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THURSDAY, NOVEMBER 10, 1870

SCIENCE AND THE WORKING CLASSES

THE spread of scientific instruction among the labouring population is a subject of greater importance than a superficial consideration might allow to it. Our scientific work is at present done almost entirely by our middle class, and mainly by those who have had such an education as is afforded by our universities or technical schools, or by foreign colleges. Not only do we look in this direction almost exclusively for the scientific training of the next generation, but also for the greater part of the work actually done in the field. Were statistics obtainable, it would surprise outsiders to learn how large a proportion of the practical observations in Astronomy, Geology, or Natural History is accomplished by men, the greater number of whose working hours are spent in towns or in some totally uncongenial occupation, and who can only devote a few precious hours stolen from their rest, or their brief summer holiday, to those pursuits which they have done so much to encourage.

This ought not so to be. Every one of these urban lovers of Nature must have returned from his annual retreat in the country with the thought how much more he could do for Science, how much greater scope he would find for the exercise of a keen eye or a cunning hand, if only fortune had so far favoured him that the prime of his life could be spent far from the tumult and dust of cities. And yet we find that, as a rule, those who do live in the country are very slow in making use of their glorious opportunities. The class who spend their whole time in the midst of every varying phase of the phenomena of Nature, have, taken as a whole, contributed little to the advancement of Science. How many a rare fossil, whose determination would have thrown light on some of the occult problems of geology, has been smashed by the careless hammer of the quarryman! How many a phenomenon of animal or vegetable life, the recording of which might have forestalled the discoveries of a Darwin or a Wallace by half a century, has remained unnoticed by the field-labourer before whose vacant gaze alone it passed! How many a strange monster or exquisite unknown form of life, for the possession of which all the museums of Europe would have eagerly competed, has been passed with merely an exclamation of awe or wonder by the untutored fisherman or sailor! The wealth of scientific knowledge which has in this manner been lost is incalculable. Now and then a Hugh Miller rises from the ranks to the command of a brigade in Science; now and then a George Stephenson, invincible from the feeling of conscious power, fights against difficulties of which those in the middle class can have but a faint conception, and comes to the fore by the force of inherent genius; but these are but a few solitary landmarks in the midst of the dead level of the intellects of our rural population.

The cause of this failure is not far to seek. It is simply want of education,—education, in the first place, of the powers of observation, and in the second place of the reasoning faculties, by the use of which alone the observations can be utilised and made to subserve the ends of Science. Everyone who has had the opportunity must have been struck with the dormant condition of the habist of observation of our agricultural population. An adaptation of purpose to end, or a deviation from the ordinary course of the laws of Nature, which would at once strike the educated eye, does not for a moment arrest their attention. Even of occurrences which pass daily before their eyes they are profoundly ignorant. We were once asked by a very intelligent labourer, whose occupation took him abroad frequently by night, if we could explain how it was that the moon shifted her place among the stars so much in the day time, while she remained stationary all night! The first thing to do with the working man is to teach him how to make use of his eyes. And in this first elementary lesson we are afraid our teachers of Science have hitherto lamentably failed. The error of "popular" scientific lectures, of evenings with working men at mechanics' institutes, is that which is so commonly attributed to clergymen, that of speaking over the heads of their audience. It does not really profit your Hodge or Styles to be discoursed to for an hour about the wonders of astronomy, the uninterrupted chain of organic beings from the Amœba to the Elephant, or the grandeur of the uniformitarian theory of geology. Take him out on a starry night and let him look through a telescope, and see for himself that Saturn is a round orb suspended in the air. with its marvellous rings and its attendant satellites; take him where with his own hands he may exhume a shark's tooth from a chalk-pit, and show him how this absolutely proves that the spot on which he stands was once fathoms deep beneath the ocean; put a flower in his hand, and point out the structure and the function of every organ, and you will at least have made a beginning. You cannot be too simple or practical. Treated in this way, we believe that every science-teacher who has tried the experiment will testify to the eagerness of the working man to increase his store of knowledge. In the case of those classes of the labouring population who live in towns, the only substitute is to take them to the Museums or Collections of Natural History, which are the best representatives of Nature herself. For our country gentlemen and tradesmen, the Naturalists' Field Clubs, which now flourish in so many counties, are doing a good work: something of a similar kind is wanted for the less educated class of the population.

But when the eye has been trained to observe, the whole work of education has by no means been accomplished. It is the portion of the work on which there is most need to insist at the present time, because it has hitherto been almost entirely neglected. The system pursued till recently in our Universities and public schools was based on a high cultivation of the reasoning faculties, to the almost entire exclusion of any recognition of the perceptive powers. There may be a danger now of running into the other extreme, and already we hear some zealous devotees of Natural Science exclaim against book knowledge, as if it were opposed to a practical scientific training. There cannot be a greater mistake. Unless a man thinks to live the life of a recluse, and to profit nothing by the labours of others in the same field, the greater part of his knowledge must always be derived from books. What we insist on is that the learner must be taught *first* to use his own eyes, before he has recourse to the experience of others. No man can be considered to be highly educated who does not possess the power of bringing his perceptive faculties to play on the phenomena that surround him, and also of exercising his reasoning powers to systematise his observations, and to compare them with those of others who have preceded him. The surest way of cultivating the Perception is by the severe study of some branch of Natural Science; the Reason is to be trained in the lecture-room and the study. Nature does not proceed on the principle of setting one of her gifts at variance with another; and so far from one of these sets of faculties being opposed to the other, neither can be cultivated to the full extent of the mental powers without the assistance of the other. No nation has distinguished itself by producing a greater number of keen and accurate observers of Nature than the Scotch, and none has set a higher value on the education that is derived from books. In the scientific education of the agricultural population of England, it will be found that the long disuse for generations of the reasoning powers is the greatest difficulty to be overcome. Although we do meet here and there with those who are more or less accurate observers of Nature, it is extremely rare to find one who has any power of forming a connected train of reasoning as the result of his observations.

We need but look around us on the events passing before our eyes on the Continent of Europe, to recognise the manner in which Education is proclaiming herself victorious along the whole line. As a nation, we are slow to learn. But that nation must indeed be both deaf and blind, which does not at the present time see the necessity of straining every nerve to redeem itself from the disgrace of ignorance. With our working classes taught to exercise those faculties which they all possess, but which so few know how to use, and thus trained to form the strength of the nation in all fresh advancements in Science and the Arts, England would quickly distance all competitors, and assume that position which it would now seem younger rivals are likely to snatch from her grasp.

HUXLEY'S LAY SERMONS

Lay Sermons, Addresses, and Reviews. By Thomas Henry Huxley, LL.D., F.R.S. (Macmillan and Co., 1870.)

IN this volume Professor Huxley has presented to the public a miscellaneous collection of essays, some didactic, some controversial, some addressed to a general audience, some to a special one, and composed at various times during a number of years extending from 1854 to within the last few months.

The subjects of which the Professor treats are as various as the occasions for which his papers were written and the audiences which he addresses, and, as may be easily believed, his essays are of very unequal value. But one great element of value they all possess in common, and that is, the thorough-going boldness, and honesty, and out-spokenness with which they deal with all subjects alike. This is no small merit in any writer, and it is an especially great one in a man occupying the position which is held by Professor Huxley. It is a remarkable condition of English society at the present time that a man who combines real scientific eminence with great general ability and special oratorical power, as Professor Huxley does, is made, with or without any consent of his own, into a kind of popular oracle. Like an oracle he is expected to have a response ready for any imaginable query, and like an oracle too he must find himself not unfrequently under special and strong temptation to "prophesy smooth things." Yet Mr. Huxley does not prophesy smooth things; on the contrary, he does not hesitate to put the most unpopular propositions in the plainest possible language when he sees that it is right to do so; and to say that a man—and that man a public teacher—lies under special temptation, and that he resists that temptation, is to say at once that his teaching must be worth listening to, and that even where we cannot accept his doctrines, we may still listen to them with advantage and gain instruction from them.

With one exception, the papers in this volume may be classed under three heads, viz.: Educational Essays, either theoretical or practical, which include Nos. 1, 3, 4, 5, 6, and 9: Scientific Controversy, consisting of Nos. 12 and 13, on the Origin of Species; and also 7, 8, and 14, of which the first is the famous "Essay on the Physical Basis of Life," and the other two are replies to the attacks made upon the former: Finally, Presidential Addresses to the Geological Society, Nos. 10 and 11, of which the latter might fairly come under our second heading, consisting as it does of a very able reply to Sir W. Thomson's strictures upon modern geology. The essay which will not come into any of these divisions is the shortest in the book; viz. that on "Emancipation, Black and White." We must however devote some space to it, since it appears to us to be almost the best reasoned and most temperate view of what its author calls the "irrepressible woman question" which we have yet seen, although we are not prepared to accept the author's conclusions without reserve. In this essay Mr. Huxley's allegiance to the facts of science comes into uncomfortable collision with his allegiance to the traditions of party. He comes before us in the character of an advanced Liberal, but he cannot forget that he is, before all things, a biologist; and the consequence very naturally is, that although he is prepared to support a policy of emancipation — apparently upon the general principle that *all* government is a mistake—yet he is compelled to admit that the arguments of *extreme* emancipationists are "to a great extent nonsensical." We are confident that this question is one which must be ultimately settled mainly upon physiological grounds, and it is just because the conventions of society very rightly do not admit of the full and fair discussion of those grounds before mixed audiences, that the extreme emancipationists have been enabled to obtain for their theory the amount of currency which has lately fallen to its lot. In the present instance, Mr. Huxley appears to have followed out his physiological argument with characteristic fairness *to a certain point*, and consequently sees that after all due emancipation, "Nature's old salique law will not be repealed, no change of dynasty will be effected;" and again, that "so long as potential motherhood is her lot, woman will be found to be fearfully weighted in the race of life." But why should Mr. Huxley halt at this admission? Why does not his Darwinian logic carry him on to its legitimate and necessary consequence? According to the law of natural selection, when once fairly engaged in the struggle for existence, no less a penalty than ultimate extinction awaits the weaker race. If the parallelism between a race and a sex can be maintained at all—and the parallelism is Mr. Huxley's, not ours—it plainly implies that, put into competition with man, woman must sooner or later cease to exist *as a competitor*, just as certainly as the black rat has been driven out before the *Mus decumanus*, or as, to adopt a different class of example, the handloom weavers have been driven from the market by machinery and steam. But while we thus doubt the wisdom, or indeed the possibility, of placing women on a level with men, and in competition with them, we would by no means therefore be understood to argue against giving them a liberal education or improving the law in regard to their property.

Passing on to consider for a short time the Educational essays, we need say but a very few words of the single specimen given of the writer's method in practical education, viz. the Lecture on a Piece of Chalk. It is certain to be well known to most of our readers. Those who do know it, for the most part recognise it as a model both in matter and in manner of what a single lecture ought to be ; those who do not had better read it at once, for till they have so done they will have but an imperfect idea of such a model. The other educational essays, viz. the first six in the book (with the exception of the second) contain an exposition of the author's views on many of those points in the theory of education which are most keenly disputed at the present time, such as the value of natural science as contrasted with mathematics or philology as an instrument of education; that of the natural history sciences as contrasted with other branches of natural science, the method by which they should be taught, &c. Now it is only fair to admit that in all these matters Professor Huxley's addresses must be looked upon as the speeches of an advocate, and moreover of an advocate who feels that he carries the feeling of the public with him for the most part, but is by no means equally sure that he can overcome the prejudices of the jury. Still, considered as the speeches of an advocate, they are admirable, and it must be remembered that an advocate may prove his case, and this, we think, Mr. Huxley has in several instances done. In regard to the most important of the questions discussed, we are disposed to believe that any one of these three instruments of education may turn out a highly cultivated and thoroughly well educated man, provided the teacher knows how to teach and the learner presents good raw material upon which he may exercise his skill ; but this by no means proves that they are all of equal value. One thing we can certainly say in regard to the classical education of our own day, that is to say, of the day of those who are the acting men of the present generation, viz., that, however well it may have served the turn of that small minority who were sure to make the best of any kind of education, and for whom, therefore, it is the least necessary to make provision, it has done nothing at all for the great majority of those who have been submitted to it. It is not too much to say, that out of the men who have gone from public schools to Oxford, and who have spent their whole lives between the ages of eight and twenty-two in learning Latin and Greek, not one in three could at the latter age read a Latin or Greek author with ease and intelligence. This may not in itself prove the case of science as an instrument of education, or even prove the inefficiency of classics ; but at least it shows that classics have failed as a fact, and reduces us to this dilemma, that we must admit either that they are but a very imperfect means of education, or that the general standard of educability among young Englishmen is unaccountably low. One other alternative indeed remains, viz. the supposition that classics have been generally very badly taught, but this seems to us hardly tenable. It is difficult to believe that so much labour has been bestowed by so many good scholars as may be found amongst the schoolmasters of the last fifty years, upon the art of teaching classics, without the elements even of the art being discovered. At any rate reformers, or even revolutionists, in education may fairly argue, that what has not been done in so many years by a method which has had the whole field of the higher education to itself,. is hardly likely to be effected by a persistent continuance in the same path. We are reminded of the physician of Laputa, of whom, when he had already almost killed his victim by his discipline, Gulliver says, " We left the doctor endeavouring to recover his patient *by the same operation*."

We have left ourselves no space in which to notice the remaining and more directly scientific portion of Professor Huxley's work. The book is not to be discussed fairly in the space at our disposal : it is, however, full of interest throughout, and we need perhaps the less regret that we are unable to direct our readers' attention to the remaining essays, inasmuch as they constitute that part of the work which deals with the scientific controversies of the day some of which have already been discussed in NATURE.

G. W. C.

FERNET'S ELEMENTARY PHYSICS

Traité de Physique Elémentaire. Par Ch. Drion et E. Fernet. Troisième Edition. (Paris : V. Masson et Fils. 1869.)

THE third edition of this well-known handbook of French physics deserves more than a casual notice. We are told in the preface that it has been entirely recast by the second of the two original authors, M. Fernet, a pupil of the lamented Verdet, who has caught something of the spirit of his master. There has been no teacher of physics in our time whose work has been, on the whole, comparable to that of Verdet. He has all the clearness of Tyndall; and, as almost all of his published lectures were delivered to audiences more strictly scientific than those to whom the famous books on Sound and Heat were originally presented, he is never diffuse. His arrangement of the essential points of his subject, and his grouping of the illustrative details and of the exceptions to the general principles which govern it, have scarcely been equalled even in France, which is the special country of precise and exhaustive exposition. It is high praise, therefore, to say of M. Fernet, that in parts his book recalls his master's method and style.

The treatment of mechanics which is common in this country places statics before dynamics. There is only one thing to be said in favour of this arrangement—that the idea which lies at the root of dynamics, that of change of rate of motion, is a little difficult for a beginner to catch, and it is very hard to get him to see how to express it in any but the simplest case. On the other hand, we have no clear notion of Force at all until we master it, and statics without that idea is a series of barren propositions, which stand to real life much in the same relation as a "uniform, weightless, perfectly rigid, straight rod" does to a real bar. M. Fernet accordingly places this conception at the commencement of his book, and we find the laws of uniformly accelerated motion given before the discussion of levers and centres of gravity.

It is extremely easy, and perhaps a little ungracious, to select for notice points the omission of which causes surprise even in an elementary work of 800 pages. The need of compression, which constrains an author who has to treat in one volume mechanics, hydrostatics, pneumatics, light, heat, sound, electricity, magnetism, and meteorology, is so great, that it is impossible to question too loudly the prudence of obvious omissions. But we should have expected to find a little discussion of the adhesion of liquid plates to the solids which they wet, and to each other, ; and one would willingly excuse the absence of the regulation picture of the balloon and its car, with the accompanying history of the brothers Montgolfier, for an account of the mercurial air-pump of Sprengel or Jolly. M. Fernet is perhaps a little open to the charge which is constantly brought against the scientific men of his country, that if they read anything but the science of the *Comptes Rendus* and the *Annales de Chimie et de Physique*, they never indicate the fact by a line or an allusion. M. Verdet alone, of well-known French scientific writers of our time, knew German and English as well as French science, and showed that he knew it. We should scarcely have found any book of his on the subject of heat, without any reference—as far, at least, as I can find—to Joule's principle of the equivalence of mechanical effect and heat, of the "mechanical equivalent" by which the one may be converted into the other, or of the fraction of the heat which can be converted into work by a perfect heat engine.

