accounts are already worked out by Le Gendre in "Progressive Japan," in Griffis's "Mikado's Empire," and lastly in Rein's "Japan." On p. 105 it is stated that the Japanese Government lays a heavy tax upon the farmer which may amount to half what he has won by patient labour; this oppressive measure would hinder future agricultural progress. In reality the legitimate tax is only 2½ per cent. of the net product.

The last (fifth) chapter treats of the historical development of foreign commerce and the balance of exports and imports. It is seen from the elaborately compiled tables that Japan is now in a favourable condition. In spite of the author depreciating and underrating what the Japanese have done, and the apparently incurably unfavourable physical conditions of the country, the author has, in the concluding chapter, a somewhat reassuring statement. He says that Japan will gradually produce more and more agricultural products if the heavy tax is taken off and serviceable roads are constructed throughout the interior. If this should be the case, the buying power of the country will be increased, and Germany will have to look for an opportunity to engross the export commerce. I must remark, lastly, that the author seems to me not fair-

minded in doing a great injustice to the Government of Japan, by which he was temporarily employed.

Munich B. KOTÔ

NOTES

AT the annual general meeting of the Society of Arts, which was held on the 27th ult., Sir William Siemens being in the chair, the following resolution relative to the death of Mr. Spottiswoode, who was a vice-president of the Society, was passed :—"That this meeting of the Society of Arts desires to express the deep regret with which it has received the news of the death of Mr. William Spottiswoode, one of its vice-presidents, and its sense of the loss which the Society has sustained by his decease. In him England loses one of her most remarkable men of science, science itself one of its greatest ornaments, and all who knew him a sincere and valued friend. Besides devoting his own time and thought to the advancement of knowledge, he was ever ready to lend to all engaged in like pursuits the assistance of his experience and his wise counsel. In thus placing on record their own appreciation of his services, the Society desires to express its feelings of sympathy with his widow and his family, and also with the Fellows of the Royal Society, of which he was the honoured and beloved President."

THE report of the Council for the past year, which was then read, makes it abundantly evident that the useful work of the Society is being carried on as successfully as heretofore. The *conversazione* of the Society previously fixed for the 11th inst. has been postponed to the 25th. On that day it will be held at the Fisheries Exhibition, South Kensington, when the Prince and Princess of Wales will be present.

INTELLIGENCE has been received from Vivi, on the Congo, of the sudden death of the well-known Swedish explorer, Capt. T. G. Een. Mr. Een, who was on his way to join Mr. Stanley on the Upper Congo, fell down dead from heart disease, just as the signal for his caravan to start was given.

THE Vienna Academy of Sciences offers two prizes of 1000 florins each (about 84*l.*) for the best treatises (1) on the capacity of various crystals for conducting electrical currents; and (2) on the chemical constitution of albumen matter.

THE well known Russian merchant Sibiriakoff is about to send another vessel to the Siberian rivers this summer. This is the steamer *Obe*, built of Bessemer steel at Motala in Sweden, and which will leave Gothenburg this week. The vessel, which is provisioned for sixteen months, is commanded by the Russian Capt. Weide, who has for many years sailed on the Yenisei and Lena. She will proceed to Tromsö, where she will meet his other steamer, the *Nordenskjöld*. A schooner with building materials will accompany the steamers as far as Novaya Zemlya, where it is intended to erect some storehouses at Yugor Scharr for the reception of cargoes when ice prevents the approach to Obi or Yenisei. At Novaya Zemlya a member of the expedition, Capt. Grönbeck, with two Samoyedes, will be left behind to study the ice and make meteorological observations during the winter. The *Obe* and *Nordenskjöld* will proceed to Port Dickson and the River Yenisei, in the mouth of which, in the Sastorovsky, the *Nordenskjöld* discharges her cargo, viz. merchandise, and loads a cargo of Siberian produce, with which she returns to Europe. The *Obe* proceeds up river with what cargo she can carry as far as Yeniseisk, and remains there for river navigation during next summer.

M. THOLLON is now working in the Observatory at Paris. We are informed that the Pic du Midi Observatory is making great progress towards completion, and that Admiral Mouchez, M. Thollon, and other astronomers will visit it towards the end of August.

THE monthly meeting of electricians has developed into a new institution, which is to be called Société des Électriciens. A committee has been established for determining the regulations to be proposed at a general meeting next October. M. Cochéry, Ministre des Postes et Telegraphes has been appointed honorary president of the society.

A REMARKABLE instance of the fidelity and sagacity of the dog happened on Friday last at Milford Haven, and is recorded in the daily papers. Two men named Davies and Taylor were out in a boat which was swamped. The former of these was the owner of a dog, and whilst the men were struggling in the water the animal caught hold of Taylor with the object of supporting him; finding, however, that it was not his master to whom he was rendering this assistance, he relinquished his grasp and went to the aid of Davies, his master, supporting him until he was rescued by a passing steamer, the other man being drowned.

ON June 13 at about 2 p.m. an earthquake was felt in the neighbourhood of Vossevangen in Norway. There was one continuous shock lasting several seconds, accompanied by a noise as that of a heavy train passing.

A NEW electric boat, exceeding in size all that have hitherto been designed, is now being fitted up at Millwall by the Electrical Power Storage Company, and is, we understand, nearly ready for her formal trial trip. The new craft is of iron, and measures forty-six feet in length. Her "engine" is a Siemens' dynamo of the D2 type, and works direct on the screw shaft without any gearing. The screw is of unusually narrow pitch, in order to enable the dynamo to run with a high velocity. She carries sixty-five accumulators of the Faure-Sellon-Volckmar pattern of the same size as those used in the smaller electric boat constructed last autumn by the same company. In the private trials made, a speed of eight miles per hour was maintained. This boat will be sent to Vienna, and will doubtless attract much notice at the forthcoming Electrical Exhibition in that city.

RIGNOLD'S panorama of the Arctic regions will be exhibited at the Royal Victoria Coffee Hall during the present month. This panorama, which was painted by the late Clarkson Stanfield, R.A., has the reputation of being the finest marine painting extant.

UNDER the title of "Hardy Perennials and Old-fashioned Flowers" Mr. L. Upcott Gill of 170, Strand, has issued the first number of what will be, if carried out on the lines here laid down, a rather bulky book, and moreover an expensive one, inasmuch as the number before us, which bears date April, is priced at 6*d.*, contains only forty-eight pages, and proceeds only to CAL. in an alphabetical arrangement of the names of the flowers which are recommended for cultivation. The aim of the work is a good one, namely, the bringing to notice many flowers for cultivation in our gardens that are now totally neglected or forgotten. Many old familiar friends are brought to mind in glancing through these forty-eight pages. The arrangement of the plants in alphabetical order of their scientific names is the best that could have been adopted. The wholesale use of capital letters for the specific names should be altered, and more care should be taken in the spelling, such mistakes occurring as *Achillea Ægyptica* for *ægyptiaca*, *Calthus* for *Caltha*, &c. Some of the figures also are extremely poor.

SINCE the above was written we have received the June number of this little work, which brings it down to *Helleborus* or the Christmas Rose. In this latest number the same lavish use of capitals occurs for the initial letter of the specific name

and the average number of mistakes also occurs. Thus under the genus *Funkia* the common name given is *Planting* Lily, whereas it should be *Plantain* Lily, a name that has been quite recently accorded to these plants by a gardening contemporary.

