THURSDAY, MAY 22, 1873

THE FUTURE OF THE ENGLISH UNIVERSITIES

AN ECHO FROM OXFORD

THE Association for the Organisation of Academical Study has inaugurated a good work, which must in the end have an important result. But in the expressions of policy as yet put out by that body we notice an omission which perhaps is intentional, but in any case a very serious, indeed a fundamental one. It is well enough to declare that the collegiate and other revenues of Oxford and Cambridge should be devoted to the encouragement of research, and to placing the highest kind of teaching in all subjects within the reach of the people of this country. It is most true that to this end prize-fellowships and non-resident sinecures must be abolished, and in their place we must have carefully-chosen professors, assistant-professors, and lecturers, teaching and carrying on original research in all departments of knowledge. With such a programme in hand the members of this association can very plausibly demand for the old Universities that they be not despoiled of their excessive wealth, but that this wealth be made operative and productive within the limits of the Universities themselves. Nevertheless there is a question which necessarily arises—whenever the future of the English Universities is mentioned—which the Association has not discussed, and which we think it ought boldly to meet, even though it should lead to a split in the ranks. That question is this—Are Oxford and Cambridge to remain as institutions exclusively for the elegant education—the "culture"—of the upper classes who may choose and can afford to allow their sons to while away certain years there? or are they to be made engines of national education where a poor man may go with as much reason as a rich one; and profitably spend his time in acquiring knowledge and training which have a real value in the world and place their possessor in the position to earn his bread and his standing among men?

It is a fact that at this moment a youth entering a college at either Oxford or Cambridge and taking his degree after four years of a very pleasant life, having spent during the process at least 800*l.* (*i.e.* 200*l.* a-year) comes away, not a whit further on in the battle of life than he was on entering. He has acquired some good habits, many very bad ones, but has received no training nor instruction which will render him useful to other men, excepting—the exception is a very significant one—as a clergyman or as a schoolmaster.

The state of things is neither more nor less than this—that a young man cannot study at the English Universities and associate with even the most steady-going of his fellow-students at a less expense than that named above; and that the University cannot, at any rate does not, teach him anything in a practical or professional way. It is useless for Cambridge and Oxford to open their classes to non-collegiate students, as long as those classes are not something more systematised and practically-pointed than they can be, when exclusively designed as parts of a so-called elegant or "liberal" education. Men who are intending to work hard in life cannot afford to pass through such a course after leaving school; and hence our University students are, with a few exceptions, drawn from the richer classes; hence, too, the amount of luxury and rarity of earnest study amongst them, which reacts on many of their teachers. The present position of the Universities with regard to education for the business of life is merely that of a preparatory school. The same limitations of subjects—the same books are in force here, with some small additions for the few "honour men," as in our public schools, such as Eton, Harrow, and Rugby. The B.A. degree—the ordinary examinations for which any average boy on leaving school at sixteen or eighteen years of age could easily pass—absorbs nearly all the activity; is, in fact, almost the highest effort of each University. Almost all the teaching, certainly all the college work, is directed and governed by the requirements of this preparatory course which prepares for nothing. Whilst the intellectual standard thus held up is childish enough, it is necessarily accompanied by a system of tutorial superintendence and direction as wearisome as it is injurious.

In fact, the best effort in Oxford and Cambridge—the most striking movement in recent times—as compared with the dead calm of some fifty years since, has been rather a retrogression than an advance; we are less of Universities now than then, and have become more like—and are daily becoming more like—the great public schools, such as Eton and Harrow. The greater part of all the college-teaching staff is employed in doing the very same work as that done in the schools, which ought never to be required at a University at all. As the arrangements and innovations of the various college-bodies are watched, it becomes obvious that the schoolmaster is abroad in a very ambitious spirit with the avowed object of making the University a great Seventh Form, similar in discipline and character of instruction to his own pedagogic institution.

This state of things is defended by a large number of persons—among them members of the Association—with two words chiefly in their mouths—"culture" and "technical." It is maintained that "technical education" (an expression which is used for the purpose of suggesting the less intellectual side of what it is better to term "professional education") is not the function of the Universities, that it cannot be conveniently undertaken in them, that it is better carried out in the great cities such as London, Manchester, Edinburgh, whilst the Universities in their academic seclusion can administer that smattering of omniscience, dilettantism, and good manners which it is so important for persons of a certain income to possess. To obtain this a youth must be prepared to sacrifice time and money; and in offering this the University is, according to the opinion of many resident fellows, doing its work in the world. The selfishness of this view of University functions is patent enough. Clearly it is an easier matter to undertake this ornamental work, and to leave to others the business of life. It appears to be overlooked by its advocates that the Universities thus may, or rather have, lost all influence, all share in the life of the country. In renouncing technical or professional education, the University renounces all those who must have such education at the age when she might receive them. Those who really value, as we do above most things, breadth of intellectual interests—who have intense repugnance to narrow "specialism"—cannot, upon due consideration, defend the separation of "ornamental" and "technical" education, as likely to conduce to increase of culture among our fellow-countrymen. It is by undertaking most fully the charge of the higher education—of those for whom without distinction such education is necessary—that the Universities can really do most for the cause of culture. When Oxford and Cambridge succeed in getting hold of all such students then only can thoroughly satisfactory results be expected by those who are anxious for the progress of the higher education. What we desire more earnestly is, that Oxford and Cambridge may be the means of giving breadth of view and interest to as large a number of young Englishmen as possible, for it is this that we understand by "culture:" not the mere ease of manner due to luxury and the select association of leisured men. Oxford and Cambridge can spread true culture, and can have pretensions to such an office only when acting up to their trust and fully providing for the very best and fullest professional study in all departments. There are some to whom it appears important that the Association should plainly declare itself on this matter, before proceeding to the question of the foundation of institutions for scientific and literary research within the University. If on the one hand the Association were to declare for the exclusion of professional study, and at the same time to advocate the foundation of increased means and material of research within the University, we should feel at once that the policy of the Association would not be accepted by all. There is a great deal of human nature in the men who occupy distinguished positions in our Universities, and in the select atmosphere of non-professional students and cultured ecclesiastics there is an inevitable languor and repose of the mind which are infectious. The most vigorous body becomes limp before the sirocco, and in this atmosphere of luxurious culture it may be doubted whether even Faraday could have carried out his investigations: probably only by investing himself in a kind of mental diver's costume. On the other hand, the presence of an active body of those who for want of a better word we may call professional students—of men who, having neither time nor money for self-indulgence, determinedly work round their professor—the presence of a whole lot of such professors each so surrounded, and the association thus established between the Universities and the progress of the body of the country in the arts and sciences, would bring about a gigantic change. Professors so surrounded might with advantage be largely increased; the purely ornamental students would be by no means dislodged—they would remain in numbers then as now—but beneficially influenced by the example of the career-seeking and professional student. These in their turn would be benefited by a duly proportioned infusion of those students seeking exclusively "culture"—the amateurs and patrons of serious pursuits.

It is, then, only on the basis of professional training in the widest sense of the term, that we should care to see a reorganisation of Oxford and Cambridge. Let the colleges be taxed, say, to the extent of fifty per cent. of their revenue in order to support the professoriate and the appliances which each faculty may deem adequate, not only for direct "student teaching," but for progressive research. Then we may hope to see our Universities elevated from the condition of mere finishing schools for young gentlemen. If such a plan cannot be carried out, it would seem useless to simply create sinecures within the old places, larger and probably less productive than those which at present exist. Sharp and painful though the measure might be—we should in that case have to yield to the removal of means which have so long lain idle. The colleges would be relieved of their excessive income to support more practical institutions elsewhere, and Oxford and Cambridge would collapse into the condition of mere theological seminaries. When the Association meets on Saturday next, it would be well that this point should be raised, lest by the silence of the leaders of the movement, any one should be lukewarm in its support.

FRICK'S PHYSIKALISCHE TECHNIK

Oder Anleitung zur Anstellung von physikalischen Versuchen und zur Herstellung von physikalischen Apparaten mit möglichst einfachen Mitteln. Von Dr. J. Frick. (Braunschweig, 1872.)

THIS most useful book has now reached the fourth edition, and has swelled to 700 pages, illustrated by 986 wood engravings. To some British physicists and teachers the work has already proved itself serviceable, but there are doubtless many to whom it is at present unknown who would find much valuable information therein.

Dr. Frick's work is not in any sense a manual of experimental physics; it is rather an elaborate treatise upon physical apparatus and the methods of physical research. Its object, we learn from the preface, is to give an introduction to the methods of conducting physical inquiry, to enumerate the precautions which it is necessary to adopt in order to ensure success, and to give ample directions with reference to the construction of apparatus and its management. This field is, comparatively speaking, untrodden before, and we have no hesitation in saying how thoroughly successful Dr. Frick's attempt to guide us over it has proved. We shall briefly indicate the contents of the book, and then point out the few matters in which we think the execution of the task has fallen short of what might have been fairly expected.

The first part contains a sketch of the arrangements necessary for the physical laboratory, and a detailed account of the methods of manipulating glass, metals, and other materials which are required for the apparatus described in the second part. This portion of the book is very interesting and useful. We find here numerous hints on turning, glass-blowing, and similar processes with which it is well for the physicist to be acquainted. In the second part we have in Chap. I. a description of the apparatus necessary for the study of the equilibrium of forces applied to solids, liquids, and gases; Chap. II. describes the apparatus used for experiments on motion

Chap. III. is on acoustics ; Chap. IV. on light ; Chap. V. on magnetism ; Chap. VI. on electricity ; Chap. VII. on heat. It may be remarked that the figures are drawn to scale, and further illustrations of the details are added whenever necessary.

As a fair specimen of the illustrations and descriptions we may refer to Article 121, wherein is described Müller's apparatus for studying experimentally the free falling of a body. This beautiful contrivance is for the purpose of causing a point vibrating horizontally to trace a curve up on a board descending vertically. From the form of the curve the law of falling bodies is deduced. In Chap. IV. we meet with many interesting contrivances : for example, Fig. 433 represents an arrangement for showing the principle of the rainbow experimentally by the aid of spheres of glass. This chapter is concluded by a practical lesson in photography. Many of the figures in Chap. VI. will be found to represent electric instruments which are manifestly great improvements on forms in ordinary use. As an example we refer to the Rheostat, Fig. 775.

Considering the book has already reached such portly dimensions we can hardly complain of omissions. We are, however, of opinion that the space at the disposal of the author might have been more judiciously employed if some of the apparatus which he has described were omitted and some instruments which he has passed over were inserted instead. To illustrate this remark we may refer to the chapters on mechanics. We there find a number of ingenious contrivances generally pretty well known, but we also meet with toys like those described in articles 66 and 67 which could, we think, have been very well dispensed with. On the other hand we seek in vain in the same chapter for a full account of Willis's system of mechanical apparatus. To say that this ingenious system would, with trifling additions, enable all the mechanical experiments described by Dr. Frick to be performed is to give a very inadequate idea of its resources. In the hands of a competent experimenter Willis's apparatus will be found to provide in a substantial form the principal parts necessary for nearly every conceivable experiment in mechanical philosophy. The framework of this apparatus is so useful in almost any physical research that we cannot conceive how it could have been omitted from "Physikalische Technik," had the author of that work been acquainted with the writings of Prof. Willis. We think also that some of the host of merely qualitative experiments described for the purpose of illustrating centrifugal tendency (Article 124) might very well be omitted. On the other hand, we miss Smeaton's machine, which, admitting as it does of exact quantitative results being determined. is perhaps, next to Atwood's machine, the most useful instrument we have for illustrating the truths of dynamics.

We are tempted to think that Dr. Frick is not adequately acquainted with English scientific literature. This opinion receives some confirmation when, on turning over 238 closely-printed pages which describe electrical apparatus, we fail to see Sir William Thomson's beautiful instruments described ; nor on turning to the Index do we even find the name of that philosopher mentioned.

Although we decidedly think this book might have been better, yet we decidedly think that it is very good, and we cordially recommend it to the notice of physicists and lecturers, who will certainly find it useful.

OUR BOOK SHELF

Electricity. By R. M. Ferguson, Ph.D., F.R.S.E. (W. and R. Chambers.)

WE regret that the Elementary Treatise on Electricity has not been revised by its author since its first appearance. For example, useful as is the chapter on the absolute measurement of an electric current, its usefulness to students would be increased by a fuller and more detailed explanation. At the foot of p. 159 it is stated that "the heating effect (of the current) depends on the strength of the current and the resistance." It should be the *square* of the strength of the current into the resistance, as is correctly stated in a preceding paragraph. On p. 153 there is a mistake in the calculation of the quantity of water decomposed by a current ; 60 c.c. × tan. $51\frac{1}{3}$ = 75 c.c., and not 80 c.c., as is stated, and afterwards assumed. A description of the sine-galvanometer ought hardly to have been omitted, and a fuller explanation, together with an engraving of Thomson's reflecting galvanometer, ought surely to be given. There is also but a meagre account of the induction coil, and the function of the condenser is not explained : the term *rheotom* instead of contact-breaker, looks pedantic, and may puzzle some readers. But the most faulty part of the book in our estimation is the singularly obscure and misleading manner in which the terms Electric Quantity and Tension are defined on p. 64. Tension is spoken of as synonymous with electric depth, or as the French say, electric thickness ; whereas the tension, pressure, or power of discharge possessed by any electrified point, varies as the *square* of the electric depth at that point.

The first part of this text-book relates to magnetism and more evident care has been bestowed on this portion. The charts of isogonic and isoclinic lines are most useful, and so also are the chronological appendices, in which a brief scientific history of each subject is given. But why could not the dip and declination be given for a later year than 1865 ? It is said on page 16 that two magnetic needles are absolutely necessary to show "the power of the earth in determining the position of the needle," and that "if it were possible to hang a needle in the air so as to leave it perfectly free to take any position, it would show us fully the directive action of the earth." Is it not possible to buoy a magnetic needle in water, or sink it in mercury, so that the action of gravity may be neutralised, and the directive influence of the earth wholly come into play? Moreover, many dipping needles are made with a swivel pivot, by means of which the declination and dip are roughly shown at the same time. Two other blunders we notice in the part on magnetism. On page 4, speaking of a "small magnetic bar or needle," Dr. Ferguson says that "if both poles of the needle are attracted indifferently by any end of it [a bit of iron], it is not magnetic." This is as slipshod in its science as it is in its English, for it is precisely the test of a magnetic body that it does attract either end of the needle ; magnetic should of course read magnetised, and so again a few lines lower down. The other blunder is on page 14, where it is said that "cobalt is attracted by the magnet at the highest temperatures." It is well known, and can easily be shown as a class experiment, that cobalt loses its magnetic character at a white heat. But in spite of these errors, Dr. Ferguson's "Electricity" is a book that has been of much use to both teachers and students of science. Its obvious merits lead us to hope that a revised edition may find it free from the defects to which we have drawn attention.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Forbes and Tyndall

At p. 387 of the recently published "Life and Letters" of the late Principal Forbes, the following passage occurs:—

"I believe that the effect of the struggle—though unsuccessful in its immediate object—will be to render Tyndall and Huxley and their friends more cautious in their further proceedings. For instance, Tyndall's book, again withdrawn from Murray's 'immediate' list, will probably be infinitely more carefully worded relative to Rendu than he at first intended."

This passage has been selected, among others, by Principal Shairp, the editor of this portion of the "Life," from a letter addressed to A. Wills, Esq., under date of November 14, 1859: the "struggle" to which it refers arose out of an attempt on the part of some influential friends of Principal Forbes, who were at that time members of the Council of the Royal Society, to obtain the Copley medal for him; and it took place at the Council meetings which were held on October 27 and November 3, 1859.

I was not a member of the Council at this time, and therefore, I could take no direct part in the "struggle" in question. But, for some years before 1859, glaciers had interested me very much; I had done my best to inform myself in the history of glacier research; I had followed with close attention the controversy which had been carried on between Prof. Tyndall and his friends, on the one hand, and Principal Forbes and his supporters on the other; and, finally, I had arrived at a very clear conviction that the claims made for Principal Forbes's work, could not be justified.