So far as it goes, in fact, the book is extremely clear and satisfactory; but it gives one less impression of a complete working up of the subject to the latest date than we had expected to find in it. Take for instance the well-known series of experiments by which Kundt established the velocity of sound in tubes of different materials. They were explained by Tyndall in this country a couple of years since, and it is impossible to conceive any which make more visible to the student those vibrations of bodies which he is constantly required to admit in the course of his reading on acoustics. The omission of all mention of the famous "singing flames" is more defensible from a purely scientific point of view, as the experiments scarcely admit precise measurements, and can only be relied on to convince any one who needs convincing how many astonishing things there are within the range of the science.

M. Fernet has given a considerable number of notes, in which there is a precise mathematical treatment of the statements in the text. We should be glad to see the plan followed more frequently in elementary treatises. In a book, the introduction of simple mathematics in a note does not distract the attention of the reader who is frightened even by a simple equation, and it gives precision to statements which can scarcely be fixed without them, or without a long and extremely tedious paraphrase. We are more doubtful of the advantage of a practice common in France, and which has crept into some English books; the introduction into the text of *notes non exigées.* These are parts which the student who is preparing for a special examination may omit if he chooses. The text of a book of this kind ought, it seems to us, to be either one thing or another—to be composed with a perfectly definite object, and to be one and indivisible. As it is, it produces something of the impression that is given occasionally when a single picture is used to illustrate two different propositions. The lines which belong to the first get so mixed with those of the second, that the student can follow neither. A book too full of *notes non exigées* is apt to be too little systematic and scientific for the more advanced student for whose benefit these notes are inserted, and to be made too difficult for the simpler readers whose wants are mainly kept in view.

WILLIAM JACK

OUR BOOKSHELF

Cryptogames Vasculaires du Brésil. Par Prof. A. L. Fée avec le concours de Monsieur le Dr. Glaziou. Pp. 268, 4to., 78 Plates. (Paris: Baillière.)

PROFESSOR FEE is by many years the oldest amongst living fern-authors. He has held for more than a quarter of a century the chair of botany at Strasburg, and has concentrated his attention principally upon ferns and the other allied higher orders of cryptogamic plants. He published a general treatise upon the classification of the order as long ago as 1844, and since then many consecutive years have never passed without producing some memoir upon the subject from his fertile pen. Having recently received a fine collection of ferns from Dr. Glaziou, the superintendent of the Botanic Gardens at Rio Janeiro, he has been stimulated to add one more memoir to the series, and that is the work now before us. All the series of his monographs, several of which are in folio, are illustrated beautifully and copiously, not only with full-sized figures of the plants, but also with careful magnified analytical details; and together they form by far the most extensive and excellent series of fern-plates which anyone upon the Continent has published. The present memoir is quite upon a par with its predecessors in this respect. It is in quarto, and contains seventy-eight quarto plates and a list of all the ferns and fern-allies known to the author as inhabiting Brazil, with a list of special stations, but with descriptions of novelties only. But there is one drawback to the value of Fée's works, and that is a very great one. Living at a distance from the great metropolitan herbaria, our author has apparently worked almost entirely upon his own private collections, and has continually failed to recognise well-known plants, and has made new species in great numbers out of the specimens which his correspondents have sent him, which no one else has been able to understand as such. In none of his works—we have no alternative but to say—has this tendency been carried to a greater excess than in the present one. For Brazil alone he describes and figures in the present memoir upwards of 180 new species, so called. These are not from tracts of country which the collectors whose gatherings have been already reported upon have not visited, or have left unexplored, but nearly all from the vicinity of the capital, and from the gatherings of Glaziou. Now the neighbourhood of Rio Janeiro is exceedingly rich in ferns; but there is, perhaps, no other part of Tropical America from which herbaria, both in England and on the Continent, have been more bountifully supplied. The consequence is, that out of this 180 we do not think that more than from twelve to twenty species are really new, in any sense in which we understand in this country what is meant by a species. For instance, we have some seven or eight species elaborately characterised and figured from what cannot be called anything else than so many individual fronds of that most cosmopolitan of ferns, our common English *Aspidium* or *Polystichum aculeatum.* Or, to take one of the exclusively Brazilian species, *Cyathea Gardneri*, a very distinct tree-fern, is included in the list under five different names—*Gardneri* (Dr. Gardner's number on which Hooker described the species quoted), *incurvata* (a name of Kunze's published in the Linnæa from Regnell's specimens), *mamillata*, *taunaysiana* and *attenuata*, the last three new species here named and figured for the first time; but the figures, beautiful as they are, might, any of them, have been drawn from Gardner's specimens. The author does not seem to have any knowledge of numerous English and German books and papers in which Tropical American ferns are described, as for instance, Grisebach's excellent Flora of the British West Indies; and this leads to further name-crossing. In short, although one cannot but admire the excellence and the copiousness of the illustrations in these memoirs, and ought not to leave out of sight the example of devotedness to science which they show, expenditure of time devoted to one object through a long course of years, and of money, only a very small proportion of which their sale can possibly repay, yet still the predominant feeling on the mind must needs be that to deal with plants in this way has a direct tendency to bring species-botany at a very rapid rate into a state of utter confusion.

J. G. BAKER

The Laboratory Guide. A Manual of Practical Chemistry for Colleges and Schools, especially arranged for Agricultural Students. By A. H. Church, M.A., Professor of Chemistry in the Royal Agricultural College, Cirencester. Second edition, enlarged and revised, pp. 170. (London: Van Voorst, 1870.)

THIS little book, as its title indicates, is intended mainly for the use of students of agricultural chemistry, and we fear it might cause disappointment to anyone who wished to employ it as a guide to general analysis. The science of chemistry is so rapidly increasing, that it would seem almost hopeless, at the present time, to give students a complete knowledge of chemistry and leave them to apply their information to the special subject they intend to follow. Professor Church's book is intended to obviate this difficulty, and after a few introductory lessons of universal application, the student commences experiments on materials with which he is certain to come in contact in agriculture, such as superphosphate, milk, soils, &c. Part I. treats of chemical manipulation, and consists of a number of lessons intended to accompany the course of lectures, and from which the student will learn the mode of performing some simple operations, as solution, filtration, crystallisation, specific gravity, and will become acquainted with the modes of preparation and properties of the principal elements and compounds. Each lesson commences with a list of the apparatus required, the ordinary reagents, and the special materials and tests necessary for the performance of the experiments, which are detailed with great clearness. This arrangement is calculated to cause the student to be careful to have everything ready before commencing work, and will thus save him much time and inconvenience, for few things are more likely to endanger the success of an experiment than leaving it at a critical moment in order to obtain some piece of apparatus or reagent which should have been previously prepared. Part II. treats of qualitative analysis, of which Chapter I. deals with the elements, reagents and tests, and reactions; and here we find the terms univinculant, bivinculant, trivinculant, &c., as equivalent to monad, dyad, triad, &c. The principal distinguishing characteristics of the different groups of elements are here given. The section on reagents and tests will be found useful, for it contains the modes of testing for impurities, and indicates the strength of the different solutions employed, two things to which attention should always be paid. The second chapter of this part describes the methods of qualitative analysis, all rare elements and those with which the agricultural student is not likely to meet being omitted. The third part is devoted to the general processes of quantitative analysis, and the fourth to the examination of manures, soils, water, and food. This book will doubtless be invaluable to agricultural students, besides being useful to those requiring special information on the subjects of which it treats. The appearance of such a work is a satisfactory indication of the extension of the application of scientific chemistry to the useful arts.

The Book of the Roach. By Greville Fennell (of the *Field*). 16mo. pp. 118. (London: Longmans and Co. 1870.)

WHILST Mr. Pennell has instructed us in catching *lege artis* all the various fish in British rivers and lakes, Mr. Fennell has been content to devote a little volume to the natural history and fishing of the Roach. Let no one smile at the man in the punt with his humble notions of enjoyment. Maybe he has been toiling hard the whole week in the noisy, murky town; the quiet sport of the Saturday afternoon suits his purse exactly, and there will be real enjoyment over the dish of fried roach "caught by father." Nay, if we could measure the amount of pleasure, healthy recreation, and renewal of vigour obtained by the multitude in the unpretentious sport of roach-fishing, and compare it with that sought for by the select few who have the privilege of finding their amusement on a salmon river, we should probably find the balance very much on the side of the former. No apology, therefore, was needed from Mr. Fennell for the publication of his little book on the Roach. He has divided it into eight chapters, of which the first two are devoted to the natural history of this fish, and the five following to a description of the tackle and various kinds of baits, and to the methods of roach-fishing generally as well as at certain localities. In the last chapter hints are given on the roach as an article of food, on the method of cooking, &c.

A. G.

LETTERS TO THE EDITOR

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

Hypothesis regarding the Corona

HAVING read in Nos. 34 and 35 of the valuable periodical NATURE (of June 23 and 30) the two articles about the Corona, I beg leave to direct your attention to an hypothesis concerning its nature, and especially the origin of the *beams*, which I sent to the Physical section of the Amsterdam Academy of Sciences, of which I have the honour to be a member.

I have just received No. 1776 of the *Astronomische Nachrichten*, for October 15, where the American astronomer, Dr. Gould, in a notice regarding the total eclipse of the sun of August 7 (1869) says:—

"Of the Corona I made some hasty measurements both with the telescope and without it. Its form varied continually, and I obtained drawings for three epochs at intervals of a minute. It was very irregular in form, and in no apparent relation with the protuberances of the sun, or the position of the moon. Indeed, there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon. One of the beams was at least 30′ long."

This passage induced me to submit to the trial and judgment of my fellow-members of the Physical section of the Royal Academy a very simple hypothesis regarding the nature of the Corona, which entered my mind shortly after the observation of the total eclipse of August 18, 1868.*

I think—and, if I am not mistaken, all astronomers agree with me—that a part of the luminous phenomenon which we call the Corona belongs to an atmosphere of the sun,† having a feeble reflecting power, or being itself luminous. But the beams in the Corona, whose variability is now confirmed anew, are necessarily an optical phenomenon. They originate, I believe, in the inequalities of the moon's surface. If the sunlight slants somewhere along the moon's limb through a *valley*, we observe from our point of view a *beam*, provided there exist between the moon and us particles able to reflect the sunlight, or to transmit it like semi-transparent bodies. There is no need to have recourse to diffraction.

To look for these particles in the atmosphere of the earth, as Mr. Gould does, is, in my opinion, not tenable, as the Corona and the beams have also been observed in eclipses, where the cone of the shadow even had‡ a breadth of thirty-six German leagues. The molecules of the atmosphere, which we see around the eclipsed sun, are wholly within the cone of the shadow.

These reflecting particles are undoubtedly to be looked for beyond our atmosphere, between the moon and the earth, and I believe that they may be regarded as identical with those particles which float in the ether, and under other circumstances cause the zodiacal light.

That the zodiacal light, or rather the particles which cause it, reach the earth's orbit, is, as regards nearly its whole circumference, not subject to any doubt, as the apex of the zodiacal light is "mostly farther off the sun than 90°. Only in the months of March and April is it not possible to follow the zodiacal light so far" (Schmidt, "Das Zodiacallicht," Braunschweig, 1856). But then, at those particles which are situated on the apex of the zodiacal light, the sun's ray makes a right angle with the line which joins the earth, and the circumstances are not favourable for small and widely-dispersed particles to reflect much sunlight in the direction of the earth. It is, therefore, probable that in March and April these particles would show themselves farther from the sun if the illumination were stronger. At a total eclipse of the sun that angle is 179¼° for a particle visible at 30′ distance from the moon's limb, and 179¾° for particles in the immediate neighbourhood of the moon's limb; and it is an acknowledged fact that, under these circumstances, there is much more light reflected than if reflected at a right angle. Moreover, the intensity of the transmitted light increases equally with this angle.

Schmidt mentions, in his above-quoted work, that he and other observers, during the total eclipse of July 28, 1857, looked out for the zodiacal light, but in vain. He thought the sky not dark enough, and expresses himself as follows:—"Das Ansehen der den schwarzen Mond umgebenden vielstrahligen Corona war im ganzen betrachtet nicht gerade geeignet, sie *sogleich* in Beziehung zum Zodiacallichte zu denken."

Thus it seems that the beams made Schmidt conclude, "This is no zodiacal light." If my explanation is the true one, and if the sun's atmosphere is accepted as unlimited, and gradually passing over into the ether, then the light of the Corona, which equally surrounds the moon, falls altogether into the same category as the beams, only that it originates in the reflecting particles beyond the moon. Accordingly, my hypothesis is expressed thus: *Both the Corona and its beams have the same origin as the zodiacal light.*

I remark, finally, that the strangely curved form of some beams, as well as their variability, may be very well accounted for on this hypothesis—the curved form by the irregularity of the moon's surface. For instance: I take a particle of light of a beam of the Corona; I imagine myself in the molecule which, according to my hypothesis, corresponds with that particle of light, and which is situated between the moon and my eye. Looking, then, from that stand-point to the moon, I must see the light of the sun slanting over a valley; but if I move myself laterally—*i.e.* in the direction parallel to the moon's radius which corresponds with that valley—then it is not certain that I shall see on the same point an extraordinary amount of light, for it is possible that at the same place a ridge has intervened. It is, however, very possible that, if I move in a direction perpendicular to the former (and also perpendicular to the direction towards the earth), I come into a region where that ridge does not intervene, but where the continuation of the valley is again visible, through which the sunlight slants. In this case, the beam of the Corona—*i.e.* the effect produced by those particles which receive more light than others at equal distances from the surface of the cone, having its apex in the eye, and which surrounds the moon—has a curved form.

The variability is, I think, satisfactorily explained by the motion of the moon passing by the sun.

This hypothesis forced itself upon me when at Toli-holi* I saw the moon rising behind a hill. Before she made her appearance her light shone over the trees, and produced, in a hazy air on my side of the hill, beams which very much resembled those of the Corona. I do not know whether the zodiacal light has already been analysed by the spectroscope; if so, the comparison of the spectra of zodiacal light and the Corona will serve to test my hypothesis.

A second test is this: total eclipses observed in the months of December and January should show less brilliant beams than total eclipses observed in other months, since in December and January the earth is near the direction of the perihelion of the circumference of the zodiacal light. The next total eclipse of December 12, 1871, will perhaps give some information on this point.† J. A. C. OUDEMANS

Batavia, Sept. 2

* At the Island of Mantawalu-kéké (near Celebes), where I had the pleasure of meeting the Commander (Captain Bullock) and the Etat-Major of H.M.S. *Serpent*, and the Spanish astronomers of the municipal Athenæum at Manilla, the fathers Faum, Nonnell, and Ricardo.

† This is probably the Chromosphere, as seen in the Eclipse.—ED.

‡ (*Subintellige*): at the surface of the earth.

* *Vulgo*, erroneously, Tontoli, north coast of Celebes.

† We hope also something from next month's eclipse.—ED.

The Fuel of the Sun

IN your impression of October 6 Mr. Murphy adds another to the frequent attempts that are still made to galvanise the expiring hypothesis that attributes the solar heat and light to a meteoric bombardment. Many very strong and sufficient objections have been already brought against it, but as Mr. Murphy states that he is "not mathematician enough to form any opinion on the merits of the controversy," I will add two arguments which to my mind are quite sufficient to annihilate this explanation—both of which may be sufficiently understood without mathematics, and neither of which have I ever seen fairly stated.

1st. The advocates of the meteoric bombardment usually start from the fact that great meteoric showers fall upon the earth. Thus, Dr. Tyndall, in his lectures on "Heat considered as a Mode of Motion," introduces Mayer's hypotheses, with an account of the number of meteors counted during the August and November showers; and these observed meteors and a few comets are the only actual observed material upon which this bombardment theory rests—all beyond them are mere figments of mathematical imagination, and any supplies derived from the zodiacal light, or otherwise exclusively from the space within the earth's orbit, must have been exhausted within the period of human existence.

Now, it is quite obvious, without any detailed calculation, that if these meteoric bodies, coming from anywhere you please outside of the earth's orbit in sufficient quantities to maintain the heat and light of the sun, had fallen, as they must have done, upon the earth in a proportion due to its magnitude and position, they must in the course of a few millions of years—say from the era of the Laurentian rocks to that of the London sewage deposits—have covered the earth with a very important superficial stratum, instead of merely supplying a few rare specimens for our museums. Every slowly deposited sedimentary rock upon the face of the earth should be thickly peppered and conglomerated with meteoric dust and nodules. With these considerations and the well-known geological facts before us, I need scarcely state the obvious conclusion, viz. that the evidences in support of the theoretical terrestrial requirements of this bombardment hypothesis are contemptibly insufficient.

