THURSDAY, MAY 29, 1873

THE ZOOLOGICAL STATION AT NAPLES

ROME was not built in a day, says the proverb,—and so far, at least, the Zoological Station resembles the Eternal City,—for it is not yet quite finished.

The difficulties have been sufficient to explain this delay. The complexity of a building of this kind, which had to combine so many technical arrangements with scientific requirements without neglecting beauty of appearance and the comfort of a dwelling-house for the principal, assistant naturalists, and other officials, will easily be conceived by those who have ever attempted to carry out the plan of an establishment *sui generis*. Add to this, that the dimensions of the building were limited before a stone was laid, that the sums allotted for the construction were by no means unlimited, that all had to be done in so difficult a place as Naples, by a foreigner who never had experience in practical pursuits of this intricate nature, but is a naturalist, and not a business man.

At the same time, one must not believe that this delay has been altogether a misfortune. Though the Zoological Station had to pass through more than one "crisis," it has been particularly lucky: dangerous as the aspect of all these critical situations seemed, nevertheless it has always escaped, and now finds itself in better circumstances than it would have been without them. This seems principally due to the fact that in struggling against difficulties and enemies, one is forced to strengthen and augment one's auxiliary troops, and thus the army of supporters gets greater and greater, and triumph is more easily secured than before.

As the outlay had been considerably increased in consequence of greater dimensions, and some internal arrangements, it became necessary to find additional funds. I am happy to say, that on my application, the German Empire, after having consulted the Berlin Academy of Sciences, consented to contribute 1,500*l*. The Italian Government likewise promised, on my personal application to the Minister of Finances, Dr. Sella, to remit the not unimportant sums that had to be paid as duties on the importation of the machinery and the great glasses.

On the other hand, I formed a new scheme for keeping up the establishment. Some of the readers of NATURE may remember, perhaps, that the whole place was founded upon the income of the Aquarium, which is combined with the Zoological Station. The bulk of the capital being augmented, and the whole establishment in all its parts increased, the sums necessary for supporting it likewise must increase. Instead of ten places to be given to foreign naturalists, who come to work in the Zoological Station, there are now twenty. The number of officials, scientific and unscientific, will increase at the same rate, and everything else, too. Desirable as such an event must be for science' sake, much as it would increase the importance of the new Institution, there can be no doubt that it would also greatly increase its annual wants.

I pursued, therefore, as much as I could, the plan for letting the tables in the laboratories,—a plan which has been spoken of in NATURE (vol. vi. p. 362). I am happy to say that at present Italy as well as Prussia has consented to hire each two tables. Bavaria, too, is likely to take one table, and further applications have been made to Saxony, to Baden, and some other places, which at present cannot be indicated, as negociations are still impending.

The Library of the Station has made very important progress. The Zoological Society of London has generously granted the complete set of their Proceedings; the British Association the complete set of their Transactions. Dr. Engelmann, the Leipzig publisher, has again made a splendid gift of all that he has published since 1870; Viet and Co., of Leipzig, have given the eight last volumes of the *Archiv für Anatomie und Physiologie*: Friedlander, of Berlin, has sent some of his most valuable books; and single naturalists constantly send in their publications. The Catalogue of the whole Library will soon appear, and be delivered to the scientific public as Appendix to the *Zeitschrift für wissenschaftliche Zoologie*.

The Station has already made its presence felt in the world of Zoology, by sending to Universities and Laboratories collections of Mediterranean animals. What makes this especially valuable is, that by the careful way in which the required specimens have been prepared and preserved, they are always capable of being dissected and even studied in a histological way, which seldom is the case with museum specimens. Thus the Universities of Marburg, Göttingen, Munich, Strasburg, Jena, and others, have received such collections as were asked for by the Professors of Zoology; besides this, the zoologists that passed during the last winter to Naples or Messina, have been always assisted by the scientific staff of the Station.

We have also succeeded in sending animals alive to distant places. Thus it has become very generally known that a small parcel containing some specimens of Amphioxus has been received as a charged letter in the Crystal Palace Aquarium; and I hear from Mr. Lloyd that the small animals are still alive. We succeeded also in sending some large crabs over by steamer.

It is my intention to develop as much as may be this department of the activity of the Station, and I take this opportunity of stating that the Station will send Mediterranean animals of every kind and in any state of preparation to those who make application for them. The charges will be as moderate as possible, always in accordance with the self-supporting principle, so as to enable every part of the establishment to provide for its own wants.

ANTON DOHRN

Naples, May 8

GAUDIN'S "WORLD OF ATOMS"

L'Architecture du Monde des Atomes, devoilant la structure des composés chimiques et leur cristallogénie. Par Marc-Antoine Gaudin. (Paris: Gauthier-Villars, 1873.)

IT is now more than forty years since Ampère, in his lectures at the Collége de France, was discussing the evidence in favour of the existence of atoms, and the difficulties of any scientific investigation of their properties and relations. M. Gaudin, one of his hearers, was struck, as he tells us, with the importance of this investigation, and then and there devoted the efforts of his whole life to carry it out. Accordingly, in 1832 he presented a very extensive work to the Academy of Sciences, a report on which, by MM. Gay-Lussac and Becquerel, is annexed to the volume before us.

The ideas developed in this work were derived from two sources—crystallography and chemistry. Haüy had endeavoured to explain the regularity of the forms of crystals by regarding them as built up of molecules, the form of each molecule being similar to that of the simplest solid which can be obtained from the crystal by cleavage. The absolute size of these integrant molecules, as they were called, was left, of course, indeterminate.

Wollaston preferred to regard the arrangement of the ultimate molecules in a crystal as resulting, not from their accurately fitting one another as bricks do in a wall, but from their tendency to crowd together into the smallest possible volume as peas do in a bag. The form of the molecules, according to Wollaston, was not polygonal, but spherical or ellipsoidal.

At this point Ampère took up the theory. His atoms were no longer either closely fitted together, or even touching one another at isolated points, but were maintained by attractive and repulsive forces at distances exceedingly great compared with their own dimensions. The forms of the atoms themselves were therefore no longer considered as of any importance; the molecules, formed of groups of these atoms, were represented in diagrams as systems of points; and the explanation of the geometrical properties of the substance in the crystalline form was sought in the geometrical arrangement of these atoms.

The proportions in which the atoms of different kinds were to be represented in the molecules were determined in accordance with the atomic theory of chemistry, established by Dalton, and the absolute number of such atoms in the molecule was arranged so as to satisfy the law of gases, recently discovered by Gay Lussac, which asserts that the mass of every gaseous molecule is proportional to the specific gravity of the gas at the standard pressure and temperature.

The theory of M. Gaudin may be regarded as founded upon that of Ampère, with certain modifications. Instead of assuming with Ampère, that when two molecules combine, the form of the compound molecule is the resultant of the forms of its components, he supposes that the atoms of the combining molecules are all thrown into a common stock, to be arranged, according to some principle of equilibrium or of symmetry, in a form having no necessary relation to the forms of the combining molecules.

In the work before us M. Gaudin gives us, as the result of his long-continued meditation on compound molecules, actual diagrams of their supposed forms, showing not only their outward shape, but the arrangement of the molecules in each of the layers in which they are disposed. The ingenuity with which he has arranged in a symmetrical manner groups sometimes amounting to 279 atoms must be seen in order to be appreciated. But the merit of these arrangements as an explanation of facts must be tested, first by a careful comparison of those forms whose chemical relations are similar, and then by a comparison of each diagram with the crystallographic properties of the substance which it is supposed to represent. The author has, to the best of his ability, applied both these tests, and we shall not here pronounce sentence upon the result of such an examination.

We may remark, however, that M. Gaudin began his labours forty years ago, using the methods of investigation which we have briefly described. Since that time he has been patiently arranging his atoms by rows and groups, and representing them in models by means of pearls of various hues. He has shown no symptom of being attracted towards any of those newer paths which Joule, Clausius, and others have opened up into the higher regions of kinetic molecular science. Indeed we not only find no mention of the names of any of these men, but we look in vain for any indication of a desire to pass beyond mere geometrical arrangements of atoms, and to inquire into the forces with which they act on each other or the motions with which they are agitated. There is a chapter, indeed, entitled "Hémiédrie et pouvoir rotatoire," but though there is something about hemihedry, there is nothing there at all about the power of rotating the plane of polarisation of light. The only piece of dynamics in the book is the theory of capillary phenomena at p. 197, about which the less we say the better.

M. Gaudin is favourably known to science as an adept in the management of the blow-pipe. He has melted the most refractory bodies, and compounded the oriental ruby from its elements. He has not only established the chemical formula of silica and modelled its molecule, but he has fused quartz into beads, and drawn it into threads like spun glass.

His experimental researches have displayed great ingenuity and manipulative skill, but have often been brought to an untimely end for want of funds to carry them on. In his theoretical speculations he has been guided by geometry alone, without the powerful if not absolutely necessary aid of dynamics; and in the great work of his life he has met with very little encouragement, and has been sustained only by his conviction of the scientific value of the treasure of which he is in search.

OUR BOOK SHELF

A Manual of Photography. By George Dawson, M.A. Eighth edition. (J. and A. Churchill.)

THE new edition of this excellent manual of photography, which is founded on and incorporates as much of Hardwick's "Photographic Chemistry" as is valuable in the present further advanced stage of the art, retains its position as the best work on the subject for amateurs, as well as professionals. The many new methods and materials which are so frequently being introduced, make it essential that any book professing to keep up to the times must be frequently revised, and Dr. Dawson has in this work presented the subject in its most advanced position. The earlier chapters, after giving a short sketch of the history of photography, enter into a description of the most important experiments, the expansion of which make up the subject itself. This is followed by a review of the various lenses required for the many different purposes to which photography is applied, and their peculiarities are rendered more evident by the introduction of very clear diagrams of them in section. After a full description of the various points connected with the wet-plate collodion process, considerable space is devoted to the more modern subject of dry-plate photography. The many precautions necessary in the employment of the collodio-bromide negative process, as introduced by Messrs. Sayce and Bolton, and improved by Mr. Carey Lea and Colonel Wortley, are fully entered into; and the very rapid method introduced by the latter gentleman, in which the collodion is saturated with nitrate of silver, is given with some very recent formulæ. The subject of printing in pigments, so important in the present day, which "doubtless would become universal were the processes unfettered by patents," is fully described, with the difficulties attending the "double transfer" of the gelatin film. Following the details of photolithography, photozincography is that of collotype printing, which has become so prominent of late. A vocabulary of chemicals ends this valuable and suggestive work, of which, from want of space, we have had to omit the mention of many points.

LETTERS TO THE EDITOR

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

Science at Cambridge

THERE are some points in two articles which have lately appeared in your periodical upon which I should like to make a few remarks. First, however, allow me to congratulate the author upon having deprived the opponents of "Science" of a time-honoured monopoly. For a certain quiet insolence of conscious superiority and an inability to see more than one side of a question, his articles equal any diatribes that I have heard or read from the most intolerant supporter of "the old ecclesiasticism and false culture."

Let that pass, however, and let us proceed to examine one or two statements in detail. "Science is all but dead in England, . . . perhaps deadest of all at our Universities." Now on reading the word science, one has always to ask what a writer means, and the probability is that he means what is commonly called natural science; our writer, however, kindly gives us a definition—"that searching after new knowledge which is its own reward."

Most certainly the more eminent among our Professors and resident Fellows (and some of them are known even in Germany) cannot be said to have followed learning for any other reward; or if so they have taken their pigs to a very poor market. He will, perhaps, say that they have fellowships and professorships. Yes, the aggregate value of which will vary from probably six to nine hundred a year, coupled, in some cases, with conditions which seriously diminish their value. Is this so great a prize?

Again, we are represented as encouraging by prize fellowships and "that kind of liberal education which softens the character and prevents it being strong." I hardly know what the writer means; however, the following are the studies which we do endeavour to encourage:—

(1.) Mathematics.—Does the writer seriously mean to apply his remark to this study? If so, I can only say that to those who can appreciate sound and unsound reasoning, there is a marvellous difference between the work of (say) such a geologist as the late Prof. Sedgwick, trained in the school of mathematics, and not a few, whom it would be offensive to name, who have never had that advantage.

(2.) Language.—Perhaps in comparison with looking at mites in a microscope, or analysing some very rare but useless mineral, the attempt to enter into communion with the thought of the master-minds of our race in the past time is a contemptible pursuit; but though with a great regard for both the above pursuits myself, and with no pretension to refined scholarship, I cannot—and hope but few can—agree with this opinion.

(3.) The moral sciences.—If this is meant, I give the writer over to the tender mercies of the philosopher, who, I think, will be able to give an account of him: so also as regards legal studies.

(4.) Natural Sciences.—These are encouraged in precisely the same way as the others (in the case of most colleges). Is this then culture which produces effeminacy?