Under these circumstances I thought it would be a most unfortunate occurrence if the Council of the Royal Society, containing as it did, not a single person who had made the glacier question his especial study, should practically intervene in the controversy then raging, and throw its weight upon the side of one of the combatants, without due consideration of what was to be said on the other side.

A friend of mine, who was a member of the Council, shared these views; and, in order to enable him to enforce them, I undertook to furnish him with a statement which he could lay before the Council when the award of the Copley medal came up for discussion.

It is not necessary to state what took place at the meetings of the Council—suffice it to say that the Copley medal was not awarded to Principal Forbes.

So far, therefore, as my statement may have contributed to this result, my efforts were completely successful. Principal Forbes's very influential champions in the Council were left, as I am informed, in a hopeless minority; and instead of tending to make me more cautious in my "future proceedings," what occurred on this occasion should have emboldened me.

The notion expressed by Principal Forbes that I and Prof. Tyndall's other friends were in any way discouraged by the results of our battle, is therefore strangely erroneous; however, I do not know that the error would have been worth correction, if Prof. Tyndall had not been referred to as one of those who took part in the fray. But, in justice to Prof. Tyndall, I am bound to say that he knew nothing about the battle until after it was over. My ally in the Council and I, agreed, for reasons which will be obvious to any honourable man, that Prof. Tyndall, though an intimate friend of ours (and largely because he was so), ought not to have any knowledge of the action we took; and, in a note dated November 4, 1859, I find myself suggesting to my friend in the Council, that Tyndall ought to be kept in his then ignorance "until his book is out." I have every reason to believe that this suggestion was carried into effect; at any rate, Prof. Tyndall did not see the drift of my statement till a year ago when (on May 13, 1872) I sent it to him accompanied by some other documents and the following note:—

"Routing among my papers yesterday I came upon the inclosed cinders of an old fire, which I always told you you should see some day. They will be better in your keeping than mine."

I am informed that there was not even an attempt to controvert the leading points of my statement on the part of the advocates of Principal Forbes's claims; and therefore the assertion that Prof. Tyndall was led to word "infinitely more carefully" what he had already written about Rendu, by anything which occurred in the Council, is simply preposterous.

In making these remarks I have no intention of throwing the slightest blame upon the late Principal Forbes; who surely had a perfect right to express to an intimate friend whatever impression was left upon his mind, by such reports as reached him of the occurrences to which he refers. But I confess I find it difficult to discover any excuse for the biographer, who deliberately picks the expressions I have quoted out of a private letter, and gives them to the public, without taking the trouble to learn whether they are, or are not, in accordance with easily ascertainable facts.

T. H. Huxley

May 17

Forbes and Agassiz

In the review of Dr. Tyndall's book on the "Forms of Water" which appeared in Nature, vol. vii. p. 400, the following words occur:—"But surely it was not unnecessary to rake up again the Forbes-Rendu controversy, nor to renew the claims of Agassiz and Guyot." Mr. Alexander Agassiz takes exception to this (see Nature, vol. viii. p. 24) and makes the following assertions:—That when a guest of Agassiz on the glacier of the Aar in 1841, Forbes returned the hospitality of Agassiz "by appropriating what he could" from the work of the latter, and "misrepresenting the nature of his intercourse with Agassiz." This refers to a matter of facts and may be proved or disproved by the facts. It refers to an attack made upon Forbes in 1842, which was immediately answered by him in a manner that left no room for further discussion. I must necessarily be brief in stating the facts. They may be found fully detailed in the *Edin. New Phil. Journal*, 1843, or in the "Life and Letters of James David Forbes, 1873." They are as follows:—In 1841 Forbes enjoyed the pleasure of a visit to Agassiz on the Unteraar Glacier. On the first day of their sojourn (August 9), their only companion was Mr. Heath, of Cambridge. They were afterwards joined by friends of Agassiz. On this first day Forbes pointed out to Agassiz the veined structure of the ice. Agassiz had spent five summers studying the glaciers (see Mr. Alexander Agassiz' letter in Nature), but he replied "that it must be a superficial phenomenon, that he had on a previous occasion noticed such markings, and that they were caused by the sand of the moraines causing channels of water to run." Forbes showed him that the structure was general, even in the body of the glacier. Agassiz expressed a doubt "whether the structure had not been superinduced since the previous year." Forbes afterwards showed him that in a crevasse three or four years old the markings extended across the crevasse and were visible in continuation from one side to the other. Further, Forbes insisted upon its intimate connection with the theory of glaciers. When in the ensuing winter M. Desor wrote to Prof. Forbes denying his claims to the discovery, the latter sent him a statement of the above facts, begging that M. Agassiz should state whether they were correct or not. M. Agassiz wrote an answer to this letter. He does not deny a single one of the facts supplied by Forbes in connection with the observations of August 9. This letter was printed and circulated by M. Agassiz. Furthermore, when these facts were published by Forbes, even then M. Agassiz did not deny any of them. Moreover, Mr. Heath, the only other witness, gives his evidence in support of the accuracy of the above facts (see "Life of Forbes," Appendix B, Extract I.). Other friends of Agassiz, who joined them afterwards, wrote to Forbes stating their belief that to him alone belonged the discovery. After leaving the Aar glacier Forbes extended his observations. He showed (1), that the structure was common to most, if not all, glaciers (see "Forbes' Life," p. 550, note); (2), that this was the cause of the sand lying in lines ("Life," p. 548); (3), that this was also the cause of the supposed horizontal stratification of the terminal face of some glaciers (Royal Soc. Edin., 1841, Dec. 6); (4), he showed that these blue markings were the outcroppings of blue ice that formed lamellar surfaces in the interior of the glacier; (5), he actually determined the shape of these surfaces in the case of the Rhone glacier (R. S. E., 1841, Dec. 6); (6), he remarked that "the whole phenomenon has a good deal the air of being a structure induced perpendicular to the lines of greatest pressure," though he did not assert the statement to be general. This was in 1841. In later years he extended these observations. I have said enough to prove (1), that although Agassiz carried with him "a geologist, a microscopic observer, a secretary, a draughtsman, and many workmen," and though he had spent five summers studying the glaciers, he did not see these markings (or at any rate recognise them as a structure of the ice) until Forbes showed them to him; and (2), that Forbes recognised this structure as an important "indication of an unknown cause" ("Occasional Papers," p. 4), and worked out the subject thoroughly.

In the *Comptes Rendus* for Oct. 18, 1841, a portion of a letter from Agassiz to Humboldt was published. Here he lays claim to the discovery without mentioning the name of Forbes. He speaks of it as "le fait le plus nouveau que *j'ai* remarqué." Forbes felt deeply annoyed at this conduct of his friend, but contented himself with publishing his own discovery. A rupture between the two friends now commenced. About this time M. Guyot recollected that he had described this appearance in 1838 to the Geological Society of France, at Porrentruy ("Agassiz Etudes," p. 207). Several people had seen the same thing previously. Among others, Sir David Brewster writes as follows:—"The Mer de Glace is like the waves of the sea, as if they had been fixed by sudden congelation; when the ice is most perfect, which is on the sides of the deep crevices, the colour is a fine blue. There is an appearance of a vertical stratification in the icy masses stretching in the direction of the valley in which the glacier lies. . . . The surface of the glacier exhibits also the appearance of veins exactly like blocks [?] of stone" (*Journal*, 1814). In 1820, M. Zummstein saw it ("Bibliothèque Universelle," 1843). Col. Sabine and M. Elie de Beaumont had also seen it ("Travels in the Alps," p. 29). But though seen it had not been studied, nor did any printed description of it exist. M. Guyot did not even print an abstract of his communication. It remained an isolated, unprinted, forgotten fact until Forbes appeared upon the scene. Professor Tyndall has most justly said that neither Forbes nor Agassiz knew of it in 1841 ("Forms of Water," p. 187). Yet though, as has just been proved, Forbes pointed it out to Agassiz in 1841, the latter tried to show that he had known of Guyot's observation (letter from Agassiz to Forbes, "Life of Forbes," Appendix B), and endeavoured to give the credit to Guyot rather than to Forbes (his own claims having been now disproved). If it be true that he knew what Guyot had done, then (1) why did he not mention it to Forbes and Heath, both of whom affirm (in contradiction to the statements of Agassiz) that Guyot's name was not mentioned? (2) Why did he not perceive the importance of the structure? (3) Why did he say that it was superficial? (4) Lastly, how could he reconcile it with his conscience to describe it to Humboldt as "le fait *le plus nouveau* que *j'ai* remarqué?"

The facts show (1) that Forbes was seriously wronged by the conduct of Agassiz; (2), that he discovered independently the veined structure; (3) that he was the first to study the subject and give it its true place in reference to glacier theories. I have limited myself to the accusation contained in the letter of Mr. Alex. Agassiz. Whether he is correct in his appreciation of the estimate put upon Forbes' labours, in Dr. Tyndall's last popular work, I need not at present discuss. I know so well to what conclusion a comparison of that book with the writings of Forbes and other workers on glacier theories would lead, that I leave it confidently to the judgment of those "fair-minded investigators" of whom Mr. Alex. Agassiz speaks.

GEORGE FORBES

P.S.—Mr. Heath's testimony, to which I have referred, is given in the following extract from a letter dated Trinity College, Cambridge, Feb. 25, 1842:—"I will witness—1st, that he (Agassiz) knew nothing about it; 2nd, when he did see it he said it was superficial *sand*; 3rd, that he was the last to believe that it went to any depth. I think your account very true, and not claiming one jot more than fully belongs to you."

Cambridge, May 20 G. F.

Perception and Instinct in the Lower Animals

THE suggestion made by me in your issue of February 20, that animals which had been deprived of the use of their eyes during a journey might retrace their way by means of smell, had the effect of letting loose a flood of illustration, fact, and argument bearing more or less directly on the question; and as the stream now seems to have run nearly dry, I ask permission briefly to review the evidence adduced, so far as it affects the particular issue I brought forward. Several of the writers argue as if I had maintained that in all cases dogs, &c., find their way, wholly or mainly, by smell; whereas I strictly limited it to the case in which their other senses could not be used. The cases of this kind adduced by your correspondents are but few. The first, and perhaps the most curious, is that of Mr. Darwin's horse; but, unfortunately, the whole of the facts are not known, As Mr. Darwin himself pointed out, the horse may have lived in the Isle of Wight, and been accustomed to go home along that very road. I would suggest also that the country might resemble some tract in the neighbourhood of his own home; or that the horse, having been brought from home by a route and to a distance of which it had no means of judging, thought its master was riding home on the occasion in question, and therefore objected to turning back. Anyhow, the case is too imperfect to be of much value as evidence in so difficult a matter. "J. T." (March 26) quotes the case of the hound sent "from Newbridge, county Dublin, to Moynalty, county Meath," thence long afterwards to Dublin, where it broke loose, and the same morning made its way back to its old kennel at Newbridge. I can find no "Newbridge, county Dublin," although there is a Newbridge, county Kildare, which is 26 miles from Dublin, on a pretty direct high road. That the dog never attempted to return during its "long stay" at Moynalty seems to show that some special facilities existed for the return from Newbridge. What they may have been we cannot guess at in the total absence of information as to the antecedents of the dog, the route by which he returned, and the manner in which he conducted himself on first escaping in Dublin.

The next case, of the two dogs returning from Liverpool to near Derby, is vague, and also without necessary details. It happened 50 years ago, and the only evidence offered as to the mode of the dogs' return is that "*it is said* they were seen swimming the Mersey." "N. Y.'s" case (April 24) of the dog who "did not make haste back," and therefore could not have returned by smell, is also most inconclusive. The distance was only 20 miles, and we know nothing of the route the dog followed, or the time it took. How do we know the dog did not wait the three weeks till it saw someone it knew living at or near its former house, and followed that person? This appears to me to be an exceedingly probable way of accounting for many of these returns where the distance is not very great. This brings me to the case of Mr. Geo. R. Jebb, who seems to have gone to the trouble of making an experiment which, with a little more trouble, might have been very complete and satisfactory. The dog was taken by rail very circuitously from Chester to a place 10 miles from Chester. It "hung about the station for about an hour and a half," and in three hours more arrived at its home. But we are still left totally in the dark, both as to the route it took or the process by which it decided on that route. What is required in such experiments is, that a person not known to the dog should be ready to watch and follow it (on horseback), noting carefully on the spot its every action. We should then perhaps know why it "hung about the station" an hour and a half before commencing its journey home, and afterwards, whether it showed any hesitation as to its route, and whether it followed the road or went straight across country. A few experiments carefully made in this way, at distances varying from 10 to 30 miles, and with a thorough knowledge in each case of the animal's antecedents, would, I venture to say, throw more light on this interesting question than all the facts that have been yet recorded. The only experiment of this kind I have met with is in the work of Houzeau ("Etudes sur les Facultés Mentales des Animaux"), and it is so curious that I give the passage literally. He says (vol. i. p. 156): "I have succeeded in making young dogs of five or six months lose themselves on first going out with me. They would begin by seeking for my trace by smell; but not succeeding in this, they would decide to return home. If there was a path, they followed the route by which they had come. If it was an untrodden virgin country, they shortened the circuits they had made in coming, but did not altogether depart from them. One would say that memory furnished a certain number of points which divided the route, and they went towards these by memory of directions. Thus inscribing chords to the curve by which they had come, they returned to the house." M. Houzeau's general conclusion from a considerable body of observations made with this point in view is, that animals find their way by exactly the same means as man does under similar circumstances, that is, by the use of all their faculties in observation of locality, but especially by a memory of directions and by a ready recognition of places once visited, which serve as guide-posts when they are again met with. This seems to me a very sound theory, and quite in accordance with all that is known of the manner in which savages find their way.

The more general objections to my little theory which are made in your leading article appear to depend on the denial, to such animals as dogs and horses, of that amount of common sense and reasoning power which I believe them to possess, and also to the assumption that in the case supposed they would recollect merely the odours, not the objects the presence of which these odours had indicated. I imagine that animals know, just as well as we do, that some sights, sounds, and smells are caused by permanent, others by evanescent or changeable causes. The smell or sound of a flock of sheep would indicate to a dog the presence of an actual flock of sheep, just as surely as the sight of them would do, and he would no more lose his way because those sheep were not in the same place the next day or the next week, than he would had he travelled the road on foot with his eyes open. The smell of a wood, of a farmyard, of a ditch, a village, or a blacksmith's shop, with the more or less characteristic sounds accompanying these, would tell the dog that corresponding objects were there just as surely as the sight of them would do. On his return he would recognise the objects, not the smells and sounds only, and he would be no more puzzled by the absence of certain moveable objects he had recognised by smell than he would be had he seen them. I quite believe that mistakes would often be made owing to the discontinuousness of sufficiently characteristic odours; but the process of "trial and error," suggested by F.R.S., would be constantly used, and this is in accordance with the length of time usually taken in these journeys, often very much longer than would be required for a return by the shortest route and at moderate speed.

A friend has communicated to me a most remarkable fact, of a different character from any which have been referred to during the course of this discussion; and as I have it at first hand and took the exact particulars down as narrated to me, I think it will be of value. Many years ago, my friend lost a favourite little dog. He was then living in Long Acre. Three months after, he removed to a house in another street about half a mile off, a place he had not contemplated going to or even seen before the loss of the dog. Two months after this (five months after the dog was lost) a scratching was one day heard at the door, and on opening it the lost dog rushed in, having found out its master in the new house. My friend was so astonished that he went next day to Long Acre to an acquaintance who lived nearly opposite the old house (then empty) and told him his little dog had come back. "Oh," said this person, "I saw the dog myself yesterday. He scratched at your door, barked a good deal, then went to the middle of the street, turned round several times, and started off towards where you now live." My friend cannot tell, unfortunately, what time elapsed between the dog's leaving the old and arriving at the new house. If every movement of this dog could have been watched from one door to the other, much might have been learnt. Could it have obtained information from other dogs (and that dogs can communicate information is well shown by Mr. A. P. Smith's anecdote in your issue of three weeks back)? Could the odour of persons and furniture linger two months in the streets? These are almost the only conceivable sources of information, for the most thoroughgoing advocates for a "sense of direction" will hardly maintain that it could enable a dog to go straight to its master, wherever he might happen to be.