My second objection attacks the fundamental basis of this hypothesis, and I think destroys it altogether. I maintain that any explanation of the sources of solar light and heat which does not equally and necessarily account for the radiations of all the other self-luminous orbs that people the whole immeasurable depths of space, is philosophically worthless. It is thus worthless if it does not also account for the perpetual renewal, the constancy, the eternal permanence, of all these radiations. The pale nebulæ, as well as the brighter suns, should be equally included in its grasp.

Tested thus broadly and philosophically, the meteoric bombardment hypothesis appears in its true colours as a monstrous physical absurdity. It assumes a perpetual flow of solid masses *converging* continuously from everywhere towards everywhere; or otherwise a state of things which could only endure through the time which these meteoric masses would occupy in travelling the semi-distance between the neighbouring suns. These little journeys ended, the interstellar space must, according to this hypothesis, become a sterile vacuum, all the lights of heaven must go out, eternal darkness must rest upon the face of the deep, and everlasting death pervade the universe.

W. MATTIEU WILLIAMS

The Cockroach

I HAVE only to-day noticed the Rev. C. J. Robinson's letter on this subject in your issue of the 29th Sept. A friend of mine, whom I have known all my life, who occupied an important trust as Bank Manager in India last year, and who is at present home on sick leave, assures me that Dr. Norman Macleod is wrong when he denies the nail-nibbling propensities of the cockroach. My friend had been in Kurachee for some time, and on his journey from that town to Bombay by sea he was annoyed one night in his berth by some insect crawling over his face; half asleep and half awake he put up his hand to his face and sent the insect to the foot of his berth. Shortly after he was awoke by a pain at his great toe, and on looking at it he discovered that a cockroach had nibbled off all the nail down to the quick.

JAMES DURIE

Aberystwith, Oct. 8

Were Cockroaches known to the Ancient Greeks and Romans?

YOUR correspondent, Rev. C. J. Robinson, drew attention in your columns (NATURE, Sept. 29) to the question whether these troublesome insects were known to the Ancient Greeks and Romans; he says, "there is a good deal to lead one to suppose that the *μυλακρίς* mentioned by Aristotle, and the *Blatta pistrinorum* of Latin writers was the same as our loathsome pest." I think Mr. Robinson is mistaken in supposing that the *μυλακρίς* is mentioned by Aristotle, at least I can find no mention made of this insect in the writings of the Stagirite. The word *μυλακρίς*, meaning some kind of insect, occurs in the fragments of Aristophanes preserved by Pollux, who amongst other meanings of the term gives the following one:— *ζῶόν η ἐν τῷ μύλωνι γινόμενον*, and then quotes this couplet from Aristophanes,

Ἵνα ξυνῶσιν ᾧπερ ἥδεσθον βίῳ,
Σκώληκας ἐσθίοντες, καὶ μυλακρίδας.

"where they may partake of the food of which they are fond, eating worms and *mylocrides*." It would not be possible to say what the *μυλακρίς* here denotes, but from the creature being often produced in mills, it may possibly mean a "Cockroach," though a "meal-worm" (*i.e.*, the larva of the beetle, *Tenebrio mulitor*) would suit equally well. The Greeks, however, had a word which may well represent the Cockroach, though it is even here impossible to speak with certainty. The word, *υίλφη*. it is probable denotes this insect. Aristotle (Hist. Anim. viii. 19. § 4) uses the word once; he enumerates the *silphe* amongst insects which cast their skins. The Scholiast in the "Peace" of Aristophanes says the *silphe* is an ill-smelling insect (*δυσώδμος*). Aetius (8. 33.) speaks of "the fat of the stinking *silphe* which inhabits houses." The epigrammatist Evenus (Analect, i. p. 167) speaks of the *silphe* of the booksellers' shops, and applies to it the epithets, page eating (*σελιδηφάγος*), destructive (*λωβήτειρα*), black-bodied (*μελαινόχρως*)." Lucian speaks of the mere book collector as providing pastime for mice and habitations for *silphe*, and cuffs his slaves for not keeping the mice and *silphe* away. (Advers. Indoct, iii. 114, Ed. Hemsterhus). The Scholiast here gives a description of the *silphe* which Schneider with some reason refers to some kind of *Lepisma*. Ælian (H. A. i. 37) says that the *silphe* infest swallows' nests; these cannot be cockroaches. Galen and Paulus Aegineta apply the epithet, *βδέουσαι*, to the *silphai.* Dioscorides (ii. 38) says that the inside of the *silphe* found in bake-houses when pounded with oil is good for pains in the ear. This leads me to the *Blatta* of the Romans. "On pulling off," says Pliny, "the head of a *blatta* it gives forth a greasy substance, which, beaten up with oil of roses, is said to be wonderfully good for affections of the ears." He speaks of the disgusting nature of this insect, one kind of which is known by the name of *Myloecon*, and found in mills (Nat. Hist. xxix. 39). In another place (xi. 34) Pliny says, "It is the nature of the *blatta* to seek dark corners and to avoid the light; they are very often found in baths." According to Virgil, "the light-avoiding *blattæ*" find their way into bee-hives (Geor. iv. 243). Horace (Sat. ii. 3, 119) ridicules an old miser for sleeping on straw and leaving his bed clothes in his chest, the food of *blattæ* and *tineæ*, "Blattarum ac tinearum Epulæ." Martial (Lib. iv. Ep. 37.) says unless his books are well put together they become the prey of *tineæ* and *blattæ*.

Constrictos nisi das mihi libellos
Admittam tineas trucesque blattas.

From the above passages it will be seen that the *blatta* was a destructive insect to clothes, books, &c., that it avoided the light, and was fond of warm places, that it frequented mills and exuded a greasy substance from its head, that it was a disgusting creature (probably in allusion to the smell) all of which particulars are true of cockroaches, and as there are many species of the family, and are widely distributed over all parts of the globe and must have been known to the ancients, I think there is good reason for concluding that the cockroach was known to the Greeks by the name of *σίλφη*, and to the Romans by that of *blatat.*

W. HOUGHTON

The Aurora Borealis

I SHALL be obliged if you will put on record a few scattered notes which I took of the splendid Aurora Borealis of October 25, seen from Arthingworth, Northamptonshire. When I first observed it at half-past five P.M., a crimson glow extended in an irregular band from N.N.E. to W., most prominent at about 20° to 30° above the horizon. This increased in height and breadth until it nearly reached a point S.W. of the zenith, and about 15° W.N.W. of the star Vega. At this time the northern part of the sky was perfectly free from aurora; gradually that part and the whole dome of the heavens, with the exception of a section from W. to nearly S., became filled with luminous streamers. These, for about 20° on each side of N., were white, the others crimson striped with white or rather greenish light, but the green I believe to be an effect of contrast, as where similar streamers were distant from the red light they were white.

The white or green steamers appeared to eclipse the red light, they changed their size, shape, and position, while the red continued comparatively unchanged. There were also dark streamers which, at first, I believed to be mere spaces without light, and to be caused by the darkness beyond, but I became ultimately convinced (as far as one could be convinced by appearances so subject to illusion) that they formed a part of the phenomenon itself. These streamers or long brushes could be seen beyond and clear of the luminous portion of the aurora, leaving the normal light of the sky between them and it, and hanging like long horse-tails, or like the fringes of rain seen on the edges of a distant rain cloud; changing their shape and position just as the luminous streamers are seen to do.

The most remarkable part of the phenomenon, however, was the circle of sky, or what may be called the pole of the aurora, to which the streamers converged. It appeared to embrace about from 7° to 10° of space. To an ordinary observer it might have appeared occasionally to shift its position to some extent, but, as far as I could judge during an hour's observations, this was not really the case, flickerings at times covered portions of it, and at other times the whole became faintly luminous; but by marking its position with reference to some small stars, this seemed to me to be unaltered. Most singular were the terminations of the streamers they culminated at this circle, not being undefined or gradually evanescent, but having angular tips far brighter than the portions immediately beneath, the nearest illustration to which I can give is an inverted fish-tail or bats-wing gas burner, except that this gives a feeble light at the point, while the aurora tips were whitest and brightest there, the streamers now fading off, and now becoming brighter and tinged with red as they got to 40° or 50° from the horizon; the tips varied constantly, but preserved the mean distance from the pole or focus of the aurora. The position of this was, as far as I could ascertain without star maps or instruments for observation, about 15° W.N.W. of Vega. The convergence of the beams was not in appearance conical, but dome or cupola shaped; this was, however, in all probability an optical illusion. Whether there was really a convergence or whether the beams were parallel, and the convergence an effect of perspective, can only be decided if some approximative measures of the distance of the streamers be ascertained. It appears to have been at a greater distance from the earth than is usually attributed to aurora borealis, having been seen in different parts of Europe and I believe in America. Doubtless the comparison of these observations will give some parallax or approximation to measurement of the distance. I remember about seven or eight years ago seeing an aurora at Chester, where the flashes appeared close to the observer, so that gleams of light continuous with the streamers could be seen between the houses of the town and myself, like the portions of a rainbow intervening between terrestrial objects and the observer. I tried then to ascertain if there was any reflection or other cause of optical illusion, but could not see it as other than a real effect; I seemed, so to speak, to be in the aurora. The effect on the 25th was very different, and gave me the idea of great distance.

The light was sufficient to enable me to tell the time by my watch easily, but not to read newspaper print.

Between half-past six and seven o'clock it faded away, and at from half-past seven to ten had become an ordinary white aurora, confined to the northern portion of the heavens.

115, Harley Street, Nov. 2 — W. R. GROVE

ON the evening of the 24th ult. the aurora was most beautifully seen here, and if you have space for it, I will add a further spectroscopic observation to those you have already recorded. I found no continuous spectrum, but two of the lines described by your other correspondent.

1. A line in the light green, much reminding one of the line from the larger nebulæ, but more brilliant and with a peculiar flickering in it. This line was well seen in all parts of the sky, but was specially bright in the auroral patches of silver light.

2. A line in the red, very much like the lithium line, but rather more dusky. This line was only well seen in the rosy patches of the aurora, but could be faintly traced wherever the rose tint at all extended.

When the display of rose-coloured light was at its height, the spectrum from the most vividly coloured portion gave the red line very distinct, while the green line still remained bright by its side. I am quite inclined to agree with your correspondent, T.F., in the conjecture that both these lines are due to hydrogen, though (probably through difference in temperature or pressure) they do not quite agree with the lines of that gas as taken from the discharge in a vacuum tube.

The spectroscope was one of Mr. Browning's small direct-vision 5-prism instruments adapted for star purposes.

It may be worthy of note that the belts of Jupiter are highly coloured at the present time. The equatorial zone is of a distinct dark ochre colour, deepening to red brown as it approaches the lower edge (in an inverting telescope); two thin belts above are slate purple, and a darker belt below is of a deep purple, with a faint trace of rose colour.

The planet was thus seen on Nov. 2, at 9 p.m., not far above the horizon, and in bright moonlight, in a 8½ Browning's silvered spectrum with achromatic eye-pieces—144, 305, and 450; best I think with 144.

Guildford, Nov. 5 — J. R. CAPRON

Clouds

I DO not think Prof. Poey's "New Classification of Clouds," published in NATURE of Sept. 8th, does much to advance science. I see no use in any classification of clouds, unless it is based on their mode of formation, and, so far as I see, there are but three ways in which it is possible for clouds to be formed. These are:—

1. The cooling of a mass of air *in situ* by radiation. This forms stratus.

2. The cooling of a mass of air by diminished pressure when it flows in an ascending column. This forms cumulus. A modification of this process is when (according to Espy) sudden expansion takes place above, so as to diminish the pressure through the entire height of a column of air, and, in consequence of the cold due to the diminution of pressure, to produce condensation of vapour throughout the column. This is Espy's explanation of waterspouts.

3. The cooling of a mass of air by coming into contact with a cooler mass of air than itself. This forms cirrus.

Of course these three modes of formation may be modified and combined in endless ways. To mention one of the simplest: A cloud which has begun to form as a cirrus or cumulus, may become a centre from which heat is radiated, and thus go on forming as a stratus.

It is in the highest degree unphilosophical to reject stratus as a species of cloud on the ground that it is "not a cloud properly so called, but a mist or hoar frost." A cloud and a mist do not differ fundamentally.

Prof. Poey is, however, right in saying that cumulus is not a distinct species of cloud. It is only a cloud which (in consequence, I believe, of the loss of electrical tension) has begun to run together into raindrops. — JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

Extreme Seasons

A GREAT deal of speculation has been indulged in to account for the extreme seasons that have prevailed over so large a part of the northern hemisphere during the last few months. In this country, as we are subject to extreme seasons, more particularly as regards the rainfall, the subject is one of peculiar interest. In a paper read before the California Academy of Sciences in February on the subject of our extreme seasons, I brought forward a number of observations to show that these were due to broad polar and equatorial currents occupying large portions of the earth's surface continuously, and without much perpendicular or horizontal disturbance, except at the borders where the currents meet. The facts I then brought forward showed that from October to the middle of February a northerly current prevails over this portion of the American continent, extending from one to two hundred miles to the westward of San Francisco to the eastern edge of the Mississippi valley, whilst a southerly current prevails over the eastern side of the continent as far as the Atlantic. The southerly current to the westward extends uninterruptedly across the whole breadth of the Pacific to the coast of Japan. This same distribution of air currents without much perpendicular or horizontal mixing has apparently continued during the summer, and acounts, I think, satisfactorily for the extreme heat that has marked the continental climates over so large a part of the northern hemisphere. Nor is it surprising that the summer temperature on the continents should be so universally hot, as a horizontal wind, either from the north or from the south, blowing over the land in summer must necessarily be a hot wind. That there is no cosmical cause for this elevated temperature is proved by the extremely low summer temperature prevailing over the Pacific between this place and Japan. The mean temperature, as ascertained by observations made on board the mail steamships between here and Japan was, for Nov. 1869, 70°·2, for January, 62°·9, for May, 1870, 61·9, for July, 65·7, giving a mean of 2°·7 less for May and July than for January and February. The difference in favour of the winter temperature would be still more marked were the coast temperatures eliminated, as they perhaps should be; as these were much above the mean in summer and below the mean in winter. As to the causes that lead to the peculiar distribution of the air currents in certain seasons, I have not the slightest idea, but I think that, admitting the fact, it affords a satisfactory explanation of anomalous temperatures both in winter and summer.

San Francisco, California, Sept. 4 — JAMES BLAKE

Cyclones

CYCLONES are commonly regarded as exceptional phenomena of the atmospheric circulation; and we see in text-books statements as to the seasons of the year at which they are most apt to occur, descriptions of the premonitory signs which herald their approach, and directions to aid ships in avoiding the most dangerous portions of the storm-field. In short, each cyclone is regarded as an exceptional fact, an isolated burst of fury from the old storm-god Hurakan.

The writer has lived all his life on the great highway of cyclones, at Charleston, South Carolina; and from the observations of many years, has been led to conclude that this commonly received view embraces only those cyclones which, on account of their rotatory violence, really do threaten destruction on land and sea; and that consequently it overlooks a most important series of phenomena, which, though they do not so forcibly arrest attention, are even perhaps more significant in a scientific point of view. Though *destructive cyclones* or hurricanes are ortunately rare, cyclones or grand rotatory movements of the atmosphere are, at least on certain portions of the earth's surface, of *every-day occurrence.* In Charleston, Savannah, and along the coast of South Carolina generally, the writer knows from experience that very few, if any, changes of wind are to be observed, but such as are due to the cyclone which happens just then to be passing on its northward journey; and even the apparent exceptions are probably not difficult of explanation.

There is in short an atmospheric "Gulf Stream," whose course, beginning somewhere eastward of the Caribbean Sea, is nearly the same as that of the oceanic "Gulf Stream," and this atmospheric stream is composed of an endless succession of cyclones chasing each other ceaselessly up towards the polar regions, along the track recognised as that of great hurricanes. These cyclones vary within very wide limits both as to velocity of rotation and velocity of translation, as well as in diameter, and all the characters usually ascribed to such atmospheric movements. Many of them exhibit no wind stronger than a pleasant breeze in any part of their field; and a few have so gentle a motion, at least in some parts of their circuit, as will not agitate an ordinary vane; a few are almost wholly without clouds, and very many wholly without rain or lightning. Their effect upon the barometer, when appreciable, must generally be very slight; but in temperature they are usually divided into a warm and a cool semicircle by a line which, in Charleston, lies about S.W. and N.E.