THE annual Reports of Colonial Botanical Gardens are so frequently reaching us and the matter contained in them is of such value and importance that we regret we have not space at our disposal to give a more extended notice of some of these records of scientific work in our widely spread dependencies. Two of these reports lie before us, namely, that of Dr. Trimen on the Royal Botanic Gardens, Peradeniya, Ceylon, for the year 1882, which report is dated at Peradeniya on January 1 of the present year, and that of Mr. Charles Ford on the Hong Kong Gardens, or rather on his work as Superintendent of the Botanic and Afforestation Department, Hong Kong. A large portion of Dr. Trimen's report is given to the consideration of economic plants, the first mentioned being coffee. Under this head it is with no satisfaction we learn that "Leaf disease has in no degree diminished, and the continued failure of crop during the past year has added to the difficulties of all concerned in the planting enterprise of the colony." Dr. Trimen continues, "No combined effort whatever to prevent the disease on the lines indicated by its known nature has been even attempted, whilst the waste of money and time in local applications of 'cures' has continued. As at the same time high cultivation and liberal manuring have become generally impossible from pecuniary necessities, the existing state of things, however much to be lamented, cannot be considered surprising. A remarkably wet season, too, has aggravated the condition of the badly nourished trees, and the low prices ruling for coffee have intensified the loss by short crops. Thus the cultivation of coffee has been in many places found not to cover expenses, and the necessity of growing other products has been more than ever forced upon proprietors." From this we gather that the prospects of coffee cultivation in Ceylon are anything but promising, and with regard to Liberian coffee, upon which the hopes of planters were at one time founded, we find that it likewise has had to bear the severe attacks of leaf disease, and consequently rises and falls in the estimation of planters. In suitable soils and localities, however, it does well, and the old trees now between eight and nine years old, though badly diseased, show no diminution in their crop-bearing capabilities. No record however is kept of the exports of Liberian coffee from Ceylon distinct from the produce of the other kind. Dr. Trimen remarks that the *Hemileia* not unfrequently attacks the fruits of Liberian coffee. As might be supposed, the subject of cinchona cultivation occupies a large portion of the report, and next to it comes tea and cocoa. The past year, we are told, has witnessed a very striking rise in the export of the first-named beverage, the exports amounting to 623,292 lbs., an advance on the previous year of 345,702 lbs. Tea estates have been opened at all elevations, and many old coffee estates not suited for cinchona culture are now cropped with tea. Indiarubber, gutta percha, and many other industrial and medicinal plants come under Dr. Trimen's review of a year's work at Ceylon, proving once more, if proof were needed, the value of the Peradeniya Gardens amongst others in promoting the advance of applied botany; and the same may be said of Mr. Ford's report of the Hong Kong Garden, for we find there that a considerable amount of attention has been given to the growth of such plants as *Cinnamomum cassia*, the tree furnishing Cassia Lignea of the London market, the Chinese varnish tree (*Aleurites cordata*), and the mahogany tree (*Swietenia mahogani*).

A NEW form of dry pile has been described in *Wiedemann's Annalen* by J. Elster and H. Geitel. In the previous forms of dry pile, from the time of Zamboni downwards, the disks of foil and paper have been placed in glass tubes, with the result that the film of moisture collecting on the inner surface of the tube has always exerted a more or less destructive influence. In the new dry piles the disks are strung with a sewing-needle upon a single strong silk thread, which insures better insulation. Messrs. Elster and Geitel have made the very interesting observation that piles of this type can be charged from a Holtz machine. An ordinary Zamboni pile of 11,000 pairs of disks of tin and copper foil gave, after ten minutes' charging, sparks one millimetre long, and was able to illuminate a small Geissler tube for some time with a discharge continuous at first and afterwards intermittent. Following up this analogy dry piles were constructed on the plan of a Planté battery. Thin disks of lead foil alternating with disks of silk paper painted with a mixture of soluble glass and peroxide of lead were strung upon silk strings. A charged pile of 7000 such plates gave for ten minutes a spark one millimetre long; and after twenty four hours still showed electrification.

THE last report of the British Consul at Tientsin supplies us with information respecting the only colliery at present in complete working order in China. This is at Kaiping, not far from Peking. The coal is said to belong to the true carboniferous system, and the bed dips to the south some forty-five degrees, forming a large basin under the Gulf of Pehchihli. No fear is entertained that the measures will run short. So far as has been ascertained, the coal bearing stratum is about one thousand feet, containing thirteen seams. During the winter months two hundred tons per day of the inferior kinds of coal can be sold to natives in the vicinity, who use it for pottery, brick, and lime-kilns; indeed, one of the most important results achieved by the opening of the colliery has been the revival of several industries in the vicinity which were languishing or extinct, on account of the surface coal of the district being mostly worked out, and the price of other coal being too high to be used with profit. In connection with the colliery is a small railway, the only one in all China. Its length is but six and a half miles, and at the terminus the coal is placed in barges and carried down by canal. After a little opposition the locomotives were allowed to run freely. But ironworks, which it was also intended to start, could not get over the superstitious opposition raised on the score of the proximity of the Imperial tombs, and the consequent geomantic disturbances caused by sinking shafts, &c. The iron ore is said to exist in enormous quantities, but it is not easy to work owing to the amount of silica present.

WE have received Parts 4–6 of the *Transactions of the Yorkshire Naturalists' Union*, which do credit to that energetic body of local naturalists. They are entirely occupied by lists and notes concerning the fauna and flora of this, our largest, county, so arranged that each subject has a separate pagination, and most of the authors give evidence of considerable bibliographical research; some of the articles are of far more than local importance.

FROM several parts of Sweden the appearance of an unknown caterpillar, which consumes the crops, is reported. Its length is from one inch to one and a half, and its colour grey-brown with green stripes. In one place it put in an appearance immediately after a violent storm with rain. The Academy of Agriculture has despatched an entomologist to visit the places from which it is reported.

THE German Society of Analytical Chemists offers two prizes, of 25*l.* and 15*l.* respectively, for the best treatises on cocoa and cocoa manufactures, with reference to their commercial value and efficacy in nutrition.

ADMIRAL MOUCHEZ will not be reappointed at the expiration of his term of office. The Government is fully convinced that it is useless to resort to this formality, and that it would be better to continue his appointment by *toute réconduction*, as is customary in France under peculiar circumstances.

Miss Fielde, an American missionary lady stationed at Swatow, has, it is stated, completed a voluminous dictionary of the Swatow dialect, which will be published shortly.

The additions to the Zoological Society's Gardens during the past week include a Feline Dourocouli (*Nyctipithecus vociferans* ♀) from Columbia, presented by Mr. H. H. Thiele; an Indian Civet (*Viverricula indica* ♀) from India, presented by Capt. Wilson; two Squirrel-like Phalangers (*Belideus sciureus*) from Australia, presented by A. Pretyman; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mr. J. E. Dothie; an Australian Crow (*Corvus australis*) from Australia, presented by Mrs. A. H. Jamrach; a Nicobar Pigeon (*Calænas nicobarica*) from the Philippine Islands, presented by Mr. Hugh Low; two Common Gulls (*Larus canus*), British, presented by Mr. C. W. Jervis Smith; a Spotted Mud Frog (*Pelodytes punctatus*) from the South of France, presented by Mr. H. P. Cambridge; a Cape Ant Bear (*Orycteropus capensis*), twelve Derbian Zonures (*Zonurus derbianus*) from South Africa, two Canadian Beavers (*Castor canadensis* ♂ ♀) from Canada, a Viperine Snake (*Tropidonotus viperinus*) from North Africa, a Tree Boa (*Corallus hortulanus*) from South America, purchased; a Hairy-footed Jerboa (*Dipus hirtepes*) from Arabia, a Simon's Dwarf Jerboa (*Dipodillus simoni*) from Algeria, received in exchange; a Japanese Deer (*Cervus sika* ♂), a Hybrid Syrian Wild Ass (between *Equus hemippus* ♂ and *Equus onager* ♀), an Impeyan Pheasant (*Lophophorus impeyanus*), four Amherst's Pheasants (*Thaumalea amherstiæ*), bred in the Gardens.