Not to trespass further on your space, I would venture to hope that some persons, having means and leisure, would experiment on this subject in the same careful and thorough way that Mr. Spalding experimented on his fowls. The animals' previous history must be known and recorded; a sufficient number of experiments, at various distances and under different conditions, must be made, and a person of intelligence and activity must keep the animal in sight, and note down its every action till it arrives home. If this is done I feel sure that a satisfactory theory will soon be arrived at, and much, if not all the mystery that now attaches to this class of facts be removed.

ALFRED R. WALLACE

The Origin of Volcanic Products

I HAVE not yet had the advantage of seeing Mr. Mallet's translation of Palmieri's late work on Vesuvius, but have read with interest Mr. Forbes's review thereof and Mr. Mallet's reply in NATURE of Feb. 6 and March 20. I have no desire to enter into a controversy, but as I have for the past fifteen years taught and defended a theory of the origin of volcanic products identical with that now maintained by Mr. Mallet, I may be permitted to say a few words. That the source of all such matters was to be found not in the earth's nucleus but in sedimentary strata, was taught by Referstein in his *Naturgeschichte des Erdkörpers*, in 1834; and again, doubtless independently, by Sir J. F. W. Herschel in 1837; while, for my own part, I was led to the same conclusion before I became aware of the views of either of my predecessors, solely from a consideration of the varying composition of plutonic rocks and of the stony and vaporous products of volcanic action. To the views of Herschel I first called attention in the *Canadian Journal* for March 1858, and again in the *Quar. Geol. Journ.* for November 1859, pp. 488–496, § vii.).

In the first of these I have said: "If we admit that all igneous rocks, ancient plutonic masses, as well as modern lavas, have their origin in the liquefaction of sedimentary strata, we can at once explain the diversities in their composition. We can also understand why the products of volcanoes in different regions are so unlike, and why the lavas of the same volcano vary at different periods. We find an explanation of the water and carbonic acid, which are such constant accompaniments of volcanic action, as well as the hydrochloric acid, sulphuretted hydrogen, &c." The nature of the reactions between siliceous, calcareous, and aluminous strata, holding carbonaceous matter, gypsum, sea-salt, &c., was then discussed, and the products of their transformations under the influence of water at an elevated temperature considered. In both of these papers referred to, the inadequacy of the views of Phillips, Durocher, and Bunsen, to explain the origin of these various products, was maintained.

In the *Geological Magazine* for June 1869, I returned to this subject in a paper on "The Probable Seat of Volcanic Action," where, after repeating and enforcing the above views, I said: "Two things become apparent from a study of the chemical nature of rocks; first, that their composition presents such variations as are irreconcileable with the simple origin generally assigned to them; and second, that it is similar to that of the sedimentary rocks whose history and origin it is, in most cases, not difficult to trace." In what follows I endeavour to show in the latter the source of such "eruptive rocks as peridolite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen."

Mr. Mallet has, however, made a very important advance in this theory of volcanic action by pointing out a source of heat independent of the cooling nucleus. Referstein had supposed heat to be generated by chemical action in the sediments, and his view has lately been brought forward, in a modified form, by Leconte; but this I have always rejected as untenable. The chemical actions supposed to be involved in the processes would consume rather than generate heat. I have hitherto followed Herschel and Babbage in regarding the heat as directly derived by conduction from an incandescent nucleus, but Mr. Mallet has now shown that the work expended in the crushing of the strata which takes place in certain regions of the globe where the contraction which attends the slow refrigeration of the globe is displayed in corrugations of the crust, is more than adequate to explain volcanic heat. To this it must be added that, inasmuch as the crushing process takes place in strata which, from their depth, are already at an elevated temperature, the heat developed by the mechanical process comes in to supplement that derived by conduction from the igneous centre. Vose had already, in a general manner, pointed out the same thing, suggesting in terms which are, it is true, wanting in scientific precision, the notion that the mechanical force at work in the crushing of the strata was the source of heat. This, however, in no way detracts from the great merit of Mr. Mallet, who may rightly claim "to have been the first to apply weight, measure, and number to volcanic theory," and we await with great interest the publication of his quantitative results. Apart from his thermo-dynamic theory, however, his views of volcanic action are apparently identical with those of Referstein and Herschel, to which I have for many years been endeavouring to give form and consistency. I may here call attention to a paper, "On some Points of Dynamical Geology," published in the *American Journal of Science* for this month (April 1873), in which I have already alluded to the foregoing questions, and to the endeavours which I have for fifteen years been making "to reconstruct the theory of the earth on the basis of a solid nucleus." I have there rehearsed the views which I have all this time maintained as to the causes which determine the process of corrugation of the earth's crust, the accumulation of sediments, and the development of volcanic activity in certain regions of the earth; thus giving a theory of the geological and geographical distribution of past and present volcanoes.

T. STERRY HUNT

Institute of Technology, Boston, Mass., April 25

Kinetic Theory of Gases

On page 300 of the second edition of Maxwell's excellent little text-book on the "Theory of Heat," it is stated, as a result of the kinetic theory of gases therein set forth, that "gravity produces no effect in making the bottom of the column" (of gas) "hotter or colder than the top."

I cannot see how this result follows from the kinetic theory of gases. On the contrary, it seems obvious that thermal equilibrium can only subsist according to the kinetic theory, where the molecules encounter each other with equal average amounts of *work* or *vis viva*, and in order that this may be the case, the velocity of the molecules (and consequent temperature) of any upper layer must be less than that of the molecules in the layer next below; since, in order to encounter each other, the former must descend, and acquire velocity, while the latter must ascend and lose it. This would establish a diminution of temperature from the bottom to the top of a column of air at the rate (in the absence of any counteracting cause) of 1° F. for 113 ft. of height, as can easily be verified from the fact that on account of the specific heat of air 1 lb. requires 183 foot-pounds to raise its temperature 1° F. Radiation may diminish this and tend to produce equilibrium, but nevertheless it seems obvious from these two opposing tendencies a residual inequality of thermal condition would result, and that the top of a column would be cooler than the bottom. That this would be the case if the air were in general motion in the form of upward and downward currents, will not, I presume, be disputed; and surely molecular is on the same footing. If the particles of air are moving in every direction with great absolute velocity, in what respect does this differ from air currents? In fact, all the particles which at any epoch of time are moving in any given direction constitute an air-current in that direction, mingled, it is true, with currents in other directions, but moving with accelerated velocity if descending, and with retarded velocity if ascending, and thus always tending to produce a diminution of temperature with height as a condition of gaseous thermal equilibrium. J. Guthrie

Graaf Reinet, Cape Colony, April 2

Kerguelen Cabbage

I would like to know, through your paper, whether the naturalists of the *Challenger* have orders to attempt to collect the seeds of the Kerguelen Land cabbage (*Pringlea antiscorbutica*). It has often occurred to me that the attempt ought to be made to introduce this plant on the seashores of Northern Europe and America. John R. Jones

Milwaukee, Wisconsin, U.S. April 14

Yorkshire Terrier Story

The anecdote of the instinct of dogs given in the number of Nature, May 1, p. 6, is identical with one to be found in Bewick's "History of Quadrupeds," p. 367, 1800, which he calls the well-known story of the "Dog at St. Alban's."

The same story precisely, with some dramatic embellishments and names, occurs in "Bingley's Animal Biography," vol. 1, p. 223. A.

Dorking

BICHROMATE PHOTOGRAPHS

A SINGULAR discovery has recently been made touching the action of light upon substances rendered sensitive by the bichromates of potash and ammonia, which threatens to revolutionise photographic printing altogether, at any rate so far as the production of permanent prints is concerned. The printing by means of silver salts in the ordinary way, which is still in vogue with nearly all portrait photographers, will always find application, by reason of the simplicity of the manipulations and the delicate and pleasing nature of the results, albeit all silver photographs enjoy the unenviable notoriety of being perishable. First of all, they lose their pristine brilliancy and freshness, then a sickly yellowness gives place to the glossy whites of the picture, and finally the deep bronze shadows become of a flat brownish tint, which grows weaker and weaker as time goes on. To secure permanent photographs, which shall possess all the beauty and detail exhibited by silver prints, has been for many years the aim of photographic experimenters, and it was not until Swan and Johnson had contributed their well-known improvements that the production of a delicate photograph in permanent pigments became at all possible. Mechanical photographic processes, where the pictures are printed off in a press, are still beset with many difficulties of a practical nature, the most perfect of them—Woodburytype—requiring further elaboration before perfect prints of large dimensions can be secured.

Pigment photographs, or carbon prints, as they are generally termed, require three elements for their production—a pigment (such as Indian-ink, lamp-black, or some such substance), gelatine, and bichromate of potash, or ammonia. A compound of these three substances is spread upon paper, and termed pigment or carbon tissue. This tissue is printed under a transparent negative in the sun, the light acting more or less energetically upon the sensitive pigment, and rendering it insoluble in parts, so that when it is immersed subsequently in warm water certain portions refuse to wash away, and these form the image; during the exposure of the tissue to light, these parts have in fact become fixed by its action. This, as we all know, is what takes place in the formation of a carbon print.

It has been found that the action of light upon a bichromate film is very different in its nature to the result produced by the sun upon iodide of silver. A film of pure iodide of silver, as Dr. Reissig and Mr. Carey Lea have abundantly shown, may be impressed with an image which will fade out altogether if the film is afterwards preserved for a sufficient time screened from light. Indeed it is possible to impress iodide of silver with an image, allow the same to fade away in darkness, and then impress the film with a second and different picture. The photographic image, therefore, on iodide of silver is of an evanescent nature, becoming weaker and weaker, and, if preserved for any time, ultimately fading away altogether. Now, with a photograph upon a bichromate film, the reverse is the case. If an impression of the slightest kind is produced upon a film of gelatine sensitised with bichromate, and put away in the dark, the action of the light still goes on, and progresses until the image has become a perfect and vigorous one. This continuation of the solar action has been turned to good account by carbon printers, who in winter time and busy moments have printed their photographs in darkness instead of light; that is to say, in lieu of exposing their sensitive tissue in the sun under a negative for hours and hours, they merely do so for a few minutes, the slight image thus impressed being allowed to gain in vigour subsequently by preservation for some time—half-a-day or so—in darkness, before development in warm water. In the ordinary way only half-a-dozen copies can be obtained from one negative during the day, if all of them are fully printed in the sun, whilst if only incipient prints are produced, a score of impressions may easily be secured.

Within the last few days we have progressed a step further in carbon printing. M. Marion of Paris has discovered that if you take a bichromate image printed in the sun, and put it into contact with another bichromate surface, you produce upon the latter a similar impression. You can in fact take a carbon picture fresh from the frame and employ it as a printing block, from which any number of impressions are procurable. It is a most singular fact that a solarised surface should be capable of setting up an action upon another sensitive surface placed in contact with it. But so it is. The impression made by light upon a bichromate film is capable of transmission to another surface of like nature merely pressed against it. We have, as it were, stored up in the original print a quantity of sunlight which has been absorbed and may afterwards be communicated to other surfaces.

The importance of this discovery can scarcely be overrated, and there is no doubt but that it will work an era in the matter of carbon printing. We need secure but one single photograph printed in the sun in order to obtain a large number of copies, all of which shall be as delicate and vigorous as if they had been printed by sunlight. A sheet of gelatine sensitised with bichromate of potash is put under a negative and printed; it is withdrawn from the printing frame and immersed in a weak solution of bichromate of potash which swells up those portions of the surface that have not been attacked by light, and thus produces a picture in relief. The sheet of gelatine is then put into a press and impressions from it taken on sensitive carbon tissue, the block being moistened from time to time with bichromate solution. The copies thus produced upon the tissue are not fully printed and cannot be developed at once; they are simply incipient, or nascent, pictures, it must be mentioned, and they require preservation in the dark for some hours to allow the action of the light to continue, exactly in the same way as if the carbon tissue had been exposed to sun-light for a few minutes. When the prints have been kept sufficiently they are developed in warm water, and fine vigorous copies are the result. Naturally enough if the tissue is kept too long after, the mordant action of the light continues rendering the film insoluble, and then the development of the image in warm water obviously becomes impossible.

Another application of the same principle has been made by M. Marion, in which carbon printing is assimilated to silver printing, to such a degree, that those accustomed to the ordinary method of printing photographs on albumenised paper, would find no difficulty in adopting it. H. BADEN PRITCHARD

ON THE METHOD OF COLLECTING AND PRESERVING ENTOMOSTRACA AND OTHER MICROZOA

CONSIDERING the varied interest which attaches to the Entomostraca, it has long seemed to me that they attract a remarkably small share of attention from microscopists. In the case of so widely distributed and numerous a group, this cannot arise from any real difficulty in procuring materials for study; but I believe it does arise in great measure from a want of information as to the best means of capturing and preserving specimens. I propose, therefore, briefly to point out some of the methods which in my own hands have best answered these ends.

Classification.—The Entomostraca constitute, as all microscopists know, a division of the class Crustacea, and for the purposes of the present paper we may with sufficient approach to accuracy consider them as forming four groups—*Cladocera*, of which the common *Daphnia*, or water-flea, is the type; *Ostracoda*, typified by the little hard-shelled, bivalve, mollusc-like *Cypris*; *Copepoda*, represented by the well-known *Cyclops*; and the parasitic species, *Pœcilopoda*, commonly known under the name "fish-lice."

Respecting the last-named group, I shall have nothing to say here; the mere knowledge of their mode of life indicates the method of capture.

Habitat.—All collections of still-water, large and small, from the mere road-side pool to the mountain lake and the ocean, support, with scarcely an exception, their quota of entomostracan inhabitants; nor is purity an essential condition of their existence, for sometimes they are found in great numbers when one would think the foulness of the medium too much for animal existence of so high a grade. Doubtless, however, a moderate purity of water is necessary to the presence of any great variety of species; a luxuriant aquatic vegetation is also very favourable to the growth of most Entomostraca, affording them probably not only food, but shelter. For this reason the weedy margins of lakes are as a rule much more prolific than the clear central portions, where, indeed, but little microscopic life usually exists. Rapidly flowing water is of course unfavourable to the existence of these organisms, but the sea, both between tide-marks and in the open, abounds with them. Ostracoda, except the fresh-water Cyprides, live for the most part on the bottom, and are therefore to be obtained chiefly by dredging. The brackish water of salt-marshes and estuaries supports its own peculiar species, some of which often occur in prodigious numbers; and even the highly saline waters of brine springs and salt lakes have been found to contain Entomostraca.