Observation of the winds, during a voyage in a sailing vessel from Charleston to Liverpool, along the course of the Gulf Stream, has satisfied the writer that this *stream* continues unbroken between these two points, and this conclusion was strengthened by repeating these observations between Liverpool and New York. In the former voyage, hardly one of the cyclones which passed over gave more than a stiff breeze, while in the latter, from Cape Clear to Sandy Hook, every cyclone was a storm, and one of them was reported by the captain, on his arrival, as a "hurricane."

The causes of this aërial current, and its connection with the circulation of the whole terrestrial atmosphere, it is not the writer's purpose at present to discuss, though he considers the discussion one of almost *cosmical* importance. But the existence of such a stream is a fact of practical commercial value, in fixing the natural highways for sailing vessels between Liverpool and the Atlantic and Gulf ports of the Southern States. Obviously the short route from Northern Europe to those ports will be that southward along the coast of Europe until reaching the trade winds, then westward to strike the cyclonic current in the neighbourhood of the West Indies, and then, if bound to Atlantic ports, northwestward with that current. When bound, on the contrary, from the Southern ports to Northern Europe, the short route is obviously that along the Gulf Stream, which is also that with the current of the atmospheric stream. To reverse this practice, either way, is deliberately to sail "against wind and tide," if such a stream exist.

The flow of atmospheric *waves* which, in a recent work, has been described as setting from the coast of America towards Europe, though the writer has not seen that work, he believes cannot be other than the flow of cyclones in that portion of the atmospheric stream lying between the vicinity of New York and the English Channel. The cyclonic character is not always distinct, and sometimes is completely masked by the great distance of the observer from the centre, and the consequent apparently rectilinear course of the wind; and the chances of mistake are still further increased when the observer is moving in a course parallel to the path of the centre of the cyclone.

These observations have already been brought to the notice of the Smithsonian Institution, and the writer hopes that something will be done in America towards the comprehensive, precise, and detailed inquiry which the subject demands. But unless attention of the same kind be given in Great Britain, and in the voyages of the Atlantic steamships, the resulting information will remain incomplete.

JOHN M. CRADY
Curator of the Museum of the College of Charleston

Charleston, U.S., Sept. 13

Singing of Swans

IN times ancient and modern "singing of swans" has been reckoned by naturalists among "vulgar errors" and groundless superstitions. It may therefore be interesting to your readers to hear that swans actually do sing, which I can testify by my own personal experience.

From my ninth to my eighteenth year I lived at a place in the west of Iceland, called Gufudalur. It is situated at the end of a small firth, called Gufufjördur, which is so shallow that by low water it is almost dry: the bottom of the firth is covered with sea-grass (marhálmur). In this firth hundreds of swans gather together all the year round, except during the winter months, when the firth is covered with ice; and in the month of August, which is their moulting season, when all of them leave this firth and go to another not far off, called Gilsfjördur. There is no apparent reason for this migration, as Gufufjördur seems in every way as safe and convenient for them during this season as Gilsfjördur. Tradition therefore accounts for this migration in the following manner:—Once upon a time two widows lived one on each side of Gufufjördur. At that time the swans did not go away during the moulting season, and the widows used to gather great quantities of swans' feathers, which are sold in Iceland at the present day at a halfpenny a piece. Thus the swans' feathers formed a considerable item in the income of the two widows. Once, however, one of the widows gathered feathers on a piece of land belonging to the other. A quarrel arose, and one of the widows uttered a spell to the effect that henceforth all the swans should leave Gufufjördur during the moulting season. I will not vouch for the correctness of this tradition, but the fact remains that this migration takes place annually during the above-mentioned season.

During nine years I have heard the singing of the hundreds of swans which gather together in Gufufjördur. In the morning and evening their singing is so loud that it can be heard miles away, and the mountains on both sides ring with the echo of it, for at that time every individual swan seems to join in the chorus. This is, indeed, a wonderful concert. The singing of the swan has not the least resemblance to the cackling of geese or the quacking of ducks. In fact, its voice is unlike the voice of any other bird that I have heard; it seems so clear and full, and has, as it were, a metallic ring in it. When it is calm and clear in the morning or the evening, the swans fly along the valley towards the mountains in parties of seven or nine, sometimes only three; as far as I can remember they are always in odd numbers. During their flight, they either keep in a straight line, one after another, or they form a triangle, leaving an open space in the middle: the foremost swan sometimes emitting single sounds at short intervals. The tradition of the singing of the swan being sweetest just before its death is well known in Iceland; but I am unable either to deny or to confirm this tradition, because I have never been present at the death of a swan.

The swans of Gufufjördur do not lay eggs there, and I am inclined to think that the most of them do not lay eggs at all, for their number in this firth does not seem to be less from the middle of May to the end of July, which is the season during which swans in Iceland lay eggs and bring up their young ones. On the mountains round Gufufjördur there are many small lakes or tarns, and on the banks of those lakes I have seen swans build nests and lay eggs; as a rule there is only one pair on each lake, and, strange to say, these swans sing but very seldom.

JÓN A. HJALTALIN (Icelander).

152, St. Paul's Road, Camden Square, N.W.

State Aid to Science

I REGRET that I should have worded my lecture on Cosmical Physics in such a way as to leave it doubtful how the central establishment I spoke of was to be supported.

Unable myself to conceive the possibility of such an institution being properly supported otherwise than by State aid, I fear I did not sufficiently realise that others might not be of the same opinion. At the time of the establishment of the present Meteorological Office, it was acknowledged that private scientific enterprise cannot be expected to furnish the money requisite to carry on an extensive system of meteorological observations, and the same conclusion equally applies to the other branches of cosmical inquiry.

The most convincing proof of the justice of this conclusion lies in the fact that the British Association, who have hitherto contributed a large portion of their income to advance terrestrial magnetism, find that they cannot do so much longer without detriment to other subjects which have an equal claim upon their liberality. They have therefore resolved to give up their connection with the Kew Observatory after the autumn of 1872. Further proof is surely needless.

B. STEWART

*THE THEORY OF NATURAL SELECTION FROM A MATHEMATICAL POINT OF VIEW**

THE fascinating hypothesis of Darwinism has, within the last few years, so completely taken hold of the scientific mind, both in this country and in Germany, that almost the whole of our rising men of science may be classed as belonging to this school of thought. Probably since the time of Newton no man has had so great an influence over the development of scientific thought as Mr. Darwin; and no one can over-estimate the debt which Science owes to his patient researches and his clear insight into some of the hidden ways of Nature. The advocates of Darwinism have, however, almost invariably failed to recognise that the theory consists of two essentially distinct portions, one of which may be admitted while the other is denied. The first portion is that with which the name of Darwin is popularly associated, although its origination is by no means due to him, namely, the probable ancestry of all forms of living organism from a single or a few original germs; the other portion, and that which we especially owe to his genius, is the theory that the infinite modifications of existing forms owe their origin to a process of Natural Selection from spontaneous variations. These two perfectly distinct hypotheses have generally been so confounded together that those who have attacked or defended the one have also attacked or defended the other. My object in the present paper is to show that, while the former hypothesis may be considered as established, as nearly as it is possible to establish a theory which requires thousands or millions of years for its complete development, the arguments in support of the second hypothesis are far less satisfactory.

The principle that new forms of organic life have been produced by modifications of older nearly-allied forms is by no means a new one; its inherent reasonableness and probability commended it to Lamarck and the author of the "Vestiges of Creation" long before it was elaborated in a more scientific form by Mr. Darwin and Mr. Wallace. It has been opposed, of course, by theologians; but, were it not that the theological mind is inherently averse to the reception of new ideas, it would have been seen that the supposition that the Creative Power works by continuous modification and adaptation of contrivance to end, by a constant exercise of His prerogative, is a far higher tribute to His exalted attributes, than the popular dogma that all living things were created as we now see them by one single gigantic effort, after which the power collapsed, and has never since been exercised. Why should organic life be the one thing in the world not subject to change? The *coup de grace* may be considered to have been given to the anciently received theory by the investigations so ably carried out by Mr. Darwin and Dr. Hooker on the characteristics of Insular Floras. The fact that no island which has been separated from the mainland during recent geological epochs has genera, and scarcely even species, of animals or plants peculiar to itself, while islands which have remained isolated during lengthened geological periods have faunæ and floræ almost entirely peculiar to themselves, is inexplicable on any other hypothesis than that of the gradual differentiation of species by long-continued separation. No more striking instance of this law has been given than that afforded by the East Indian Islands, as shown in Mr. Wallace's "Malay Archipelago." Two great types of animals and plants are found in different regions of the archipelago, the Indo-Malayan and the Australian; and these two types are separated, not by any diversity of climate and soil—not even by any of the wide but shallow channels which indicate recent separation, such as that between Borneo and Sumatra—but by the narrow but very deep channel separating Bali from Lombok, which indicates a lengthened geological separation of two continents at this point.

The hypothesis that the prime agent in all these infinite modifications is the principle of Natural Selection from spontaneous variations, has been recently further illustrated by Mr. Wallace's volume of Essays, "Contributions to the Theory of Natural Selection;" and it is mainly from the illustrations furnished in this work that I propose to derive my arguments as to its inadequacy. In the first place I wish to call attention to the fact which the Duke of Argyll has already acutely pointed out in his "Reign of Law," that the theory does not even attempt to explain the most inexplicable phenomenon in the development of these organic changes, namely, the first commencement of a tendency to variation. The title of Mr. Darwin's famous work, the text-book of the theory, seems to me, indeed, altogether a misnomer: "The *Origin of Species* by means of Natural Selection." Mr. Darwin admits the existence of what he terms a "spontaneous" tendency to variation among the offspring from a common ancestor; this "spontaneous" tendency is the only natural law which can correctly be termed that of the *origin* of species; all that Mr. Darwin and his disciples attempt to explain is the survival and propagation of certain among the diverse forms thus resulting in preference to others. Throughout the whole of Mr. Wallace's volume he appears to have no consciousness that his theory does not go to the root of the matter. When once the tendency to change has set in, there can be no doubt that "Natural Selection," "The Survival of the Fittest," whatever you like to term the principle, is one among many causes which tend to the perpetuation of certain forms. When, however, Mr. Darwin asserts, "I am convinced that Natural Selection has been the main, but not exclusive, means of modification,"* I am by no means prepared to go with him to that extent. Some of Mr. Darwin's disciples go even further, and seem to consider it, in fact, as almost the only means.

There is no phenomenon in Natural History which is more thoroughly relied on by the advocates of Natural Selection as furnishing a decisive argument in favour of their theory, than the one which forms the subject of the longest of the essays in Mr. Wallace's volume, that of Mimicry or Mimetism. I propose, therefore, to occupy the greater part of this paper with an inquiry how far the facts which have been adduced support the conclusions first brought prominently forward by Mr. Bates in his "Naturalist on the Amazons," and more fully elaborated and illustrated by Mr. Wallace. There can be no doubt about the frequent occurrence of "protective resemblances" in the animal kingdom. Certain classes of animals enjoy, from various causes, exceptional immunity from the attacks of their natural enemies. In order to share in these immunities, it is found that other animals, belonging to an entirely different class or order, whilst retaining all the structural characters of their own class, so closely resemble in external features of colour and form particular species of the favoured races as to be readily mistaken for them. How do the advocates of the theory of Natural Selection attempt to account for this superficial resemblance? By the continuous preservation, through countless generations, of those particular individuals which spontaneously approach most nearly to the ultimate forms.

Now, there are two principles admitted or insisted on by every advocate of Darwinism, which it is necessary to bear very clearly in mind in the following argument. The first is, that, in a state of nature those differences which ultimately become specific or generic are brought about by exceedingly slow gradations. And it is obvious that it must be so. For if by chance any strongly abnormal form is produced, even should it survive to generate offspring, which is in itself doubtful, it must necessarily cross with other less abnormal individuals, and its descendants would thus have a tendency to revert towards the parental form. * On this point Mr. Darwin himself says: "It may be doubted whether sudden and great deviations of structure, such as we occasionally see in our domestic productions, are ever permanently propagated in a state of nature." † And again, "Natural Selection always acts with extreme slowness." ‡ The other point which I wish to be borne in mind is, that no change can possibly take place by the process of Natural Selection which is not directly of advantage to the individual. On this point again all the supporters of the hypothesis are agreed. Mr. Darwin distinctly affirms that "only those variations which are in some way profitable, will be preserved or naturally selected;" § and Mr. Wallace even more emphatically speaks of "the principle which Mr. Darwin so earnestly impresses upon us, and which is, indeed, a necessary deduction from the theory of Natural Selection, namely, that none of the definite facts of organic nature, no special organ, *no characteristic form or marking*, no peculiarities of instinct or of habit, no relations between species or between groups of species—can exist, but which must now be or once have been *useful* to the individuals or the races which possess them." ‖

We have, therefore, established at the outset these two data: that the passage from the ordinary to the mimetic form is effected by a number of exceedingly small steps, and

* Paper read before Section D of the British Association, at Liverpool, September 20th, 1870.

* "Origin of Species," 4th ed., p. 6. † Ibid, p. 47. ‡ Ibid, p. 121. § Ibid, p. 131

‖ "Contributions to the Theory of Natural Selection," p. 47.

that every one of these changes must present some advantage to the species which undergoes it. Now let us apply these two principles to the recognised facts of Mimetism; and for this purpose we may take a single instance, one of the most remarkable and best authenticated, recorded by Mr. Bates in his "Naturalist on the Amazons," and more fully in his paper on the "Lepidoptera of the Amazon Valley," in the "Transactions of the Linnean Society." There is in South America a tribe of butterflies of very gaudy colour, the *Heliconidæ*, which appear to enjoy exceptional immunity from the attacks of birds, from the exudation, when attacked, of a nauseous fluid, and are consequently extremely abundant. Another South American genus of Lepidoptera, the *Leptalis*, belongs structurally to an entirely different class, the *Pieridæ*, and the majority of its species differ correspondingly from the *Heliconidæ*, in their size, shape, colour, and manner of flying, being nearly pure white, and of the same family as our common cabbage butterfly. There is, however, one particular species of *Leptalis*, which departs widely in external facies from all its allies, and so closely resembles a species of *Ithomia* belonging to the *Heliconidæ*, as apparently not only to deceive the most experienced entomologists, but even to take in its natural enemies also, and, although perfectly harmless, to share the immunity of the butterfly it simulates. Mr. Bates and Mr. Wallace have both attempted to show, with great ingenuity and plausibility, that this entire change from the normal form to that resembling the *Ithomia* has taken place through the agency of natural selection acting through a long series of generations. I believe, however, on careful examination, the line of argument will be found to break down, and that at its very outset, on the ground that the early stages of the transformation will be perfectly useless for the protection of the species.

Applying the rigid test of mathematical calculation to the problem, I think it may safely be assumed that it would require, at the very lowest calculation, one thousand steps to enable the normal *Leptalis* to pass into its protective form. Mr. Bates indeed assumes that the change may have taken place much more rapidly, but this appears a very unsafe and unsupported deviation from the sounder principle laid down by Darwin and Wallace. It is indeed obvious that any marked variety resulting suddenly must inevitably revert, as already observed, more and more towards the parent type by crossing, unless, indeed, we are to suppose that a pair, male and female, are simultaneously produced with a deviation in exactly the same direction, and that their offspring keeps itself apart, interbreeding only with itself as a separate colony,—an assumption contrary to all experience. At all events, we may safely say that within the historic period no such change has been effected within a vastly larger number of generations, where human agency has not come into play. The next step in my argument is, that the smallest change in the direction of the *Ithomia*, which we can conceive on any hypothesis to be beneficial to the *Leptalis*, is at the very lowest one-fiftieth of the change required to produce perfect resemblance. I believe myself that a very much larger fraction, say one-fourth or one-third, would be practically useless; as I am told by practical entomologists that birds will distinguish with accuracy caterpillars suited for their food from other species scarcely distinguishable to our eyes, which are not so suitable. For the sake of argument, however, I will suppose that a change to the extent of one-fiftieth is beneficial to that small extent *after which* natural selection may begin to come into play. Mr. Wallace, indeed, argues that an infinitesimal and inappreciable distinction may make the difference of a slightly longer span of life being allowed to the butterfly, to lay its eggs in safety; but this is a deductive piece of reasoning derived from the theory, because necessary to it, and not inductive observation from nature; and I altogether decline to be carried further, for the sake of the theory, than the limit I have indicated. Suppose a parallel instance: that our common brown owl has a *penchant* for mice, while moles are abhorrent to its palate; is it conceivable that, supposing a mouse was born approaching a mole by the one-hundredth part in external appearance, say with feet a fraction of a line broader, or eyes slightly deeper set, the shortest-sighted of owls would for a moment mistake *Mus* for *Talpa*? Or, a still more parallel instance: suppose a blue-bottle fly were born blessed with a slightly narrower waist, or a faint band of yellow on its body, will any one maintain that it stands the least chance of escape from destruction by those birds which do not feed on wasps? And no one who has examined Mr. Bates's or Mr. Trimen's beautiful drawings, or, still better, the insects themselves, will say that I have exaggerated the extent of the passage from the normal to the imitative *Leptalis*.