But perhaps the writer will say that only classics and mathematics are encouraged. To this I reply that the other studies are of recent growth in the University—I admit that they ought to have taken root long ago—that however is not our critic's charge—he is dealing with the present—and I have no hesitation in affirming that in all the important colleges the students of natural science have just as good a chance of honour and rewards as those of other branches of learning. The number of rewards that have been given is small, because the number of really first-class students has hitherto been small. The number of students (and their quality) increases year by year. I have no fear that as a rule they will meet with their deserts.

Or does the writer mean to say that our fault is teaching and examining, that we ought to open laboratories (for notwithstanding his definition, I think his science means only one thing) and simply exercise a general superintendence over students? After an experience of some years I can only say that though I do not worship either lectures or examinations (especially the latter), with a blind "idolatry," I believe without them the majority of young students are very apt to become slipshod and slovenly in their work.

But the trumpet's sound is so uncertain that I know not exactly what the writer does mean. I have read over and over again his *tertium quid* (p. 41), in supporting which he confesses he is with a select few—doubtless the salt of the earth—and I still am doubtful. They be very "brave words"—but "to make the University a place where anyone and everyone may be trained for any and every respectable path of life," is just the aim of every change that has been made since I have known the place. Our students—of subjects not classical—certainly increase yearly, and I have not heard of any marked deportation to the Elysian fields of either Manchester, London, Newcastle, or Germany. Neither have I found that such "master minds of the age" as are within our walls (and I think, subject to the writer's approval there are some) inaccessible to students.

In conclusion I must state in self-defence that I am not usually considered a conservative. I have done all that was in my power to help the cause of University Reform, and especially of Natural Science. But much as I delight in the latter I decline to regard it as the only culture, the only training worthy of respect. I trust to live to see yet greater changes. These however will not be obtained by vague declamations or reckless accusations—such as "long years of misrule have left suckers of jobbery, like bind-weed in an old garden, which come up refreshed with every stirring of the soil." After twelve years of active life in the University—and often failing to obtain what I wanted—I unhesitatingly assert that there is no place where there is so little jobbery, or where the motives that actuate men, even if mistaken, are so generally pure as here. There are jobs everywhere now and then, even in scientific societies or coteries.

There are indeed difficulties in the way of reform, but the writer of the articles, I venture to think, has not hit the nail on the head. May I, before ending this long letter, in a few words indicate one or two—1. The workers in the University should govern it. At present a body called the Senate, consisting chiefly of non-residents, has the final decision of everything. Throw all the power into the hands of the working bees, and we will reform ourselves fast enough. 2. The waste of money in non-resident fellowships. There are very few here who would defend the present system, but we have no power to change it ourselves. 3. The poverty of the Universities. To relieve this we must have power given to colleges to alter their statutes, and so long as the University is governed by the Senate, we in the colleges do not care to put beyond our control funds which might then even be applied to political or theological squabbles. 4. An improvement of the Professorial System—more teachers and rather less routine work, with greater unity of action between the former. Give us powers, and we will soon settle that. We are bound, like the Jews, "by a chain of ordinances," and till that is broken cannot help ourselves. T. G. BONNEY

St. John's College, Cambridge

Arctic Exploration

THE news of Mr. Hall's Arctic Exploration is important from two points of view, and I shall be obliged if you will allow me the space to point out the lessons to be derived from it, and the way in which the new facts strengthen the arguments for Polar exploration.

But first it is most important that the attempt of the *Times* to injure the cause of Arctic discovery, in an article published last Saturday (May 24), should be examined; for if all the arguments of the enemies of knowledge are summed up in that article, they are weak indeed. Such as they are, these arguments are propped up by three incorrect statements which must, in the first place, be knocked away. First it is alleged that "men of science cannot tell us what are the problems they hope to clear up" by Arctic discovery. This assertion is disproved by the documents published by the *Times* itself on December 16, in which the objects of Arctic exploration are clearly and distinctly enumerated. That this enumeration may be, and should be made more exhaustive, you pointed out at the time, and your suggestions have been adopted. But the objects are clear enough, and have been clearly stated. Briefly they are the investigation of the geography, hydrography, geology, meteorology, fauna, flora, and ethnology of an unknown area covering several million square miles of the earth's surface. Secondly, the *Times* alleges that Arctic Explorers "made little of the dangers of the proposed expedition." This is a mistake. Arctic Explorers have done nothing of the kind. They are perfectly aware of the extent and scope of the dangers, and how they can be reduced to a *minimum*, and they have furnished the Government with the results of their experience. Thirdly, it is asserted by the *Times* that geographers "confidently cited the pending close of Mr. Hall's Expedition as a conclusive fact to be argued from." This also is incorrect. Mr. Hall's expedition was simply mentioned in their address, without any comment, and no argument whatever was derived from it; for the very good reason that its history was then unknown. Very strong arguments, however, will now be based upon the facts stated by the boat's crew of the *Polaris*.

These erroneous statements being refuted, the whole argument in the *Times* article, falls to pieces. There remains a highly-coloured version of the story told by the boats' crew of the *Polaris*, garnished with sensational sentences, of which the following are examples:—"*Death, in a hundred ghastly shapes, dogs the shadow of this phantom ship*" . . . "*Are we morally justified in exposing human beings to the slow torture of a solitary death by famine or by cold?*" and the like; the moral being, that because the writer in the *Times* has imagined some fanciful nightmare, therefore no Englishman is again to venture into the Arctic Regions.

Let us turn to the plain facts. The *Polaris* is a vessel wholly unsuited for ice navigation; she was commanded by a landsman, and her crew was undisciplined and not under proper control. Yet she passed safely through Baffin's Bay and far up Smith Sound, where at least two exploring parties made journeys to the north; she wintered, and was drifted out into Baffin's Bay last summer, where part of the crew deserted with all the boats. But she had plenty of provisions, could easily winter in Whale or Wolstenholme Sounds, where there are friendly Esquimaux, could construct a boat if necessary, and a fleet of whalers will be in the "North Water," ready to give assistance, this summer. Obviously the blame of any disasters that may have befallen her cannot be imputed to the Arctic Regions. Under the circumstances, she would have been leaky, her crew would have been mutinous, and she would have lost her boats in any other climate. These events are due to the way in which the expedition was organised, not to the temperature.

An English Arctic Expedition, consisting of two vessels adapted for ice navigation with a picked crew under naval discipline, and commanded by an experienced seaman, will run no such risks. One vessel, as a depôt, could be stationed near the entrance of Smith Sound, while the other pressed to the north; so that, in the improbable event of the advanced ship being lost, her crew could retreat to the consort. The dangers of Arctic exploration are involved in the travelling and boat-work, and in exposure to frost-bites and over-fatigue. They are not such as Englishmen may not freely and prudently encounter in the cause of science and discovery. They are such as our ancestors were eager and anxious to meet and overcome, and as their descendants, in spite of the *Times*, intend to encounter again and again. They apply to individuals, not to the expedition as a body, and have been reduced to a *minimum* by modern science and experience. The climate is the healthiest in the world, the scenery enchanting, the work most interesting and fruitful. The *Times* alleges that former Arctic expeditions have brought back nothing but "a few insignificant facts, and a multitude of barren conjectures." This is the exact reverse of the truth. They have brought back a multitude of important facts in all branches of science, and priceless collections. To them is due the lucrative whale and seal-fisheries, great stores of knowledge, the materials for such papers as that by Dr. Hooker on the *Arctic flora*, and for many others of similar value.

The news brought by the boat's crew of the *Polaris*, if at all accurate, is very important. It proves that even such a vessel as the *Polaris* may advance up Smith Sound, in one season, to 82° 16′ N. It is stated, also, that at, or near this point, the land, both of Greenland and Grinnell Land, was still trending northward. From such a point, an extended party, with depôt parties, organised on McClintock's principles of sledge travelling, could reach the North Pole and return to the ship. Another important fact is that the *Polaris* was beset in 80° 2′ N. and drifted out into Baffin's Bay. This shows that there is not a constant block of ice in the strait, but that the floes drift down with the current, leaving, as a consequence, an occasional navigable lane between the drifting ice and the land floe. These facts are most satisfactory, and increase the prospect of a successful exploration of the unknown region by way of Smith Sound. I think that you have already announced the nomination of an Arctic Committee by the Council of the Royal Society, to confer with the Committee of the Geographical Society; and we may fairly anticipate that when these bodies again bring the subject to the attention of Her Majesty's Government this summer, the case, both as regards the important objects to be attained by Arctic exploration, and the measures to be adopted, will be materially strengthened.

London, May 27 CLEMENTS R. MARKHAM
Late of H.M.S. *Assistance* in the Arctic Expedition of 1850-51.

Forbes and Tyndall

PROF. HUXLEY is at a loss "to discover any excuse for the biographer" having published, at pp. 386-7 of the "Life and Letters of the late Principal Forbes," a letter, with an extract from which he heads his own letter in the last number of NATURE. For publishing that letter no excuse need be offered, because a sufficient reason can be given.

The discussions regarding the glacier question, and the decision of the Council of the Royal Society regarding the Copley Medal in the autumn of 1859, are matters well known to all who take interest in such subjects. Some further light has been thrown upon the history of the latter transaction by the recent letter of Prof. Huxley. Neither into the overt facts, nor into their secret springs, was it my duty as a biographer to enter. But it was my duty to record the impression made on Forbes's mind by the treatment he then received. This I did, not by "deliberately picking expressions out of a private letter," as Prof. Huxley phrases it, but by giving, without note or comment, nearly the whole of a letter written by Forbes at the time to his friend Mr. A. Wills.

Instead of objecting to the few lines on this subject which have been allowed to appear, Prof. Huxley may rather appreciate the reserve which has passed over so lightly a transaction in which the late Principal Forbes felt that he was deeply wronged. But it was the desire of the biographers to exclude, as much as possible, all controversial matters, not from any doubt as to the justice of Forbes's claims as a glacier discoverer, but in order, as far as might be, to avoid strife. If they have not succeeded in doing so as completely as they wished, this has not been their fault. When the book was almost through the press, they found themselves, by the appearance of Prof. Tyndall's work on the "Forms of Water," constrained to depart somewhat from their original intention, and to include two statements which Forbes had written on the subject of his glacier discovery, and which are now to be found in Appendix A and Appendix B of his "Life." These contain the gist of the whole question, as far as Forbes was concerned. Neither the one nor the other has ever yet been refuted in any point. If Prof. Huxley desires to justify the action of himself and others who opposed Forbes in 1869—and to deal with the question in the only way in which the world is at all concerned with it—let him try to disprove the facts and refute the statements contained in these two appendices. If he succeeds in this attempt, he will have removed the grounds on which Forbes rested his claims to be held as a glacier discoverer. Till this has been done, to discuss merely incidental personal allusions is to miss the point, and to evade the main issue.

J. C. SHAIRP

Houston House, Linlithgowshire, May 26

Clerk-Maxwell's Kinetic Theory of Gases

YOUR correspondent, Mr. Guthrie, has pointed out an, at first sight, very obvious and very serious objection to my kinetic theory of a vertical column of gas. According to that theory, a vertical column of gas acted on by gravity would be in thermal equilibrium if it were at a uniform temperature throughout, that is to say, if the mean energy of the molecules were the same at all heights. But if this were the case the molecules in their free paths would be gaining energy if descending, and losing energy if ascending. Hence, Mr. Guthrie argues, at any horizontal section of the column a descending molecule would carry more energy down with it than an ascending molecule would bring up, and since as many molecules descend as ascend through the section, there would on the whole be a transfer of energy, that is, of heat, downwards; and this would be the case unless the energy were so distributed that a molecule in any part of its course finds itself, on an average, among molecules of the same energy as its own. An argument of the same kind, which occurred to me in 1866, nearly upset my belief in calculation, and it was some time before I discovered the weak point in it.

The argument assumes that, of the molecules which have encounters in a given stratum, those projected upwards have the same mean energy as those projected downwards. This, however, is not the case, for since the density is greater below than above, a greater *number* of molecules come from below than from above to strike those in the stratum, and therefore a greater number are projected from the stratum downwards than upwards. Hence since the total momentum of the molecules temporarily occupying the stratum remains zero (because, as a whole, it is at rest), the smaller number of molecules projected upwards must have a greater initial velocity than the larger number projected downwards. This much we may gather from general reasoning. It is not quite so easy, without calculation, to show that this difference betwen the molecules projected upwards and downwards from the same stratum exactly counteracts the tendency to a downward transmission of energy pointed out by Mr. Guthrie. The difficulty lies chiefly in forming exact expressions for the state of the molecules which instantaneously occupy a given stratum in terms of their state when projected from the various strata in which they had their last encounters. In my paper in the *Philosophical Transactions*, for 1867, on the "Dynamical Theory of Gases," I have entirely avoided these difficulties by expressing everything in terms of what passes through the boundary of an element, and what exists or takes place inside it. By this method, which I have lately carefully verified and considerably simplified, Mr. Guthrie's argument is passed by without ever becoming visible. It is well, however, that he has directed attention to it, and challenged the defenders of the kinetic theory to clear up their ideas of the result of those encounters which take place in a given stratum. J. CLERK MAXWELL

Additional Remarks on Abiogenesis

SINCE my communication in NATURE, March 20, a further investigation of the subject has shown me that the experiments there recorded do not yet fully prove the reality of abiogenesis. My argumentation based on those experiments is liable to the following objection:—

The principal experiment (water, potassium-nitrate, magnesium-sulphate, calcium-phosphate, glucose, and peptone) is conducted in a *neutral* solution. In the control-experiments neutral ammonium-tartrate is used as nutritious substance for the supposed germs. But this salt disassociates by boiling, loses ammonia, and the reaction becomes *acid*. When, therefore, Bacteria appear in the principal experiment and not in the control-experiments, this result can be explained by admitting that the germs resist a temperature of 100° in a neutral liquid, but are killed by the same temperature in an acid solution. This explanation agrees very satisfactorily with the fact proved by Pasteur, that an acid reaction is much more deleterious to living germs than a neutral reaction at the same temperature.