ON THE CAUSES OF GLACIER MOTION[1]

THE question of the causes which produce the movement of glaciers, which was at one time so eagerly discussed, would appear to have slumbered for the last ten years. This cannot be said to arise from the fact that a perfectly satisfactory theory has been developed, and recognised as such by all inquirers. The ambiguous allusion to the subject in Sir John Lubbock's presidential address to the British Association is an evidence that such certainty has not been attained. It is indeed generally supposed that the fact of the melting-point of ice being lowered by pressure is somehow at the root of the matter; but a full explanation of the origin of this pressure in the case of glaciers and of the mechanical features of the problem has yet to be given. I may therefore be pardoned if I draw attention to a different solution, proposed not by myself but by one of the greatest of English mechanicians. My apology for doing so is that I approach the question as an engineer, not as a physicist; and that it is in its essence, as will be shown immediately, a mechanical rather than a physical problem.

The following are leading facts of glacier-motion which must be accounted for by any valid theory on the subject:—

(1.) The phenomena of the movement of a glacier are simply those of a solid body in a state of flow.

(2.) The present glaciers of Switzerland and Norway, which are the only ones which have been critically examined, are mere shrunken fragments of the glaciers of the Great Ice Age. To take one instance, the present glacier of the Rhone is about 6 miles long and perhaps 500 feet deep; but the old glacier of the Rhone, which abutted against the Jura, was 120 miles long, and must have been 2000 to 3000 feet deep. The movement of such glaciers as this must also be accounted for in any satisfactory theory.

(3.) The glaciers of the present day are not confined to the temperate region; they are found in much larger numbers and of much greater size in the Arctic regions.

(4.) Both in the temperate and in the Arctic regions glaciers move in winter as well as in summer, and by night as well as by day.

That a glacier is in a state of flow was first proved by Forbes, and has since been confirmed by the measurements of Tyndall and others. Whilst the whole mass moves downwards, the top moves faster than the bottom and the sides than the middle; the upper layers must therefore be continually shearing over the lower, and the medial over the lateral. A glacier, being a body in a state of flow, must move under the influence of forces powerful enough to overcome its resistance, and so produce this condition.

The general phenomena of the motion of a glacier are exactly reproduced when a viscous body moves through a channel under the influence of its own weight. We have therefore first to inquire whether the shearing resistance of ice is sufficiently low to enable us to regard a glacier as a viscous mass.

The only experiments known to me on the shearing resistance of ice, are those of Moseley (*Phil. Mag.*, January, 1870). He found that, with pressures from 100 to 110 lbs. per square inch, cylinders of ice sheared slowly across the two planes in contact, sliding over each other without losing continuity. The distance sheared through was about five-eighths of an inch in half an hour. A load of 119 lbs. per square inch was sufficient to shear through a cylinder of $1\frac{1}{2}$ inches in diameter in two to three minutes. From these experiments it would appear that the lowest shearing stress which will cause ice to flow is about 100 lbs. per square inch; but sufficient time was not allowed in the experiments to make this a matter of certainty.

There is another way in which the shearing resistance of ice may be tested. In the case of a block of ice of vertical sides, gravity of course produces a shearing resistance along all planes passing through the base. Let h be the height of such a block in feet, and consider the shearing force due to gravity on any square foot of a plane making an angle θ with the vertical. This shearing force is given by—

$$\frac{\frac{wh \times h \tan\theta}{2} \times \cos\theta}{h \sec\theta} = \frac{wh}{2} \sin\theta \cos\theta.$$

This expression is a maximum when $\theta = 45°$, and its value is then—

$$\frac{wh}{4}.$$

What is the greatest height at which a vertical cliff of ice will stand? I am not able to state this precisely, but it is very considerable. Mr. Whymper mentions crevasses in South America 300 feet deep. Cliffs of fully that height have been seen standing out of water in the case of icebergs, and as so small a part of an iceberg projects above water, these cliffs probably extend below to a considerable depth. Taking, however, only 300 feet for the value of h, or for the maximum height of an ice cliff, this would give about 30 lbs. per square inch as the lowest shearing force upon a plane of ice which would cause it to assume the condition of flow.

Let us now suppose a glacier of thickness a, lying upon a slope whose inclination to the horizontal is β; then the force per square foot, tending to shear the ice at its junction with the slope, is clearly $awo \sin\beta$.

Supposing $\sin\beta$ to equal $\frac{1}{4}$, and that the shearing resistance is 30 lbs. per square inch, we get a = about 290. Hence we may say that a glacier lying on a slope of 1 in 4 will not move at all under its own weight unless it be at least 300 feet thick, and that, if it be more than this, the upper 300 feet will move as one solid mass, the part below alone representing the conditions of flow.

It is needless to say that there are hundreds of glaciers which are less than 300 feet thick, and which at no part of their course have a slope anything approaching 1 in 4.

We have now to show that the theories generally propounded for glacier action are all of them negatived by some of the foregoing considerations. These theories may be stated as follows:—

(1.) The glacier simply slides over its bed as a solid body. This is negatived by the fact that some parts move faster than others.

(2.) The glacier flows under the action of its own weight, exactly as a viscous body flows. This is the theory of Forbes. It is disproved by the facts given above, which show that even on a slope of 1 in 4 a glacier would not flow unless it was at least 300 feet thick.

(3.) The glacier moves by the crushing of its base. This has been disproved by Moseley's experiments, which showed that the crushing resistance of ice was considerably higher than the shearing resistance.

(4.) The glacier moves by the melting of its base. This is the theory of Hopkins. He placed a block of ice at 32° F. on a slab at a small angle, and found that it slowly descended as it melted. On this view the bottom of the glacier must always

[1] Paper by Walter R. Browne, M.Inst.C.E., read at the Royal Society, June 15, 1882.

be in a melting state. But glaciers are of all sizes and thicknesses, and they move in winter as well as summer. Bessels ("Die Amerikanische Nordpol Expedition," p. 398) measured the motion of an Arctic glacier (not apparently very thick), in the month of April, which is just when the winter cold would have sunk deepest, and found it considerable. Again in the *Zeitschrift des deutschen Geologischen Gesellschaft*, vol. xxxiii. p. 693, is an account of measurements of a Greenland glacier, both in winter and summer, which show that the motion in winter is only 20 per cent. less than in summer. It has been suggested to me that the interior heat of the earth may be sufficient to keep the bottom of the ice from freezing; but this cannot apply near the sides, where the ice is shallow, and the freezing of a very small strip on each side would be sufficient to keep the whole mass from descending. Moreover, this cause would apply to masses of snow as much as to ice. But it is known that masses of snow, though lying on steep slopes, do not descend in this way, even in summer, but melt away where they lie.

(5.) According to the theories of Tyndall, Croll, and others, the glacier moves not in the form of ice but of water. These theories are based on the known fact that the freezing point of ice is lowered by pressure. Hence it is supposed that certain parts of a glacier are continually being exposed to so much pressure that they melt. The water escapes downward, and the pressure being relieved it freezes again. The continuity of the glacier is further kept up by the process of regelation, according to which two pieces of ice, if placed in contact, form into one solid mass.