Methods of Collecting

1. *Freshwater.*—An ordinary "ring-net," made of "hard muslin," or "crinoline," from six to twelve inches in diameter, and fitted to the end of a walking-stick, will be found the most convenient apparatus for the capture of such swimming species as haunt the weedy margins of ponds and lakes. For such shallows as are matted with a growth of *Littorella*, *Lobelia*, or other dwarf ground-plants a "horse-shoe" net, with a frame made after the fashion of a Dutch hoe, is very serviceable; while in working from a boat in the centre of a lake the ordinary ring-net on a stick will be quite sufficient. In this way the net will, after working for a few minutes, usually be partially filled with fragments of weed and other *débris*, amongst which there will also be found a fair sample of the Microzoa inhabiting the locality. The coarsest fragments, such as stems of rushes and portions of water weeds, may conveniently be picked out with the fingers, and thrown away, while the rest of the contents of the net must be transferred to a bottle of clear water, an eight-ounce being a convenient size for the purpose. The Microzoa may then be readily separated by filtering into another bottle through a net of sufficiently wide mesh to allow of their passage through it: "mosquito-netting" I have found to answer well for this purpose. Having thus obtained our Entomostraca in a condition tolerably free from admixture with extraneous matter, they may easily be collected in a patch on the centre of a piece of fine muslin by passing the whole through a piece of that material, arranged over a funnel. They should then be transferred at once (if it be not wished to keep them alive) to a small phial of some preservative fluid. This may be effected easily by a penknife, but a very convenient instrument for the purpose is an ordinary quill toothpick. This process, which appears somewhat cumbrous in writing, is in reality very easily performed, but it may be still further simplified, according to the fancy of the collector, by fitting an outside funnel with a muslin net, and having a small inner one of perforated zinc, so as to do all the filtering at one operation. The collecting net may also be protected from the entrance of very coarse rubbish by a light, moveable wire grating. The species obained by these means will often include numerous representatives of all three orders, Cladocera, Ostracoda, and Copepoda. For the capture of such Ostracoda as haunt the bottom in parts too deep to be reached by a walking-stick, a small hand-dredge is required: this will be more particularly noticed in the marine section.

2. *The Sea.*—The free-swimming species, the great majority of which belong to the order Copepoda, may be most conveniently captured by the walking-stick net held over the side of a row-boat in gentle motion. Care should be taken that the lower end of the net is as wide or wider than its mouth, and that the material, while close enough to retain the Entomostraca, is yet open enough to allow a free current of water through it: if those points be not attended to the result will be a back-wash, carrying back out of the net much which should have been retained.

A towing-net dragged by means of a line from the side or stern of the boat may be used, but is not so much under control, and seldom produces so much spoil: such a net, however, attached in a tide-way during the night to some stationary object, and made with the precautions mentioned above, will often do good work, especially if its specific gravity be adjusted so as to sink very slightly below the surface. As a rule, indeed, the hours from dusk to midnight seem to be the best for capturing pelagic species near the surface. In tidal pools on the shore the same appliances are required as for freshwater ponds.

Ostracoda and other deep-dwelling species require, of course, the use of the dredge; and where Microzoa only are the objects sought, the dredge may conveniently be made of a size much smaller than those in ordinary use. The mouth need not be more than 6 in. in its largest diameter, the bag being made of coarse canvas or "cheese cloth," and from 18 in. to 2 ft. long. The material so dredged up, after having been passed through suitable sieves, so as to separate the coarser portions, should be washed in a muslin bag for the purpose of removing all the impalpable mud, which often constitutes a very considerable proportion of the bulk: this operation may most easily be performed over the side of the boat in the sea, or in some large vessel of sea-water. The washed material is then to be put up in canvas bags, duly labelled, and hung up in a warm position to dry; the more rapidly this part of the process is conducted the better chance will there be of preserving the internal parts, as well as the valves of the Ostracoda, in good condition. But should it be wished to secure the animals actually alive, the best plan will be, after washing the mud as above explained, to immerse a quantity of it in a basin of sea water, allowing it to stand for an hour or more, when many of its inhabitants will have made their way to the surface of the water. They will, indeed, continue to come to the surface for many hours, but the later ones will probably be sickly or dead.

But besides Ostracoda, there are often great numbers of Copepoda in or on the ooze and sand of the sea-bed. These require for their separation a different method of procedure; the following, so far as I know, being the most convenient. After the process of sieving described in the preceding paragraph, all the minute swimming animals will be found in the water in which that operation has been conducted; all that is necessary, therefore, is to pour the water off through a muslin net in which the Microzoa will be retained—in a dirty state, however, which will render careful washing desirable, or still better, the transference of the whole to a bottle of clean sea water for an hour or two; in this way the little creatures will clear themselves of adherent dirt better than we can do by any amount of washing.

A very rich field for the collecting of Copepoda is found in the groves of Fuci and Laminariæ so common on rocky shores at and beyond low-water mark. The fronds of these weeds having been dragged up in any convenient way, are to be washed, a handful or two at a time, by brisk agitation in a tub of sea water, after which the water is to be filtered as directed above. It is best not to macerate weeds in the water for any great length of time, because much mucus exudes from the Laminariæ, enveloping the Entomostraca, and rendering it an extremely difficult and tedious matter to examine the gathering properly. It should be mentioned that, although all weeds harbour numbers of Entomostraca, *Laminaria saccharina* is, as a rule, by far the most productive, apparently on account of the rugosities of the frond affording more efficient shelter to their minute inhabitants: sheltered pieces of coast and land-locked bays are much the most productive hunting grounds.

Treatment of Dredged Material.—The separation of Ostracoda, Foraminifera, and other Microzoa from dredged sand or mud, is best accomplished by the process of "floating." For this purpose the material should be thoroughly well dried and sifted, so as to insure the fine division of the whole mass, then placed in a vessel of water and thoroughly stirred. By this means all the lighter organised particles—chiefly Ostracoda, Foraminifera, minute Mollusca, fragments of Polyzoa, &c.—will, owing to their contained air, be brought to the surface, and may be removed in any convenient way, but best, perhaps, by pouring off the supernatant water through a very fine gauze sieve. Some of the larger and heavier species will, however, sometimes remain at the bottom, and must be picked out with the help of a hand lens.

Fossiliferous Clays and Shales.—These, after repeated maceration in water, should be passed, time after time, through fine sieves, so as to wash out the impalpable suspended mud; at last drying the residuum and floating out the organic particles, as previously directed. When much fossilised, however, the Microzoa will not float. In this case they must be picked out one by one from the residuum left after the repeated washings.

Preservation of Specimens.—Soft-bodied species, *e.g.*, Copepoda, Cladocera, &c., are best preserved in methylated spirit, either of full strength or diluted with an equal quantity of water, the latter, in my opinion, being preferable, as it does not so readily evaporate entirely if left unattended to in small bottles for a length of time. The great disadvantage of alcohol is that it coagulates the albuminous tissues, rendering the animals almost opaque, at the same time destroying the natural colour; but most other preservative solutions possess these properties to a greater or less extent, and have likewise other drawbacks, such, for instance, as becoming cloudy, permitting the growth of fungi, &c. When, however, it is especially wished to preserve the colours, a mixture of equal parts of glycerine and distilled water answers admirably. Indeed, the only hindrances to its general use as a preservative for Microzoa are its strongly solvent action on calcareous tissues and its inconvenient stickiness. For microscopic mountings (of non-calcareous objects) some kind of "glycerine jelly" answers admirably; especially that described by Dr. Carpenter in his book on the microscope, which preparation is, however, improved by saturating with arsenious acid the water used in its manufacture. Ostracoda and other dry specimens require, of course, no preparation beyond mounting on slides of wood or cardboard. An excellent plan of mounting, so as to show at one view all the Ostracoda or Foraminifera obtained in any locality, is shown in the accompanying diagram, the

slides being made of the ordinary size, of stout cardboard or millboard. The central part of the slide is cut out, and the marginal portion mounted on another slide having a dull black ground. The slide is ruled transversely, so as to divide it into any convenient number of spaces, and if needful, ruled also with one line lengthwise down the middle. Each space is marked with a figure or letter of the alphabet referring to the species mounted within it, and an index to the whole kept in a book of reference. The diagram is a facsimile of a mounting so prepared in my collection.

GEORGE S. BRADY

ON THE ORIGIN AND METAMORPHOSES OF INSECTS*

IV.

ON THE NATURE OF METAMORPHOSES

IN the preceding articles we have considered the life history of insects after they have quitted the egg. It is obvious, however, that to treat the subject in a satisfactory manner we must take the development as a whole, from the commencement of the changes in the egg, up to the maturity of the animal, and not suffer ourselves to be confused by the fact that all insects do not leave the egg in the same stage of embryonal development. For although all young insects when they quit the egg are termed "larvæ," whatever their form may be (the case of the so-called Pupipara not constituting a true exception), still it must be remembered that some of these larvæ are much more advanced than others. It is evident that the larva of a fly, as regards its stage of development, corresponds in reality neither with that of a moth nor with that of a grasshopper. In fact, insects quit the egg in very different stages. The maggots of flies, in which the appendages of the head are rudimentary, belong to a lower grade than the grubs of bees, &c., which have antennæ, mandibles, maxilla, labrum, labium, and, in fact, all the mouth parts of a perfect insect. The caterpillars of Lepidoptera are generally classed with the vermiform larvæ of Diptera and Hymenoptera, and placed in opposition to those of Orthoptera, Hemiptera, &c. But, in truth, the possession of thoracic legs places them, as well as the similar larvæ of the Tenthredinidæ, on a decidedly higher level, while in the development of the cephalic appendages there is, as already mentioned, a marked difference between the maggots of flies and the grubs of bees. Thus, then, the period of growth (that in which the animal eats and increases in size) occupies sometimes one stage in the development, sometimes another; sometimes, as for instance in the case of *Chloëon*, it continues through more than one, or, in other words, growth is accompanied by development. But, in fact, the question is even more complicated than this. It is not only that the larvæ of insects at their birth offer the most various grades of development, from the grub of a fly to the young of a grasshopper or a cricket; if we were to classify larvæ according to their development, we should have to deal not with a simple case of gradations only, but with a series of gradations, which would be different according to the organ which we took as our test.

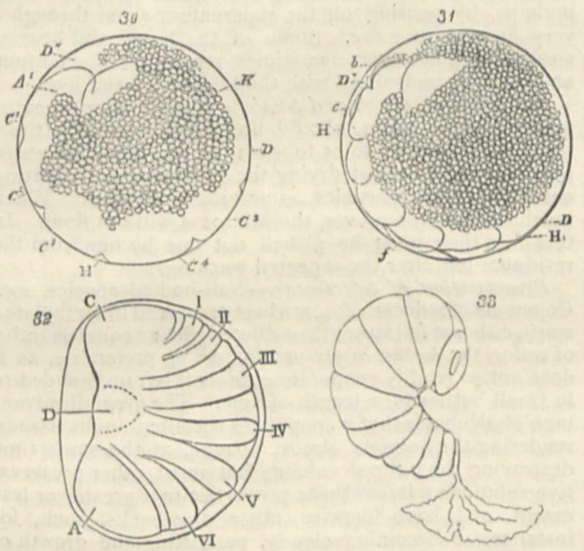


FIG. 30.—Egg of Phryganea (Mystacides). A^1, mandibular segment; C^1 to C^5, maxillary, labial, and three thoracic segments; D, abdomen. (after Zaddach). 31, Egg of Phryganea somewhat more advanced. b, mandibles; c, maxillæ; $c\ f\ s$, rudiments of the three pairs of legs. 32. Egg of Pholcus opilionides (after Claparede). 33, Embryo of Julus after Newport).

FIG. 34.—Colony of Bougainvillea fruticosa, natural size, attached to the underside of a piece of floating timber (after Allman). 36, The medusa from the same species.

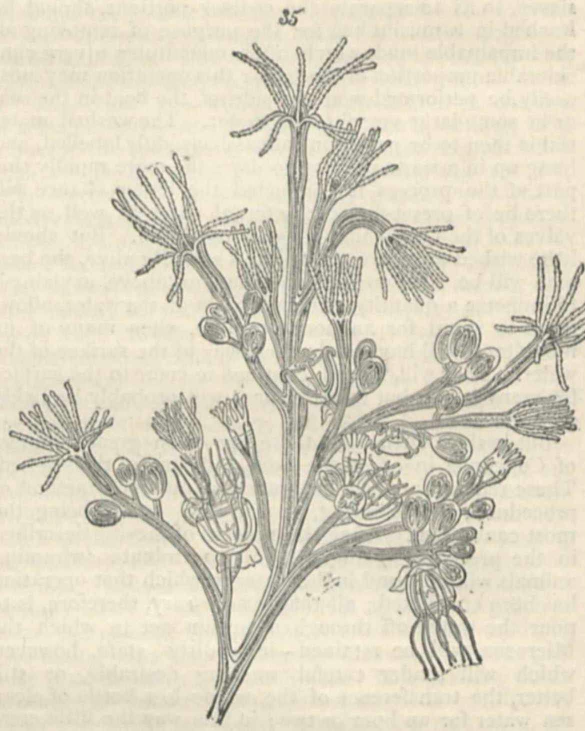


FIG. 35.—Portion of Colony of Bougainvillea fruticosa, more magnified.

Apart, however, from the adaptive changes to which special reference was made in a previous article, the differences are those of gradation, not of direction. The development of a grasshopper does not pursue a different course from that of a bee or wasp, but the embryo attains a higher state before quitting the egg in the former than in the latter; while in most Hymenoptera the body-walls and internal organs are formed before the thoracic appendages; in the Orthoptera, on the contrary, the legs

* Continued from p. 31.

make their appearance before the body-walls have completely closed round the yolk.

Prof. Owen,* indeed, goes so far as to say that the Orthoptera and other Homomorphous insects are, "at one stage of their development, apodal and acephalous larvæ, like the maggot of the fly; but, instead of quitting the egg in this stage, they are quickly transformed into another, in which the head and rudimental thoracic feet are developed to the degree which characterises the hexapod larvæ of the *Carabi* and *Petalocera*."

I quite believe that this was originally true of such larvæ, but from the tendency which large and important organs have, to appear at an early stage of embryonal development, the fact now appears to be, so far at least as can be judged from the observations yet recorded, that the legs of those larvæ which commence life with these appendages, generally make their appearance before the body-walls have closed, or the internal organs have approached to completion. Indeed when the legs first appear they are merely short projections, which it is not always easy to distinguish from the segments themselves. It must, however, be admitted, that the observations are neither so numerous, nor in most cases so full, as could be wished.

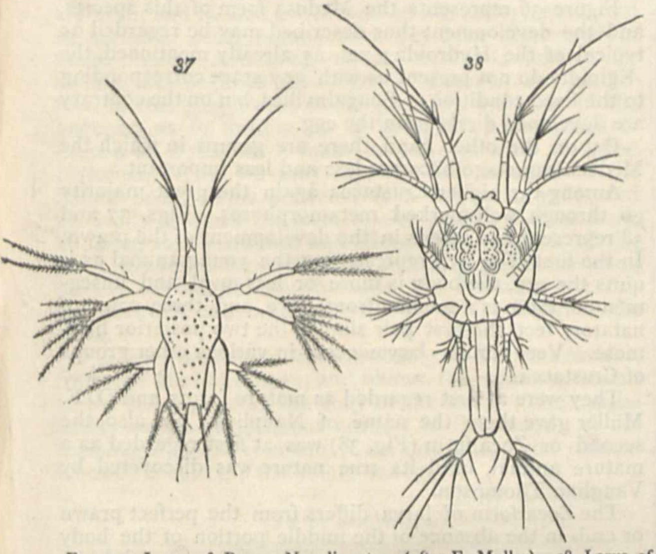


FIG. 37.—Larva of Prawn, Nauplius stage (after F. Muller). 38, Larva of Prawn, more advanced, Zoea stage.

Fig. 30, for instance, represents an egg of Phryganea, as represented by Zaddach in his excellent memoir,† just before the appearance of the appendages. It will be seen that a great part of the yolk is still undifferentiated, that the side walls are incomplete, the back quite open, and the segments only indicated by undulations. This stage is rapidly passed through, and Zaddach only once met with an egg in this condition; in every other specimen which had indications of segments, the rudiments of the legs had also made their appearance, as in Fig. 31, which, however, as will be seen, does not in other respects show much advance on Fig. 30.