This objection is very rational, but it does not throw over my conclusion respecting the reality of abiogenesis, for the following reasons:—

It is now obvious that in the control-experiments ammonium-tartrate cannot be used, a nitrogenous body must be sought, not too complex, that remains neutral by 100°. For this end I have found urea to answer well. Pure urea is perfectly fit to furnish nitrogen to the Bacteria, but not to furnish them their carbon. Bacteria sown in a solution of urea and mineral salts do not develop themselves, but when sugar is added their growth goes forth rapidly. The following solution—100 c.c. water, 0·2 grms. potassium-nitrate, 0·2 grms. magnesium-sulphate, 0·04 grms. calcium-phosphate, 1 grm. glucose, 0·5 grm. urea, is eminently fit for the development of Bacteria. Also a solution that contains, instead of the sugar and the urea, 0·5 grm. peptone.

These solutions were now used in the control-experiments. For instance:

a. Principal experiment. 100 c.c. salt-solution,* 2 grms. glucose, 0·3 grms. peptone boiled and treated in the ordinary manner (See NATURE, vol. vii. p. 380). On the third day the liquid contains countless swarms of Bacteria.

b. Control experiment. 100 c.c. salt solution, 1 grm. glucose, 0·5 grm. urea, boiled exact. No Bacteria appear; on the eighth day the liquid is perfectly clear.

c. Control experiment. 100 c.c. salt solution, 0·5 grm. peptone, boiled, &c. On the eighth day complete absence of Bacteria.

In each of these experiments the reaction is neutral. They are therefore fully comparable. The experiments *b* and *c* prove, moreover, that the closing tiles exclude completely the atmospheric germs, a fact that was also proved by direct experiments, wherein the solutions *b* and *c* were used and dust strewn on the closing tile in the manner formerly described.

But is it not possible to generate Bacteria in a liquid which has been boiled when acid?

To elucidate this point, the above-named solution *a* was rendered acid (2-4 c.c. of a 1 per cent. solution to 100 c.c.) and treated as usual. No Bacteria appeared, whether the liquid was, after boiling, neutralised with soda or not.

But this negative result is easily conceivable; for the acid alters essentially the calcium-phosphate, changes $CaHPO_4$ into $Ca_2H_4P_2O_8$. And that this alteration is not without influence, is rendered probable by the fact, which I have recorded in the *Maandblad voor Natuurwetenshcappen*, No. 7 (April 23, 1873), namely, when in the principal experiment instead of $CaHPO_4$ is used a mixture of $Ca_3P_2O_8$, and $Ca_2H_4P_2O_8$ the result (the genesis of Bacteria) is much less constant. The neutral calcium-phosphate by boiling with water breaks up in the basic and the acid salt, but this division must take place in the presence of sugar and peptone.

On the other hand, the acid modifies the peptone. This is easily demonstrated by comparing, in the polariscope, the rotating power of a neutral peptone-solution with the power of the same solution. After boiling with acid a notable difference is observed.

The acid can, nevertheless, be employed with the following modification:—In 100 c.c. water are dissolved 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, and 2 grms. glucose; 2 c.c. of a 1 per cent. solution of tartaric acid are added, so that the liquid has a strong acid reaction. It is then boiled for ten minutes. Then with a red hot platinum spatule a little soda is taken from a hot crucible and thrown in the flask. The quantity of soda required is approximately ascertained by a preliminary trial. Care should be taken not to render the liquid alkaline. Then 0·05 grm. calcium phosphate and 0·3 grm. peptone are added together, and the boiling continued for ten minutes. The flask is closed as usual, and deposited in the hatching-bath. Three days after, it swarms with Bacteria.

When, instead of calcium-phosphate and peptone, are added 0·05 grm. calcium-phosphate and 0·5 grm. urea, nothing appears; and the result is equally negative when the following solution is taken:—100 c.c. water, 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, 0·05 grm. calcium-phosphate, 1 grm. potassium-natrium-tartrate, 0·3 grm. peptone. In this latter case no acid is used. The addition of the tartrate is made to have a sufficient quantity of carbon in the liquid. These control experiments prove that none of the employed materials, neither the glucose, nor the calcium-phosphate, nor the peptone did introduce germs.

By these experiments the above-stated objection is, in my opinion, satisfactorily refuted.

In concluding these remarks, I must mention an important fact. For the above-described experiments, I employed mostly the ordinary glucose, an amorphous, yellowish white mass, not chemically pure. By crystallisation from strong alcohol, I purified this sugar. In three different preparations I obtained thus three samples of perfectly white more or less pure glucose. One

* Composed of 1 grm. potassium-nitrate, 1 grm. magnesium sulphate, 0·2 grm. neutral calcium-phosphate in 500 c.c. water.

of these samples yielded, with peptone, Bacteria; not so the other two. All three were prepared with the utmost caution respecting atmospheric dust, &c. That, moreover, the positive result could not be caused by an accidental admixture of germs was amply proved by the often repeated control-experiments. It appears, therefore, that, besides the glucose and the peptone, a third substance is needed for generating Bacteria, a body present in the ordinary glucose (starch-sugar), but removed by purification. The nature of this body I have not yet been able to ascertain. But however important, this matter has no direct bearing upon the question of abiogenesis. For that this third unknown body cannot be (as some will probably presume) a germ, my control-experiments and also the above-described experiment, wherein the sugar was boiled with acid, do sufficiently prove.

D. HUIZINGA

Groningen, May 23

Flight of Birds

SOME time since I had occasion to ascend a mountain in the neighbourhood. The wind was blowing over the ridge-like crest of the mountain with a velocity of, I should say, ten or twelve miles an hour, sweeping with increased rapidity through certain transverse gorges cutting the ridge at right angles. In one of these I observed a hawk hovering in search of prey. In the midst of this rapid air current the bird remained apparently *fixed* in space, without fluttering a wing, for at least two minutes. After a time it gently changed its position a few feet with a slight motion of its wings, and then came to rest again as before, remaining apparently as motionless as the rocks around it. From my nearness to it a change of position of an inch would have been clearly visible, and yet except when it seemed to desire to change its point of observation no motion of any kind could be detected. How is this to be accounted for? Does a bird possess the power of giving an extremely rapid tremulous motion to its wings invisible even at a small distance, similar in its nature to the wing vibration of certain insects, which, as any one may have noticed, have a similar power of apparently fixing themselves in space over a flower, for example, notwithstanding a condsiderable amount of motion in the air in which they are suspended?

If any of your correspondents would kindly take the trouble to throw some light on these points they would greatly oblige one who is unfortunately placed out of reach of the ordinary means of reference.

J. GUTHRIE

Graaff Reinet, Cape Colony, April 2

THERMO-ELECTRICITY

THE subject I have chosen is one intimately connected with the names of at least two well-known members of this University—the late Prof. Cumming and Sir William Thomson. It possesses at present peculiar interest for the physicist; for, though a great many general facts and laws connected with it are already experimentally, or otherwise, secured to science—the pioneers have done little more than map the rough outlines of some of the more prominent features of a comparatively new and almost unexplored region. Some of its experimental problems are extremely simple, others seem at present to present all but insuperable difficulties. And it does not appear that any further application of mathematical analysis can be safely, or at least usefully, made until some doubtful points are cleared up experimentally.

The grand idea of the conservation, or indestructibility, of energy :—pointed out by Newton in a short Scholium a couple of centuries ago, so far at least as the progress of experimental science in his time enabled him to extend his statements :—conclusively established for heat at the very end of last century by Rumford and Davy; and extended to all other forms of energy by the splendid researches of Joule :—forms the groundwork of modern physics.

Just as, in the eye of the chemist, every chemical change is merely a re-arrangement of indestructible and unalterable matter; so to the physicist, every physical change is merely a transformation of indestructible energy; and thus the whole aim of natural philosophy, so far at least as we yet know, may be described as the study of the possible transformations of energy, with their conditions and limitations; and of the present forms and distribution of energy in the universe, with their past and future.

It is found by experiment that some forms of energy are more easily or more completely transformable than others, and thus we speak of higher and lower forms, and are introduced to the enormously important consideration of the degradation, or, as it is more commonly called, the dissipation, of energy. The application of mathematical reasoning to the conservation of energy presented no special difficulties which had not, to some extent at least, been overcome in Newton's time: but it was altogether otherwise with the transformations of energy. And it is possible that, had it not been for the wonderfully original processes devised by Carnot in 1824, we might not now have secured more than a small fraction of the immense advances which science has taken during the last thirty years.

For a transformation of heat we must have bodies of different temperatures. Just as water has no "head" unless raised above the sea-level, so heat cannot do work except with the accompaniment of a transference from a hotter to a colder body. Carnot showed that to reason on this subject we must have *cycles* of operations, at the end of which the working substance is restored exactly to its initial state. And he also showed that the test of a *perfect* engine (*i.e.* the best which is, even theoretically, attainable) is simply that it must be *reversible*. By this term we do not mean mere backing, as in the popular use of the word, but something much higher—viz. that, whereas, when working directly, the engine does work during the letting down of heat from a hot to a cold body; when reversed, it shall spend the same amount of work while pumping up the same quantity of heat from the cold body to the hot one. As a reversible engine may be constructed (theoretically at least) with any working substance whatever, and as all reversible engines working under similar circumstances must be equivalent to one another (since each is as good as an engine can be) it is clear that the amount of work derivable from a given amount of heat under given circumstances (*i.e.* the amount of transformation possible) can depend only upon the temperatures of the hot and cold bodies employed. In this sense we speak of Carnot's Function of Temperature, which is as imperishably connected with his name as is the Dynamical Equivalent of Heat with that of Joule.

Building upon this work of Carnot, Sir W. Thomson gave the first *absolute* definition of temperature—that is a definition independent of the properties of any particular substance. Perhaps there is no term in the whole range of science whose meaning is correctly known to so few even of scientific men, as this common word temperature. It would not, I think, be an exaggeration to say that there are not six books yet published in which it is given with even an approach to accuracy. The form in which the definition ultimately came from the hands of Joule and Thomson enables us to state as follows the laws of transformation of energy from the heat form.

1. A given quantity of heat has a definite transformation equivalent.

2. But only a fraction of this heat can be transformed by means even of a perfect engine: and this fraction is DEFINED as the ratio of the range through which the heat actually falls to that through which it might fall—were it possible to obtain and employ bodies absolutely deprived of heat.

This definition has two great advantages. 1st, The utmost amount of work to be got from heat under any circumstances of temperature is determined by precisely the same law as that assigning the work to be had from

* Abstract of the Rede Lecture delivered in the Senate House, Cambridge, May 23, 1873.

water under similar circumstances of level. In this case the sea-level corresponds to what is called the Absolute Zero of temperature. [It is well to observe here that it is the potential energy of the water, not the quantity of water itself, which corresponds in this analogy to the quantity of heat. In this simple remark we have all that is necessary to correct Carnot's reasoning in so far as it was rendered erroneous by his assumption of the materiality (and consequent indestructibility) of heat.] 2nd, Temperatures thus defined correspond, as Thomson and Joule have shown by elaborate experiments, very closely indeed with those given by the air-thermometer—the absolute zero being about 274° of the Centigrade scale below the freezing point of water. I have made this digression as I shall have frequently to use the word temperature, and I shall always employ it in the sense just explained.

The subject of Thermo-electricity of course includes all electric effects depending on heat, but in this lecture I shall confine myself to the production by heat of currents in a circuit of two metals.

The transformation of heat into the energy of current electricity was first observed by Seebeck in 1820 or 1821. His paper on the subject (Berlin Ac., 1822-3, or Pogg. vi.) is particularly interesting, as he gives the whole history of his attempts to obtain a voltaic current from a circuit of two metals without a liquid, and the steps by which he was led to see that heat was the active agent in producing the currents he eventually obtained. In this paper Seebeck gave the relative order of a great number of metals and alloys in the so-called thermo-electric series, and showed that several *changes of order* occurred among them as the temperature was gradually raised.