If, therefore, this reasoning is sound, one thousand steps being necessary to effect this change in external appearance, and one-fiftieth of the whole change, or twenty steps, being the smallest amount that is really profitable to the animal, it follows that the first twenty steps of the transformation are not due to natural selection, but must have taken place by an accumulation of chances. Let us investigate the value of this chance. Suppose there are twenty different ways in which a *Leptalis* may vary, one only of these being in the direction ultimately required, the chance of any individual producing a descendant which will take its place in the succeeding generation varying in the required direction, is $\frac{1}{20}$; the chance of this operation being repeated in the same direction in the second generation is $\frac{1}{20^2}$ or $\frac{1}{400}$; the chance of this occurring for *ten* successive generations (instead of twenty, as I have assumed above) is $\frac{1}{20^{10}}$, or about one in ten billions. Now another factor comes into the calculation, and that is the number of individuals among which this chance is distributed. Mr. Bates and Mr. Wallace agree in stating that both in South America and in the Malay Archipelago the imitative species are always confined to a limited area, and are always very scarce compared with the imitated species. We will assume that the number of individuals of the imitative *Leptalis* existing at any one time is one million; the chance of there being among these million a single individual approaching the *Ithomia* to the extent of one-hundredth is $\frac{1000000}{10000000000000}$, or the chance against it is ten million to one.

It will be seen that in the above calculation I have endeavoured to throw every advantage into the scale of the natural selectionist. I believe myself, and I think most naturalists will agree with me, that vastly more than a thousand generations, each characterised by a small change, must be conceded; and that, on the other hand, a change to the extent of even greatly more than one-fiftieth would be absolutely useless. This idea receives great confirmation from observing the most wonderful identity of the marking in the mimicked and mimicker. If a rough imitation is so useful, it must be a mere freak of Nature to produce so absolute an identity, and we are landed in the dilemma that the *last* stages are comparatively useless. If, again, I had carried on the calculation to $\frac{1}{20^{20}}$ instead of $\frac{1}{20^{10}}$, it would have been difficult to have stated the result in figures; and if, on the other hand, it is objected that a million is too low an estimate of the number of individuals existing at one time, and a hundred million or a thousand million is substituted (an altogether inconceivable estimate for a rare conspicuous butterfly limited to a small area *), the result will not be materially affected. For, supposing the chance is reduced from one in ten million to one in ten thousand—and it is said that the world has existed quite long enough to give a fair chance of this having occurred once—it is not a solitary instance that we have. Mr. Bates states that, in a comparatively small area, several distinct instances of such perfect mimicry occur; Mr. Wallace has a store in the Malay Archipelago; Mr. Trimen records several of wonderful beauty and exactness in South Africa; and the more attention is turned to the subject, the more numerous do instances of mimicry become.

I have left out of account altogether those still more remarkable instances, which are even more difficult to explain on the theory of natural selection (as the number of steps must be infinitely greater), in which animals not only imitate others belonging to entirely different natural orders, as Diptera mimicking Hymenoptera, and caterpillars snakes, but where they resemble inanimate objects. The weird and uncanny resemblance of the *Phasmata* and *Mantides* to dry leaves and sticks has long been known: not only is the veining of the leaves accurately reproduced, but the attacks of parasitic fungi are simulated; and Mr. Wallace records instances of larvæ bearing the most minute resemblance to the droppings of birds, and spiders to the axillary buds of plants. Through what countless generations must these transformations have been effected! and by what mathematical formula could we express the chance against their occurrence, if

* The latter number would give 150 individuals per acre over an area 100 miles square, or 50 per acre for an area as large as Ireland.

natural selection only had been at work in their production? The difficulties in the way of the natural selection explanation are also materially increased when we find, as is often the case, that it is one sex only (the female) which undergoes these mimetic changes, and that the changes have to take place simultaneously in the direction of colour, size, form, and habit.

It may now fairly be asked, if the principle of natural selection is abandoned as the main cause of these wonderful modifications, what other theory can be substituted in its place? I do not know that the objector to a theory is always bound to provide another theory as a substitute. Mr. Darwin, in his "Variation of Animals and Plants under Domestication," quotes with well-deserved approval Whewell's aphorism, that "Hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error." Mr. Darwin's and Mr. Wallace's hypothesis of natural selection has been of signal service to science; but if this hypothesis has been too rashly handled and too widely applied, it may be equally serviceable to point out its incompleteness or its error, as the first step to a still more scientific explanation. In the following remarks, I merely wish to call the attention of naturalists to one or two points which I think have almost been lost sight of in the discussion.

I have already adverted to the inaccuracy of the title of Mr. Darwin's great work, "The *Origin of Species* by means of Natural Selection." The opponents of Darwinism, even so acute a reasoner as the Duke of Argyll, appear to see no alternative between the theory that species have arisen through the agency of external causes, and the theory that species have remained immutable since their creation. I can accept no such alternative. Indeed we may say that external influences *cannot* be the primary cause of the transmutation of species. The utmost claimed by the theory of natural selection is, that it selects the fittest from already existing so-called "spontaneous" varieties. Every page of Mr. Darwin's work teems with reference to this pre-existing tendency to variation, with respect to which he says: "Our ignorance of the laws of variation is profound." Mr. Bates, when speaking on the subject of mimicry, makes the following very remarkable admission:—"It would seem as though our *Leptalis naturally produced* simple varieties of a nature to resemble *Ithomiæ*."* By a careful study of the context, I can only conclude that Mr. Bates means the same thing by his "natural" varieties as Mr. Darwin does by his "spontaneous" variations, namely, an innate tendency to vary *not caused by natural selection*, but on which tendency natural selection operates, and without which it would be perfectly inoperative. The use of the term "spontaneous" is open to objection from a philosophical point of view. It either means that the phenomena in question are subject to no law, or that they are the result of some law with which we are unacquainted. The former hypothesis will probably be rejected by every scientific naturalist, and must be utterly abhorrent to the believer in a "Reign of Law." This tendency to variation in the offspring meets us on every side in our investigation of nature. Every gardener knows how uncertain is the produce of seeds compared with the produce of buds or offshoots from the same plant. The ordinary mode of obtaining new varieties of strawberries or other fruits is from seeds. An endless variety of the commonest florist's flowers is produced by sowing seeds from the same capsule. Of the laws of this variation we are, as Mr. Darwin says, "profoundly ignorant;" but it does not follow that a patient interrogation of nature pursued in the true Darwinian spirit, may not reveal to us something of these laws. Of one thing we are certain, that natural selection here plays no part. If then we must admit that the first beginning of change takes place without the operation of this principle, why should we claim for it the main, almost the exclusive agency, in the changes which follow? Some other principle, at present unknown to us, originates these variations; what right have we to say that this principle, whatever it may be, then ceases to act, instead of being the main agent in all the other subsequent changes?

But are we limited to negative evidence in tracing the transmutations of species mainly to some unknown internal law? A single sentence in Mr. Wallace's Chapter on Mimicry seems to me pregnant with results for the future inquirer. He incidentally remarks how frequently it is the case that, when mimicry has once set in by the action of natural selection, new habits and instincts come into play to assist in the mimicry. It does not, however, appear to occur to Mr. Wallace to trace any connection between the instinct and the mimicry. The connection will be found, I believe, to be very close. Passing by for the moment any definition of instinct, let us trace its range in the organised world. From the whole vegetable kingdom it is conspicuous by its absence. In the lowest classes of the animal kingdom, the Protozoa and Cœlenterata, it is found, if at all, in a very low form; and though there is a popular superstition that oysters may be crossed in love, yet we cannot attribute to the Mollusca as a class any strong development of the instinctive faculty. When, however, we come to the Articulata, and especially to the Insecta and closely allied Arachnida, we meet at once with developments of instinct rivalling, if not exceeding in perfectness, those found in the highest forms of animal life. In the lower orders of Vertebrata again, the Pisces and Reptilia, we apparently come to a retrogression in the instinctive faculty, which is once more strongly developed in the Aves and Mammalia. Now let us compare this with what is known of Mimicry. From the vegetable kingdom it is absent. There are, it is true, resemblances, and resemblances of the most wonderful and perfect kind, in the marking and venation of the leaves of plant belonging to entirely different natural orders, equal in extraordinary closeness to those of which I have spoken in the animal kingdom; but these are in no sense mimetic or protective. Mere protective resemblances of colour I consider of far less importance than of form or habit; since colour may unquestionably be affected directly by the external circumstances of light, &c., and varies "spontaneously" in both the animal and vegetable kingdom to a far greater extent than does form. In the lowest forms of animal life we have no well-authenticated instances of mimetism, the most striking among the Mollusca with which I am acquainted is one pointed out to me by Mr. G. S. Brady in the beautiful *Lima hians*.* But when we come to insects, we find protective resemblances of the most extraordinary kind, in marking, in form, in habit, presented to us on every side. Among fishes and reptiles the principle appears to be again comparatively in abeyance, and to be once more strongly developed in birds. The parallelism is indeed almost complete. In short, the power of mimetism, as far as is known at present, runs almost *pari passu* with the development of the nervous system.

But what is instinct? Modern naturalists are pretty well agreed in abandoning the old distinction in kind between reason and instinct, and in considering the nest-building instinct of birds and the cell-constructing instinct of bees, as but a lower form of the same faculty which we call reason in ourselves. It is admitted that this instinct teaches the bee which flowers to rifle for its honey, and even to modify its habits in accordance with the circumstances in which it is placed; but, according to the prevalent theory, it has no power to modify its proboscis so as to enable it to obtain the honey from the flower, or to modify its wings to suit to its new habit. In short its own body is almost the only thing over which the animal has no power. To me such a restriction appears to be unphilosophical. I cannot but believe in the existence of an unconscious Organising Intelligence, an idea which Mr. J. J. Murphy has ably and logically advocated in his "Habit and Intelligence." And if this inherent innate power of change is admitted, it at once harmonises the tendency to variation which exists in all created beings, with the perpetuation of those forms best adapted to resist the struggle of life, and lends to natural selection the assistance of a fellow-worker far more powerful and of more universal operation.

A powerful argument in favour of this view may be drawn from Mr. Wallace's volume. Every reader of that book must have been struck with the remarkable manner in which he completely abandons and casts aside his own theory when he comes to treat of man. Natural selection is amply sufficient to account for all the other transmutations in the animal kingdom; only give time enough, and it is competent to develop the elephant out of the *Amœba*—the one step in the animal creation which is beyond its power is that from the ape to man; all the infinite forms of the brute creation have resulted from this principle,—to produce the different races of mankind some other power is needed. In a singularly able review of this work in the *Archives des Sciences Physiques et Naturelles*, M. Claparède, of Geneva, points out with great acumen the singular inconsistency of this reasoning; and shows how great a want of faith in his own principle it betrays on the part of its author. Mr. Wallace's line of argument is very interesting. We may take only a single instance. Man is the only terrestrial mammal with a bare hairless back. All savage nations feel the want of a covering to their back; in cold countries to protect them from the cold, in

* Transactions of the Linnean Society, vol. xxiii., p. 512.

* See NATURE, Vol. ii., p. 376.

hot countries to protect them from the heat of the sun. It is impossible to conceive, therefore, that this absence of covering was ever directly beneficial to the race or the individual; and hence it cannot have been produced by the operation of natural selection; but must have been in some way connected with those reasoning powers which lead to the construction of clothing and dwellings on which his civilisation so largely depends. Mr. Wallace, however, appears to forget that he had previously stated his conclusion that "those great modifications of structure and of external form which resulted in the development of man out of some lower type of animal, must have occurred *before* his intellect had raised him above the condition of the brutes."* This principle, therefore, whatever it may be, other than natural selection, which produced man's bare back, must have been in operation before the intellect of man was developed. This strange inconsistency of Mr. Wallace's appears to result from the fact that he is unable to shut his eyes to the inevitable conclusion that the development of man from the ape, and the production of the different races of mankind, have not resulted from the operation of natural selection, pure and simple, but that this principle has been powerfully assisted by man's reasoning faculties. This reasoning seems to me perfectly sound and inevitable, admitting, for the sake of argument, Mr. Wallace's hypothesis, that man *is* descended from the apes; but, if we consistently believe in the action of general laws which govern the whole of animated nature, we must carry the argument back a step further. Reason is but a higher development of instinct. If man's reason has assisted him so to modify his body as to adapt himself to the circumstances with which he is surrounded, we are unable to bring forward any valid argument why the instinct of animals should not also assist them to modify their bodies, by slow and gradual degrees, so as to adapt them to the circumstances with which they are surrounded.

In the essay alluded to above, M. Claparède, himself one of the few genuine Darwinians among French writers, points out the dangerous and unscientific manner in which the theory of natural selection is made, in the hands of its too zealous advocates, to explain phenomena which are probably due to other causes. The discovery of this law marked an era in the history of natural science, and gave a wonderful impulse to original research. The danger now is that the law will be pressed into services which have no claim upon it; and that, in the hands of injudicious partisans, it will become a hindrance rather than an aid to science, by closing the door against further investigations into other laws which lie behind it. To claim for Natural Selection the main agency in the creation of the countless forms of organic life with which we are surrounded, is straining it beyond its strength. An era of equal importance will be marked by the discovery of the law which regulates the tendency to variation which must necessarily underlie natural selection.

The argument of "design" was undoubtedly pushed by pre-Darwinian writers to too great an extent. The most recent phase of Darwinianism, however, is a complete denial of the existence of design in Nature. It is the carrying into Natural Science of the Hobbesian principle of Self-love. Every individual and every species exists for its own advantage only, and has no *raison d'être* except its own welfare. To my mind the beauties and wonders of Nature seem, on the other hand, to teach a different lesson, that,

> All are but parts of one stupendous whole,
> Whose body Nature is, and God the soul;

that there are laws, albeit almost unknown to us—not laws merely of external circumstance, but laws of internal growth and structure,—which actively modify each individual organism, not only for its own advantage in the struggle for life, but for the higher end of subordinating every individual existence to the good of the whole.

ALFRED W. BENNETT

* "Contributions to the Theory of Natural Selection," p. 319.

THE PROFESSORSHIP OF NATURAL HISTORY, QUEEN'S COLLEGE, BELFAST

IN a late number we announced that Professor Wyville Thomson, of the Queen's College, Belfast, had been appointed by the Crown to the Professorship of Natural History in the University of Edinburgh. This will necessitate the resignation by Professor Thomson of his chair in the Queen's College, Belfast, a resignation which we may presume will be made before the commencement of the next term, and a resignation in which some of our readers and many of our men of science will take an interest, for the places of honour or emolument open to the student of Natural Science in this country are so very few, that there is naturally much excitement when one of the few is to be filled up. Already we hear of a whole host of young and meritorious workers setting their faces towards the city that boasts to be the Athens of the North of Ireland. The mere mention of the names of Dr. Cunningham, who in the Straits of Magellan earned his Natural Science spurs so well, of Mr. E. Ray Lankester, whose numerous papers show an intimate acquaintance with zoology, of Dr. Macalister, whose comparative anatomy memoirs are so well-known, or of Dr. Traquair, whose papers on fossil fish and on the skull of recent Pleuronectidæ, are of high merit, not to name others, will show that the post of Professor of Natural History in the Queen's College, Belfast, will be contested for by a little army of well-educated and accomplished gentlemen, the selection of any one of whom would reflect credit on the College.