Again in Aphis, the embryology of which has been so well worked out by Huxley,‡ the case is very similar, although the legs are somewhat later in making their appearance. "In embryos," he says, "$\frac{1}{140}$th of an inch in length (Pl. xxxvii. Fig. 6), I have found the cephalic portion of the blastoderm beginning to extend upwards again over the anterior face of the germ, so as to constitute its anterior and a small part of its superior wall. This portion is divided by a median fissure into two lobes, which play an important part in the development of the head, and will be termed the "procephalic lobes." I have already made use of this term for the corresponding parts in the embryos of *Crustacea*. The rudimentary thorax presents traces of a division into three segments; and the dorso-lateral margins of the cephalic blastoderm,

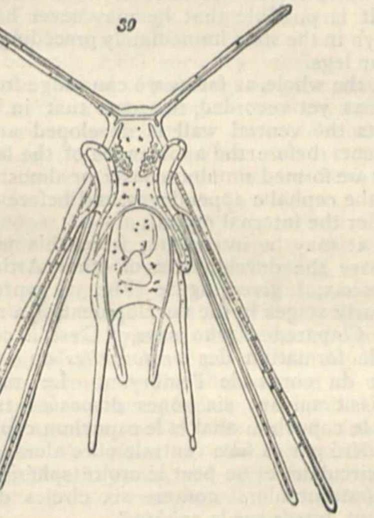


FIG. 39.—Larva of Echinocidaris, seen from above $\times \frac{6}{10}$ (after J. Muller).

behind the procephalic lobes, have a sinuous margin. It is in embryos between this and $\frac{1}{100}$th of an inch in length, that the rudiments of the appendages make their appearance, and by the growth of the cephalic, thoracic, and abdominal blastoderm, curious changes are effected in the relative position of those regions."

In *Chrysopa oculata*, one of the Hemerobiidæ, Packard has described* and figured a stage in which the body segments have made their appearance, but in which "there are no indications of limbs. The primitive band," he says, "is fully formed, the protozorites being distinctly marked, the transverse impressed lines indicating the primitive segments being distinct, and the median furrow easily discerned." Here also, again, the dorsal walls are incomplete, and the internal organs as yet unformed.

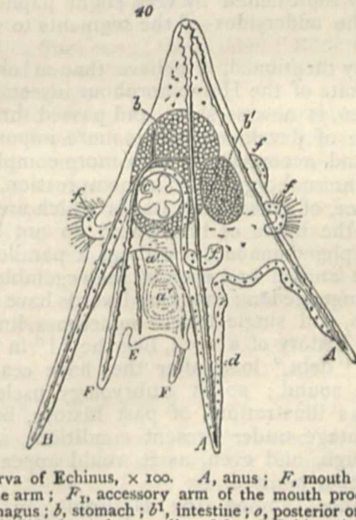


FIG. 40.—Larva of Echinus, × 100. *A*, anus; *F*, mouth process; *B*, posterior side arm; F_1, accessory arm of the mouth process; *a*, mouth; a^1, œsophagus; *b*, stomach; b^1, intestine; *o*, posterior orifice; *d*, ciliated bands; *f*, ciliated epaulets; *c*, disc of future Echinus (after J. Muller).

* "Lectures on the Anatomy, &c. of the Invertebrate Animals."

† "Untersuchungen uber die Entwickelung und den Bau der Gliederthiere," 1854.

‡ "Linnean Transactions," v. xxii. 1858.

* "Embryological Studies on Hexapodous Insects." Peabody Academy Science. Third memoir.

In certain Dragonflies (Calypteryx), and Hemiptera (Hydrometra), the legs, according to Brandt,* appear at a still earlier stage.

According to the observations of Kolliker† it would appear that in Donacia the segments and appendages appear simultaneously. Kolliker himself, however, admits that "meæ de hoc insecto observationes satis sunt manca," and it is possible that he may never have met with an embryo in the state immediately preceding the appearance of the legs.

On the whole, as far as we can judge from the observations as yet recorded, it seems that in Homomorphous insects the ventral wall is developed and divided into segments before the appearance of the legs, but that the latter are formed simultaneously, or almost simultaneously, with the cephalic appendages, and before either the dorsal walls or the internal organs.

As it may be interesting from this point of view to compare the development of other Articulata with that of insects, I give a figure (Fig. 32) representing one of the early stages in the development of a spider (Pholcus) after Claparede,‡ who says, "C'est à ce moment qu' a lieu la formation des *protozorites* ou segments primordiaux du corps de l'embryon. Le rudiment ventral s'épaissit suivant six zônes disposées transversalement entre le capuchon anal et le capuchon céphalique. L'œuf considéré par sa face ventrale offre alors un contour à peu près circulaire et on peut le croire sphérique. Les zônes se montrent alors comme six circles d'un blanc plus éclatant, tracés sur la sphère."

Among Centipedes the development of Julus has been described by Newport.§ The first period, from the deposition of the egg to the gradual bursting of the shell, and exposure of the embryo within it, which, however, remains for some time longer in connection with the shell by a distinct funis, lasts for twenty-five days. The segments of the body, originally six in number, make their appearance on the twentieth day after the deposition of the egg, at which time there were no traces of legs. The larva when it leaves the egg is a soft, white, legless grub (Fig 33), consisting of a head and seven segments, the head being somewhat firmer in texture than the rest of the body. It exhibits rudimentary antennæ, but the legs are still only represented by very slight papilliform processes on the undersides of the segments to which they belong.

As already mentioned, I believe that at one time the vermiform state of the Homomorphous insects, which, as we have seen, is now so short, and passed through at so early a stage of development, was more important, more prolonged, and accompanied by a more complete condition of the internal organs. The compression, and even disappearance, of embryonal stages which are no longer adapted to the mode of life, which do not benefit the animal, is a phenomenon not without a parallel in other parts of the animal and even of the vegetable kingdom. Just as in language long compound words have a tendency to concision, and single letters sometimes linger on, indicating the history of a word, like the "l" in "alms," or the "b" in "debt," long after they have ceased to influence the sound; so in embryology useless stages, interesting as illustrations of past history, but without direct advantage under present conditions, are rapidly passed through, and even, as it would appear, in some cases altogether omitted.

For instance, among the Hydroida, in the great majority of cases, the egg produces a body more or less resembling the common Hydra of our ponds, and known technically as the "trophosome," which develops into the well-known Medusæ or jelly-fishes. The group, however, for which Prof. Allman has proposed the term

* Mem. de l'Acad. Impé. des Sci. de St. Petersburg." 1869.
† Observationes de Prima Insectorum Genesi, p. 14.
‡ Recherches sur l' Evolution des Araignées.
§ Philosophical Transactions, 1841.

Monopsea,* and of which the genus Ægina may be taken as the type, is, as he says, "distinguished by the absence of a hydriform trophosome, the ovum becoming developed through direct metamorphosis into a medusiform body, just as in the other orders it is developed into a hydriform body." Figure 34 represents, after Allman, a colony of *Bougainvillea fruticosa* of the natural size. It is a British species, which is found growing on buoys, floating timber, &c., and, says Allman,† when in health and vigour, "offers a spectacle unsurpassed in interest by any other species—every branchlet crowned by its graceful hydranth, and budding with Medusæ in all stages of development (Fig. 35), some still in the condition of minute buds, in which no trace of the definite Medusa-form can yet be detected; others, in which the outlines of the Medusa can be distinctly traced within the transparent ectotheque; others, again, just casting off this thin outer pellicle, and others completely freed from it, struggling with convulsive efforts to break loose from the colony, and finally launched forth in the full enjoyment of their freedom into the surrounding water. I know of no form in which so many of the characteristic features of a typical hydroid are more finely expressed than in this beautiful species."

Figure 36 represents the Medusa form of this species, and the development thus described may be regarded as typical of the Hydroida; yet, as already mentioned, the Æginidæ do not present us with any stage corresponding to the fixed condition of Bougainvillea, but on the contrary are developed direct from the egg.

But on the other hand there are groups in which the Medusiform stage becomes less and less important.

Among the higher Crustacea again the great majority go through well-marked metamorphoses. Figs. 37 and 38 represent two stages in the development of the prawn. In the first (Fig. 37), representing the young animal as it quits the egg, the body is more or less oval and unsegmented, there is a median frontal eye, and three pairs of natatory feet, the first pair simple, the two posterior biramose. Very similar larvæ occur in various other groups of Crustacea.

They were at first regarded as mature forms, and O. F. Müller gave them the name of Nauplius. So, also, the second or Zoea form (Fig. 38) was at first regarded as a mature animal, until its true nature was discovered by Vaughan Thompson.

The Zoea form of larva differs from the perfect prawn or crab in the absence of the middle portion of the body and its appendages. The mandibles have no palpi, the maxillipeds or foot-jaws are used as feet, whereas in the mature form they serve as jaws. Branchiæ are either wanting or rudimentary, respiration being principally effected through the walls of the carapace. The abdomen and tail are destitute of appendages. The development of Zoea into the perfect animal has been well described by Mr. Spence Bate‡ in the case of the common crab (*Carcinus mænas*).

All crabs, so far as we know, with the exception of a species of land crab (*Gegarcinus*), described by Westwood, pass through a stage more or less resembling that shown in Fig. 38. On the other hand the great group of Edriopthalma, comprising Amphipoda (shorehoppers, &c.) and Isopoda (woodlice, &c.), pass through no such metamorphoses; the development is direct, as in the Orthoptera. It is true that one species, *Tanais Dulongii*, though a typical Isopod in form and general character, is said to retain in some points, and especially in the mode of respiration, some peculiarities of the Zoea type; but this is quite an exceptional case. In Mysis, says F. Muller,§ "there is still a trace of the Nauplius-stage; being transferred back to a period when it had not to

* Monog. of the Gymnoblastic or Tubularian Hydroids. By G. J. Allman, F.R.S., &c., Roy. Society. † l.c., p. 315.
‡ Philosophical Transactions, 1859, p. 589.
§ "Facts for Darwin," Eng. Trans., p. 127.

provide for itself, the Nauplius has become degraded into a mere skin; in *Ligia* this larva-skin has lost the traces of limbs, and in *Philoscia* it is scarcely demonstrable."

Once more, the Echinodermata in most cases "go through a very well-marked metamorphosis, which often has more than one larval stage. The distinctive character of the metamorphosis appears to be the possession by the larvæ of at least a mouth and pharynx, which, whether absorbed or cast off, is never converted into the corresponding organs of the perfect Echinoderm developed inside of the provisional organism. The mass of more or less differentiated sarcode, of which the larva, or pseudembryo, as opposed to the Echinoderm within it, is made up, always carries upon its exterior certain bilaterally-arranged ciliated bands, by the action of which the whole organism is moved from place to place, and it may be strengthened by the superaddition to it of a framework of calcareous rods."*

Thus Fig. 39 represents a larva of *Echino-cidaris*, after Muller; † The body is transparent, $\frac{6}{10}$ in length, shaped somewhat like a double easel, but with two long horns in front, which, as well as the posterior processes, are supported by calcareous rods. These larvæ swim by means of minute vibratile hairs, or ciliæ. They have a mouth, stomach, and in fact, a well-defined alimentary canal, but no nerves or other organs have yet been discovered in them. After swimming about in this condition for awhile, they begin to show signs of change. An involution of the integument takes place on one side of the back, so as to form a pit or tube, which continues to deepen till it reaches a mass or store of what is called blastema, or, as we may say, the raw material of the animal body. This blastema then begins to grow, and gradually assumes the form of the perfect Echinoderm. In doing so it surrounds and adopts the stomach of the larva, but forms for itself a new mouth or gullet, throwing off the old mouth, together with the intestine, the calcareous rods, and in fact all the rest of the body of the larva.

Fig. 40 represents a larva probably of *Echinus lividus*, from the Mediterranean, and shows the commencement of the sea egg within the body of the larva. The capital letters denote the different arms, *a* is the mouth, *a'* the æsophagus, *b* the stomach, *b'* the intestine, *f* the ciliated lobes or epaulets, *c* the young sea-egg.

JOHN LUBBOCK

(*To be continued.*)

* "Rolleston—" Forms of Animal Life," p. 146.

† Uber die Gattungen der Seeigellarven. Siebente Abhandlung. Kon. Akad. d. Wiss. zu Berlin. Von Joh. Müller, 1855, Pl. iii. fig. 3.

EXTIRPATION BY COLLECTORS OF RARE PLANTS AND ANIMALS

THE Legislature, having very properly provided for the preservation of small birds, might extend its protection to other animals and to plants; for although it would be inexpedient to prevent individuals from taking rare insects and botanical specimens, it is surely expedient to deter persons or societies from offering premiums which are leading to the extirpation of such species.

Some years ago a judicious and formal protest against this culpable practice was published by many of the most eminent British botanists, and it has constantly been deplored by all true lovers of natural science. The respected president (the Rev. Dr. Mitchinson) of our East Kent Natural History Society, in his address at the last annual meeting thereof at Canterbury, made such strong observations on the subject as might raise the question whether local societies may not do as much harm by promoting the extirpation of rare plants and animals as good in other respects; and I have always been insisting, at the meetings of the same society and elsewhere, that it is our duty to cherish, and not destroy the precious plants and animals of the district. Whenever a rare plant or animal is exhibited at those meetings, we have always a wail about its having been "not long since often seen, though now fast disappearing." A chief cause of this is the deplorable rapacity of collectors of and traffickers in specimens; since the preposterous notion prevails that botany and entomology consist in a recognition of the mere physiognomy, without the least regard to the physiology, of species, and being able to call them by their scientific names.

And so it will be while local societies continue to encourage such errors, instead of promulgating the essential principles of botanical or entomological science, and obstructing the injurious operations of mere collectors or pretenders. And this desirable end, so far as regards taxonomy, might be easily attained without the least harm to rare species. Prizes for the best display, illustrated by microscopic drawings and preparations of the generic and specific characters of sections or the whole of many natural orders would afford really good tests of the industry and attainments of the candidates. For example, why not try for this purpose the Willows, Grasses, or Sedges? Two of these orders have the further recommendation of being of great economic value. Again, as specific distinctions seem to be the ultimate aim of these societies, certain cells or tissues, such as the pollen, epidermis, hairs, and stomata, would afford good subjects for investigation in this point of view, as would also raphides and other plant-crystals, and very likely disclose valuable characters not yet recognised in the books of systematic botany.

I have been led to these remarks by the increasing frequency of the practice now deplored. As the "West Kent Natural History, Microscopical, and Photographic Society" is much and deservedly respected, and exercises justly considerable influence in its department, an extract from its last "Council's Report," p. 19, will suffice as a sample of the mischief:—"With a view to promote the study of Entomology and Botany among the members of the Society and their families, the Council, in the early part of the year, announced their intention of giving two prizes of 5*l.* 5*s.* each, one for the best Botanical collection, the other for the best collection of Lepidopterous Insects; all specimens to be gathered or taken within the West Kent district." This quotation is by no means intended for blame to any particular society, but merely as an example taken from one of the printed "Reports" that has lately reached me of what is still being sown broadcast generally throughout the country.

And here we have plainly not only a reward of money for the best collection of plants and Lepidoptera in a given district, but a temptation or inducement to unscrupulous collectors, in their anxiety to win the prize and defeat their competitors, to destroy such rare specimens as they may not take away. Such nefarious conduct is not meant to be insinuated of the West Kent Society; but my object is simply to assert that which I know has too often been the effect of such prizes, and to invoke the aid of NATURE in suppressing the evil.

GEORGE GULLIVER

A FRENCH PHYSICAL SOCIETY

THE scientific movement increases in France; it began about the end of the Empire, under the ministry of Durüy, and has since taken greater proportions, especially after the last war. The new French Association for the Advancement of Science,* it is well known, is modelled after the British Association, the success of which has surpassed expectation.

The physicists of Paris have assembled for several years in the laboratories of the Superior Normal School, placed at their disposal by M. Berlin, the director of the scientific studies of this school. They conversed about physics

* See NATURE, vol. v. p. 357.

recent theories were set forth, the new or little known instruments were shown and explained. Thus Sir Wm. Thomson's electrometer, and several experiments of Prof. Tyndall called forth the curiosity and attention of the assistants. But those amicable meetings are no longer sufficient; the necessity of a more formal gathering was felt, as well as of writing and publishing Transactions, that the notes and observations might not be completely lost. The members of the Institute of the physical section encouraged the new society by their warm approval.