The advocates of this theory hardly seem to consider how very small the lowering of the freezing point is for any ordinary pressure. It is only ·0075° per atmosphere. In other words, it will require a pressure of 2000 lbs. per square inch to liquefy ice at 31° instead of 32°. This is equivalent to the weight of a column of ice about 5000 feet high. It is needless to ask whether such a pressure can exist within an ordinary glacier, while on the other hand glaciers undoubtedly move at temperatures far below freezing point—in the Arctic regions below zero.

It seems to be generally supposed that the pressure in the lower part of a glacier is due to the steeper upper portions: the glacier channel is spoken of as a mould, through which the ice is forced by pressure from behind. But in the upper glacier, slopes of ice or *nevé* are not uncommon at angles of 30° or even more. Such slopes usually do not even touch the more level parts of the glacier below them, but are separated from them by a wide, deep crevasse called a Bergschrand. Of this the well-known ice wall of the Strahleck is a conspicuous example. In other cases such slopes do not end in a glacier at all, but die away upon the mountain side. It is certain, therefore, that ice or *nevé* is able to maintain itself at a high angle upon its slope of rocks, and therefore cannot possibly exercise pressure upon the parts of the glacier far in advance of its foot. The fallacy of this idea may be further illustrated by referring, not to modern glaciers, but to those of the Great Ice Age. Can we suppose that the pressure of the snows about the sources of the Rhone was sufficient to drive that glacier down the valley to Martigny, round a sharp angle to the Lake of Geneva, through the bed of that lake, and on to the slopes of the Jura, a distance of more than 100 miles, in which the average slope was about 1 in 200; giving a propelling force per ton of ice of about 11 lbs. only?

All these theories have this in common, that they regard gravity as the sole and direct agent in the movement of glaciers, and the above considerations seem to prove that it is an agent far too weak for the work it has to do.[1]

The only other agent which has been suggested, or seems likely to be suggested, to account for the motion of glaciers, is heat. This suggestion, as is well known, is due to the late Canon Moseley, F.R.S., and was to some extent worked out by him in papers published in the *Phil. Mag.*, 1869 and 1870.

The mode of operation, on this theory, is well known. Ice is here considered merely as a solid body, obeying the ordinary laws of expansion and contraction under differences of temperature. This it is known to do, the coefficient of linear expansion, for 1° F., being ·00002856 (Moseley, *Phil. Mag.*, January, 1870), which is very high. When a mass of ice, such as a glacier, suffers a rise in temperature, either through conduction or radiation, it will expand; this expansion will take place mainly in the direction where movement is easiest, that is, down the valley. If from any cause the temperature falls, the glacier will again contract; but since the expansion is assisted by gravity whilst the contraction is opposed by it, the latter will be somewhat less in amount than the former, and when the ice has returned to its original temperature, its centre of gravity will have moved a certain small distance down the valley. By such alternate expansions and contractions the glacier moves gradually from the top to the bottom of its course.

That variations of temperature do take place in a glacier cannot be doubted, whatever be the condition in which it lies. This granted, the fact that it should move in the way described appears to me no more surprising than that the sheets of lead on which Canon Moseley made his well-known experiments did so move; and that the motion thus produced is of the character which answers to all the facts of the case, so far as they are at present known, can, I believe, be established.

The controversy occasioned by Canon Moseley's articles was unfortunately terminated by his illness and death, before the matter had been fully cleared up. The main objections urged to his theory were two. The first was that a glacier is not one continuous body (as assumed by Canon Moseley in his mathematical investigation), but is broken up into many parts by crevasses. But in the first place, the assumption above mentioned is merely one of convenience, and not in the least necessary to the theory. A detached piece of ice would move in the same way as a glacier, or as the sheet of lead did in Canon Moseley's experiments. Secondly, if a glacier is anywhere divided in its whole thickness by a crevasse, this is absolutely fatal to the gravitation theories, since there can be no pressure between the portions above and below this division. The only possible explanation of crevasses, on these theories, is that they are due to the glacier bending over a convex part of its bed. In that case the bottom half will be in compression, and only the top half in tension, so that the crevasse cannot possibly extend more than half way through the thickness.

The second objection was that the conductivity of ice is low; hence the effect of the heat would be confined to the layers near the surface, and could not account for the motion of the glacier as a whole. This objection does not seem to be confirmed by careful reflection upon the way in which such forces act. Let us suppose a glacier 100 feet deep, of which each successive foot expands and contracts alike throughout, but adheres with a definite shearing resistance to the layers above and below. Let there be a rise in temperature, which does not extend beyond the uppermost 10 feet. This layer will expand, and if it were free would expand to the full amount due to the increase in temperature. But its lower surface is not free. In expanding it will therefore drag the next layer after it, or in other words will cause it to expand also. The amount of expansion, however, will not be so great, because there will be a certain shearing extension at the plane of division between the two. The second layer will similarly cause an expansion in the third, and so on to the bottom. In consequence the energy which would all have been exerted on the top layer, had that been free, will be distributed over the whole of the layers; and the extension of the top layers will of course be much smaller than it otherwise would have been. Should the temperature then remain constant, the layers will retain their position, and adapt themselves to the new circumstances. If the temperature falls, the layers will contract, but from the now opposing effect of gravity they will not return to their original position. The top layer, which has extended furthest, will be the furthest below its original position; the second layer next and so on. If we suppose the layers to be indefinitely thin, we have the condition of things in an actual glacier. The ice in any vertical section will, on the whole, move down the slope, but the top will move faster than the middle, and the middle than the bottom, exactly as it is known to do. The same holds with regard to a horizontal section. At the sides the ice will be held back, not only by the friction, but also by the protuberances of the rock, which compel the ice to shear over them. Hence the velocity there will be retarded, and will be less than that in the middle, which is comparatively free.

A more important objection remains to be considered, which is this. On the present theory the motion at any point on the surface of a glacier will be not continuous, but oscillating alternately downwards and upwards, and the net distance by which it has descended, say, in a day, will be a mere fraction of the total distance through which it has moved in that period. If

[1] Another evidence against pressure from behind as a cause of motion is furnished by the very small size of many glaciers. Some of these, notably those of the class called "glaciers remanés," are only a few hundred yards long, and cannot be many feet deep.

so, this alternate motion ought to have been noticed in the various observations which have been made upon glaciers, and this does not appear to have been the case. But, in reply to this, it may be remarked that most of the observations have only given the net movement of points on the glacier during intervals of a day or more, and therefore would not show the oscillations. Again, such observations have always been at points near the end of a glacier. Now the variations in temperature of a glacier will be very different at different parts, and the motion of the end of the glacier will, to a great extent, show the average result of these different advances and retreats in different parts of the higher regions. This average result will, of course, be a steady progression down the valley, and the oscillatory movement at the end of the glacier may be so much masked by this as not to be readily observable. Lastly, it may be suggested as possible that a certain amount of expansion by heat may have the effect of giving a *set* to ice, so that it does not return to its original length when brought back to the same temperature. If this be so, the oscillations would be much less marked, and at the end of the glacier would probably be indistinguishable.

I may now draw attention to some phenomena of glacier action, which are explained by the heat theory, but which do not seem explicable on the gravitation theory.