But a rumour reaches us that there may be no election to the Professorship after all—that the spirit of economy is to annihilate the spirit of competition; that, in order that the Government of this great country may save certain paltry trifling possibilities of pension, it is their intention to translate to Belfast one of the four Professors of Natural Science in the Queen's Colleges of Cork and Galway. It is necessary to explain how this can be done. Each of the Queen's Colleges had originally a Professor of Geology and Mineralogy, and a Professor of Zoology and Botany. Their income was that of a junior assistant in the British Museum, and for common decency's sake, it was found necessary to raise it; this was done on the condition that each of the Professors undertook to lecture on the subjects at the time lectured on by his colleague, on the death or resignation of that colleague, without further increase of pay. So when Prof. Dickie, who was Professor of Botany and Zoology in the Queen's College, Belfast, resigned, on his removal to Aberdeen, Prof. Thomson had to lecture in zoology and botany, in addition to his own subjects of geology and mineralogy. Thus it happens that should the Government confer the vacant Belfast chair on one of the four existing Professors of Natural Science in the other two Queen's Colleges, his post in the college which he leaves will be filled up by his colleague, and the Crown will have to deal in the matter of pension, &c., with but four persons instead of with five, as they will have if they appoint a candidate who is not one of these four Professors to the vacant post. Nor can the Crown confer this Professorship on one of the present Professors, and then fill up the place thus left vacant by a new appointment, because, although the yearly salary of the colleague of the Professor thus elected will not be increased thereby, yet his fees, to a slight extent, will; and so, to break the bargain made, would be to the detriment of the individual—a thing, we believe, no Government would do. But why, we ask, should they, for a paltry saving, do detriment to the cause of Science in this country—courted when she is needed—kept at more than arm's length when it is imagined she may be done without? Science is but badly cared for in our country, and we here allude to the above facts for the purpose of urging those to whose care this appointment falls, to forget, for the once, all considerations except those for the good of the College, and to quicken the already expanding life of the Queen's University in Ireland by the infusion of fresh blood into this one of its Colleges.

It is in the interest of Science that we write, not in the interest of candidates, one of whose names we would not mention above another.

X

PITCHER PLANTS

DIFFERENCE of opinion has been expressed as to the nature and use of the liquid found in the so-called pitchers of various plants, such as Nepenthes, Rafflesia, and certain Orchidaceæ. The popular idea that these curious receptacles collect pure water for the refreshment of the thirsty in arid places, would seem to be set at rest, by a consideration of the fact that these plants grow in moist and marshy places. There would seem, moreover, to be some improbability that plants should secrete pure water.

In this country, where these plants are grown under exceptional conditions, there is some difficulty in settling these questions experimentally. In such cases, extraneous water often finds its way into the pitchers, so that several ounces may frequently be gathered from a single receptacle of Nepenthes, the greater part of which is accidental.

In August last I had an opportunity of collecting the liquid from two flowers of Coryanthes, one of the Orchidaceæ, which had just opened, in one of the well-known stove-houses of Mr. Wilson Saunders.

Though the quantity collected was small, amounting only to about three cubic centimetres, or 1·18 cubic inches, an examination showed the following properties :—

Clear and somewhat glutinous in consistence. Possessed of a high refractive power, and a specific gravity of 1·062.

Odour pleasant but faint, becoming more marked by a gentle heat. Neutral to test papers. Becoming milky, by concentration on the water-bath, it finally yielded a transparent gum, insoluble in alcohol.

Oxalates produced no precipitate of lime, but basic lead acetate gave a curdy reaction. Concentrated hot sulphuric acid blackened the liquid.

Although the taste was not acrid, the mawkish flavour would render it quite unpotable.

This examination therefore proved the liquid to be something else than pure water.

100 parts of liquid contained :—

Water and volatile oils . .	98·51
Non-volatile residue. . . .	1·49
	100·00

G. B. BUCKTON

SPECTROSCOPIC OBSERVATIONS OF THE SUN

PROF. C. A. YOUNG has obligingly sent me an account of his recent work, which is very rich in promise, as he tells me that he has now the dispersive power of 13 prisms of heavy flint, each with an angle of 55°. It is now some time ago since I announced to the Royal Society that over spots prominences, built up of different vapours, were sometimes observable by means of their lines, *bright and thin*, overlying the thick absorption lines in the spot spectra. This observation is, I hold, a clear proof of the truth of the theory put forward by Dr. Frankland and myself, namely, that changes in spectra, notably the thickening of the lines, are due to pressure, and not to temperature; for according to the theory of exchanges, the bright prominence must be hotter than the absorbing vapour which underlies it, and still the lines are thinner.

Dr. Young has now observed these phenomena with exactly the same result. He writes to Professor Morton :—

"I write to inform you that last Thursday, Sept. 22, about 11 A.M. Hanover mean time, I was so fortunate as to see the sodium lines D_1 and D_2, reversed in the spectrum of the umbra of a large spot near the eastern limb of the sun. At the same time the C and F lines were also reversed, but with the great dispersive power of my new spectroscope I see this so often in the solar spots, that it has ceased to be remarkable.

"The figure gives the appearance of the sodium lines. In the umbra of the spot the D_3 line was not visible, but in the penumbra was plainly seen, as a dark shade, represented in the figure.

"I am not aware that this reversal of the sodium lines in a spot spectrum has ever been observed before; its reversal in the spectra of prominences is not very unusual. A small prominence on the western limb of the sun, which was visible the same forenoon, presented all the following bright lines, viz.: C, D_1, D_2, D_3, 1474·, b_1, b_2, b_4, 1989·5, 2001·5, 2031·, F, 2581·5, 2796·, and h; 15 in all.

"In the spot spectrum the magnesium lines b_1, b_2, and b_4 were not reversed, but while the shade which accompanies the lines was perceptibly widened, the central black line itself was thinned and lightened."

Further, Prof. Young has succeeded in obtaining photographs of protuberances on the sun's limb, of which he has been good enough to forward me a specimen. They were obtained by attaching a small camera to the eye-piece of the telescope and opening the slit somewhat widely, using the hydrogen line near G. He adds :—"As a picture, the little thing amounts to nothing, because the unsteadiness of the air and the maladjustment of the polar axis of the equatorial caused the image to shift its place slightly during the long exposure of three-and-a-half minutes which was required, thus destroying all the details. Still, the double-headed form of the prominence is evident, and the possibility of taking such photographs is established."

In a letter to myself Prof. Young adds :—"I should not have published so imperfect a success were it not that my engagement as a member of Prof. Winlock's eclipse party prevents me from following up the matter at present. The experiments were tried on the 28th, and on the 30th the equatorial was taken down to be packed up and sent to the rendezvous, at Alvan Clark's factory, where all the instruments are collected and put in order previous to sailing." J. NORMAN LOCKYER

NOTES

SINCE our last issue the Joint Committee of the Royal and Royal Astronomical Societies and the Council of the British Association have met to consider the question of the Eclipse Expedition, and in consequence of these meetings Mr. Gladstone has been asked to receive a joint deputation to urge upon the Government the importance of the proposed expedition. The Joint Committee have appointed the Presidents of the Royal and Royal Astronomical Societies, the Astronomer Royal, and Mr. Lockyer to plead its cause; while the Council of the British Association will be represented by the President and officers of the Association, Sir John Lubbock, M.P., and Dr. Lyon Playfair, M.P. Up to the time of our going to press, however, no time had been fixed for the deputation to wait upon the Prime Minister.

THE medals in the gift of the Royal Society have this year been awarded as follows :—The Copley medal to Dr. Joule; the Rumford medal to M. Descloiseaux; and the Royal medals to Prof. W. H. Miller and Mr. W. Davidson.

PROF. SIMON NEWCOMB has arrived in this country from the United States Naval Observatory. His mission among us is to examine and report on the great Newall telescope. He will then proceed to Gibraltar to observe the approaching eclipse.

ALL members of the British Association will be concerned to learn that Dr. Hirst feels called upon by the pressure of his new duties to resign the General Secretaryship of the Association, an honorary post which he has long filled with the greatest advantage to Science.

AMONG the mass of correspondence which has recently reached us are several interesting letters on early notices of the Aurora, for some of which we hope to find room next week.

WE rejoice to see several men of science, with Professor Huxley at their head, coming forward as candidates for the London School Board. We shall be glad to be informed if this example is followed elsewhere.

WE read in the *Echo* that the Electric Telegraph has been put to a new use in Canada. At Mimouski, when the late earthquake came upon them, they sent at once to Quebec, a distance of 200 miles, to ask, "How do you feel?" While the operator there was at his work the shock arrived. He at once sent to Montreal, about 200 miles further on, to ask if they had felt it. They had just time to say "No" before the earthquake came up.

THE new buildings of the Glasgow University were formally opened on Monday last. The proceedings were conducted within the Hunterian Museum. The Duke of Montrose, Chancellor of the University, presided, and congratulated the principal professors upon the success which had attended their efforts, and that they had lived to see the opening day of the new University. The merchants of Glasgow had made princely fortunes, which was creditable to their talents and their industry; but it was still more creditable to them that they had made such a use of their riches as to enable this noble building to be erected for the education of the rising generation. After Prof. Lushington had delivered an address, Mr. A. Orr Ewing, M.P., stated that from subscriptions and from Government 254,000*l.* had been obtained, and 117,000*l.* had been received for the ground upon which the old college stood. Everything in connection with the new building was paid. Of the 150,000*l.* in public subscriptions, Glasgow had given nearly all. The various classes met on Tuesday.

WE believe that Mr. W. Spottiswoode will succeed the late Dr. W. A. Miller as Treasurer of the Royal Society.

ON the 15th inst., Dr. Grey will read a paper at the Statistical Society "On the Claims of Science to Public Recognition and Support." We are glad that the attention of such a powerful body as the Statistical Society is to be so authoritatively drawn to such an important subject.

THE Polytechnic School of Zürich has been suddenly deprived of its chemical teachers. In addition to the death of Prof. Bolley, which we have already recorded, Prof. Hädeler, giving way to the demands of failing health, has relinquished his chair of Pure Chemistry.

IT was recently determined to erect a statue to Prof. Morse, the eminent American electrician and inventor of the telegraph which bears his name, and for this purpose subscriptions were opened in the States. The full amount has been very speedily subscribed, and the erection of a marble statue is to be at once proceeded with. It is to stand in the Central Park, New York, a suitable position having been willingly granted by the Commissioners.

IT is a question of some interest and curiosity how to compare the forces of steam and gunpowder. The following calculation, each step of which can be easily followed, may therefore be acceptable to our readers. The force exerted by an exploding charge of powder in a gun requires for its calculation two considerations, *viz.* the amount of force given to the shot, and the time in which that force is imparted. Taking as our example the 300-pounder Woolwich gun, the first element of the calculation is as follows:—One steam horse lifts 33,000lb. one foot in one minute, or 550lb. in one second. The 300lb. shot leaves the muzzle of the gun at the rate of 1,300 feet per second. Let H represent the force in horse-power in the shot, then, by the well-known equations $W = \frac{1}{2} m v^2$, and $m = \frac{w}{g}$,

$$H \times 550 = \frac{300}{2g} \times (1{,}300)^2 = \frac{300}{64} \times 1{,}690{,}000,\ H = 14{,}403{\cdot}409$$

The force, *i.e.* the work in the shot, is therefore measured by 14,403·409 horse power. That is, it would require that amount of horse power acting for the space of one second to give to a 300lb. shot the velocity with which it is driven from the gun by the explosion of the charge (43lb. of gunpowder). But this is done by the powder during that minute portion of a second in which the shot moves down the bore of the gun. The results of Captain Noble's chronoscope make it appear that this time is somewhat less than one two-hundredth part of a second. The force exerted by the powder must, therefore, be 14,403·409 × 200 : that is 2,880,681·8 horse power. Some comparatively small considerations, as the friction of the shot in the gun, are neglected in this calculation. It does not, therefore, err in excess, and is sufficient to give some idea of the enormous force exerted. Nor is it uniformly exerted throughout the whole time of the shot's movement in the gun, nor does the calculation above made necessarily give the greatest intensity of action. This much we may state, that at some *instant* during the 200th part of a second in the case taken, the force of the expanding gas was to be measured by nearly three million horse power.

THE unpublished manuscripts left by the late Sir James Simpson on the important subject of Hospitalism have been confided to the care of Mr. Lawson Tait, of Birmingham, for completion and editing.

IN addition to the outbreak of the long-quiescent volcano of Tongariro in New Zealand, to which we lately called attention, we hear from American sources that "a volcano, near San Rafael Valley, Lower California, which has been in a dormant state for years, has commenced a violent eruption, emitting columns of smoke and scattering ashes and cinders for miles around its base. St. Diego telegrams say it is plainly visible from there."

THE islands in the Sea of Okotsch, off the north-east coast of Asia, are being visited by ships in search of seals. A vessel recently arrived at San Francisco from Jones Island, between lat. 52° and 53° and long. 145° and 146°, with 11,500 seal-skins on board, and another vessel with a still larger cargo is expected. This island is half a mile in circumference, and is uninhabited, and is remarkable for the great abundance of seals.

CONSIDERING the many uses to which india-rubber is now applied, one of the most important being its recognised superiority over gutta-percha for deep-sea telegraphs, and remembering the fears entertained some time back of the probability of a decrease in the supply, owing to the exhaustion of the forests consequent upon the immense demand, it is gratifying to learn that the quantity of rubber exported from Para during the past year exceeded that of the previous year by 22,731 arrobas (an arroba is equal to about 25½lb.), and by 241,250*l.* in market value. It is true that the more accessible rubber districts are becoming exhausted, and give a smaller yield than in former years, but the rubber-bearing country is so extensive, and its rivers so incompletely explored, that the newly-discovered sources will, no doubt, more than make up any deficiency arising from the exhaustion of the old. It is difficult, however, to obtain accurate or reliable information from those engaged in the collecting of the rubber. The continued demand for rubber, which is collected with comparatively little labour, and requires but little skill and experience, absorbs all the attention of the natives over other products, and the constant rise in its value so stimulates its production, that it is more than probable there will be for some years to come an annual increase in the quantity imported of at least ten per cent.

In the State of Santander, Colombia, one of the most important sources of state revenue is the manufacture of so-called straw hats. These hats are chiefly made in the Bucaramanga district, and are of a very fine and white material, but still not equal to the celebrated Panama hats, as they soon become dirty, owing to the plait not being drawn sufficiently tight. The weekly sale of hats in the above district averages from 600 to 800 dozen, the lowest quality of which fetch about 1*l.* 4*s.* the dozen, and the finest quality often realising as much as 1*l.* 10*s.* to 1*l.* 16*s.* each. These latter are principally made near Zapatoca, which also has a large trade, bnt not to the same extent as Bucaramanga. They are for the most part exported to Havana and the United States, where there used to be a great demand for them, but now the trade seems to have somewhat diminished. In the first half of last year 250 cargoes were exported, and as each cargo contains 1,200 hats, some idea may be formed of the large quantities manufactured. The above is gathered from a report on the industrial resources of the State of Santander. The hats referred to are described as being made of a white kind of "straw." We have not seen any of the actual material, but think, in all probability, the so-called straw may be the split leaves of some palm, perhaps *Thrinax argentea*, which was imported in considerable quantities some years ago, and manufactured at St. Alban's into "chip" hats.

The astronomical and meteorological observations made at the United States' Naval Observatory during 1867 have just been published in a large quarto volume. There is an appendix of reports on the observations of the total eclipse of the sun of Aug. 7, 1869, the various phases of which are beautifully illustrated by chromo-lithographs, and the various instruments made use of are particularly described.

A piece of meteoric iron fell on the 18th of September last near Santa Clara, California, in the barn-yard of Michael Sanor, where it set the straw and *débris* on fire. When picked up it was exceedingly hot, and hissed when thrown into water. The meteorite which fell in October 1869 in Stewart County, Georgia, will be analysed, and a full description of its fall and accompanying phenomena given in the next number of *Silliman's Journal*, by Prof. Willet.

In consequence of the depression caused by the war, the private enterprise for working coal near Manita in Asia Minor has been postponed, but the Government is taking measures at length for working the coal formations near Constantinople for the artillery factories, carried on under the direction of Messrs. Siemens. This coal was known and worked seventy years ago, as described in old books of travels, and then abandoned. The Heraklea coal mines are going on slowly.

The Government of India is now cooling in its reduction fit, consequent on the supposed deficiency in the Budget, and is paying more attention to the discharge of its duties. The appointment of an assistant curator for the Geological Museum of India has been authorised, and an increase allowed for the literary purposes of the department.

It is reported from that interesting region, the Argentine States, that two enormous fossil skeletons have lately been discovered at Fray Bentos, but they are so large as to be beyond the local means of transporting them with safety.