In a note attached to this paper, Seebeck recognises that in this further discovery he was anticipated by Cumming (who seems, in fact, to have made an independent discovery of Thermo-electricity). Cumming showed that when wires of copper, gold, &c., were gradually heated with iron, the deflection rose to a maximum, then fell off, and was *reversed* at a red heat.

[Seebeck's original experiment and Cumming's extension of it were exhibited.]

You see that, keeping one of the copper-iron junctions at the temperature of the room and gradually heating the other, I produce a current which increases in intensity more and more slowly till it reaches a maximum, then falls off faster and faster till at last it vanishes and thereafter sets in the *opposite* direction. We are still far below the melting point of copper, yet further heating up to that point produces but little additional effect. The reason of this will be apparent from some facts to be described towards the end of the lecture. At the moment of maximum current the two metals are thermo-electrically *Neutral* to one another.—The temperature in the present case is about 280° C.

Seebeck pointed out that bismuth and antimony (to the choice of which he had been led by a very curious set of arguments) were very far removed from one another in the series, and therefore gave large effects for small differences of temperature. This is still taken advantage of in the Thermo-electric Pile, which, when combined with a sufficiently delicate galvanometer, is even now by far the most delicate thermometer we possess. It has recently enabled astronomers to detect and measure the heat which reaches us from the moon, and even from the brighter fixed stars. In the skilful hands of Forbes and Melloni this instrument was the effective agent in demonstrating the identity of thermal and luminous radiations—a step which, as regards the simplification of science, is as important as the discovery of magneto-electricity; and which was completed by Forbes when he succeeded in polarising radiant heat.

But when we come to look at this question from the point of view of transformation of energy, we have to ask *where* is the absorption, and *where* the letting-down of heat, to which the development of the current considered as a rise of energy is due. Very remarkably, an experiment of Peltier supplies us with at least part of the answer. Peltier showed that, given a metallic junction which when heated would give a current in a certain direction, then provided a battery were interposed in that circuit (initially at a uniform temperature) so as to send a current in that direction, the passage of the current *cooled* the junction, while a reversal of the current heated it. This, considering the circumstances under which it was made, and the deductions since drawn from it, is one of the most extraordinary experimental discoveries ever made. Water was frozen, in an experiment by Lenz, by means of the Peltier effect.

Here then is a reversible heat effect, and to it we may reasonably assume that the laws of thermodynamics may be applied; although from the very nature of the experiment the reversible effect must always be accompanied by non-reversible ones, such as dissipation by heat-conduction, and by heat generated in consequence of the resistance of the circuit. The latter of these is in general small in thermo-electric researches, but the former may have large values.

It is known from the beautiful experiments of Magnus that no thermo-electric current can be produced by unequal heating in a homogeneous circuit, whatever be the variations of section—a negative result of the highest importance. Sir W. Thomson, to whom we are indebted for the first and the most complete application of thermodynamics to our subject, showed that the existence of a neutral point necessitates the existence of some other reversible effect besides that of Peltier. And even if the circuit varied in section, the result of Magnus, just referred to, showed that this could only be of the nature of a convection of heat by the current between portions of the same metal at different temperatures. Thomson's reasoning is of the very simplest character, as follows:—Suppose the temperature of the hotter junction to be that of the neutral point, there is no absorption or evolution of heat there; yet there is evolution of heat at the colder junction, and (by resistance) throughout the whole circuit. The energy which supplies this must be that of the heat in one or both of the separate metals; but reasoning of this kind, though it proves that there must be such an effect, leaves to be decided by direct experiment what is the nature and amount of this effect in each of the metals separately. By an elaborate series of ingenious experiments Thomson directly proved the existence of a current convection of heat, and (curiously enough) of opposite signs in the first two metals (iron and copper) which he examined. In his own words, "Vitreous Electricity carries heat with it in an unequally heated copper conductor, and Resinous Electricity carries heat with it in an unequally heated iron conductor." This statement is not very easy to follow. It may perhaps be more intelligible in the form:—In copper a current of positive electricity tends to equalise the temperature of the point it is passing at any instant with that of the point of the conductor which it has just left, *i.e.*, when it passes from cold to hot it tends to cool the whole conductor; when from hot to cold, to heat it, thus behaving like a real liquid in an irregularly heated tube. The effects in iron are the opposite; and Thomson therefore speaks of the specific heat of electricity as being thus positive in copper and negative in iron. He gives a very remarkable analogy from the motion of water in an endless tube (with horizontal and vertical branches), produced by differences of density, due to differences of temperature. Here the maximum density of water plays a prominent part. Neumann has recently attempted, by means of the laws of motion of fluids, and the unequal expansibility of different metals, to give a physical explanation of thermo-electric currents. But, not to speak of the fact that positive electricity is by him considered as a real fluid, there are the fatal objections that his method makes no provision for the explanation of the Peltier, or of the Thomson, effect; and therefore cannot be looked upon as having any useful relation to the subject. Similar remarks apply to the attempt of Avenarius to account for thermo-electric currents by the variation with temperature of the electrostatic difference of potentials at the points of contact of different metals.

By employing the thermo-electric pile instead of the thermometers used by Thomson, Le Roux has lately measured the amount of the specific heat of electricity in various metals, and has shown that it is very small, or altogether absent, in lead. Strangely enough, though he has verified Thomson's results, he does not wholly accept the theoretical reasoning which led to their prediction and discovery.

One of Thomson's happiest suggestions connected with this subject is the construction of what he calls a thermo-electric diagram. In its earliest form this consisted merely of parallel columns, each containing the names of a number of metals arranged in their proper thermo-electric order for some particular temperature. Lines drawn connecting the positions of the name of any one metal in these successive columns indicate how it changes its place among the other metals as the temperature is raised. Thomson points out clearly what should be aimed at in perfecting the diagram, but he left it merely as a preliminary sketch. The importance of the idea, however, is very great; for, as we shall see, the diagram when carefully constructed gives us not merely the relative positions of the metals at various temperatures, with the temperatures of their neutral points, but also gives graphic representations of the specific heat of electricity in each metal in terms of the temperature, the amount of the Peltier effect, and the electromotive force (and its direction) for a circuit of any two metals with given temperatures of the junctions. In short, the study of the whole subject may be reduced to the careful drawing by experiment of the thermo-electric diagram, and the verification of Thomson's thermo-dynamic theory will then be effected by a direct determination either of Peltier effects or of specific heat of electricity at various temperatures, and their comparison with the corresponding indications of the diagram.

The diagram is constructed so that abscissæ represent absolute temperatures, and the difference of the ordinates of the lines for any two metals at a given temperature is the electromotive force of a circuit of these metals, one of the junctions being half a degree above, the other half a degree below, the given temperature.

It will be seen by what follows that nothing but direct measurement of the value of the specific heat of electricity at various temperatures can give us the actual form of the line representing any particular metal; but if the line for any one metal be assumed, those of all others follow from it by the process of differences of ordinates just described. So that it is well to begin by assuming the axis of abscissæ as the line for a particular metal (say lead, in consequence of Le Roux's result); and if, at any future time, this should be found to require change, a complex shearing motion of the diagram parallel to the axis of ordinates will put all the lines simultaneously into their proper form.

Thomson's theoretical investigation may be put in a very simple form as follows:—Let us suppose an arrangement of two metallic wires, one end of each of which is heated, their cold ends being united, and in which the circuit can be closed by a sliding piece or ring, always so placed as to join points of the two metals which are at the same temperature t. Let E be the electromotive force in the circuit, Π the Peltier effect, and σ_1, σ_2 the specific heats of electricity in the two metals. Then, if the sliding piece be moved from points at temperature t to others at $t + \delta t$, the first law of thermodynamics gives by inspection the equation

$$\delta E = J\,(\delta\Pi + \overline{\sigma_1 - \sigma_2}\,\delta t),$$

and the second law gives

$$0 = \delta\left(\frac{\Pi}{t}\right) + \frac{\sigma_1 - \sigma_2}{t}\,\delta t.$$

These equations show at once that, if there were no electric convection of heat, or if it were of equal amount in the two metals, the Peltier effect would always be proportional to the absolute temperature; and the electromotive force would be proportional to the difference of temperatures of the junctions; so that there could not be a neutral point in any case. In fact, the lines in the diagram for all metals would be parallel: and, on the former of the two hypotheses, parallel to the axis of abscissæ.

Eliminating $\sigma_1 - \sigma_2$ between the equations, we have

$$\delta E = J\,\frac{\Pi}{t}\,\delta t.$$

Now, by the construction of the diagram, $\frac{dE}{dt}$ is the difference of the ordinates of the lines for the two metals at temperature t. Hence, *whatever be the form of the lines for two metals*, the Peltier effect at a junction at temperature t is always proportional to the area of the rectangle whose base is the difference of the ordinates, and whose opposite side is part of the axis of ordinates corresponding to absolute zero of temperature. This area becomes less and less as we approach the neutral point, and changes sign (i.e., *is turned over*) after we pass it; the current being supposed to go from the same one of the two metals to the other in each case.

The electromotive force itself, being the integral of $\frac{dE}{dt}$ between the limits of temperature, is proportional to the area intercepted between the lines of the two metals, and ordinates drawn to correspond to the temperatures of the junctions respectively.

Again, the second of the preceding equations shows us that the difference of specific heats in the two metals is proportional to the absolute temperature and to the difference of the tangents of the inclinations of the lines for the metals to the axis of abscissæ. If we assume this axis to be the line of a metal in which the electric convection of heat is wholly absent, the measure of this convection in any other metal is simply the product of the absolute temperature into the tangent of inclination of its line to the axis. Thus, if the thermo-electric line for a metal be straight, electric convection is in it always proportional to the absolute temperature; and it is positive or negative according as the line goes off to infinity in the first or in the fourth quadrant. If the lines for any two metals be straight, and if one junction be kept at a constant temperature, the electromotive force will be a parabolic function of the temperature of the other junction—the vertex of the parabola being at the temperature of the neutral point of the two metals, and its axis being parallel to the axis of ordinates.

For the benefit of such of my audience as are not familiar with mathematical terms, I may give an illustration which is numerically exact. Let time stand for temperature, years corresponding say to degrees. Let the ordinate of one of the metals represent a man's income, that of the other his expenditure. The difference of these ordinates represents the rate of increase of his capital or accumulated savings, which here stands for electromotive force. As long as income exceeds expenditure, the capital increases; when income and expenditure are equal (*i.e.*) at a "neutral point," capital remains stationary, indicating, in this case, a maximum value; for in succeding years expenditure exceeds income, and capital is drawn upon. P. G. TAIT

(*To be continued.*)

ON THE SPECTROSCOPE AND ITS APPLICATIONS

IX.

NOW let me state to you how the discovery mentioned on p. 12 was finally established by Kirchhoff. In my notice of the spectroscope in the earlier articles, I had so much to say that there were several details it was absolutely essential I should curtail. One of these details was the scale by which the positions of the different bright or dark lines which are

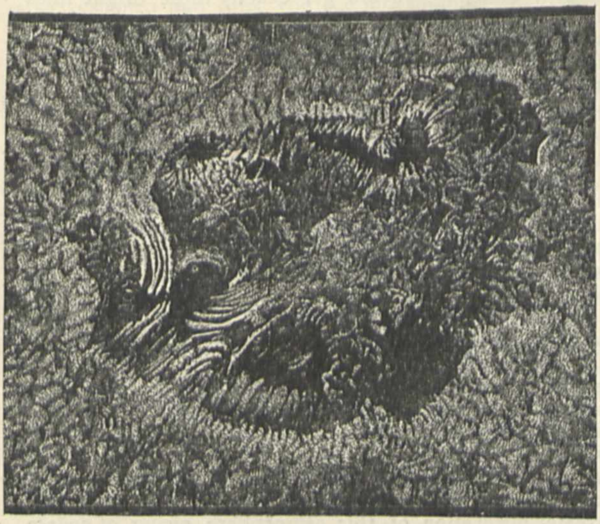

FIG. 50.—A sun-spot (Secchi), showing the "straws" in the penumbra, and the irregular masses on the general surface.

seen in the different spectra are registered, so that we may say that such a line occupies such and such a position, and such another line occupies such another position, with regard to something else. When Kirchhoff and Bunsen, two German chemists, were engaged in mapping the spectra of the elements—a research which at its commencement had nothing whatever to do with the sun—they came across this difficulty of a scale. How could they get a good scale? I have already referred to some very obvious arrangements that might determine the actual position; for instance, the observing telescope may be made to move along a graduated arc, so that by moving the telescope for the different rays and fixing it when in a proper position to see a particular ray, you might read off the index placed on the arc to a great nicety by means of a graduated *vernier* working on the

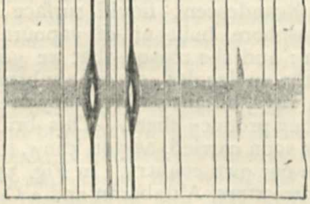

FIG. 51.—Spectrum of sun-spot. (Young.)