(1.) It is well known that glaciers, when they emerge from a narrow gorge into a comparatively wide valley, spread out into a fan shape. The Rhone glacier is a well-known instance. A still better one is a small glacier in Norway, mentioned by Prof. Sexa, which spreads out to five or six times its previous width. Now the effect of gravity, acting on a mass as a whole, is to carry it in one single direction, that of the steepest slope. The only way in which gravity can produce such a spreading out is by the parts of the glacier shearing over each other in the manner of a viscous solid. But the phenomena of ice cliffs, as mentioned above, show that ice does not spread from this cause, so that the fact seems impossible to explain by gravitation alone. On the heat theory it is, of course, perfectly easy : the expansion and contraction will take place in all directions where there is freedom to move.

(2.) Connected with this phenomenon is that of the longitudinal crevasses seen near the edges of glaciers, and particularly where they spread out in the manner just described. Now on the gravitation theory, as remarked above, the only possible explanation of a crevasse is that the ice is bending over a convex surface, and that its upper part is thus placed in a state of tension, under which it breaks. Since, on the gravitation theory, every part of a glacier is exposed to a severe pressure from behind, this explanation does not fit very well even for transverse crevasses ; but to longitudinal crevasses it is clearly inapplicable, since the bottom of a valley is seldom or never convex in the direction of its width. On the heat theory the explanation is simple. We may suppose the heat energy communicated per square foot of surface to be about the same, whether near the middle or edge of a glacier. This energy is expended in producing an expansion throughout the whole thickness of the glacier, as described above. Hence the smaller this thickness, the greater will be the amount of expansion, and the greater therefore the net motion which results. Hence the thinner parts of a glacier will always be tending to tear themselves off from the thicker, and thus longitudinal crevasses will frequently be found.

(3.) The striæ which are so marked a feature of glacier-worn rocks become more easily explained on this theory. I have seen such striæ, even in the hard hypersthene of Skye, which were a considerable fraction of an inch in depth. When we consider the enormous force necessary to plough out such a furrow in hard rock, it is almost impossible to believe that it was done by the simple passage over it, once for all, of a stone imbedded in the ice. If, however, the stone descended by a series of oscillations, so that it passed many times over the same spot, this difficulty is greatly lessened.

(4.) In conclusion I may point out that the advocates of the gravitation theory are bound to explain what becomes of the heat energy which is poured into a glacier. When the sun is shining this radiant energy is always very large, although the temperature of the air may be low. In such cases the glacier does not melt ; it is perfectly clear that it must expand, as any other solid must expand under the action of heat. If so, it seems unreasonable not to hold that the gradual descent by alternate expansion and contraction must follow, as it is known to follow in the case of other materials.

On the subject of the motion of Arctic ice, Dr. Rae, F.R.S., has kindly permitted the publication of the following particulars :—

"When in Greenland, in the autumn of 1866, I was ice-bound at the head of one of the fiords, and slept a couple of nights at an Eskimo's house. A glacier about half a mile distant was then in full activity, the movement of which might, I believe, have been as visible to the eye as it certainly was audible to the ear.

"My own idea is that Arctic glaciers must have a downward motion more or less during the whole year, summer and winter. I believe the alternation of heat and cold—or, I should rather say, of temperature—would of itself cause motion, especially near the upper surface.

"We know that ice 2 or 3 feet or more thick contracts very considerably in a few hours by a sudden fall of 15 or 20 degrees of temperature. I have found cracks in Lake Winnipeg 3 or 4 feet wide, formed by this cause during a single night, almost stopping our sledge journey. This gap soon freezes up. Then the weather gets milder, the ice expands, and with the new additional formation is too large for the lake, and is forced up into ridges. This process goes on at every 'cold snap,'[1] alternating with milder weather. Now supposing a glacier for 10 or more feet of its depth contracts by cold, as lake ice is known to do, it will get a series of cracks probably in its longest axis, say from inland seaward ; the first snowdrift will fill up these cracks or some of them, and this filling up will to some extent perform the same office as the freezing of the cracks in the lakes. The longitudinal extent of the glacier will be increased. A snowstorm always brings milder weather, which would expand the glacier, but as this expansion would naturally tend downhill, instead of up, the whole motion would be downwards. But even if the cracks I mention did not take place, the contraction by cold would pull the ice downhill, not up, whilst the expansion by increase of temperature would tend to *push* the glacier downhill, so that these opposite actions would produce similar effects in moving the glacier, or such part of it as could be acted upon by external temperature, downwards.

"I may also add that when a crack, however slight, is formed by contraction, the cold is admitted into the body of the glacier, and increases the contracting power or influence."

SCIENTIFIC SERIALS

Annalen der Physik und Chimie, xix., part 4.—Electrical experiments : electric pressure on solids, by G. Quincke. This paper forms a continuation to a series of experiments in electrostatics published by the author in previous numbers of the *Annalen*, under the title of "Electric Expansion." It is illustrated with twenty-six cuts, and will be followed by a communication on the resistance of insulating fluids to electric force.—On electric disturbance at contact of gases with bodies in combustion, with four illustrations, by Julius Elste and Hans Geitel. The authors arrive at the general conclusion that all flames may be regarded as streams of hot gas, which generate negative electricity in burning electrodes introduced from without, as well as in small bodies in combustion suspended in them.—On electric vibration, and more especially on the phenomena of polarisation produced by vibratory movements, with four illustrations, by A. Overbeck.—On the dependence of gases as heat conductors on the state of the temperature, with three illustrations, by A. Winkelmann.—On the fundamental equations of E. Ketteler's theory of optics, by W. Voigt. The author shows that, so far from flowing from the principles of the doctrine of elasticity, Ketteler's fundamental equations are diametrically opposed to them.

THE *Journal de Physique* (May, 1883) contains the following original papers :—On the difference in barometrical pressure at two points in the same vertical line, by J. Jamin.—On the action of heat upon boracite and upon sulphate of potash, by E. Mallard.—On the penetration of actinic rays into the eye of man and that of vertebrates, and on their vision of ultra-violet rays, by E. de Chardonnet.—On a new apparatus for verifying the laws governing the fall of bodies, by M. Paquet.—On an experimental demonstration of the unequal velocity of the transmission of sound in gases and solids, by F. Griveaux.

[1] "Cold snap," an American term meaning a rather sudden increase of cold.

Zeitschrift für wissenschaftliche Zoologie, Bd. xxxviii. Heft 2 (April 27, 1883), contains:—Contribution to a knowledge of the infusoria, by Dr. G. Entz (Plate 8).—On the primordial skull of some mammalia, by Fred. Decker (Plate 9).—On some Cœlenterata of the South Sea, by Dr. R. v. Lendenfeld, of Melbourne, Part II.—On new Aplysinidiæ (Plates 10 to 13).—On the embryology of Hydra, by Dr. A. Korotneff (Plate 14).—On the larval development of *Phoxichilidium plumulariæ*, nov. sp., by Dr. R. v. Lendenfeld, with woodcuts.