On the 17th of January of the present year, in the Salle Gerson, an *annexé* of the Faculté des Sciences of Paris (*Sorbonne*), a number of physicists met. They accepted provisional statutes and elected a board. The provisional statutes proposed by a committee composed of MM. d'Almeida, Alfred Cornu, Gernez, Lissajous, Mascart, expressed, in a few articles, the basis of the new association.

The purpose of the society is to promote physics; it will have two sittings a month alternately with the Chemical Society, and will publish transactions that will be sent to the members. The members are divided into resident, non-resident, and honorary members, the last chosen by election from among the most eminent men in France and abroad. In the first year six will be elected, and two only in each following year.

The society will be glad to receive such gifts as will facilitate its work, and will inscribe in its Transactions the names of the givers.

The board is thus composed:—President, M. Fizeau, Member of the Institute; Vice-President, M. Bertin, Director of the Scientific Studies to the Superior Normal School; General Secretary, M. d'Almeida, Director of the new Journal of Physics; Secretary, M. Maurat, Professor of Physics to the Lycée St. Louis, of Paris; Vice-Secretary, M. Alfred Cornu, Professor of Physics to the Polytechnic School; Treasurer-Archivist, M. Philippon, Secretary of the Faculté des Sciences of Paris.

The venerable M. Becquerel, who, notwithstanding his 89 years, assisted at the meeting, in order to give by his presence a proof of his adhesion to the new society, has been designed, by acclamation, an honorary member.

MAXIME CORNU

NOTES

PROF. OWEN has been appointed to a Civil Companionship of the Bath. If this is intended as an acknowledgment of Prof. Owen's services to science, it is not to the credit of Government that the honour was not conferred years ago.

PROF. TAIT'S Rede Lecture on Thermo-dynamics will be delivered to-morrow.

HITHERTO the London "Companies," whose "fatness" is notorious, have done little or nothing for the promotion of scientific researches or education. It is therefore with the greatest pleasure we record that the Fishmongers' Company have handsomely presented to Mr. W. K. Parker, F.R.S., so well known for his valuable researches on the shoulder-girdle and skull in vertebrated animals, the sum of 50*l*., in addition to an allowance of 20*l*. a year for the next three years in order to enable him to pursue such parts of his work as relate to the Anatomy of Fish. This we certainly think a step in the right direction, and the Fishmongers' Company deserve all praise for having been so original and generous as to be the first to take it. We hope their award to Mr. Parker is only an earnest of what they will do in the future, and that their example will not be lost on the other notoriously wealthy companies of the City of London. A few thousands a year would never be missed out of their enormous revenues, and would not diminish by a single dainty the sumptuousness of their numerous feasts; whereas the amount of original and practically beneficial scientific work that could be done with the money, would yield them and the country generally a rich return. We daresay those who have the management of the funds of the various companies would be willing enough to divert a portion into scientific channels if they only knew how to go about it; the example of the Fishmongers' Company may afford them a hint. Moreover they need be at no loss, for there are plenty of eminent men of science competent and judicious enough to lend advice to the companies in this matter. Commerce, with which these companies are all more or less connected, owes much of its present gigantic dimensions and great prosperity to the discoveries and advances of science; gratitude and self-interest ought to urge our London merchants not to be indifferent to scientific progress. Let us also add, that their award to Mr. Parker is on a scale which shows a very slight acquaintance on the part of the City magnates with the value of time.

A FUSION has taken place between the local committee at Munich for erecting a statue to Justus von Liebig, and the committee appointed by the German Chemical Society at Berlin; the latter, in order to insure unity of action, giving way in the question as to where the statue should find its place. Notwithstanding the serious nature of the claims of Giessen, it was generally thought that the resting-place of the great chemist would unite the majority of votes of his admirers. A considerable number of leading German statesmen and foreign ambassadors have joined the committee, the full list of which will shortly be published.

FRESENIUS, who twenty-five years ago founded a school of chemistry at Wiesbaden, has celebrated the anniversary of its foundation amidst the festive concourse of his friends and pupils, and of the Government and learned societies of his country. A gloom was unfortunately cast over this event by the death of Mrs. Fresenius, which almost coincided with its celebration.

WE regret very much to announce the death of Emanuel Deutsch, at Alexandria. His premature death is a very great loss to Eastern scholarship.

THE Alexandra Palace, under new management, reopens on Saturday. We hope the managers will not neglect the interests of science.

WE recently announced that the French Society for the Encouragement of National Industry had awarded its grand medal to Sir Charles Wheatstone. The following is an extract from the report of the Committee on the Economic Arts:—While the kaleidophone of Sir Charles Wheatstone has been the point of departure of the method which permits sounds to be studied by the aid of the eye; while his researches on the qualities of sound, on the production of vowels, while the creation of his speaking machine, have elucidated many points in the theory of the voice; while his ingenious apparatus, illustrating the propagation and the combination of waves, has facilitated the understanding of these delicate phenomena, and contributed to throw light on the mechanism of the undulatory motions, his numerous researches on the applications of electricity, in which he has shown, at the same time, profound science and a genius marvellously inspired, occupy a great place in the history of the electric telegraph. It is he who first realised, under conditions really practicable, this admirable means of communication between men and between nations, and we ought not to forget that, more than once, he has come *personally* among us to prepare its organisation and stimulate success. The unanimous choice made by the committee of the economic arts and cordially ratified by the Council honours our society as much as him who is the object of it. We are happy to give, on this occasion, a testimony of sympathy to a nation in which science is held in such high esteem. Those among us who have had the good fortune to visit the scientific men of England in their own country have not forgotten that we have always received from them the most cordial and the most generous hospitality. In conferring on Sir Charles Wheatstone a reward rendered valuable by those who have already received it, the Council performs a pure act of justice, and acquits, at least for some among us, a debt of gratitude.

Dr. von Dollinger has been appointed President of the Bavarian Academy of Science and Conservator-General of Scientific Museums in Bavaria, which became vacant by the death of Baron Liebig. King Louis advised the doctor of his appointment by an autograph letter.

The Institution of Civil Engineers hold a *conversazione* in the West Galleries of the International Exhibition, on Tuesday, the 27th inst.

Mr. Arthur Gamgee, M.D., F.R.S., Lecturer on Physiology at Surgeons Hall, Edinburgh, and Examiner in Forensic Medicine in the University of London, has been appointed Brackenbury Professor of Practical Physiology and Histology in Owens College, Manchester.

Prof. H. de Lacaze-Duthiers, member of the French Institute, Professor of Zoology at the Faculté des Sciences of Paris, and Director of the Zoological Station of Roscoff, will accompany Commander Mouchez, in the *Narval*, that officer being engaged in completing the hydrographic map of the Algerian shores. The professor will make frequent soundings, and study the fauna of the Mediterranean. He will be assisted in the geological determinations by a distinguished young geologist, M. Vélain, Répétiteur of the Faculté of Sciences of Paris. The cruise will last five months. The ship left Lorient on May 1. M. de Lacaze-Duthiers will join them in July, at the termination of his lectures at the Faculté. Let us hope that these new explorations, under the guidance of an ardent, learned, and experienced man, will procure materials as valuable as those which were obtained by Agassiz, Wyville Thomson and others.

We understand that there is a plan in hand for building a new museum at Vienna, to which the contents of the Imperial Zoological Cabinet, including the important collections of Natterer and other well-known naturalists, are to be transferred.

The following telegram was received on Saturday at the Foreign-office from Colonel Stanton :—Alexandria, May 17, 1873.—The Egyptian Government has just received a despatch from the Governor-General of Southern Loudan, dated 15th March, reporting the arrival at Gondokoro of the reinforcements sent to Sir S. Baker, confirming the private intelligence recently forwarded to your lordship as to the safety of the party, and adding that in compliance with Sir S. Baker's demand, 200 soldiers, with a supply of salt and ammunition, had been sent on to him. Sir S. Baker had not reached the lake.

Dr. Petermann has recently received a letter from Dr. Nachtigal, who in 1869 was sent out to Africa on a mission from the Emperor of Germany to the Sultan of Borneo. The letter is dated February 1872, and gives some brief details of Dr. Nachtigal's visits to the countries lying to the N.E. of Lake Tchad, the greater part of the region visited being new to European exploration. A most important discovery made by Dr. Nachtigal is that Bahr-el-Gazal, put down on some maps conjecturally as flowing into Lake Tchad, really flows out of that lake north-eastwards for about 300 miles. He has also discovered a range of mountains extending probably a distance of upwards of 800 miles from Tibesti to Darfur ; one of the passes is at least 7,878 ft. above sea-level. At the date of the despatch of his letter, Dr. Nachtigal was about to undertake a journey into Bagirmi, the country lying to the south-east of Lake Tchad. It will thus be seen that this traveller is collecting materials which will add greatly to our knowledge of Central N. Africa.

A message has been received by the *Daily Telegraph* from Mr. George Smith dated Mosul, May 19. "Since my last message," he says, "I have come upon numerous valuable inscriptions and fragments of all classes, including very curious syllabaries and bi-lingual records. Among them is a remarkable table of the penalties for neglect or infraction of the laws. But my most fortunate discovery is that of a broken tablet containing the very portion of the text which was missing from the Deluge tablet. Immense masses of earth and *débris* overlie whatever remains to be brought to light in this part of the great mound. Much time and large sums of money would be required to lay it open. I therefore await instructions from you and the Museum, as the season is closing." The *Daily Telegraph* and the British Museum have now an opportunity of showing that they have really at heart the advancement of historical research, and we are sure Mr. Smith's hint will be met by a hearty response. We feel confident that the liberality of the *Daily Telegraph* will be continued until Mr. Smith's researches are completed to his own satisfaction.

Some time ago we were able to give authentic news of the safety of the Russian explorer of New Guinea, Dr. N. von Miklucho-Maclay, who had been reported dead in several newspapers. Dr. Maclay has himself sent a letter to Dr. Petermann, dated on board the Russian clipper, *Isumrud*, March 11, with a postscript dated Manilla, March 22, saying he is alive, though not very well, and was about to despatch to the St Petersburg Geographical Society an account of his exploration of New Guinea, his main object in visiting that country being to collect material for its ethnology. He intended to visit Luzon and the Sunda Islands, and then return to New Guinea.

An important step has been taken in the carrying out of the decisions of the International Metric Commission which met at Paris in October last year. The form and mode of execution of the standard metre having been settled, the Commission entrusted to the French Section the manufacture and comparison of the new metres with the original standard in the Archives of France. We learn from *Les Mondes* that before proceeding to cast the definitive metres, the French Commission has thought it advisable to execute the first types, with which to test successively all the methods that will ultimately be applied to the definitive metres. This first experiment took place in the laboratory of M. H. Sainte-Claire Deville, who, with the assistance of M. Debray, has succeeded in obtaining the iridio-platinum alloy perfectly pure. The operation of casting this first international metre was considered of so much importance, that the President of the Republic and some of his Ministers, and other eminent Frenchmen, "assisted" at it. Nine kilogrammes of platinum, with one kilogramme of iridium, were melted under the action of an oxyhydrogen flame from a blow-pipe in three-quarters of an hour. The ingot was then cast, perfectly limpid, in a mould formed, like the furnace itself, of a block of carbonate of lime, whose interior walls alone were burned under the influence of the excessive temperature which was developed ; consequently with this substance there is no risk of breakage. The metal was allowed to cool in the mould, and preserved its bright surface ; in this condition it will be submitted to all the processes necessary to give it the definitive form which it ought to possess. The operation was considered, by all who witnessed it, as perfectly successful.

The following further particulars with reference to the American Arctic exploring ship *Polaris*, Captain Hall, have been obtained by the correspondent of the *New York Herald ;* they are dated Bay Roberts, *via* St. John's, N.F., into which the steamer *Tigress* had come, having on board nineteen survivors, including H. C. Tyson, assistant-navigator of Captain Hall's-expedition. This party, which had been landed from the *Polaris*, were driven from her by a gale which burst her moorings on October 15, 1872, in latitude 72·35. When they last saw the *Polaris* she was under steam and canvas, making for a harbour on the east side of Northumberland Island. She had no boats left of the six which she brought with her from the port of New York. Two were lost in a northern expedition, two were landed on the ice with Captain Tyson's party, one was burnt as fire-wood to make water for the crew, and the other is on board the *Tigress*. The *Polaris* was in command of Captain Buddington, who had thirteen of a crew along with him, and a plentiful stock of provisions. She was making a good deal of water, but, as Captain Tyson informed the *Herald* correspondent, she was not more leaky than when he was on board all the previous fall and winter. Her bow was somewhat damaged, and it is the opinion of the survivors they will be unable to get clear until July, and even then, if the ship is unseaworthy, they would have to make new boats to effect an escape. On October 8, 1871, in latitude 81·38, longitude 61·44, Captain Hall died of apoplexy, and was buried on shore, where they erected a wooden cross to mark his grave. He had recently returned from a northern sledge expedition, in which he had attained the latitude of 82·16. In September 1871, the *Polaris* entered winter quarters, and left August 12, 1872. The ice was very heavy, and set in a southern direction. She was forced south, and so continued drifting till Captain Tyson and party were driven from her. The sledge party crossed Kane's Polar Sea, which they pronounced to be a strait about 15 miles wide. There was an appearance of open water to the north.

THE Education Department propose to send on loan, to local schools in which it will be useful, what they call Travelling Apparatus for illustrating Instruction in Naval Architecture. The following is the list of articles included under that title:—1. Model of a half-midship section of an iron-clad ship, showing the mode of forming and combining the keel, frames, beams, &c., &c. 2. Ditto of an ordinary wooden ship. 3. Block-model, showing the lines used in laying off the fore-body of a ship. 4. Ditto, after-body. 5. Diagram showing the lines used in laying-off. These models and diagram are intended to be placed in the school or class-room for reference during the hours of study, in order that the students may better understand the nature of the work under consideration, and also to aid the teachers in illustrating their ideas when imparting instruction to their classes.

PROF. MARSH, in the current number of the *American Journal of Science and Art*, describes several new species of mammalia from the tertiary deposits of the Rocky Mountains region. *Orohippus agilis* is a new species of a genus intermediate between *Anchitherium* and *Palæotherium*, which has four functional digits, the first premolar tooth nearly as large as the second, no antorbital fossa, and an incomplete bony orbit. *Colonoceras*, a new genus, nearly allied to *Hyrachyus* (Leidy) and *Helaletes* (Marsh), is peculiar in having a pair of rugosities on the nasal bones, to support dermal horns. It was about the size of a sheep. Prof. Marsh separates the genus *Dinoceras* from *Tinoceras*, on account of the maxillary horn-cores being more anteriorly situated, and the parietal crests more elevated in the former, at the same time that the canine tusks are more compressed and trenchant. A new species of *Oreodon*, and two others of *Rhinoceros*, are also described.

A *résumé* of our knowledge, strikingly incomplete as it is, on the subject of sneezing, is given by Dr. Seguin in the third number of the new and excellent *Archives of Scientific and Practical Medicine*. The author's attention was drawn to the subject from his observing a fact, previously well known, that sneezing may be frequently stopped by pressing the fingers on the lips or sides of the nose. No new theory is given to explain the physiology of the phenomenon, and it is stated that naturally most of the air expired during a sneeze escapes through the nose, but that custom has brought about the discharge of a part through the mouth. This we cannot agree with, as it is difficult to believe that custom has much influence on so abrupt an act.

WE learn from *Ocean Highways* that Major Branfill, of the great Indian Trigonometrical Survey, has discovered that a peak of the Anamully Range attains a height of 8,837 ft. above the sea, 500 ft. higher than Dodabetta, in the Nilgiri Hills, hitherto supposed to be the loftiest peak in Southern India.