curve of the arc; or you may, by a modification of the instrument, use a reduced photographic picture of a scale, so that the thing to be measured and the actual scale would appear in the field of view at the same time. Kirchhoff and Bunsen tried these methods, but they did not like them. Then it suddenly struck them that, as they made their experiments in the day-time, they might use as a scale the black lines in the solar spectrum, which had not been known to change since the time of Wollaston, who discovered them. When working in the day-time, they had thus the solar spectrum visible in one half of the field of view of the telescope, which was easily managed by placing a reflecting prism over one half of the slit, as is shown in the enlarged slit in Fig. 46, so as to light one half of the slit by the sun, and the other half by whatever substance was under examination. With this arrangement they set to work with infinite care, and made a map of the solar spectrum. Such was their pro-

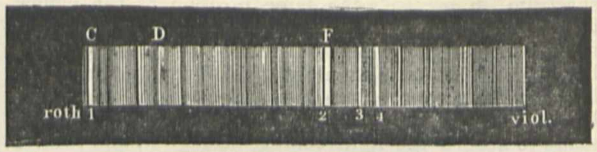

FIG. 52.—Spectrum of τ Coronæ. (Huggins.)

posal: first to map the unchangeable solar spectrum, and then, having this unchangeable scale, about which there could be no mistake, always visible, they would be able to refer to the dark lines in it all the unknown phenomena they were about to investigate in the bright lines of different vapours and gases. Having got this idea of the scale well into their minds, they were exceedingly anxious to test this question, which, as I have told you, was raised by Fraunhofer and many other men before them, of the asserted coincidence of the bright sodium line with the dark solar sodium lines; with a very delicate instrument, Prof. Kirchhoff made the following remarkable experiment:—"In order," says Kirchhoff, for these are his own words, "to test in the most direct manner possible the frequently asserted fact of the coincidence of the sodium lines with the lines D"—(that is to say, of the bright double lines of sodium in the yellow part of the spectrum, with the double line D of the solar spectrum)—"I obtained a tolerably bright solar spectrum, and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones." That is to say, in the spectrum of the sodium which was burning in the flame were lines so exactly coincident with the two dark lines in the solar spectrum, that the bright lines of the sodium spectrum put these dark lines out altogether, so that they seemed to vanish, as it were, from the solar spectrum. He goes on:—"In order to find out the extent to which the intensity of the solar spectrum could be reduced without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine

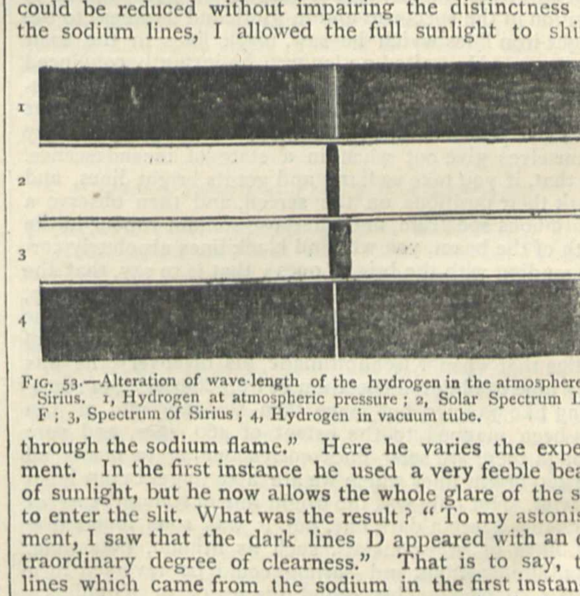

FIG. 53.—Alteration of wave-length of the hydrogen in the atmosphere of Sirius. 1, Hydrogen at atmospheric pressure; 2, Solar Spectrum Line F; 3, Spectrum of Sirius; 4, Hydrogen in vacuum tube.

through the sodium flame." Here he varies the experiment. In the first instance he used a very feeble beam of sunlight, but he now allows the whole glare of the sun to enter the slit. What was the result? "To my astonishment, I saw that the dark lines D appeared with an extraordinary degree of clearness." That is to say, the lines which came from the sodium in the first instance, were sufficiently bright to entirely eradicate the dark lines from the solar spectrum, but the two lines D were now so utterly powerless compared with the light of the sun, that they actually appeared as black lines, and coincident with the two lines D in the solar spectrum.

We have seen that the bright line due to the radiation from sodium vapour can be very easily obtained by placing some sodium in a colourless gas flame, but if we now pass the continuous light coming from the carbon points of an electric light, or from the oxyhydrogen lime-light, through this same sodium flame, the result will be that we obtain a black absorption line on a continuous spectrum, in precisely the same position as the yellow line was originally. This is Kirchhoft's crucial experiment, which at once determined not only that the dark line in the sun was absolutely coincident with the bright line of sodium vapour, but that, under certain conditions, bright, incandescent sodium vapour could actually be made to absorb the light coming through it, and reverse its own spectrum. Kirchhoff goes on:—"I then exchanged the sunlight for the Drummond or oxyhydrogen lime-light, which, like that of all incandescent solid and liquid bodies, gives a spectrum containing no dark lines." When this light was allowed to fall through a suitable flame, coloured by common salt (or chloride of sodium), dark lines were seen in the spectrum in the position of the sodium lines." You may imagine that this conclusive experiment—perhaps the most wonderful experiment that has been made during the century—gave Kirchhoff food for thought, and at once his genius travelled to a possible explanation of this strange fact he had observed; a fact, as you know, entirely in accordance with the previsions of Prof. Stokes, Dr. Balfour Stewart, and Foucalt. Kirchhoff said to himself, "I have now got the bright lines in the spectrum of the vapour of sodium coincident with the two dark lines in the solar spectrum. What does it mean?" And again the philosopher was not at fault. He said to himself—it is almost possible to see the train of his reasoning in his memoirs—"Sodium has a most simple spectrum; suppose I take the most complicated spectrum I can find." He took for this purpose the spectrum of iron, which I think you will acknowledge to be one of sufficient complication, for the spectrum is traversed by lines throughout its whole length, and I may tell you at once that no less than 460 lines have been already mapped, and their positions are now thoroughly well known to us—as well known as the position of any star in the heavens. Kirchhoff tried the iron spectrum, and he found, absolutely corresponding in position in the spectrum and in width and darkness to the bright iron lines which he saw, black lines in the solar spectrum. He waited no longer; he instantly convinced himself, and soon convinced the world, that he had discovered this very remarkable fact, that gases and vapours have the power of absorbing those very rays which they themselves give out when in a state of incandescence. So that, if you take sodium, and get its bright lines, and mark their positions on the screen, and then observe a continuous spectrum, and interpose sodium vapour in the path of the beam, you will find black lines absolutely corresponding with the bright ones; that is to say, that the sodium vapour has the faculty of entirely eating up, absorbing, or stopping that light which would otherwise go on to the screen. In the case of iron, it is worthy of notice that when Kirchhoff made his discovery, he was only able to obtain a spectrum of iron consisting of something like 90 lines, but since then the spectrum of iron has been mapped to the extent of 460 lines, and sure enough there are solar lines corresponding to nearly all the 460 bright lines which we are able to get in our laboratories. Not only was the bright line of sodium *reversed* or changed into a dark one, but it was soon found that the lines of other metals, such as lithium, potassium, strontium, calcium, and barium, could be reversed in a similar manner. This grand discovery of Kirchhoff's met with immediate acceptance, and with it you see at once the explanation of the wonderful black lines discovered by Wollaston, about which I said something in my first lecture. The riddle of the sun was read to a certain extent, and Kirchhoff read it in this way. He said:—"There is a solid or a liquid something in the sun, giving a continuous spectrum, and around this there are vapours of sodium, of iron, of calcium, of chromium, of barium, of magnesium, of nickel, of copper, of cobalt, and aluminium; all those are existing in an atmosphere, and are stopping out the sun's light. If the sun were not there, and if these things were observed in an incandescent state, we should get exactly these bright lines from them." Later researches by many distinguished physicists have shown that the following terrestrial elements are present in the vaporous condition round the sun:—

1. Sodium.	6. Chromium.	11. Cobalt.
2. Calcium.	7. Nickel.	12. Hydrogen.
3. Barium.	8. Copper.	13. Manganese.
4. Magnesium.	9. Zinc.	14. Aluminium.
5. Iron.	10. Cadmium.	15. Titanium.

Kirchhoff further imagined that he had reason to believe that the visible sun, the sun which we see—and we may take the sun as an example of every star in the heavens—was liquid.

In the sun we have, first, a bright, shining orb, dimmed to a certain degree at the edge; and here and there, over the sun, we see what are called spots. Kirchhoff wished, not only to connect his discoveries with the solar atmosphere, but was anxious to connect them with this dimming near the limb and the spots. He said that the solar atmosphere, to which all the absorption lines were due, extended far outside the sun, and formed the corona; and that this dimming of the limb was really due to the greater absorption of this atmosphere, owing, of course, to the light of the sun travelling through a much greater length at the limb than at the centre of the disc. Furthermore, he said that the sun-spots, which astronomers, from the time of Wilson, had asserted to be cavities, were nothing but clouds floating in this atmosphere of vapour. Such was the very bold hypothesis put forward by Kirchhoff—an hypothesis which you see at once explains these strange observations from Wollaston upwards, including Fraunhofer's observation of the spectrum of the sun and stars, and the brilliant ideas of Prof. Stokes, Dr. Balfour Stewart, and others in other lands. A little simple experiment, made by means of a little sodium vapour and a beam of sunlight, with the powerful aid of a little prism, gave us this tremendous knowledge about distant worlds so immeasurably remote that it seemed absurd for men to try and grapple with any of the difficulties that are presented to us. Such, then, is Kirchhoff's theory of the sun, which I hope I have been able to make clear to you. There is a something—Kirchhoff said it was a liquid—which gives us a continuous spectrum, and between our eye and that incandescent liquid surface there is an enormous atmosphere, built up of vapours of sodium, iron, and so on; and the reason that we get these dark lines is, that the molecules of the substances named absorb certain rays, because when they are in an incandescent state they produce them. This brilliant idea of Kirchhoff's was soon carried, as you know, to the stars by Mr. Huggins in our own country. In Fig. 34 will be seen the spectra of two stars, Aldebaran and *a* Orionis (Betelgeux), which are so distant that it is absolutely impossible to measure their distance from us. We know a great deal about our own sun, but these suns are so lost in the depths of space that it is quite impossible that we can get anything like a correct knowledge of their size, or know much of their belongings. By means of the prism, however, we learn in a moment a great deal. In the first star we get three lines, due to the absorption of magnesium vapour, as we get them in the sun. We know, therefore, that magnesium vapour is present in the atmosphere around that sun (Aldebaran) in exactly the same way as round our own. We also get some of the iron lines, the lines of sodium, and the lines of hydrogen, calcium, and a few other elements—nine in all. At the base of the diagram you see indications of the elements, with the bright lines of which Mr. Huggins has compared the black lines which you see in the spectrum of these heavenly bodies. By means of the star spectroscope and of the induction coil, Mr. Huggins tested these lines, as Kirchhoff did in the case of the sun, by actually getting the vapour of magnesium visible at the same time in the spectroscope: and thus you see in a moment that there is no difficulty at all in determining their coincidence, you have the two things brought so closely side by side. If I had time I might remark on the presence of some elements here and the absence of others; but there is one remarkable fact about this lower star (α Orionis) which I must mention. As far as its spectrum goes, it appears that the gas hydrogen, which is a very important element in our sun's atmosphere, as we gather from the great distinctness of the hydrogen lines in the solar spectrum—and not only in our sun, but in a great many others—is absolutely absent, whilst magnesium, sodium, calcium, &c., are present.

So far, then, you see that this little prism has enabled us to read a great many secrets of the sun and of the more distant stars; and we must acknowledge that Stokes' and Kirchhoff's hypothesis is a very magnificent one, and we can but wish that there were more men like them, who, undismayed by the failure of those who, for nearly a century before their time, had been endeavouring to unravel these secrets, were still prepared to go on, and endeavour to find them out by means of a prism and a simple sodium flame.