Rendiconti of the *R. Istituto Lombardo di Scienze e Lettere*, May 10 and 17.—Preliminary inquiry into Zanardelli's proposed penal code (continued), by Prof. A. Buccellati.—On the commentaries of Gaius and the paraphrase of Theophilus, by Dr. C. Ferrini.—A few remarks on the first five sections of Ricardo's chapter on value, by Prof. E. Nazzani.—On the mortality of infants in the various provinces of Italy, by Prof. G. Sormani. The death-rate during the first month is shown to be much higher in winter than in summer, and in the northern than in the southern provinces. Thus: 50 per 1000 in Palermo, 190 in Padova, medium for the kingdom 91·9.—On the formation of the primitive line and primitive cleft in the gastrula of the Mexican axolotl, by Prof. G. Bellonci.—Alterations in the lower hollow vein aggravating hepatic cirrosis, by Prof. A. de Giovanni.—Observations on the comet of Brooks made in the Brera Observatory, Milan, by G. V. Schiaparelli.—On a deposit of fossiliferous Pliocene clay recently discovered near Taino, to the east of Angera, in Lombardy, by Prof. T. Taramelli.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, June 21.—Prof. P. M. Duncan, F.R.S., vice-president, in the chair.—The following gentlemen were balloted for and elected Fellows of the Society, viz. Messrs. E. J. Baillie, J. Borland, K. McKean, E. C. Malan, and H. A. A. Nicholls.—A specimen of *Polyporus sulfureus* was exhibited for the Rev. A. A. Harland, obtained from the stem of a yew tree in the Cliveden Woods, Bucks.—A series of fossil fruits, &c., from Australia were shown for Dr. Charles E. Barnard; among these were species of *Phymatocaryon*, *Eisothecaryon*, *Ochthodocaryon*, *Spondylostrobus*, *Plesiocapparis*, and others.—Mr. W. T. Thiselton Dyer exhibited several interesting vegetable economic products, and made remarks thereon. Of a species of wax extracted by Mr. D. Morris of Jamaica from *Myrica microcarpa*, it was stated that while the berries are used for obtaining wax in South Africa, the West Indian fruits had not hitherto been used for this purpose. A gray, camphor-like substance, the product of *Artemisia noxa*, he mentioned as a rare example among the Compositæ; and there was a probability that this camphor was used in the production of Indian ink by the Chinese, and gave the peculiar aromatic odour to the true China ink. A rosary was shown made of fruits of *Trapa verbanensis*, De Not. (locally called Frutti de' Lago), from the Lago di Varese, Italy; also specimens of wax and candles made from *Rhus vernicifera* of Japan; the latter preparation is quite a local industry, which unfortunately is now ceasing on account of the rivalry of the cheap American oils.—The following plants were exhibited, viz., *Arnoseris pusilla* and *Hypochæris glabra*, obtained by Mr. Thomas Howse in West Surrey, and specimens of the Cheddar Pink (*Dianthus cæsius*), which had been grown freely by Mr. C. F. White on his garden wall at Ealing.—A paper on the structure of the hard parts of the Fungidæ (part 2, Lophoserinæ), was read by Prof. Duncan, and another by Mr. R. A. Rolfe of Kew, on the Selagineæ described by Linnæus, Bergius, and Thunberg.—A communication was read from Mr. H. G. Doran, on the malleus of *Rhytina stelleri*, based on a specimen obtained in the voyage of the *Vega*, and exhibited in the Swedish Department of the International Fisheries Exhibition, under the charge of Prof. Smitt of Stockholm. The author concludes that this auditory ossicle in the extinct Northern Sea Cow (*Rhytina*) is larger than in the Manatee (*Manatus*), and therefore it is the largest and bulkiest malleus to be found in the whole section of the animal kingdom where such a bone exists. In the character of its body it resembles that of the Manatee rather than that of the Dugong (*Halicore*); while in the manubrium it differs in *Rhytina* from the other Sirenia, and is far more generalised.—The following paper was taken as read, Notes on some new economic products recently received at the Royal Gardens, Kew, by W. T. Thiselton Dyer. Therein he treats of the West African indigo, the Inhambane copal, and the Ogea gum as exhibited at a previous meeting.—On the testis of *Limulus*, formed a communication from Mr. W. B. S. Benham. He describes the structures in question, noting the apparent isolation of many of the spermatic sacs, and the probability that they are not diverticula of the spermatic duct, but secondarily acquire connection therewith, the two structures being independently developed. He remarks that in no crustacean do the ducts of the generative glands form a network, whereas in the King Crab, as in the Scorpion and other Arachnids, they do.—There followed a paper on the Mollusca of H.M.S. *Challenger* (part xx.), by the Rev. R. Boog Watson. This contains a continued descriptive account of the family Bullidæ, dealing with the genera *Atys* and *Scaphander*, along with the group *Aplysiidæ*, genus *Dolabrifera*.

Zoological Society, June 5.—Osbert Salvin, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited and made remarks on two birds obtained near Lima by Prof. W. Nation, C.M.Z.S., and on a collection of birds made in New Britain, New Ireland, and the Solomon Islands, that has been sent to him for examination by the Rev. George Brown, C.M.Z.S.—Mr. Sclater also called the attention of the meeting to a Condor from Peru, living in the Society's Gardens since 1877, which he was induced to believe was a specimen of the "*Condor pardo*," or *Sarcorhamphus æquatorialis*, Sharpe.—Mr. G. French Angas exhibited a collection of butterflies made during a recent visit to the island of Dominica, W.I.—A communication was read from Prof. Owen, C.B., entitled "Embryological Testimony to General Homology."—A communication was read from the Rev. O. P. Cambridge on some new genera and species of spiders. Eight spiders, representing as many new genera, were described: two of them belonged to the family Theraphoridæ, one to the Drassidæ, and the others to the Thomisidæ. Three of these species were from Ceylon, three from Caffraria, one from New Zealand, and one from California.—A communication was read from Mr. A. G. Butler containing an account of the Lepidoptera collected by Mr. H. O. Forbes in the islands of the Timor-Laut group. Examples of twenty-three species were obtained.—A communication was read from Mr. Herbert Druce containing descriptions of some new species of moths of the families Zygænidæ and Arctiidæ, mostly collected in Ecuador by Mr. C. Buckley. The number of new species described was fifty, belonging to twenty-four genera.—A paper was read by Messrs. Godman and Salvin, containing remarks on the variations of certain species of butterflies of the genus *Agrias*.—Mr. G. A. Boulenger read a report on a collection of reptiles and Batrachians from the Timor-Laut group of islands, formed by Mr. H. O. Forbes. Two new species were described—the one a lizard of the Australian genus *Lophognathus*, and the other a snake of the Indian genus *Simotes*, proposed to be named respectively *L. maculilabris* and *S. forbesii*. The snake was of special interest, as no species of the genus *Simotes* had hitherto been previously known to occur eastward of Java.

Chemical Society, June 21.—Dr. W. H. Perkin, president, in the chair.—The following gentlemen were elected Fellows:—G. S. Bowler, C. Beringer, T. H. Coleman, A. Esilman, H. E. Harrison, C. Hulke, H. Heap, B. Hobbs, C. T. Heycock, W. J. Livingston, B. P. Lascelles, H. R. Mill, M. F. Purcell, J. E. Richardson, F. G. Roberts, W. R. Reffel, A. Smith, E. H. B. Stephenson, A. W. Soward, A. H. Samuel, D. Wilson, and R. Williams.—The following papers were read:—On evaporation in vacuo, by H. McLeod. The author has contrived several forms of apparatus, and in the present paper describes two. One in which the water was evaporated in a glass dish with ground top, at a temperature not exceeding 50°, 50 c.c. evaporated in two hours; a Körtings water pump was used to obtain the vacuum. Instead of the dish a test tube or a combustion tube may be employed. In the second form of apparatus sulphuric acid was allowed to trickle down the tube into which the aqueous vapour passed, and thus the use of a condenser was avoided.—Note on a hydrocarbon and some substitution derivatives from camphor, by H. E. Armstrong.—On the preparation of the pentathionates, by G. S. Shaw. The author has reinvestigated this subject, because Prof. Spring states in *Liebig's Annalen* that he was unable to obtain pentathionates by using the method described by V. Lewes. The author completely confirms the results obtained by Lewes, and has obtained beautifully crystalline salts in which the ratio of potassium to sulphur was as 2 atoms to 5. A note is appended to the paper by Watson Smith.—On the decomposition of ammonium nitrate;

an investigation into the rate of chemical change, by V. H. Veley.—Note on the action of allylic iodide upon phenol in the presence of zinc or aluminium foil, by P. Frankland and T. Turner. Orthopropyl phenol was obtained.—On a new gas burner for heating combustion tubes, by W. Ramsay.—On a by-product of the manufacture of aurin, by A. Claparède and Watson Smith. When aurin is prepared from phenol, oxalic acid, and sulphuric acid, some quantity of white crystals appears on the lids of the aurin-pots. These were examined by the authors, and were found to consist of a phenyl ortho-oxalic ether.