A great earthquake has taken place in the town of Santo Tomas, the capital, and in the district, of Chumbivilcas, department of Cuzco, Peru. On the 10th of July, about 1.30 P.M., it was felt as the people were coming out from mass. The upper part of the two side towers of the church fell in, the vaulted roof was rent, as well as the walls, and much more damage done. Other damage also took place in the town and district. On that same day the River Santo Tomas, which runs abo[illegible] a league to the west of the town, suddenly rose and overfl[illegible]d its banks, producing great destruction to the farms, horses, and cattle, but no lives were lost. It was afterwards ascertained that by force of the shock the waters of the Lake Quenacocha had broken out into the river. The lake is about 20 miles in circumference, and lies at the base of the western chain of the Andes. In the town of Ccolqquemarca, the tower and body of the church were injured, and many houses overthrown. Up to the 12th the earthquakes were felt every few minutes, at least every quarter of an hour, and the river was still flooded. On the 13th August there was a slight earthquake in the evening at Lima. On the 27th August an earthquake was felt in Chile—we presume at Valparaiso.

Efforts are being made to obtain improved instruments and to extend the observations at the Meteorological Observatory at Durban, in Natal.

In continuation of our comments on the caution necessary in dealing with scientific statements in the Indian press, we may advert to a case in which the Welsh fasting-girl is eclipsed. A correspondent of the *Indian Daily News* at Nuddea affirms that a Sudra woman, forty-five years old, has abstained from food twenty-five years. She bathes twice a day. Of course it is to be added that many natives have satisfied themselves of the correctness of this statement, as many Welshmen did with regard to their fasting-girl.

The *Inverness Courier* of October 13 states that a number of hollow glass globes of a dark colour, and measuring about eighteen inches in circumference, have lately been found washed ashore at various parts of the coast on the west side of the Island of Lewis. They are hermetically sealed, and have certain characters, such as IV or VI impressed on the sealed part. Some of them are partially filled with a colourless liquid. The question is asked: "Have these been used for some experiment made for the purpose of ascertaining the course of some ocean current?" Can any of our readers throw light on this subject?

In a recent number of the *Pharmaceutical Journal* a paper appears, by Mr. Cooke, on the Guarana, the seeds of a tree termed the *Paulina sorbilis*, belonging to the order *Sapindaceæ*, and abundant in the province of the Amazonas. The fruit is scarcely as large as a walnut, and contains five or six seeds, which are roasted, then mixed with water and moulded into a cylindrical form resembling a large sausage, and finally dried in an oven. Before being used it is grated into a powder, very like powdered cacao in appearance. Two spoonfuls of the powder are mixed in a tumbler of water, and this drink is regarded as a stimulant to the nerves, and like strong tea or coffee is said to take away the disposition to sleep. The active chemical principle is an alkaloid which Dr. Stenhouse has shown to be identical with theine. Guarana contains more than double as much of this alkaloid as good black tea, and five times as much as coffee, the proportion being 5·07 per cent in Guarana. It is rather a singular coincidence that the same alkaloid should prevail in all the principal substances employed in a similar manner as beverages in different parts of the world, in the tea of China and India, the coffee of Arabia, the cacao of Central America, the matê of South America, and the guarana of Brazil. Guarana is a nervous stimulative and restorative.

An attempt is again being made, with hope of success, to work the quicksilver mine of Punitaqui, in Ovalle Department, Chile. It was worked for the crown in the Spanish times, but the war of Independence and the Indian incursions stopped it, as the latter did again in 1830.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

On the Temperature of the Air at four feet, twenty-two feet, and fifty feet high.—J. Glaisher, F.R.S. In his opening remarks, Mr. Glaisher spoke of the erroneous opinions which were entertained previous to his balloon ascents with regard to temperature at different heights; it was supposed that the temperature of the air always decreased from the earth upwards, and followed some constant law; this was, however, found not to be the case; and in the Report to the British Association at Nottingham, in 1866, the conclusions were, "that the law of decrease of temperature with increase of elevation was variable throughout the day, and also in different seasons of the year; that at about sunset the temperature was sensibly the same up to 2,000 feet; and that at night (conjectures from the results of the observations taken in the only two night ascents) the temperature of the air increased from the earth upwards." It was therefore evident that a very large number of ascents would have to be made to determine the real laws.

Fortunately, in the second year of the balloon experiments, he placed a dry and wet bulb thermometer at the height of 22 feet above the ground, readings of which have been taken four times daily, viz. at 9 P.M., noon, 3 A.M., and at 9 P.M. Although from these observations, and also from those made at the different ascents, it was known that sometimes readings at the higher point were above those at 4 feet from the ground, no particular attention was paid to the above readings until after the results of the observations made in M. Giffard's Captive balloon were known; these, however, were of such importance, proving that "the decrease of temperature with increase of elevation had a diurnal range, and was different at different hours of the day, the change being greatest at mid-day, and least at or about sunset (see Report to the B. A. for 1869 at Exeter), and that sensible changes occurred within 30 feet of the earth," that Mr. Glaisher caused the observations taken at the height of 22 feet to be reduced; collecting the observations recorded during the period 1867-1870, the differences between the readings of the two thermometers were taken, and affixing the sign *plus* (+) to the difference when the temperature at the higher elevation exceeded that at the lower, and the sign *minus* (−) when *vice versâ*.

On taking the monthly means of these differences, it was proved that the *mean temperature* of the air at 22 ft. high differs from that at 4 feet by :—

	9 A.M. deg.	Noon. deg.	3 P.M. deg.	9 P.M. deg.
In January	+ 0·5	+ 0·2	+ 0·4	+ 0·6
,, February	+ 0·2	0·0	+ 0·4	+ 0·5
,, March	— 0·3	— 0·2	0.0	+ 0·4
,, April	— 0·6	— 0·5	+ 0·2	+ 0·5
,, May	— 0·6	— 0·4	— 0·4	+ 0·5
,, June	— 0·8	— 0·9	— 0·6	+ 0·8
,, July	— 0·8	— 0·8	— 0·8	+ 0·7
,, August	— 1·0	— 0·5	— 0·1	+ 0·9
,, September	— 1·0	— 0·6	0·0	+ 0·7
,, October	— 2·2	— 0·1	+ 0·6	+ 1·0
,, November	+ 0·2	+ 0·1	+ 0·6	+ 0·8
,, December	+ 0·5	+ 0·3	+ 0·4	+ 0·4

Therefore the monthly mean temperature of the air at 22 feet was higher than at 4 feet, at all hours of the day and night, in January, February, November, and December; in the afternoon and during the night hours in the months of March, April, August, September, and October; in the evening hours and during the night, in the months of May, June, and July; and that the results in one year agreed very closely with those in the same months in other years.

By selecting the largest number with a + sign, and the largest with a — sign in each month, it was found that in the winter months the temperature at 22 feet high ranged from 2° to 4° above, and from 1° to 2° below that at 4 feet, and in the summer months from 4° to 5° above and from 4° to 5° below that at 4 feet high.

The ratio of — readings to + was, in January and February as 1 to 5 at all hours. In March, April, August, and September, during the day, one of equality. In May, June, and July, as 3 to 2 during the day hours. In October, as 1 to 4; in November, 1 to 7; and in December, as 1 to 10. At the hour of 9 P.M. throughout the year, it was as 1 to 7.

Thus the — sign preponderates, indicating lower temperature above, during the day hours, in the months of May, June, and July; the two signs are about equal in number in the months of March, April, August, and September, and the + sign preponderates, denoting higher temperature during the day and night, in January, February, October, November, and December, and during the night throughout the whole year.

A second thermometer, carefully protected from radiation, was placed in the middle of the year 1869 at the height of 50 feet, and since then its readings have been regularly taken.

The *mean monthly temperature* of the air at 50 feet high was found to differ from that on the ground as follows :—

1869	9 A.M. deg.	Noon. deg.	3 P.M. deg.	9 P.M. deg.
In October	+ 0·2	— 0·5	+ 0·7	+ 1·5
,, November	+ 0·6	+ 0·5	+ 0·8	+ 1·4
,, December	+ 0·9	+ 0·3	+ 0·5	+ 0·5
1870				
,, January	+ 1·1	+ 0·3	+ 0·7	+ 0·9
,, February	+ 0·1	— 0·3	+ 0·3	+ 0·5
,, March	— 0·3	— 1·8	— 0·7	+ 0·7
,, April	— 0·9	— 2·2	— 1·7	+ 1·4
,, May	— 2·4	— 3·6	— 2·8	+ 1·1
,, June	— 2·4	— 3·8	— 3·1	+ 1·1
,, July	— 1·8	— 2·9	— 2·8	+ 1·1
,, August	— 1·7	— 2·7	— 2·0	+ 1·7

Thus we have the unexpected results that the *mean monthly temperature* of the air at 22 feet and at 50 feet high is higher during the evening and night hours throughout the year than at the height of 4 feet, and also higher night and day during the winter months.

In conducting the above investigation, it was known that the clouds had great influence on the temperature; Mr. Glaisher therefore selected those days with a sky covered with dense clouds, and it was found that there was on such days no difference between the temperature at the heights of 4, 22, and 50 feet. At the height of 50 feet, in the summer months, the temperature during the day was frequently 6° or 7° lower, and at night 5° or 6° higher than at 4 feet.

SECTION B.—CHEMICAL SCIENCE

Experiments on the Preservation of Building Stones.—Prof. A. H. Church, M.A.

This paper gave a brief account of a process for preserving stone in which solution of monocalcic phosphate, barium hydrate, and dialysed silica are successively employed. Very numerous and extensive experiments have been made with this process upon public and private buildings. The New Midland Terminus, St. Pancras, has been treated with these solutions, and so have the Chapter House, Westminster, and portions of Canterbury Cathedral and the Houses of Parliament. The process is now the property of the Patent Concrete Stone Company.

Contributions to Mineralogical Chemistry.—By Prof. Church, M.A.

The author gives an account of his researches into the constitution of numerous mineral species. The paper gives a list of the nine species which he has discovered, including the rare cerium phosphate from Cornwall. The paper also contains the chief results which its author has obtained in the analyses performed by him during the last seven years with a view to the revision of the formulæ of the mineral phosphates and arseniates. References to the original memoirs are given under the description of each mineral. Prof. Church lays great stress, in the prefatory remarks to his paper, on the importance of ascertaining the hygroscopic water of minerals, and of obtaining the samples for analysis in a state of freedom from admixture by foreign and intruding bodies.

SECTION D.—BIOLOGY

Department of Zoology and Botany

Prof. Huxley, President of the Association, read a paper on the relations of *Penicillium*, *Torula*, and *Bacterium*, in which he showed the extreme probability, if not amounting to absolute demonstration, perhaps going as far towards it as the extreme difficulty of the investigation of the subject admitted, that these two latter forms were but stages of the first. Prof. Huxley gave an account of his experiments and researches, which were carried on with extreme care, and mentioned that he had become convinced that the movements of the minute germs so much alluded to by experimenters was to be accounted for by the explanation that it was the well-known Brownian movement. The bearing of this important paper seemed to be to account for the presence of Bacteria on more ordinary principles than those of spontaneous generation.

Mr. A. W. Bennett read a paper on the *Theory of Natural Selection, looked at from a Mathematical Point of View.**

Prof. Huxley paid a high compliment to the author of this paper, which he said was the first that he could recollect having heard read at Section D, which, taking up the side against Mr. Darwin, still did so in a proper and philosophic manner. He had often mentioned objections that had struck him to Mr. Darwin, who always, however, had ready a quiver full of facts which generally settled the question, and he thought it probable that when Mr. Darwin read Mr. Bennett's paper, he would have a few facts ready which might alter his view of the case.

Dr. Anton Döhrn read a paper on the *Foundation of Zoological Stations*. In this he insisted on the importance of there being zoological stations throughout the world, just as there now were meteorological and astronomical stations. The author mentioned that he had commenced at his own cost the establishment of one such station at Naples. In it he would have large and small aquaria, a constant flow of salt water, microscope rooms, and there would be a resident working zoologist, a library, and a series of bed-rooms for foreign naturalists. At this station, not only would collections of living marine animals be made for purposes of study and for supplying the aquaria of Florence, Berlin and Vienna, but a collection in spirits would also be kept to supply working zoologists at a distance with the means of research. The President of the Section, Professors Newton and E. Perceval Wright, spoke strongly in favour of the station to be established at Naples, and expressed the hope that perhaps at some future day others would be established at such outposts as Dingle in Ireland, Aden in the Red Sea, &c. &c.

Dr. J. Barker read a paper in which he described an interesting little Infusorium called *Pleuronema doliarium.*

Professor Dickson read notes *On the Embryo of the Date Palm.* The author criticised the descriptions given in the books where the slit of the cotyledon is represented sometimes as a transverse fissure near the upper part of the cotyledon (Schnizlein's Iconographia), or as a vertical one near the upper part (Le Maôut and Decaisne). Dr. Dickson described the slit as a vertical one, situated near the base of the cotyledon, and called attention to the fact that there was here no fixed relation between the medial plane of the cotyledon and that of the seed, an exception to the general rule for monocotyledons, as laid down by Hofmeister, to the effect that in vertical seeds (erect or pendulous) the medial plane of the cotyledon coincides with that of the seed, while in horizontal seeds the plane of the cotyledon is at right angles to it.

Mr. Tyerman exhibited drawings of a young healthy plant of the double cocoa-nut (*Lodoicea sechellarum*), which he had succeeded in growing at the Botanical Gardens, Liverpool. He mentioned that he had some difficulty in keeping the strangely elongated basal portion of the cotyledon from penetrating too deeply into the ground, but after it had grown to a distance of some two feet, the germ developed a single sheathing leaf, and then shortly afterwards two of the ordinary characteristic leaves of the plant made their appearance.

Profs. Balfour and Wright congratulated Liverpool on having such an interesting collection of plants as that they had seen at their Botanical Gardens, and on having so able and intelligent a curator as Mr. Tyerman, and hoped that when next they visited Liverpool they would find at the gardens a range of glass worthy of the gardens and of the most interesting collection of plants at present preserved in a few tumble-down houses.

On the Staperaythr Whale of the Icelanders.—Mr. Bird.

On the Affinities of the Sponges to the Corals.—Mr. W. S. Kent. The author criticised the views of Haeckel.

On the Effects of the Pollution of Rivers on the Supply of Fish.—Sir James Alexander. This paper gave rise to a lengthened discussion.

Mr. Moore, director of the Liverpool Museum, exhibited some of Captain Mortimer's Sea Aquaria, by means of which he had been enabled to transport from the tropics many delicate marine fishes, crustacea, and sea anemones. He also exhibited a young Lamatin and the jaw of a fish, the rami of the mandible of which, instead of being united by a ligamentous union and forming a bony symphysis, were most compactly hinged together, admitting of a considerable amount of lateral motion. Mr. Moore also exhibited a stuffed skin of that most wonderful shark from the Seychelles, called *Rhinodon typicus*. This was the only perfect specimen known in any museum. This shark, which grows to the length of 50 feet and upwards, was known to Mr. Swinburne Ward, late Civil Commissioner of the Seychelles, as common off those Islands. But it was not known to be described until identified by Prof. E. Perceval Wright as the *R. typicus* of Smith.

* This paper will be found in full at p. 30.

A Statement in Reply to two Objections of Prof. Huxley relative to certain Experiments.—Dr. Bastian.