Now, astronomers—who, as I told you, from the time of Wilson had imagined that the sunspots were cavities—very soon began to quarrel with this hypothesis of Kirchhoff's, who said that the sunspots, instead of being cavities, were really clouds floating in the atmosphere. They remarked, and I think with truth, that to make such an assertion was altogether opposed to the evidence of the telescope. And I think I may say that the astronomers have now carried the day, for another line of independent research altogether—I mean the researches into the constitution of the sun by means of the spectroscope—has come to the aid of the astronomers, and it looks very much as if we must still hold to the opinion that Wilson in his observations, now more than a century old, was perfectly right, and that Kirchhoff's analysis, as far as it deals with the sun-spots, is susceptible of improvement. In the remarks I made in my former lecture on radiation in connection with the red prominences visible during eclipses, I drew your attention particularly to the hydrogen lines, and told you that the red flames are, for the most part, composed of hydrogen. There the prism comes to our aid in a very remarkable way indeed. It is clear to you, I think, after what I have said about absorption, that the darkening of the sun's surface, which we call a spot, is really a thing about which the prism can tell us a great deal. For instance, take a sun-spot, in which the usual brilliancy of the sun in the other parts of its disc is altogether wanting. There is not only great darkness here and there, but wonderful turnings and twistings and bendings of this solar envelope, which I have already told you Kirchhoff asserts to be a liquid one, but which I think a little consideration of Fig. 50 will show you is more probably gaseous, or cloudy, than liquid. It is obvious, I say, in this case that there was a great probability of the spectroscope being able to tell us something about this absence of light, for an absence of light means one of two things; it means either that there was a defect in radiation, or that there was some excess of absorption, and I may say that this difference—which I hope you now all thoroughly understand—really formed the battle-ground between the English and French astronomers until a few years ago. Long after Kirchhoff's experiment, M. Faye, a distinguished member of the Institute of France, went all over the work again, and declared that the sun-spot was dark, because we there got the light, not from the brightly shining envelope, but from some feebly radiating gas inside the sun; that the sun was a gigantic bubble, the bubble being nothing else than the photosphere—the liquid sphere of Kirchhoff—the interior being composed of gas, glowing at such an enormous temperature that the light we got from it was extremely feeble. You will see in a moment that, if the sun-spot were really due to the radiation from gas, we should get from that sun-spot a selective spectrum, that is to say, a spectrum with bright lines. The English astronomers said: "No; a sun-spot is not due to defective radiation at all; there is something over the bright portion of the sun which eats away the light:" whether the light was eaten away generally—whether, in fact, we had an instance of general or selective absorption—was not stated, but what they did distinctly state was, that the sun-spot was simply an indication of absorption. So that, you see, here was a thing which a spectroscope might settle almost at once, provided always that a good sunspot could be obtained for the experiment. This was done in 1866. Fig. 51 gives an idea of what is seen when we observe a small sunspot, and it is one which is full of meaning. Here is a very clear image of the solar spectrum near the double line D, and also the double D itself. If it were possible to have given you the whole of the sun's spectrum on the same scale as this, it would require an engraving yards in length, but it would be almost impossible to make my meaning clearer than I hope I can do by this small portion; and I must therefore ask you to take for granted that the dark line which you see running along this yellow portion of the spectrum would really run along the whole length of the spectrum, from the extreme red to the extreme violet. This, then, you see in a moment, was an indication of general absorption; that is to say, in the way in which the light is affected by its passage through the prism, we have the problem settled in an instant, that a sunspot is due to general absorption at all events. Further, in observing the spectra of different sunspots, it was found that the spectrum of the middle of the sunspot is much darker than the outside. So that you see this simple experiment tells us not only that the sunspot is due to general absorption, but that there is more general absorption in the middle of the spot than at its edge. This is the way in which this little prism is able to deal with these great problems.

J. NORMAN LOCKYER

(*To be continued.*)

MIND IN THE LOWER ANIMALS

I RECENTLY received a letter from Mons. J. C. Houzeau, the author of the "Études sur les Facultés Mentales des Animaux comparées a celles de l'Homme," published at Mons, Belgium, in 1872, and reviewed by Mr. Wallace in NATURE of October 10, 1872. The latter eminent writer asserts that M. Houzeau's work "contains a mass of curious facts, acute observations, and sound reasoning, which fully entitle its author to take high rank among philosophical naturalists" (p. 471). I quite agree with him in his estimate of M. Houzeau's labours, being disposed to place his two volumes of "Études" on a par with the works of Mr. Darwin; and with another work, which, while little, if at all, known in this country, deserves, nevertheless, the highest consideration at the hands of all interested in comparative psychology—the "Traité de la Folie des Animaux de ses Rapports avec celle de l'Homme," by Dr. Pierquin, published in Paris (in 2 vols.), so long ago as 1839.

I need not say that any suggestions coming from an observer of such experience as M. Houzeau deserve the attention of the now many earnest students of the subject of "Mind in the Lower Animals;" and I therefore make no apology for bringing under the notice of your readers certain remarks contained in the letter aforesaid.

In the first place, M. Houzeau begs to direct attention to "the high importance of sparing—at least for observation—what remains of anthropoid animals in Asia and Africa. It is my deep regret that there are none in the country where I live" (Jamaica); "and that I am thereby deprived of an opportunity to study them. They should be tamed, domesticated, and studied in their own climate—at home. The gorilla, for instance, should be perpetuated in Guinea in domesticity. As I stated in my book, it does not appear impossible that apes might learn to talk. Should the attempt succeed even partially, what would not be the bearing and importance of it physiologically and historically? Could not some means of study be devised in the English colonies? To save the Anthropoids from destruction, and to promote the study of their mental capacity, is worthy surely of the earnest exertions of naturalists."

I quite concur with him as to the desirability of educating by domestication—so far as possible, and studying the results of such education in the anthropoid apes, and indeed the whole group of the Quadrumana. We know what has been the result in the dog of centuries of association with, and training by, man; though even in that familiar animal we do not yet know the extent of his capabilities, because training in certain directions has scarcely been attempted. Man has, for his own ends, directed special attention and effort to the development, in the dog, of his power of scent, swiftness, vision, courage, watchfulness, and other qualities that render him useful in the chase, as a watch-animal, as a companion, and so forth. But no similar persistent efforts have been made to cultivate, for instance, his moral sense—to produce an animal good in a moral point of view—honest, affectionate, benevolent, conscientious, in the highest degree. And yet that it is quite as possible to produce or educe moral greatness or goodness as physical swiftness or muscular strength, I am firmly persuaded. Notwithstanding all that has been said of the superior intelligence of the dog, horse, elephant, ant, and bee, I believe that were as much care bestowed on the training of the moral qualities of many monkeys or apes as is given to the instruction of the pointer or setter, the homing pigeon, piping bullfinch, or talking parrot, or to the training of the race-horse, results of a startling kind would be attained, or would be shown to be attainable. There are certain respects in which apes and monkeys approach more closely to man than do the dog or the other animals just mentioned: they possess potentialities or capabilities of which some of the almost marvellous stories told us by reputable traveller-naturalists give us but a glimpse.

I cannot, however, discuss that or other subjects in comparative psychology here, hoping, as I do, to have fuller and more fitting opportunity in a forthcoming volume of the "International Scientific Series" of Messrs. H. S. King and Co.

M. Houzeau expresses surprise that, at the present day, the belief should be almost universal that, while all races and conditions of man have souls, the best of other animals have none. This is obviously a matter of pure speculation, which I must not now discuss. But I may direct the attention of your readers to a curious book published in Aberdeen in 1824, by Peter Buchan, entitled, "Scriptural and Philosophical Arguments or Cogent Proofs from Reason and Revelation that Brutes have Souls, and that their Souls are Immortal." The work in question is, however, now so rare, that it may be difficult to obtain even a perusal of it. The reader of German literature may also refer to a book on the same subject by Schmarda, to which my attention was called some time ago by the late Professor Day, of St. Andrews.

M. Houzeau animadverts on the anomaly that the persons, from whom we should expect the most valuable evidence regarding the mental acquirements or capacities of the lower animals—those who are habitually and intimately associated with them—drovers and drivers, horsemen and huntsmen, shepherds and sportsmen, jockeys and grooms, butchers, and even veterinarians, are those, on the contrary, in whom we too frequently meet with the strangest ignorance or prejudice. They would seem to be, as a rule, incapable of honestly observing and of making logical inferences from facts observed; instead of using their own eyes and reason, they permit themselves to be blinded and befooled by obsolete tradition or fable.

Notwithstanding the perfectly overwhelming bulk and variety of the literature of comparative psychology—or at least of the data on which it may be founded, there are many points in the mental history of the lower animals that require and admit of elucidation by *observation and experiment*. If any person of ordinary intelligence—either abroad or at home—feels inclined to plead, as an excuse from contributing to the progress of comparative psychology, the want of proper opportunity, I would commend to his consideration the example of M. Houzeau as a noble one of the successful "pursuit of knowledge under difficulties." He modestly describes himself as a traveller-naturalist: and in the letter above referred to thus refers to the circumstances under which he collected the materials for the two bulky volumes of *Études*, that constitute one of the most important contributions yet made to the science of comparative psychology. "It was rather occasionally that my attention was called to the subject of the 'Mental faculties of animals,' having been almost exclusively engaged, previous to my sojourn in America, in astronomical and geographical pursuits. Still the subject was pressed upon me when, in the wildernesses of Texas and Northern Mexico, I had to live in the open air, in the constant company of domestic animals and in close proximity to wild ones; far away," as he says, "from the European field of labour and even from intellectual resources," in a foreign wild land, without the means of literary or scientific reference. Under circumstances, in a word, most unfavourable to such a publication, he has nevertheless produced a work that would do honour to any of our own *savans*, with all the appliances of our large cities, large societies, and large libraries at their command.

W. LAUDER LINDSAY

NOTES

FREE admission to the lectures and courses of practical instruction in Chemistry, Physics, Mechanics, and Biology at South Kensington will be granted to a limited number of Teachers and Students of Science Classes under the Science and Art Department, who intend to become Science Teachers. The selected candidates will also receive a travelling allowance, and a maintenance allowance of *1l. 1s.* per week, while required to be present in London. The course in Chemistry will commence in October, and end in the following June. The course in Biology will commence in October and close in February or March. The course in Physics will commence about February and close in June. The course in Mechanics will probably commence about February and close in June. Students are required to attend from 9 or 10 A.M. to 4 or 5 P.M. daily, in addition to the time required in the evening for writing up their notes, &c. Candidates for these Studentships must send in their applications on Science Form No. 400, copies of which may be obtained on application to the Secretary of the Science and Art Department. For the courses in Biology and in Mechanics some power of drawing is essential, and no candidate will be admitted who cannot show that he has acquired sufficient power.

THE following courses of instruction of Science Teachers in connection with the Science and Art Department will probably be organised this summer:—1. Chemistry, Inorganic, 2. Chemistry, Organic, 4 weeks, commencing July 1, Prof. Frankland, F.R.S. 3. Magnetism and Electricity, 3 weeks, commencing June 24, Prof. Guthrie, F.R.S. 4. Heat and Light, 3 weeks, commencing July 17, Prof. Guthrie, F.R.S. 5. Botany, 4 weeks, commencing June 24, Prof. Thiselton Dyer. 6. Mechanics, 4 weeks, commencing June 25, Prof. Goodeve. 7. Geometrical Drawing, 3 weeks, commencing June 26, Prof. Bradley. Before definite arrangements can be made, however, it is necessary to know how many Teachers can and will take advantage of the courses; and therefore all Teachers who wish to attend are required to fill up and return a form (Science Form, No. 500), which may be obtained by application to South Kensington. If more Teachers apply to attend than can be accommodated at any course, those will be selected who have passed the highest examinations—in which the result of the present May Examination will be counted—and have had the most successful classes. The Teachers who are selected, and who attend one or more of the courses, will receive 2nd class railway fare and 30*s*. a week while in London.

IN connection with St. John's College, Cambridge, there will be offered for competition, in December 1873, an Exhibition of 50*l*. per annum for proficiency in Natural Science, the Exhibition to be tenable for three years in case the Exhibitioner have passed within two years the previous examination as required for candidates for honours; otherwise the Exhibition to cease at the end of two years. Candidates will have a special examination in (1) Chemistry, including practical work in the laboratory; (2) Physics (viz., Electricity, Heat, Light); (3) Physiology. They will also have the opportunity of being examined in one or more of the following subjects:—(4) Geology; (5) Anatomy; (6) Botany; provided that they give notice of the subjects in which they wish to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the Master and Seniors that excellence in some single department should be specially regarded by the candidates. They may also, if they think fit, offer themselves for examination in any of the classical or mathematical subjects. Candidates must send their names to one of the tutors fourteen days before the commencement of the examination. The tutors are the Rev. S. Parkinson, D.D., Rev. T. G. Bonney, B.D., and J. E. Sandys, Esq., M.A.

FROM Prof. E. D. Cope we have received the description of two apparently new fossil mammalian forms from the Eocene of Wyoming, which he places among the Carnivora. *Mesonyx obtusidens* forms, according to the author, a distinct family of the fissiped Carnivora, most closely related to the Canidæ, with weakly sectorial teeth, four of them being true molars (a marsupial character), and short, flattened, ungual phalanges in which there are no indications of collars for the reception of the nails themselves. *Synoplotherium lanius* may be a Carnivore, but the claws were flat, and the scaphoid of the carpus did not anchylose with the lunare, which shows that it belongs to a more generalised type. It must be remembered that Prof. Marsh has described very similar forms from the same strata.