Meteorological Society, June 20.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—The following papers were read:—On the structure of the ice-cloud disposed in threads, proposed to be called "cirro-filum," by the Rev. W. Clement Ley, M.A., F.M.S. Of the cirriform clouds one of the most important to the weather forecaster is that to which the author has given the name of "cirro-filum." Having from the time he was twelve years of age carefully studied this cloud whenever visible, and having for the last twenty-five years made it the subject of minute study, he is enabled to bring forward some results which may prove of value. The author then gives, first, a short account of the mode in which he was led to prosecute this study; secondly, a classification of the more recent and reliable observations; and lastly, an explanation of the principal phenomena observed.—Notes on a second series of experiments on the distribution of pressure upon flat surfaces perpendicularly exposed to the wind, by Richard H. Curtis, F.M.S. The results obtained in these experiments agree very closely with those of the former experiments.—On the reduction of wind records, by the Hon. Ralph Abercromby, F.M.S. The author discusses the significance and best method of deducing from anemographic records the total quantity, the quantity from different points of the compass, the relative frequency, the mean and annual velocity, the mean velocity from different quarters, the resultant, and the mean and diurnal direction of the wind.—The spectroscope as an aid to forecasting weather, by F. W. Cory, M.R.C.S., F.M.S.—Note on river temperatures as compared with air temperatures at Greenwich and Bremen, by Robert H. Scott, M.A., F.R.S. The author compares the results given in a recent paper by Sir G. B. Airy on a comparison between the records of the temperature of the Thames and those of air temperature taken at Greenwich with those published by Herr von Freeden for the temperature of the Weser as compared with that of the air at Elsfleth, close to Bremen, for the ten years 1858–67.

Physical Society, June 23.—Prof. Clifton in the chair.—New member, Mr. Stearn.—Prof. D. E. Hughes, F.R.S., exhibited a number of experiments illustrating his theory that a magnet is made up of magnetic molecules each of which is a small magnet. When a magnetic metal is in a neutral state he showed that there is a symmetrical arrangement of the molecules such as to make them satisfy their mutual attractions; not as on Ampère's theory a "higgledy-piggledy" arrangement. Prof. Guthrie stated that a piece of watch-spring magnetised retains its magnetism when impregnated with mercury. Prof. Everett, Mr. W. H. Coffin, and others remarked that Ampère's theory tried to account for the magnetism of the molecules. Professors Perry and Ayrton observed that when soft iron is between red and white hot, it ceases to be attracted by a magnet.—The new absolute sine galvanometer of Prof. Minchin was then exhibited to the meeting by Prof. G. Carey Foster. It is intended for the Cornell University, and measures less than the E.M.F. of a Daniell cell. The principle of the instrument was described at a former meeting of the Society. Prof. Ayrton, Lord Rayleigh, Mr. Coffin, and Prof. Clifton offered some remarks on the apparatus.—A note on the induction-balance effect and the densities of alloys of copper and antimony, by Mr. George Kamensky, A.R.S.M., was then explained by Prof. Chandler Roberts. These experiments were to determine whether the curve of the electrical resistance of the copper antimony alloys would be a straight horizontal line, U-shaped or of the L type. They were found to belong to the last type. It is seen from the curve exhibited that there is a rapid fall from copper to the alloy containing only 10 per cent. antimony, and this decrement is continued until the alloy $StCu_4$ is reached, when the curve turns rapidly and rises to $StCu_2$, then turns again, and passes to pure antimony. Prof. Roberts has shown that the alloy $SnCu_4$ occupies the lowest point of the curve, namely, the position that in the copper-antimony series is occupied by the alloy $StCu_4$. In the copper-tin series the second critical point is held by $SnCu_3$, and in the copper-antimony curve this point is held, not by Cu_3Sb, but Cu_2Sb, the formula for the violet alloy known to alchemists as the "regulus of Venus." The specific gravities were also plotted in curves, showing that the alloy Cu_2Sb does not stand out from the rest, while the alloy Cu_4Sb has a higher density than copper.

EDINBURGH

Royal Society, June 18.—Prof. Maclagan, vice-president, in the chair.—The Astronomer-Royal for Scotland presented a paper which was read by Prof. Crum Brown, on bright clouds in a dark night sky. This phenomenon Prof. Smyth had twice witnessed, on April 8, 1882, and April 30, 1883. On both these occasions the meteorological conditions were peculiar, the air being for a few hours remarkably dry. The explanation given was that the glow on the clouds was due to reflection of the gas-lights of Edinburgh from the hollow water-drops in the cloud, which from their floating in a very dry atmosphere had become sufficiently thin-walled to throw back a strong reflection from the two surfaces.—Prof. Tait read a mathematical note by Mr. Anglin, in which a solution was given of the problem to express x^m in terms of powers of x lower than n, when x^n is given in terms of these lower powers, and m is greater than n.—Prof. Tait communicated the results of his recent measurements of the compressibility of water. The water was compressed in a tube silvered inside and dipping with its lower and open end in a trough of mercury. The whole was placed inside the hydraulic press, and exposed to pressures of 1, 2, 2½, and 3 tons weight per square inch, the compression of the water being measured by the height of ascent of the mercury, which was given at once by the lower limit of the silver film. For water, both fresh and salt, the compressibility was found to diminish with increase of pressure, diminishing at much the same rate in both cases, although to begin with the fresh water was more compressible than the sea-water in the ratio of about 72 : 67. The results obtained for the fresh water could be very accurately represented by the formula $c = ·0072\,(1 - ·043\,p)$, where c is the true compressibility per ton at pressure p tons weight per square inch. The mean temperature of the water was 12° C. At the same temperature alcohol of density ·83 showed a much greater compressibility (·01202 for one ton weight per square inch), which also diminished with increase of pressure—·01043 being the average compressibility for 3 tons weight.