Department of Anatomy and Physiology

On Some of the more Important Facts of Succession in Relation to any Theory of Continuity.—Dr. Cobbold. This paper—which by permission had been transferred from the Department of Zoology and Botany—Dr. Cobbold stated, was generally of an elementary character. He said that for several years past the Biological Section of the Association had permitted, if it had not actually encouraged, the reading and discussion of papers having for their object the popularising (or it might be the unpopularising) of the theory of natural selection. To many besides himself the separate papers and remarks which followed were eminently unsatisfactory, perhaps arising not so much from any want of ability on the part of the authors as from the unscientific method adopted by them. The discussion which followed the reading of the Rev. F. O. Morris's paper at Norwich, "On the Difficulties of Darwinism," was lamentable in the extreme; for, so far as could be gathered, not one of those who sympathised with the reverend gentleman's position had the courage to advance a single fact in favour of the view which his "difficulties" were intended to support. At Exeter Mr. Morris renewed his exposition, but a much more vigorous effort in the same direction was made by the Rev. A. Freeman. The severity of the criticism which followed these final literary efforts in aid of anti-evolutionism could only be understood by those who were present; but the general conclusion of scientific authorities was significantly expressed in the statement made more than once to the effect that "neither of the papers ought to have been read." For his part, he thought the utmost freedom should be allowed to all desirous of opposing this or that theory; yet it should be thoroughly well understood that the sectional committee deprecated the employment of quotations from the Scriptures calculated to excite religious prejudice. A purely scientific question could only be satisfactorily discussed on a scientific basis—unless, indeed, it was insisted that theological speculations were inseparable from the domain of science. He then went on to say that to the mass of so-called educated men the acceptance of the views set forth by Mr. Darwin in his work "On the Origin of Species" must naturally present a variety of difficulties, and it appeared to him (Dr. Cobbold) that the best and truest way of showing an intelligent sympathy with those who were so situated, was to select and present some natural group of observed facts in such a clear and, if possible, attractive light, that common sense alone might be trusted to recognise the reasonableness or otherwise of honestly asserted deductions. The facts he had selected for exposition were such as represented what might be termed the apparent chronology of the organic series, or, in other words, the ascertained times of the coming and flourishing of the larger animal groups. A true conception of what was or ought to be understood by the expression "equivalences"—botanical, zoological, or geological—lay at the very basis of a correct appreciation of the significance of the records of animal, vegetable, or sedimentary rock distribution throughout all time. Further, he ventured to assert that the grandeur of the formative scheme of nature, whether testifying to an evolutionary method of production or to a series of creative acts, few or many in number, could only be adequately realised by the naturalist whose powers of allocation and grouping enabled him to grasp the magnitude and infinite import of those relations. Dr. Cobbold said he had insisted upon equivalency for years past. He then proceeded to deal with the facts of succession, and said the earliest organism as regarded time which geology had revealed was the fossil called Eozoon, which belonged to the lowermost division of the animal series. Dr. Cobbold then described the succession of the various known groups, and, glancing at the times of origin and succession of the placental mammals, said the first thing that the record suggested was the rapidity with which the most divergent groups made their appearance. Of course, there was no real basis for an assumption of a coeval creation, so to speak. It might be fairly held, on zoological grounds, that we ought not to separate man and monkeys, but retain them as one of the twelve groups under the ordinary title of primates. He adopted the division of the placental into twelve groups, not from any rigid belief as to their separate equivalences, but because they were not only sufficiently distinctive for all practical purposes, and also formed on the whole perhaps the finest expression of grouping which science could at present afford. After dwelling at great length upon the succession of the various groups, he stated that as regarded the highest of all, the placental series, he would only say that, as he understood the doctrine, the strictest demand of the development theory did not require, as was too commonly supposed, a lineal descent as between bimana and quadrumana; but it was certainly held that either of these groups, as we now knew them, might have been separately evolved from more generalised primatal types, the intermediary terms being possibly connected by a long antecedent and far more generalised common progenitor. In that connection the most advanced evolutionist must candidly own that the assumedly missing tertiary primatals constituted a great and very natural bar to the complete and popular acceptance of the theory of descent by natural selection. On the other hand, the scientific naturalist, whilst admitting these serious deficiencies, threw into the opposite scale a multitude of considerations, the collective value of which seemed to him to outweigh all the data thrown into the anti-continuity side of the balance. For himself, in conclusion, he said that his necessarily limited application of those data was amply sufficient to enforce upon him the provisional acceptance of any theory of continuity. To his mind, its clear application irresistibly implied that nature, to use an old phrase, was but a series of harmonies—wheel within wheel, there being probably but one wheel differing only from all the wheels of whose limits it was not possible for them to conceive. However, in the contemplation of the phenomena presented to them within that wheel—or that realm of "orderly mystery," as the president had called it—there was ample room and verge for the display of the highest physiological attributes with which man was endowed.

Department of Ethnology and Anthropology

The report *On the Heat Generated in the Blood in the Process of Arterialisation*, by Dr. Arthur Gamgee, was taken as read.

New Physiological Researches on the Effects of Carbonic Acid.—Dr. B. W. Richardson. The author explained that the observations he had made were new in that they related to the direct action of carbonic acid on animal and vegetable fluids, and they were interesting, equally to the zoologist and botanist as to the anatomist. The author first demonstrated the result of subjecting a vegetable alkaline infusion to the action of carbonic acid under pressure. The result was a thick fluid substance which resembled the fluid which exudes as gums from some trees. When the fluid was gently dried it became a semi-solid substance, which yielded elastic fibres. This observation had led the author to study the effect of carbonic acid on albumen, serum of blood, blood itself, bronchial secretion, and other organic fluids. When the serum of blood was thus treated with carbonic acid under pressure and general warmth, 96° F., the collodial part was separated; but when the blood, with the fibre removed from it, was treated, there was no direct separation, the blood corpuscles seeming for a time to engage the gas by condensation of it. But blood containing fibrine, and held fluid by tribasic phosphate of soda, was at once coagulated by the acid. The bronchial secretion is thickened by carbonic acid, and a tenacious fluid is obtained, resembling the secretion which occurs in asthma and bronchitis, while secretions on serous surfaces are thickened and rendered adhesive. After details of many other facts, Dr. Richardson concluded by showing what bearing this subject had of a practical kind. In the first place, the research had relation to the question of elasticity of organic substances; and, secondly, on the direct action of carbonic acid in the production of vegetable juices. But the greatest interest concentrated on the relation of the research to some of the diseases of the animal body. Thus in instances where the temperature of the body is raised and the production of carbonic acid is excessive, the blood on the right side of the heart has its fibrine often precipitated, and in many other cases fibrinous or albuminous exuded fluids are solidified, as is the case in croup. The author, in the course of his paper, explained how rapidly blood charged with carbonic acid absorbed oxygen when exposed to that gas, and he held that carbonic acid in the venous blood was as essential to the process of respiration as was the oxygen in the pulmonary organs.

SCIENTIFIC SERIALS

Journal of the Chemical Society, October, 1870.—The first paper in this number is by Dr. Divers, "On the Precipitation of Solutions of Ammonium Carbonate, Sodium Carbonate, and Ammonium Carbamate by Calcium Chloride." These results obtained by Dr. Divers are the following:—Calcic carbamate is soluble, and the presence of ammonia retards its transformation into carbonate. When carbonic anhydride is passed into an ammoniacal solution of calcic chloride, the carbamate is first formed, and is gradually precipitated as carbamate. This paper is followed by nearly two pages of Addenda et Corrigenda to the author's previous memoir.—"On the Manipulation of Gold and Silver Bullion," by Charles Tookey, Assayer in the Japanese Imperial Mint, formerly in the Royal Mint, Hong Kong. In this paper the author gives descriptions of two of the processes that he has adopted. Instead of boiling the cornets in separate parting flasks, he uses a series of perforated platinum tubes, supported in a porcelain plate. A number of cornets are, by this means, simultaneously submitted to the action of the nitric acid. Secondly, in order to clean the buttons, they are placed with the lower side uppermost on a platinum plate with depressed perforated cavities, which is plunged into hot dilute hydrochloric acid, afterwards into hot water acidulated with hydrochloric acid, and lastly into pure water. The plate is then drained by placing it on porous paper and dried over a gas flame.—"On some new Bromine Derivatives of Coumarin," by W. H. Perkin, F.R.S. On adding coumarin to bromine in the presence of carbonic disulphide, allowing the solution to evaporate, and crystallising the residue from alcohol, dibromide of coumarin $C_9H_6O_2Br_2$ is obtained. When coumarin and bromine in carbonic disulphide are digested at 140°, bromo-coumarine $C_9H_5BrO_2$ and dibromo-coumarin $C_9H_4BrO_2$ are produced, and are separated by crystallisation from alcohol, in which the latter is the less soluble. Dibromo-coumarin fuses at 174°, and distils nearly unchanged. It crystallises from alcohol in small needles. Bromo-coumarin fuses at 110°, and crystallises from alcohol in transparent prisms, often beautifully curved. When heated with solution of potassic hydrate both the bromo-compounds dissolve, producing crystalline salts, probably containing the bromo-coumaric acids.—"On Organic Matter in Water," by Mr. C. Heisch. The author has observed that certain waters which are known to be contaminated with sewage matters, give rise to the formation of a microscopical fungus when a small quantity of sugar has been added, and the mixture exposed to light for a few days at the temperature of 60°-70° F. Six drops of sewage from which the solid matter had settled, were mixed with 10,000 grains of West Middlesex and New River water; to 6 oz. of the mixture 10 grains of pure sugar were added, and 10 grains were also added to 6 oz. of the water without sewage; these solutions, and some of the mixture of water and sewage, were placed at a window. The water containing the sewage and sugar became turbid in 24 hours, the other liquids remaining clear. On examining the turbid water with an $\frac{1}{5}$ inch object glass, it was found to be filled with small spherical cells, with, in most cases, a very bright nucleus, which group themselves in bunches like grapes; they then spread into strings, with walls surrounding and connecting the cells; the original cell walls afterwards break, leaving tubular threads branched together. After several days, an odour of butyric acid is perceived. One drop of fresh urine in 10,000 grains of water produced similar effects; though without the addition of the sugar, the water might be kept for weeks without becoming turbid. Filtration through Swedish paper, or boiling for half an hour, does not prevent the growth of the fungus. The water no longer exhibits this property, however, after passage through a good bed of animal charcoal, that is, if the charcoal is frequently exposed to the air. If the filtration is continuous, the filtrate soon becomes as bad as the original water.—"On the Methods for the Determination of Carbon in Steel," by Mr. W. D. Herman. The author has obtained very concordant results by burning the iron or steel in a current of oxygen, the iron is converted into ferric oxide and the carbonic anhydride collected in potash bulbs and weighed. Some results obtained by four different methods of estimating the carbon in iron and steel are given at the conclusion of the paper.—"On the Determination of Phosphoric Acid," by Mr. W. C. Williams. The author suggests a modification of the process for separating phosphoric acid from the alkaline earths originally proposed by Reigsig.

SOCIETIES AND ACADEMIES

Bristol

Observing Astronomical Society.—Report of observations made during the period from Aug. 7 to Oct. 6, 1870, inclusive.

Solar Phenomena.—Mr. Thomas G. E. Elger writes:—The magnificent display of solar spots observed in August was repeated, though in a rather less striking manner, during September. Between the 7th and 12th the spots were small, few in number, and mainly confined to the S. hemisphere; on the 11th only three moderately-sized groups were visible. The immense group observed last month, and which was near the centre of the disc on August 30, was due at the E. limb about the 17th, but owing to unfavourable weather and absence from home, I did not notice it till the 24th, when it measured 2′ 45″ × 1′ 50″ without including the outlying penumbra which followed it; its length on the 25th was 3′ 0″ The penumbra of this group presented some remarkable features. It contained four large umbræ and many smaller ones; on the preceding side it was thickly studded with minute dots of every shade from black to light brown. When examined with a power of 180 at 3^h on the 25th, the entire group was evidently undergoing rapid and violent changes, the striation of the penumbra and the dark "spurs" and serrated edges of the umbra clearly indicating the cyclonic nature of the forces involved. The above group was preceded by a very long and narrow V-shaped spot, which occupied nearly the same position as a large spot observed in August. Several other groups were observed during the month, which presented interesting details, but they were generally smaller than the August groups. The appearances exhibited by the large group described above, and indeed by most spots of a similar class observed this year, seem altogether opposed to the "deep excavation" theory of sun spots. —Mr. T. W. Backhouse, of Sunderland, reports "a very fine group of spots passed the sun's centre in the northern zone on September 21; on Sept. 23, at $21^h\ 15^m$, it contained two very long penumbræ, which were not widely separated; the *f* one was 74,000 miles long, and the *p* one 92,000! On the 25th, at $19^h\ 45^m$, it was only 66,000 miles long, and the *f* penumbra was divided into two. Another very fine group, also in the N. zone, passed the sun's centre on the 24th. The dimensions in miles of its chief spot were as follows:—

Date.		Time.	Penumbra. Length.	Penumbra. Width.	Umbra. Length	Umbra. Width.
Sept.	21	$5^h\ 15^m$	——	47,000	26,000	11,000
	,,	$21^h\ 25^m$	abt. 72,000	50,000	31,500	——
	22	$3^h\ 25^m$	50,000	——	29,500	——
	23	$21^h\ 25^m$	63,000	——	——	——
	25	$21^h\ 30^m$	abt. 70,000	——	——	——

On the 23rd, at $4^h\ 40^m$, I found it was divided into four, apparently by a violent current in the middle from *p* to *f*".—Mr. William T. Dunning, of Bristol, observed the large spot visible on September 21; with his 4-inch metallic reflector he could very distinctly see a black nucleus in the S part of the umbra. It did not appear to be actually enclosed within the umbra, but was situated on the margin of the penumbra.—Mr. E. B. Knobel, of Burton-on-Trent, says that on Sept. 25 the large group near the centre of the disc measured 2′ 54″ by 1′ 44″; on the 26th the two largest groups were equal in length to 2′ 44″ and 2′ 36″ respectively. They were distinctly visible to the naked eye. —The Rev. S. J. Johnson, of Crediton, writes that on September 21, at $4^h\ 30^m$, a power of 70 on a 2¼-inch *O. G.* showed penumbræ on, at least, 26 spots visible on the sun. On September 20 "seven spots were very large indeed, and arranged in five groups, each scattered over a large surface."—Mr. Albert P Holden, of London, referring to the large spot, says that on September 20, "when entering on the solar disc it appeared as an elongated spot with a bright arm stretching over half the umbra till it joined a projection on the N. side. It was followed by a large broken group of broken masses of various dimensions. On the 23rd, at 8 A.M., the chief spot had enlarged considerably, while the broken ones following it had very much decreased. The great spot was very nearly divided into two by a very broad arm springing from the N.; the W portion of the umbra being again subdivided by a similar arm on the S. side. This last was on one side broken up very peculiarly, so that it presented the appearance as if a handful of bright straw had been thrown carelessly upon it. The eastern portion of the umbra was crossed by a very bright curved streak, which was so bright and so clearly distinct right up to its edges as to appear more like a carved piece of silver. On the 24th, at 8 A.M., the broken mass before referred to as following the chief spot had almost disappeared, with the exception of one small spot and a small amount of penumbra. The great spot was also a little smaller and quite divided by the broad arm. A large crack appeared in this latter. Each of the two portions of the original spot were also divided by luminous bridges across them. On the 28th the appearance of the umbra was much the same, although the penumbra was entirely changed and a great narrow branch had projected S. to an immense distance. This great arm was dotted here and there with a few patches of umbra. The broken mass which had followed the great spot at its first appearance was now entirely dissipated. On the 29th the two portions of the original spot were widely separated and much contracted, and two spots to the S. which had hitherto been of very small size much increased in dimensions. The rotation of the solar orb then carried the spot out of sight. The principal fact impressed upon the mind by these observations is that a spot becomes dissipated in consequence of its continual division and subdivision by the projection of luminous bridges across its various portions.

DIARY

THURSDAY, November 10.

London Institution, at 7.30.—Acoustics of the Orchestra: Dr. W. H. Stone.

London Mathematical Society, at 8.—Annual General Meeting. Recent Researches on Quartic and Quintic Surfaces: By Prof. Cayley.—The Retiring President's Address.

FRIDAY, November 11.

Astronomical Society, at 8.

MONDAY, November 14.

London Institution, at 4.—Chemical Action: Prof. Odling.

TUESDAY, November 15.

Anthropological Society, at 8.—Observations on the Condition of the Blood-Corpuscles in Certain Races: Dr. R. H. Bakewell—Tribal Affinities among the Aborigines of Australia: Mr. C. Staniland Wake.—Description of Australian Aborigines and Half-Castes, with Exhibition of Skulls: Dr. Robert Peel.

Zoological Society, at 9—On the Form and Structure of the Manatee (*Manatus Americanus*): Dr. J. Murie.—Observations on the Salmonidæ in Tasmania: Mr. Morton Allport.—On the Anatomy of *Ailurus fulgens*: Prof. Flower

Statistical Society, at 7.45.—On the Claims of Science to Public Recognition and Support, with Special Reference to the so-called "Social Sciences": Dr. Guy, F.R.S

Royal Geographical Society, at 8.30.

THURSDAY, November 17.

Linnean Society, at 8.—On the *Passifloreæ*: Dr. M. T. Masters.—On the White-beaked Bottle-nose: Dr. James Murie.

Chemical Society, at 8.—Mineralogical Notices: Prof. N. Story Maskelyne and Dr. Walter Flight.

London Institution, at 8 30.—Acoustics of the Orchestra; Wind Instruments: Dr. W. H. Stone.

CONTENTS