MESSRS. WILLIAMS AND NORGATE have just issued the prospectus of a unique and most elaborate work by Mr. Herbert Spencer, consisting to a large extent of the tabulated material which he has accumulated for his "Principles of Sociology." In preparation for the latter work, requiring as bases of induction large accumulations of data, fitly arranged for comparison, Mr. Herbert Spencer, some five years ago, commenced, the collection and organisation of facts presented by societies of different types, past and present. Though this classified compilation of materials was entered upon slowly to facilitate his own work, yet, after having brought the mode of classification to a satisfactory form, and after having had some of the tables filled up, the results appeared likely to be of such value that Mr. Spencer decided to have the undertaking executed with a view to publication: the facts collected and arranged for easy reference and convenient study of their relations, being so presented, apart from hypotheses, as to aid all students of Social Science in testing such conclusions as they have drawn and in drawing others. The work consists of three large divisions. Each comprises a set of tables exhibiting the facts as abstracted and classified, and a mass of quotations and abridged extracts, otherwise classified, on which the statements contained in the tables are based. The condensed statements, arranged after a uniform manner, give at one view, in each table or succession of tables, the phenomena of all orders which each society presents—constitute an account of its morphology, its physiology, and (if a society having a known history) its development. On the other hand, the collected extracts, serving as authorities for the statements in the tables, are (or rather will be, when the work is complete) classified primarily according to the kinds of phenomena to which they refer, and secondarily according to the societies exhibiting these phenomena; so that each kind of phemonenon, as it is displayed in all societies, may be separately studied with convenience. The three divisions, each thus constituted, comprehend three groups of societies:—(1) *Uncivilised Societies*; (2) *Civilised Societies—Extinct or Decayed*; (3) *Civilised Societies—Recent or still Flourishing*. Several sample tables have been sent us, and as a specimen of the classificatory headings under which the immense array of facts are grouped, we shall give those belonging to Table IX. of Division I. ("Uncivilised Races"), the Sandwich Islanders, one of the Malayo-Polynesian Races. First are given their Inorganic Environment (Climate, Surface); Organic Environment (Vegetal, Animal); Sociological Environment (adjacent tribes), Physical, Emotional, and Intellectual Characters. Then follow the tables, divided into Structural and Functional, each of which is subdivided into Operative and Regulative. The Structural Operative is again subdivided into Operative and Regulative; the Structural Regulative is subdivided into Political (*Civil*, [Domestic, (Marital, Filial), Public], *Military*), Ecclesiastical, and Ceremonial (*Mutilations*, *Funeral Rites*, *Laws of Intercourse*, *Habits and Customs*). Under Functional, the Regulative is subdivided into Sentiments (*Æsthetic*, *Moral*), Ideas (*Superstitions*, *Knowledge*), and Language; the Operative into Processes (*Distribution*, *Exchange*, *Production*, *Arts*, *Rearing*, &c.), and Products (*Land-Works*, *Habitations*, &c., *Food*, *Clothing*, *Implements*, *Weapons*, *Æsthetic Products*). Under each final subdivision ample details are given. The value of such a work to all students of sociology, and of mankind generally, will be inestimable.

SIR JOSIAH MASON, who has already built and endowed an orphanage at Erdington, near Birmingham, at a cost of more than a quarter of a million, has now arranged to erect and endow a Scientific College in Birmingham, for which will probably be expended at least an equal amount. The *Times* gives the following details:—During his long experience as a manufacturer, Mr. Mason became deeply convinced of the want of and necessi y for "thorough systematic scientific instruction, specially adapted to the practical, mechanical, and artistic requirements" of the Midland district, and to this want he has determined to devote a portion of his remaining property to supply. The institution is to be called "Josiah Mason's College," or "Josiah Mason's College for the Study of Practical Science." Regular systematic instruction is to be given in mathematics, abstract and applied physics, both mathematical and experimental; chemistry, theoretical, practical and applied; the natural sciences, especially geology and mineralogy, with their application to mines and metallurgy; botany, and zoology, with special application to manufactures; and physiology, with special reference to the laws of health. The English, French, and German languages will also be taught. The trustees have power to include mechanics and architecture and all other subjects necessary to carry out the objects of the founder. Mere literary education and instruction are excluded, as well as all teaching of theology and subjects purely theological. No principal, professor, teacher, or other officer of the college is ever to be called upon to make any "declaration as to or submit to any test whatever of their religious or theological opinions," nor are these in any wise to be considered either as qualifications or disqualifications for holding any office, fitness to give the instruction required being the sole and only test. Provision is also made for giving lectures and opening classes for popular or unsystematic instruction, at which the attendance shall be open to all persons, "without distinction of age, class, creed, race, or sex." The founder's object being to promote the prosperity of the manufactures and industry of the country, especially of the two towns so frequently named, the college will be open to qualified persons of all classes who have to rely on science, art, or manufactures for a livelihood, "especially the more intelligent youth of the middle class." Provision is also made, when the funds permit it, to provide instruction for females as well as males. The site selected for the college is in the centre of the town, and the land is therefore of the greatest value, and the generous founder has already laid out upwards of 20,000*l*. on the site. He has also conveyed landed property producing about 600*l*. a year, and there is a clause in the deed in which he states it to be his intention to devote by his will additional funds for the use of the college. The total amount of this noble endowment cannot, therefore, be positively stated, as it will, of course, depend upon circumstances. Enough, however, has already been done to render the "Josiah Mason College" one of the most princely gifts yet made to posterity in England by any of her wealthy sons.

THE forthcoming number of Petermann's *Mittheilungen* will contain an interesting article compiled from the Australian papers, giving an account of a three months' journey during August, September, and October of last year into the interior of Australia, by Mr. Ernest Giles, accompanied by Messrs. Carmichael and Robinson. They struck off from the route of the overland telegraph at Chambers's Pillar, about 133° 55′ E. long., and 24° 53′ S. lat., and journeyed in a north-west direction along Finke Creek, traversing ground which has not hitherto been explored. They passed among long ranges of hills, lying in an east and west direction, and varying in height from a few hundreds to 4,000 ft., though few of the heights are apparently above 1,000 ft. At about the 24th parallel, in 133° N., they came upon multitudes of magnificent fan palms growing along the bed of the creek; they named the place the "Glen of Palms." Their journey in this direction extended to 129° 55′ W., and about 23° 10′ S., the utter sterility of the region and the want of water compelling them to turn back. It was only during the last few days, however, of their western journey that water became scarce. The most characteristic vegetation throughout was Spinifex; Casuarina was also of frequent occurrence. Travelling for about 100 miles in a southern direction, the explorers came upon an extensive salt marsh, apparently from Petermann's map upwards of 100 miles long and from 6 to 7 miles broad; Baron von Müller has named this Amadeus Lake. After staying here for a few days, Giles and his companions struck northwards for about 40 miles, and then south-eastwards, passing numerous creeks and a range of hills, "Gill Range," and meeting the Finke again on November 16, not far from their starting-point. Altogether these plucky explorers travelled 1,300 English miles, and have added considerably to our knowledge of the interior of Australia.

UNDER the name of "Herbarium Mycologicum Œconomicum," F. Baron Thümen proposes to form a collection of those parasitic fungi which are injurious (including, also, any that are useful) in forestry, agriculture, horticulture, or in any other branch of industry. The specimens of each species will be labelled with the scientific name, diagnosis, and any needful remarks, and, where possible, will be sufficiently numerous for a portion to be submitted to microscopic examination. The collection will be issued in fasciculi of fifty species, at the price of three thalers each, and may be obtained of the collector, at Teplitz, in Bohemia.

WE regret to learn that Mr. Louis Fraser, at one time prominently connected with the Zoological Society of London, author of the "Zoologia Typica," and a professional taxidermist of high repute, is suffering from destitution, in his old age, in British Columbia. On April 7 last a communication was presented before the meeting of the Academy of Sciences of San Francisco on this subject by Mr. Henry Edwards, one of the members, and an appeal for assistance was made to the friends of science. This was answered by contributions on the part of several persons, but it is not stated to what extent.

THE anniversary meeting of the Royal Geographical Society was held on Monday, Sir Henry Rawlinson in the chair. Sir Bartle Frere was elected President, and the Earl of Derby, Sir H. Rawlinson, Sir R. Alcock, and Admiral Richards, vice-presidents. The retiring president, in his valedictory address, reviewed at some length the progress of scientific exploration during the past year.

AT the special request of Rear-Admiral Sands, the U.S Congress, at its last session, allowed an appropriation for the purpose of completing and publishing the catalogue of southern stars, observed by Gilliss in 1850–52, and the work is now being put in the hands of computers for publication as soon as possible.

A SLIGHT shock of earthquake was felt on the morning of April 14, at Goalparah, Assam.

ADDITIONS to the Brighton Aquarium during the past week: Smooth Hound (*Mustelus vulgaris*); Skate (*Raja batis*); Gurnards (*Trigla lyra*); John Dorée (*Zeus faber*); Scad, or Horse Mackerel (*Trachurus trachurus*); Lump-fish (*Cyclopterus lumpus*); Turbot (*Rhombus maximus*); Common Carp (*Cyprinus carpio*); Gold and Silver ditto (*Carassius auratus*); Tench (*Tinca vulgaris*); Herrings (*Clupea harengus*); Sharp-nosed Eels (*Anguilla vulgaris*); Sand-launce (*Ammodytes lancea*); Garfish (*Belone vulgaris*); Zoophytes, *Actinoloba dianthus*, *Tubularia indivisa*, *Sertularia cupressina*, *Obelia geniculata*, *Pleurobrachia pileus*.

THE additions to the Zoological Society's Gardens during the past week include two Cretan Ibexes (*Capra picta*), presented by Mr. T. B. Sandwith; a Macaque Monkey (*Macacus cynomolgus*); a Rhesus Monkey (*M. erithraeus*) from India, and a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. H. N. Hewett; a dark-green Snake (*Zamenis atrovirens*). and a four-lined Snake (*Coluber quadrilineatus*) from Malta, presented by Mr. C. A. Wright; a pig-tailed Monkey (*Macacus nemestrinus*) from Java; a Malabar Parrakeet (*Palaeornis columboides*) from South India; an olive Weaver Bird (*Hyphantornis capensis*), from South Africa, purchased; a Brazilian Tapir (young) (*Tapirus terrestris*) from South America; a Harpy Eagle (*Thrasaëtus harpyia*) from South America, deposited; four variegated Sheldrakes (*Tadorna variegata*), and four ruddy Sheldrakes (*T. rutila*) hatched in the Gardens.

SCIENTIFIC SERIALS

Poggendorff's Annalen der Physik und der Chemie. No. 3, 1873.—This number commences with a paper by Dr. Oudemans, jun., on the influence of optically inactive solvents on the rotatory power of optically active substances. The author, employing a Wild polaristrobometer and lime-light, experimented with cane-sugar, cinchonin, brucin, phlorizin, and other substances, with water, chloroform, alcohol, ether, &c., as solvents. He unexpectedly found that the specific rotatory power of cinchonin in various mixtures of alcohol and chloroform had not values entirely intermediate between those of cinchonin in either solvent separately (which are ↿ 212° and ↿ 228°). It rises to a maximum of over ↿ 237° in a mixture of 10 per cent. alcohol and 90 per cent. chloroform. He further compared the influence of different solvents on the specific rotatory power of active substances, with their solvent action, and he considers the greater values of the former property correspond with a greater solubility of the active substance. The numerical results are given in full.—Julius Thomsen continues his "Thermochemische Untersuchungen," examining, in this paper, the affinities of the constituents of water, of sulphuretted hydrogen, of ammonia, and of carburetted hydrogen. He finds that while there is development of heat in the formation of marsh gas, there is absorption in the formation of ethylene and acetylene, from carbon and hydrogen. The author gives a *résumé* of results from the series of researches here terminated (the affinity of hydrogen to the metalloids), which presents some points of considerable interest.—In the next paper Prof. Lubimoff of Moscow calls attention to an error current in most text-books on physics. The field of view in a Galilean telescope is stated to depend on the size of pupil of the observer's eye, and to be measurable by the angle under which this will appear from the centre of the object-glass. This, he says, gives a value five or six times smaller than the actual, which is directly dependent on the size of aperture of the object-glass. He explains and illustrates his new theory at some length.—F. Rüdorff contributes the first part of a paper on the solubility of saline mixtures, and Ed. Ketteler continues his mathematical inquiry into the influence of astronomical motions on optical phenomena.—Among the extracted papers may be specified those by Edlund on galvanic resistance, by Braun on direct photography of the solar protuberances, and by Baumhauer on hygrometry in meteorological observatories.