SYDNEY

Linnean Society of New South Wales, April 25.—The following papers were read:—Notes on a collection of fishes from the Burdekin and Mary Rivers, Queensland, by William Macleay, F.L.S., &c. The new species described are *Serranus estuarius*, *Therapon fuliginosus* and *parviceps*, *Diagramma labiosum*, *Corvina argentea*, *Caranx compressus*, *Cybium semifasciatum*, *Platycephalus Mortoni*, *Eleotris planiceps*, *Atherinichthys maculatus*, *Mugil Ramsayi*, *Chatoëssus elongatus*, *Anguilla margiuipinnis*, and *Tæniura Mortoni*.—By J. J. Fletcher, M.A., B.Sc., notes on a viviparous lizard. The author's attention had been drawn to the subject during last January, when he obtained at Burrawang several examples of female lizards in an advanced stage of pregnancy. The embryos were from two to three inches long, enveloped in a thin and transparent chorion quite devoid of the calcareous matter with which it is more or less impregnated in the oviparous species.—Notes on a method of obtaining water from *Eucalyptus* roots, as practised by the natives of the country between the Lachlan and Darling Rivers, by K. H. Bennett.—Prof. Stephens exhibited a photograph and a sketch forwarded by Mr. C. Jenkins, representing a fossil from the Devonian formation of the Murrumbidgee valley, near Yass. Mr. Jenkins is inclined to refer it to *Asterolepis* (which is closely connected with *Pterichthys*), but chiefly on account of the character of sculpture of the scales. On the same ground he doubts its relationship to *Cacosteus* or *Cephalaspis*. Prof. Stephens added that without the actual specimen before them with all its collected fragments, it would be premature to determine even the genus of this ancient fish, but pointed out that it appeared to have some points of resemblance to *Macropetalichthys* of the North American Devonians.—Mr. J. J. Fletcher exhibited a specimen of a giant earthworm, 25 inches long, from Burrawang, N.S.W. It probably belongs to Prof. M'Coy's genus *Megascolides*, and its existence in this colony is now recorded for the first time.

BERLIN

Physical Society, May 25.—Dr. Aron spoke on the glow-light coal, which, as is well known, is distinguished by its electrical conductibility and by its resistance to combustion when exposed to the atmosphere in an incandescent condition, and which thus resembles graphite, which possesses both these properties in a high degree. Experiments which were made in order to determine whether such good conducting and indestructible coal, as well as artificial graphite, could be made artificially, led to the result that organic substances, *e.g.* paper, cloth, wadding, when charred in vacuo at very high temperatures in graphite crucibles, acquire the property of resisting combustion and afterwards become good conductors. Wood-coal also, which, though it is with difficulty combustible, is a bad conductor, was converted into a good conductor by strong incandescence. When the incandescence and the subsequent cooling down were conducted in a stream of hydrogen, this had no effect upon the resistance to combustion. Soot, which was made incandescent under similar conditions, also acquired the properties of graphite in a high degree, so that for many purposes (*e.g.* in galvano-plastic work) soot that has been made strongly incandescent can be made to replace graphite. The property of leaving an impression which graphite possesses, and which makes it so well adapted to the manufacture of lead-pencils, was not acquired by the different kinds of carbon in the process of incandescence; very probably this property depends upon the crystalline composition of the graphite.

VIENNA

Imperial Academy of Sciences, March 8.—E. Mach, experiments and notes on the system of lightning-conductors of Mr. Melsens.—C. von Ettingshausen, contributions to the knowledge of the Tertiary flora of Java.—L. Pfaundler, on the mantle-ring machine of Kravogl, and its relation to the machine of Pacinotti-Gramme.—F. Hochstetter, sixth report of the Prehistoric Commission: on the mounds recently found at Watsch and St. Margarethen (Carniola).—F. Steindachner, on Japanese fishes.—G. Goldschmidt and R. Wegscheider, on the derivatives of pyrene.—R. Wegscheider, on some derivatives of opianic acid.—E. von Bruecke, on alcophyr, and on the true and the so-called biuret reaction.

April 5.—E. Mach, preliminary communication on new experiments made with the influence-machine.—F. Lukas, on the knowledge of the absolute strength of vegetable tissues.—W. Simerka, on the power of conviction (a mathematical study).—T. V. Tanovsky, on nitro and amido derivatives of azobenzene.—A. Nalepa, contributions to the anatomy of Stylommatophora.—A Lieben and L. Haitinger, preliminary communication on chelidonic acid.—E. Lippmann, on azylines.—B. Schwarz, on an eclipse of the sun mentioned by Archilochos. T. M. Pernter, psychrometrical studies.

PARIS

Academy of Sciences, June 25.—New methods of determining the right ascensions and absolute declinations of the stars (continued), by M. Lœwy.—Experimental studies in relation to the photometric observation of the eclipses of the satellites of Jupiter, by MM. A. Cornu and A. Obrecht.—A study of the deformations produced by sharp-edged tools in drilling, by M. Tresca.—On the employment of partial photographs in studying human and animal locomotion, by M. Marey. The object of this process is to avoid the great confusion caused by the superposition of numerous reflections in the case of slow locomotion. It is illustrated by a cut showing the attitude of the left leg of a man walking at a moderate pace and reflected at the rate of about sixty per second. The partial photographs obtained by this method enable the observer to analyse all kinds of motion, such as walking, running, leaping, and even action confined to one place.—On the action of mixtures of air with vapour of chloroform, and on a new process of anæsthesia, by Paul Bert. The experiments were made on dogs, which were treated with doses of chloroform diluted in varying proportions with air. From the effects observed it is hoped that many important problems may be solved connected with the action of this anæsthetic. But although all risk may thus be avoided in its application, the disadvantages inherent in chloroform itself cannot be overcome, and protoxide of azote still maintains its preeminence above all the anæsthetics.—On the reciprocal of homogeneity; similitude of mathematical formulas, by A. Ledieu.—Methods of separating gallium from ruthenium, osmium, arsenic, and selenium, by M. Lecoq de Boisbaudran.—On a case of long-standing hysteria, all the symptoms of which disappeared under the influence of aluminium, by M. Burcq.—On a method of computing secular perturbations in the elements of the orbits of planets, asteroids, comets, &c., by O. Callandreau.—A new generalisation of a formula of Lagrange, already generalised by Cauchy, by Em. Barbier.—On the relations of induction to electrodynamic action, and on a general law of induction, by M. Quet.—Automatic impression of telephotic despatches, that is, of despatches transmitted by light, memoir by M. Martin de Brettes.—On a method of determining by constant registration the slight movements of the crust of the earth. This method of recording microseismic movements was first suggested by MM. Bertelli and de Rossi, and forms the subject of a paper published in the *Engineer* for December 17, 1875.—On the sulphate of thorium, by Eug. Demarçay.—On a base derived from crotonic aldehyde, by Alph. Combes.—Researches on mesitylene, by MM. Robinet and Colson. A new glycol is described, and it is shown that the dichloride and the dibromide of mesitylene obtained by the action of chlorine and bromine on mesitylene gas are identical with the dichlorhydric and dibromhydric ethers of this gas.—Observations on the fermentation of breadstuffs, by M. Moussette.—On the concomitance of the anatomic and organographic characters of plants, by M. J. Vesque.—Borings at Rilhac, in the Brassac basin, east of Arvant, by M. Daubrée. These borings were most successful, revealing at a depth of eighty-six metres productive carboniferous strata underlying horizontal beds of clay and more or less argillaceous sandstones.—Borings at Toussieu, department of Isère, by M. Grand'Eury.—After piercing various alluvial, limestone, clay, and sandstone formations, coal was reached at a depth of 364 metres. These borings were begun after those of Chaponay had revealed carboniferous beds at the depth of 212 metres immediately below the marine molasse. The chief object of both is to determine the extension of the coal measures of the Loire basin under the tertiary plain in the north of Lower Dauphiny.—Scientific results of Col. Prejevalsky's journeys, and especially of his third expedition towards Tibet and the sources of the Yellow River, by M. Venukoff. Amongst the more important results were the animal and vegetable collections, comprising 408 specimens of 90 species of mammalia, 3425 of 400 species of birds, 976 of 50 species of reptiles, 423 of 53 species of fishes, 6000 of insects, and 12,000 of 1500 species of plants.

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