A FEW of the members of the Anthropological Institute, who did not approve of the proceedings at the annual meeting, have formed themselves into a separate society, under the name of the London Anthropological Society, with Dr. Charnock as president, and Captain R. F. Burton and Mr. Staniland Wake as vice-presidents. "This society," the prospectus says, "has been formed for the study of the science of anthropology in all its branches. The society, while adhering to the usual practice of conducting its transactions at meetings attended only by Fellows and gentlemen introduced by Fellows, contemplates placing the results of its investigations before the non-scientific portion of the community, by holding from time to time special meetings, to which the general public will be admitted."

ADDITIONS to the Brighton Aquarium during the past week:—One Alligator (*Alligator mississipensis*), 8 feet long, from South Carolina, purchased; one Australian Monitor (*Monitor gouldii*), purchased; 500 salmon, Great Lake trout, common trout, and hybrid fry (*Salmo salar, lacustris, et fario*), presented by Mr. Frank Buckland; larger and lesser Spotted Dog-fish (*Scyllium stellare et canicula*); Skate-toothed Shark (*Mustelus vulgaris*); Picked Dog-fish (*Acanthias vulgaris*); Monk-fish (*Rhina squatina*), one specimen 5 feet long; Sting Ray *Trygon pastinaca*); Common Skate (*Raia batis*); Spotted Ray (*R. maculata*); Thornback (*R. clavata*); Three-spined Sticklebacks (*Gasterosteus spinulosus*); Bass (*Labrax lupus*); Streaked Gurnards (*Trigla lineata*); the Piper (*Trigla lyra*); Greater Weever (*Trachinus draco*); Lesser do. (*T. vipera*); John Dorée (*Zeus faber*); Dragonets (*Callionymus lyra*); Sand Smelts (*Atherina presbyter*); Grey Mullet (*Mugil capito*); Carp (*Cyprinus carpio*); Roach (*Leuciscus rutilus*); Minnow (*L. phoxinus*); Loach (*Nemachilus barbatula*); Tench (*Tinca vulgaris*); Herring (*Clupea harengus*); Sharp-nosed Eel (*Anguilla vulgaris*); Greater Pipe-fish *Syngnathus acus*); Snake Pipe-fish (*Nerophis æquoreus*); Branched Seahorse (*Hippocampus ramulosus*), Mediterranean; Squids, (*Loligo media*); Masked crabs (*Corystes cassivelaunus*); Spider Crabs (*Maia squinado*).

THE additions to the Zoological Society's Gardens during the past week include a Cashmere Monkey (*Macacus pelops*), presented by Rear-Admiral Davies; a Savannah Deer (*Cervus savannarum*) from South America, presented by Capt. Bennett; a Suricate (*Suricata zenik*) from South Africa, presented by Mr. A. Benyon; a Palm Squirrel (*Sciurus palmarum*) from India, presented by Mr. W. Lovegrove; a Mocking Bird (*Mimus polyglottis*) from North America, presented by Mr. P. Frank; an Indian Eryx (*Eryx johnii*), presented by Dr. Anderson; two pied Crow Shrikes (*Strepera graculina*) from Australia; an Ursine Colobus (*Colobus polycomus*) from Sierra Leone; a Hocheur Monkey (*Cercopithecus nictitans*) from West Africa; a Wandering Tree-pie (*Dendrocitta vagabunda*), two pied Mynahs (*Sternopastor contra*), and two rose-coloured Pastors (*Pastor roseus*) from India, purchased; two Hoffmann's Sloths (*Cholopus hoffmannii*) from Panama; two black Vultures (*Cathartes atratus*) from South America; a black-handed Spider Monkey (*Ateles melanochir*) from Central America, and a Crocodile (*Crocodilus americanus*) from Mexico, deposited.

COMPARISON OF THE SPECTRA OF THE LIMB AND OF THE CENTRE OF THE SUN *

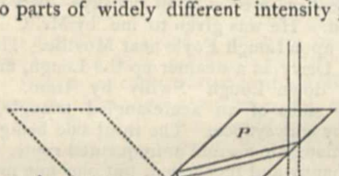
A COMPARISON of the spectrum of the edge of the sun with that of its centre is of great theoretical interest; but any comparison other than by direct juxtaposition must be very unsatisfactory, and the more so as the differences are less. In order to obtain spectra of two different portions of the sun side by side, where the slightest variations may be detected, I have constructed a small prism with four polished sides, its bases being parallelograms. This is so placed that one face rests upon the slit plate of the telespectroscope, and has its acute edge perpendicular to the slit at its middle point. The instrument may then be directed so that the image of the sun falls with its centre on the uncovered portion of the slit, while the light which forms the edge of the sun, falling perpendicularly upon the first surface of the prism, suffers two interior total reflections and a displacement depending upon the form of the prism. A glance at the figure, in which *s s'* is the slit, L L' the diameter of the sun's image, and P the prism, shows that no light from the covered part of the slit will reach the collimating lens except that which has been reflected from the two sides of the prism. The relation of the acute angle (v) and the distance between the reflecting sides (t) to the focal length of the great telescope (F) and the width of the spectrum (a) is given by the formula,

$$2t \sin v = F \tan 16' - a.$$

The sides of the prism not fixed by the equation admit of considerable latitude, but should be made to approach the lower limit in order that the planes of the direct and transmitted images may be as little separated as possible. Of course t and v should be so proportioned that the reflections may be total.

The instruments with which the following observations have been made are those belonging to the observatory of the Sheffield (U.S.) Scientific School, consisting of an equatorial telescope of 9 in. aperture, and 118 in. focal length, by Clark, and a spectroscope of Young's form by the same maker. The spectroscope has a dispersive power of 12 prisms of 60°. In most of these observations an eye-piece of high power has been adapted to it, which gives a separation of the D lines equal to 64 minutes nearly. In the small prism placed before the slit, a is equal to ·04 in., a quarter of the length of the slit.

When the instrument is properly directed and in adjustment, we see a very narrow black line dividing the spectrum longitudinally into two parts of widely different intensity; the fainter,



belonging to the limb of the sun, is marked on its edge by the bright chromosphere lines. Upon comparing these two spectra, certain differences are recognised besides that of intensity, by far the most marked of which are exhibited by the lines b_1 and b_2, which become sharper and less hazy near the limb. The line b_3 possesses the same characteristic, but to a less degree; C and F also become sharper in the same region. Excepting these and the D lines it requires very close examination to detect any variation. There is, however, a line in the red at 768·1 of Kirchhoff's scale which is strongly marked near the centre of the sun's disc, but disappears entirely, to my power at least, within 16″ to 20″ from the limb. Two other lines below F, at 1828·6 and 1830·9 of the same scale, exhibit nearly complementary phenomena, *i.e.*, they are strongly marked near the edge, but much fainter at the centre. These latter lines also become greatly strengthened over the penumbræ of spots. The line 768·1 is not thus affected. These are all the differences which I have invariably seen in repeated examinations since February 17.

Others have, however, been suspected. Certain lines, which are strengthened in a region of spots like those above mentioned, appear to be strengthened also near the edge, but do not undergo so marked a change. It is obvious that the differences should be most pronounced in the clearest sky, and such is the case. The closest examination has extended only from B to a short distance above F, as the plate glass of which the small prism is made has a decided yellow tint and absorbs the blue rays strongly.

Since the light from the border of the sun undergoes a general absorption, which reduces its intensity to much less than one-fourth that at the centre, according to Secchi's measurements, and yet the spectroscopic character is changed so slightly, it is impossible for me to escape the conviction that the seat of the selective absorption, which produces the Fraunhofer lines, is below the envelope which exerts the general absorption. But the phenomena of the faculæ prove not only that this envelope rests upon the photosphere, but also that it is very thin. The origin of the Fraunhofer lines, then, must be in the photosphere itself, which is in accordance with Lockyer's views.

Any effects which the chromosphere might produce, we would anticipate finding most evident in the lines of those gases which are readily detected there. A reference to the observations shows at once a compliance with this anticipation in the lines of hydrogen, magnesium, and sodium. The line 768·1 is not less strikingly in concordance, if it be regarded as 768·?* (the ? indicates doubt as to the tenths of the scale, and * absence of a corresponding black line) of Young's Catalogue of Chromosphere Lines. The lines 1828·6 and 1830·9, with others of the same class, probably have their origin in the medium which exerts the general absorption, and thus are allied to our telluric lines. It also seems probable that the chromosphere is too transparent to reverse many of its lines. That this is the case in the helium lines is tolerably certain.

In the apparatus described, two similar prisms were also placed over the slit in a symmetrical position. The spectra of two opposite edges of the sun were thus brought together, and the change in refrangibility due to the sun's rotation was very clearly shown.

CHAS H. HASTINGS

Newhaven, April 3

* Made at the Sheffield (U.S.) Scientific School. Communicated by Prof. Newton.

THE "INSTINCT" QUESTION

FROM the many additional communications we have received on this subject, we make the following selection.

With regard to a sense of direction, Mr. George C. Merrill, of Topeka, Kansas, writes as follows:—

I have learned from the hunters and guides who spend their lives on the plains and mountains west of us, that no matter how far or with what turns they may have been led in chasing the bison or other game, they on their return to camp always take a straight line. In explanation they say that unconsciously to themselves they have kept all the turns in their mind.

Mr. C. Bygrave Wharton, of Bushey, Herts, writes:—

As a left-handed and left-legged man who has more than once been lost in the bush in New South Wales, my experience may possibly be of interest to Mr. George Darwin and others. Invariably I unintentionally bore to the left; and once, after wandering for about six hours, just as I was giving myself up for lost, I discovered that I was within a hundred yards of the place from which I had started having performed a large circle to the left. It will thus be seen that though my left leg and arm are the stronger, there is always a tendency to walk in a circle to the left.

Mr. William Earley, of the Gardens, Valentines, sends the following interesting observations on the habits of wild rabbits:—

As is well known, the doe rabbit does not produce her young in any ordinary rabbit warren, or "run," but invariably selects a quiet out-of-the-way situation wherein to form a nursery for them. Now the reason for this peculiar practice has always been attributed to the fact that they leave their legitimate homes at this all-important period, simply because the male parents invariably destroy the offspring if an attempt be made to breed them in the permanent home or warren. I incline to believe we must look elsewhere for the explanation.

Firstly, then, a close atmosphere seems all-important to their development, as the old doe rabbit not alone denudes her breast of its natural fur covering wherein to ensconce them warmly all around, she also closes up the usual entrance to the nursery firmly, even patting the soil down to exclude the colder outer air. In due time, as the young increase in size, &c., she makes "air-holes," commencing with very minute ones, which are gradually enlarged as the inmates gain strength and size.

These are known facts, to which I add one not heretofore noticed, which seems important; it has reference to the formation of the subterranean nursery, in regard to its shape and the evident "end in view." These minor tunnels, or nursery "stops," are invariably formed by starting a downward curve, at an angle of about 45°, which is continued beyond any line of sight the eye can be guided by on the outer side. They subsequently curve abruptly upward, with almost double this initial acuteness, ending in a shelved enlargement, with the roof boundary nearly uniformly three inches from the surface of the ground above and without.

What I feel constrained to uphold in regard to these first facts is, that herein exists a most subtle sanitary arrangement; that by these means a subdued genial air is admitted, the only fresh air the nursery receives, and whereon the nurselings thrive, strengthen, and grow. The facts would seem to support the theory that the mother-parent continues what must be its hard work—doubly hard and severe in these finishing overhead excavations—until the very keen power of scent they possess assures them that the outer air is slightly admitted through innumerable interstices in the soil above.

My second proposition, or indeed belief, based upon distinct observation, is, that the parent doe rabbit does not visit its young, even nocturnally, at certain times oftener than once in each 72 hours! Certainly sometimes not more frequently than once during the 48 hours comprising two days and two nights. The latter fact I have ascertained by carefully marking and observing the neatly closed entrance to the stops, and also by marks beneath an iron garden-gate, in freshly laid gravel, which the rabbits had to scratch aside before they could enter. Furthermore, I have every reason to believe that the parent rabbit ceases to transmit the customary natural scent at the time she approaches or acts about the "stop;" if, indeed, as is the case with some kinds of game birds, during the period of incubation, she does not lose it altogether. Certain is it no appreciable amount of scent remains about the stop in the early morning after the parent rabbit has visited its nursery during the past night.

[On the question whether animals have the power of ceasing to emit a scent at certain times, see the article on Pheasants in this week's number.—ED.]

Mr. J. D. Bell, of the *World* office, New York, writes as follows on the consciousness of time in horses:—

My own experience will not allow me to speak positively as to smell, but horses that I have met and carefully observed, were not peculiarly gifted in this respect. It was a common saying on "the plains" and in the mining regions of California, that mules, by the way very sagacious animals, which would well repay observation, "scent the redskin a mile away." I have made some inquiry on this point, but have been unable to find that the olfactories of the mule are really thus acute. I can bear testimony to the extraordinary powers of sight in horses. And I am inclined to think that they take more notes by the way through their eyes than through the nose. As none of your correspondents have called attention to it, I desire to recall the fact that horses have ears as well as eyes and noses. Their hearing is very acute, and I am inclined to think that the explanation of the detection of red-skins by mules, will be found in the educated ear rather than in the educated nose. It used to be said in the cavalry service of the United States during the war that "horses were the best pickets." I have seen them again and again in the dead of night prick up their ears when the men on their backs heard nothing. I have never seen them sniff or smell first. Listening was invariably the first movement. Then came sight. Horses have scanned the woods and chapparall with a care that no man could surpass. If the moving thing first heard and then seen was an unfamiliar object—more especially if it was moving along the ground—then I have seen horses sniff, smell, and snort. In horses the snort is expressive of aversion rather than fear, or perhaps of a sentiment compounded of both.

Horses learn the notes of the bugle, and I have often seen a trained horse turn in a direction opposed to that indicated by the pressure of his less experienced rider's leg. I have known horses which, after detecting the presence of moving objects by hearing and then by sight, during which time they remained perfectly quiet, change feet, and even paw the ground if the rider did not by his movement show recognition of the presence of what might be an enemy.

And what, it will be asked, has this to do with the question at issue? Simply this—horses think, horses reason, horses classify, horses remember. But I desire to offer a few remarks on Darwin's letter about the blind mare that stopped at every public-house on the road. My own explanation of the fact, and there must be hundreds of similar instances—is that the mare, by long-continued custom, became conscious of the time which should elapse between the respective stopping places. Horses have a great memory for time. What is the interpretation of the existence and improvements of our racing and trotting horses but that these animals have the power of remembering time, and the power of transmitting this improved registering and transmitting cerebral apparatus to their progeny. I will close this letter by relating a couple of incidents. I was speaking of my belief in this equine memory for time to an enthusiastic horseman of my acquaintance, the other day, and at the same time showed him Mr. Darwin's letter. He said that in his youth he had driven a horse, sound in every respect, on a "bread" route. He always served his customers in a certain order. After a while his animal knew all the places, and stopped in front of the store or residence where bread was to be delivered, without a signal from his master. If the master remained in any place longer than usual, his horse started off, but instead of going to the next customer, returned to the stable. This, said he, occurred again and again, not at one place, but at many places.

I served, during the recent war, in a cavalry regiment in the United States' service. The horses knew the time for "the relief," and if the relief did not come they became restive. On one occasion we changed the time of remaining on post from two to four hours. For the first two hours the horses behaved admirably; after that they were in constant motion, and had to be constantly restrained. Horses recognised the time for stable call—not merely "hunger"-call, but the proper time-call.

A gentleman in the north of Ireland, who gives us his name and address, sends us the following story of a dog:—

He was a terrier—a cross upon the skye—very intelligent, like all of his kind. He was given to me by Mr. C——, a gentleman residing upon Lough Foyle near Moville. He was brought from that to Derry in a steamer up the Lough, and from Derry to Buncrana down Lough Swilly by train. He therefore travelled two sides of an acute-angled triangle, about thirty miles in all by conveyance. The third side being about fifteen miles, but a mountainous and unfrequented route. He appeared at first very happy and reconciled, but one fine morning he was seen taking the road parallel to the railway back to Derry, and after my searching for him for some days and making every inquiry, we found he had returned, tired and worn out, to his old master, Mr. C., near Moville. It was evidently hard work, and he was two or three days on the road. This I consider an interesting case.—Here the dog did not go by the third side of the triangle—which if he knew how to do he would have done instead of exhausting himself by the long route he took—following the direction along which he came by steamer and train.