Der Naturforscher for April 1873, contains a large amount of varied and interesting scientific matter. In Physics and Chemistry, there are short accounts of M. Jamin's researches on condensation of magnetism, Dr. Mayer's on measurement of sound, M. Cornu's new method of determining the velocity of light, Herr Feddersen's paper on thermo-diffusion of gases, Herr Nasse's on the nitrogen in albumenoids, Clerk-Maxwell's lecture on action at a distance, &c. Herr Nasse finds that, in the albumen-molecule, one portion of the nitrogen is combined loosely, another much more intimately, and he sets himself to determine the proportion of loosely-combined to the entire nitrogen-contents, in various albuminous substances. His observations have an important physiological bearing. In biology proper, we may note a paper giving the results of Herr Stohmann's recent study on animal nutrition. This author endeavours to formulate mathematically the digestibility of food stuffs. P. Secchi's recent communication on the solar protuberances and spots is given, and there is a meteorological paper on the temperature of air in woods and in the open, describing experiments by Herr Ebermayer. We may further call attention to a note on Baranetzky's experiments on the periodicity of outflow of sap in plants, a phenomenon he finds based on the periodical action of light. Geology, geography, technology, &c., are also represented in this serial, and the weekly "Kleinere Mittheilungen" furnish a number of well-selected scientific data.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, May 14.—Mr. Joseph Prestwich, F.R.S., vice-president, in the chair.—The following communications were read.—"On the genus *Palæocoryne*, Duncan and Jenkins, and its affinities," by Prof. P. Martin Duncan, F.R.S.—In this paper the author referred to certain minute fossils from the Carboniferous rocks of Scotland, described by himself and Mr. Jenkins in a paper read before the Royal Society, as belonging to the Hydroida, and most nearly resembling the recent genus *Bimeria*, Wright. He stated that numerous specimens since received threw some further light on the nature of these fossils, and showed especially that in all probability the base is not really cellular, but that the cellular appearance is produced by the growth of the real base of the polype over the cells of the *Fenestella* on which it grows.—"Notes on Structure in the Chalk of the Yorkshire Wolds," by Mr. J. R. Mortimer.—In this paper the author described a peculiar structure observable in chalk from Yorkshire and elsewhere, giving it a striated appearance. This structure had been ascribed by Dr. Mackie and others to slickensides. The author adduced reasons for doubting the mechanical origin of these striations, and argued that they are of an organic nature. He ascribed them to corals, and remarked that similar striæ occur in all limestone formations.—"On *Platysiagum sclerocephalum* and *Palæospinax priscus*, Egerton," by Sir P. de M. Grey-Egerton, Bart., M.P., F.R.S.—The two species of fossil Fishes noticed in this paper were described by the author in the 13th Decade of the Memoirs of the Geological Survey, published in 1872. They are both from the Lias of Lyme Regis. He now described some new specimens which add to our knowledge of their characters. An example of *Platysiagum* shows the position of the dorsal fin, which is placed very far back, occupying a place opposite to the interval between the ventral and anal fins, and the form of the trunk, which is of nearly uniform depth from the occiput to the base of the dorsal fin. The structure of the dorsal fin was described in detail. The new specimen of *Palæospinax priscus* shows especially the position of the second dorsal spine, which is placed over the 50th vertebra, the first being on the 16th, the fish thus most nearly approaching the existing *Cestracion*, which it also resembles in its dentition. In other respects it seemed to be most clearly allied to *Acanthias*.—"On a new genus of Silurian Asteriadæ," by Mr. Thomas Wright, F.R.S.E.—The specimen described showed the outline of a small Starfish, with a large disc and short rays, in a slab of Wenlock Limestone from Dudley. The outline of the ten rays was described as marked out by the border of small triangular spines, the other plates of the disc and rays being absent. Each ray was terminated by a stemlike multiarticulate process as long as the ray, from towards the extremity of which spring slender lateral processes, giving it a tufted appearance. This Starfish, which is in the collection of Dr. Grindrod, is named by the author *Trichotaster plumiformis*.

Zoological Society, May 20.—Dr. E. Hamilton, vice-president, in the chair.—Lord Arthur Russell exhibited specimens of, and made remarks upon, the different varieties of the Carp (*Cyprinus carpio*) cultivated in the German fish-ponds.—Mr. Sclater offered some remarks upon the most interesting animals observed in the Gardens of some of the continental Zoological Societies which he had lately visited.—Dr. E. Hamilton read a note confirmatory of the extraordinary fecundity of the Chinese Water-Deer (*Hydropotes inermis*).—Mr. H. E. Dresser exhibited some rare birds from the Ural, amongst which were the Smew (*Mergus albellus*) in down, nestlings of the Rustic Bunting (*Emberiza rustica*) and several specimens of Lilljeborg's *Salicaria magnirostris*, which last he believed to be identical with *Acrocephalus dumetorum* of India.—Sir Victor Brooke, Bart., read a paper on the African Buffaloes, which he considered might be reduced into two species, *Bubalus caffer* and *Bubalus pumilus*. Of these the latter exhibited two varieties in the western and eastern points of its range, while the former appeared to extend from the Cape up the eastern coast to Abyssinia without any material variation.—Mr. St. George Mivart, F.R.S., read a memoir on *Lepilemur*, *Cheirogaleus*, and other Lemurine forms, to which were appended remarks on the Zoological rank of the Lemuroidea in the natural system.—Messrs. Sclater and Salvin communicated a paper on some Venezuelan Birds collected by Mr. James M. Spence, amongst which were examples of two species believed to be new to science, and proposed to be called *Lochmias sororia* and *Crypturus cervinventris*.—A communication was read from Mr. R. Swinhoe, on the White Stork of Japan, which he referred to a species different from the *Ciconia alba* of Europe, and proposed to call *C. boyciana*.—Mr. H. E. Dresser read some notes on certain oriental species of Eagles (*Aquila*).

Royal Horticultural Society, May 16.—General meeting, —Viscount Bury, M.P., president, in the chair.—The resignation of the Assistant-Secretary was announced.—The Rev. M. J. Berkeley, who was then called to the chair, commented on the plants of interest exhibited. He called attention to specimens of *Cytisus Adami*, believed to be a graft-hybrid, which bears upon the same branches, besides its own proper intermediate flowers, the dissociated very distinct flowers of its parents.—*Tillandsia ionantha* and a large flowering specimen of *Cycas revoluta* were also alluded to.

Scientific Committee.—Dr. J. D. Hooker, F.R.S., C.B., in the chair.—Mr. Anderson-Henry sent cuttings from black currant bushes, the buds of which were swollen to an unusual size, but abortive. This was due to the presence of a four-legged acaroid, similar to those on lime and hazel. In gardens near Greenock it was seriously affecting the cultivation of the fruit; it is believed there to have been imported with plants obtained from the Low Countries.—A letter from Mr. Andrew Murray to Mr. Berkeley was read, dated Salt Lake City. He sent an *Oscillatoria*, which he had found in a hot sulphuretted spring; also specimens of a *Nostoc*, with very large-celled chains, which blackened the stones in the brooks.—Dr. Masters called attention to a mode of propagating the vine described by M. Rivière. Cuttings were planted vertically in the ground in the spring, the uppermost bud being completely covered with 3 to 4 inches of soil.

EDINBURGH

Royal Society, May 19.—Memoir on the placentation of the sloths, by Prof. Turner. After referring to the absence of any definite information on this subject in anatomical literature, the author described his dissection of the gravid uterus of a specimen of that species of two-toed sloth, which Peters has named *Choloepus Hoffmanni*. His specimen was perfectly fresh when it came into his possession, and he had succeeded in obtaining satisfactory injections both of the fœtal and maternal systems of blood-vessels. His dissections have led to the following conclusions:—The placenta of the sloth is not cotyledonary, in the sense in which the term is employed to express the non-deciduate placenta sub-divided into distinct and scattered masses, as in the ruminants. In the fullest sense of the word it is a deciduate placenta. If the inference which has been drawn from Sharpey's observations on the placenta in *Manis*, viz. that it is non-deciduate, be correct, then it is clear, if any value is to be attached to the placental system of classification, that the scaly ant-eaters can no long be regrouped along with the sloths in the order Edentata, which order must therefore be broken up. The memoir concluded with some remarks on the affinities, as regards their placental form and structure, of the sloths to the other deciduate mammals.

PARIS

Academy of Sciences, May 19.—M. de Quatrefages, president, in the chair.—The following papers were read.—A note on solar cyclones, with an answer, by S. Respighi to M. Vicaire and Father Secchi, by M. Faye. M. Vicaire in his late critique on M. Faye's solar spot theory had asked how that author could compare the barometric depressions in terrestrial cyclones which only amount to a few millimetres of mercury with the enormous lowerings of the chromosphere which ought to take place on the solar spots but which are inadmissible. M. Faye now replied that these depressions are *facts* long and carefully observed by Respighi, and quoted a letter from him on the subject. With regard to Secchi's assertion that Respighi had been deceived by the small size of his telescope ($4\frac{1}{2}$ inches aperture) he pronounced the objection utterly invalid, for, whatever might be the shortcomings of the telescope as regards minute details, it could never make the chromosphere appear very low where it was in reality very high.—Note on the mechanical properties of different bronzes, by M. Tresca.—Hydrologic studies of the Seine Part II., Agricultural applications, by M. Belgrand.—On the part played by the substratum in the distribution of rock lichens, by M. Weddell.—New observations on metallic deposits on zinc, &c., and a new heliographic process, by M. C. Gourdon.—On an electro-diapason of continuous movement, by M. E. Mercadier.—On an electro-dynamic experiment, by MM. G. Planté and Alf. Niaudet-Breguet.—On the action of dry ammonia gas on ammonic nitrate, by M. F. M. Raoult. The author found that the liquid produced by the action varies in composition with the temperature. At $-10°$ C., 100 grammes of the nitrate absorb 42·50 grammes of the gas; this gradually diminishes as the temperature rises until at $+29°$ 20·9 grm. only are retained and the product is solid, at 79° only 0·5 grm. of NH_3 remain.—On certain peculiarities observed in spectrum researches, by M. Lecoq de Boisbaudran.—On the preparation and properties of oxymalcic acid, by M. E. Bourgoin.—On the acid derivatives of napthylamine, by M. D. Tommasi.—On the different propylenic chlorides. A classification of the absorption-bands of chlorophyll; accidental bands, by M. J. Chautard. The author so calls the bands produced by the action of acids, alkalies, or other re-agents upon normal chlorophyll.—Observations on the regulation of the magnetic compass, by M. Caspari.—Experimental Researches on the influence of barometric changes on life, tenth note, by M. P. Bert.—Mineralogical determinations of the true meteoric irons (Holosidères) in the Museum, by M. Stan. Meunier. During the meeting an election to the vacant seat of the late M. le Comte Jaubert (Academecien libre) took place. M. de la Gournerie obtained 44; M. Breguet, 9; M. Sedillot, 5; M. Jacqmin, 2; and M. du Moncel, 1 vote. M. de la Gournerie was accordingly declared elected.

DIARY

THURSDAY, MAY 29.

ROYAL SOCIETY, at 8.30.—Croonian Lecture on Muscular Irritability after Systematic Death: Dr. B. W. Richardson

SOCIETY OF ANTIQUARIES, at 8.30.—Ballot for election of Fellows.

ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

FRIDAY, MAY 30.

ROYAL INSTITUTION, at 9—On the Radiation of Heat from the Moon: The Earl of Rosse.

HORTICULTURAL SOCIETY, at 3.—Lecture.

SATURDAY, MAY 31.

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

GEOLOGISTS' ASSOCIATION.—Excursion to Finchley.

MONDAY, JUNE 2.

ENTOMOLOGICAL SOCIETY, at 7.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.

TUESDAY, JUNE 3.

ANTHROPOLOGICAL INSTITUTE, at 8.—On a ready method of measuring the Cubic Capacity of Skulls: Prof. Busk, F.R.S.—Flint Implements from St. Vincent's: Prof. Rolleston, F.R.S.—Copy of a Mural Inscription in large Samaritan Characters from Gaza: Rev. D. I. Heath.—Strictures on Darwinism, Part II.: the Substitution of Types: H. H. Howorth.

ZOOLOGICAL SOCIETY, at 8.30.—The Antelopes of the genus *Gazella* and their Distribution: Sir Victor Brooke, Bart.—The Birds of the Philippine Islands: Viscount Walden.

ROYAL INSTITUTION, at 3.—Roman Archæology: J. H. Parker.

WEDNESDAY, JUNE 4.

MICROSCOPICAL SOCIETY, at 8.

THURSDAY, JUNE 5.

CHEMICAL SOCIETY, at 8.—On the Dioxides of Calcium and Strontium: Sir John Courcy, Bart.—On Iodine Monochloride: J. B. Hannay.—A new Ozone Generator will be exhibited by Mr. T. Wills.

LINNEAN SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

BOOKS RECEIVED

ENGLISH.—The Art of Grafting and Budding: C. Baltet (W. Robinson).—Elementary Crystallography: J. B. Jordan (T. Murby).—The Noaic Deluge: S. Lucas (Hodder and Stoughton).—British Rainfall, 1872: G. J. Symons (E. Stanford).—On Coal at home and abroad: J. R. Leifchild (Longmans).—The Olive and its Products: L. A. Bernays (J. C. Beal, Brisbane).—The Philosophy of Evolution (an Actonian Prize Essay): B. T. Lowne (Van Voorst).

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ERRATA.—P. 64, col. 1, line 13 from bottom, for "drift" read "draft." Col. 2, line 14 from top, for "unnecessary" read "necessary."