My theory is that the dog does preserve a very distinct, or at least tolerably distinct, notion of the route he was brought from home by, and that it is forcibly impressed upon him; but the great aid to his return is the *direction* of the sun or light. He knows that if he travels in a certain direction—say E.—he is going towards the morning sun, and W., towards the evening sun.

A correspondent, Mr. R. A. Pryor, Hatfield, sends us the following extract from the Rev. A. l'Estrange's edition of Miss Mitford's "Life and Letters":—

Miss Mitford (Letter of October 16, 1829, vol. ii. p. 277), had been dining in company with the late Dr. Routh, president of Magdalen College, Oxford, who "had a spaniel of king Charles's breed, who, losing his mamma by accident when a pup, was brought up by a cat: well, he and his brother, for there were two pups, orphans of three days old, were nursed by this cat. But what I mention him to you for is to tell you the curious account which the doctor, a man of perfect veracity, gives of his habits—he is as afraid of rain as his foster mother, will never, if possible to avoid it, set his paw in a wet place; licks his feet two or three times a day, for the purpose of washing his face, which operation he performs in the true cattish position, sitting upon his tail; will watch a mouse-hole for hours together; and has in short all the ways, manners, habits, and dispositions of his wet nurse, the cat. Is not this very singular? But it's puzzling as well as amusing, and opens a new and strange view into that mysterious subject, the instincts of animals. Mrs. Routh, and Mrs. Blagrove (the mistress of the cat, who was present at dinner to-day), confirmed all the facts of the case. They say that one can hardly imagine how like a cat Romulus (the dog's name) is, unless one lived with him."

The following is from a letter of October 20, 1835:—

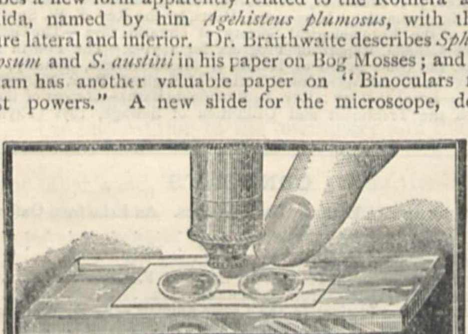
"Another characteristic of this hot dry summer (1835) has been the manner in which the large humble bees have forced open, torn apart the buds of my geraniums; an operation I never saw them perform before.

"Another novelty of this season has been that the splendid new annual, the *Salpiglossis picta*, has, after the first crop of blossoms, produced perfect seed without flower petals, a proof (if any were needed), that the petals which constitute the beauty of a flower, are not necessary to its propagation."

We may mention that Mr. C. H. Jeens has a cat and a dog, the latter now twenty months old, which, from the time the dog was a month old, have been in a relation similar to the cat and the pups in Miss Mitford's story, with a result somewhat similar. When the dog catches a mouse he treats it after the well-known manner of cats, pawing it, allowing it to run a distance, then pouncing upon it, and so on for many minutes.

SCIENTIFIC SERIALS

THE *Monthly Microscopical Journal* commences with the paper on "a new Callidina; with the results of experiments on the Desiccation of Rotifera," by Mr. H. Davis, which was read before the Royal Microscopical Society in April, and in which the author, by means of several carefully performed experiments, proves that Rotifera, which survive after being exposed to a temperature of 200° F., or in a vacuum for some time, do not get desiccated, but only covered with an impervious gelatinous covering which retains a certain amount of moisture in them. This Mr. Slack shows to have been previously proved. Mr. Parfitt describes a new form apparently related to the Rotifera and the Annelida, named by him *Agehisteus plumosus*, with the oral aperture lateral and inferior. Dr. Braithwaite describes *Sphagnum papillosum* and *S. austini* in his paper on Bog Mosses; and Mr. F. Wenham has another valuable paper on "Binoculars for the highest powers." A new slide for the microscope, designed



by Mr. D. S. Holman, is described. It is a current cel or moist chamber for studying the blood and other organic fluids. The accompanying illustration will assist in explaining it. Two shallow circular cavities are excavated in a very flat thick glass slide, not far from one another. They are united by two or three grooves, which are cut as triangles in order that they may be of unequal depth in different parts. When the apparatus is to be used, each of the shallow cavities and the intermediate grooves are partly filled with the fluid to be examined, after the slide has been warmed by the hand, and a glass cover is laid over the whole, which soon becomes fixed from the cooling of the slide and the consequent rarification of the enclosed air. The grooves between the cavities form the field for inspection, and any degree of movement may be produced in the fluid which they contain by approaching the warm finger to the top of one of the cavities, as the air inside is thus made to expand and drive some of the fluid into the other which is not heated. There is scarcely any limit to the degree of delicacy of movement which may be attained with this instrument; the slightest movement, not sufficient to remove a body from the field of vision, being produced without difficulty after some practice.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, May 15.—Dr. Odling, F.R.S., president, in the chair.—Dr. H. S. Armstrong delivered a most able and comprehensive lecture on "Isomerism," pointing out that the generally received position theory was incompetent to explain many reactions which took place in the formation of metameric and isomeric substances. He suggested that the investigation of the thermal properties of compounds would establish facts which might ultimately enable us to obtain some insight into the matter.

Anthropological Institute, May 20.—Prof. Busk, F.R.S., in the chair.—A paper was read by Mr. Hyde Clarke on the Egyptian Colony and Language in the Caucasus. This was devoted to a part of a series of investigations to ascertain the comparative chronology of prehistoric races by the correlation of comparative philology with the study of physical features, monuments, weapons, &c. It identified the Ude language of the Caucasus, that of an expiring population, with the Coptic, and still more closely with the Hieroglyphic in minute and numerous details of roots, grammar, and structure. The resemblance of the Bzyb dialect of Ude with the Bashmuric Coptic illustrated the differences between Hieroglyphic and Coptic. The paper then proceeded to point out the conformity of strata in the linguistic topography of Caucasia and the Nile regions, particularly in the earlier epochs of Agaw and Abkhas, and of Furian and Akush. Hence the conclusion was drawn that the sources of Egyptian grammar were not in the late Semitic, but in the prior epochs, and that Egyptian grammar and civilisation belong to a remote period in the annals of civilisation, but still to a relatively modern period in the history of man. The author, accepting the history of Herodotus as to the conformity between the Colchians of Caucasia and the Egyptians, did not accept his theory that the Colchians were a colony of Lesodites. In the time of Herodotus and Pindar, the Colchians, now light, were as dark as the Egyptians.

GLASGOW

Geological Society, April 24.—Mr. John Young, vice-president, in the chair.—Mr. David Robertson, F.G.S., read a note on the "Precipitation of Clay in Fresh and Sea Water." He stated that in making some observations on the gradual deposition of particles of clay held in solution by water, he found that in fresh water these were held suspended for a long time before wholly subsiding, while salt water, or a mixture of salt and fresh, became comparatively clear in the course of a few hours. The results showed that water only slightly brackish had a great power in precipitating the clay, and from this he concluded that the great bulk of the clay carried down in solution by rivers must be deposited before it could reach any great distance from the sea shore. This might throw some light on the formation of deltas, and on the silting up of river courses within the influence of the tides. It might also assist in determining how far the glacial mud, for example, could be carried into the sea by tides and currents.—The chairman read a paper which he had prepared in conjunction with Mr. Robertson, "On the Composition of the Boulder and Laminated Brick Clays of the West of Scotland." The authors stated that their object was to ascertain, if possible, the conditions under which these clays had been deposited, and how far any of them were fossiliferous. For this purpose they had collected samples of clays from upwards of fifty localities. These, after being dried, were weighed, and then carefully washed. The results led them to regard as most probable the conclusion that the till or unstratified boulder clay was a deposit that had been laid down in water and formed from materials which land ice had carried seawards, the ice extending over the submerged tracts now covered by the boulder clay. This seemed to be borne out by the large percentage of fine glacier mud found in all the boulder clays, and which they thought could not have been retained in the deposit had it been formed under a sheet of land ice above sea level, seeing that streams of muddy water continually issue from under all existing ice sheets. The laminated brick clays they viewed as having been formed by rewashings out of the boulder clay, and from the flow seawards of the muddy water from under the melting ice-sheets that bound the shores; the sea, however, being then comparatively free of ice. In nearly all the brick clays of the maritime districts they had found organisms, chiefly marine; b.t a few indicated brackish and fresh water conditions. Only in one or two instances had they found organisms in the boulder clay.

BERLIN

German Chemical Society, May 12.—President, A. W. Hoffmann. C. Engler spoke on the simultaneous action o ammonia and phosphoric anhydride on ketones, especially on acetophenone, C_8H_8O. The results are two bodies, $NC_{24}H_{18}$ and a hydrocarbon. The former crystalline, and a weak base is formed according to the formulæ,

$$2C_8H_8O + NH_3 - 3U_2O = NC_{24}H_{21}$$
$$2NC_{24}H_{21} + {}_3O = 2NC_{24}H_{18}.$$

The hydrocarbon simultaneously formed is beautifully crystallised triphenylated benzol, $C_6H_3 (C_6H_5)_3 = C_{24}H_{18}$. The reaction corresponds therefore to the formation of mesityline from acetone. Phosphoric anhydride and aniline seem to transform acetone into a base of the formula $CH_3 (C = NC_6H_5) - CH_3$ a liquid boiling between 210° – 220°.—C. Rammelsberg has investigated a so-called ozone-water, an article of trade, much advertised and praised for its medical properties. He has found no trace of ozone, but a small proportion of chlorine in it. Pursuing his researches, he found water of ordinary temperatures unable to absorb ozone without the application of pressure. The ozone was produced by Siemen's tube. Referring to a popular error: he explains what is generally considered as the production of ozone by mixing potassic permanganate with sulphuric acid, by the unavoidable presence of potassicperchlorate.—C. Scheibler referred to a gum, $C_{12}H_{22}O_{11}$ (isomeric with arabine) which he found in beetroot, and which is identical with metapectic acid. It occurs in two modifications turning the plane of polarisation to the right or to the lett. The latter is transformed by sulphuric acid into "arabin sugar" identical with the sugar he obtained by the same process from arabine. Both crystallise in identical rhombic prisms, turn the plane of polarisation to the right ($\alpha = \quad + 116$), reduce solutions of copper, and do not ferment.—N. Michaelis has made the interesting discovery that a liquid phenylic phosphide is obtained by passing benzole and tercloride of phosphorus through red hot tubes. It corresponds to the formula, C_6H_5. P. Cl_2, and boils at 222°.—A. Brückner, comparing two various mononitrophenoles, has found erroneous the supposition that there are four bodies of this formula, which he has reduced to three.—N. Baumgarten refutes the generally adopted opinion that bromine replaces chlorine in chloric acid. His experiments corroborate the doubt expressed by J. Thomson, and founded on his thermochemical researches.—C. Schorlemmer continued his valuable communications on hephylic acids and alcohols, as derived from heptan and from œnenthol.—V. Meyer recommends, for analysing commercial chloral, to heat it with a certain quantity of potassa of known strength, and to determine volumetrically the quantity of potassa that remained uncombined with formic acid.

PARIS

Academy of Sciences, May 12.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the portative force of magnets, by M. Jamin. The author thus denominates the carrying power of magnets. He exhibited two: an ordinary one weighing 6 and carrying 80 kilos, and one made on his principle, weighing 600 grammes and carrying 500 kilos; the paper described their construction.—On the causes which produce the tumefaction of obsidian at a high temperature, by MM. Boussingauld and Damour.—New researches on aldol, by M. Ad. Wurtz. The author thus names a condensed aldehyde partaking of the properties of that body and of an alcohol.—Hydrologic studies of the Seine, by M. Belgrand.—The Academy then proceeded to elect a member of the physical section in the place of the late M. Babinet. M. P. Desains obtained 32; M. Cornu, 13; M. Le Roux, 7; and MM. Bourget, Gaugain, and Lucas 1 vote each. M. Desains was therefore declared duly elected.—A report on MM. Troost and Hautefeuille's paper on isomeric and allotropic transformations was then read, and also one on a memoir on the proximate analysis of rocks, &c., by M. Fouqué.—On the water supply of Versailles during the first half of 1873, by M. E. Decaisne.—On the algebraic representation of bright lines in space, by Mr. W. Spottiswoode.—On the regulation of compasses, by M. Caspari.—On an electro-diapason of continuous movement, by M. E. Mercadier.—An answer to an observation of M. Raynaud on the conditions of maximum resistance in galvanometers, by M. Th. du Moncel.—Observations on the notes of MM. du Moncel and Thenard on the decomposition of carbonic anhydride by the silent electric discharge, by M. G. Jean.—Observations on a paper by M. du Moncel on the condensed induction spark, by M. Houzeau.—On the action of sulphur on arsenic, by M. Angélis.—On the action of gaseous hydochloric acid on the compound ammonias, by M. Ch. Lauth.—On a modification of the optical saccharimeter, by M. Prazmowski.—On the action of the dissolved oxygen of water on reducing agents, by MM. Schützenberger and Risler.—A new classification of the fresh-water algæ of the genus *Batrachospermum*, by M. Siroclot.—On spring and winter frosts, by M. Martha-Beker.

DIARY

THURSDAY, MAY 22.

SOCIETY OF ANTIQUARIES, at 8.30.—Miscellaneous Antiquities.
ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

FRIDAY, MAY 23.

ROYAL INSTITUTION, at 9—Spectra of Polarised Light: Mr. Spottiswoode.
GEOLOGISTS' ASSOCIATION.—Excursion to Eastbourne and St. Leonards.

SATURDAY, MAY 24.

ROYAL INSTITUTION, at 3.—The Historical Method: John Morley.
LINNEAN SOCIETY, at 3.—Anniversary.

MONDAY, MAY 26.

GEOGRAPHICAL SOCIETY, at 1.—Anniversary.

TUESDAY, MAY 27.

INSTITUTION OF CIVIL ENGINEERS, at 9.—Conversazione.
ROYAL INSTITUTION, at 3.—Archæology of Rome: J. H. Parker.

WEDNESDAY, MAY 28.

SOCIETY OF ARTS, at 8.
GEOLOGICAL SOCIETY, at 8.—On the Glaciation of the Northern part of the Lake District: J. Clifton Ward.—Alluvial and Lacustrine Deposits and Glacial Records of the Upper Indus Basin: Frederic Drew.—On the Nature and probable Origin of the superficial Deposits in the Valleys and Deserts of Central Persia: W. T Blanford.—On the Cephalopoda Bed and the Oolite Sands of Dorset and part of Somerset: James Buckman.
ARCHÆOLOGICAL ASSOCIATION, at 8.

PAMPHLETS RECEIVED

ENGLISH.—The Method of Quantitative Induction in Physical Science: Dr. G. Hinrichs.—Two Essays drawn up for the Official Record of the Exhibition held in Melbourne, 1872-3. 1. On Preserved Meats. 2. On Colonial Wines: Rev. J. I. Bleasdale, Melbourne.—Solar Radiation: an Account of the Experiments made at Harpenden, Herts, by the Rev. F. W. Stow, M.A.—Report of the Entomological Society of Ontario, 1872.—The Geological Survey of Canada, Report of Progress for 1871–2.—The Fourth Annual Report of the State Board of Health of Massachusetts, January, 1873.—Journal of the Royal Agricultural Society of England, Pt. 1, No. 17, Vol x. (Murray).—Local Biology: Rev. L. Blomfield (Bath).—Report of the Committee on the Treatment and Utilisation of Sewage, 1871 (Taylor and Francis)

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