THURSDAY, DECEMBER 15, 1881

CHARLES LYELL

Life, Letters, and Journals of Sir Charles Lyell, Bart., Author of the Principles of Geology, &c. Edited by his Sister-in-law, Mrs. Lyell. In two volumes. With Portrait. (London: John Murray, Albemarle Street, 1881.)

I.

"THE Principles of Geology" and "The Origin of Species" are the two books which have unquestionably exercised the most powerful influence upon the direction of scientific thought during the present century. The first of these works not only prepared the way for the second, but, as Darwin himself has told us, may actually be regarded as its progenitor, for it was the study of the "Principles" which induced the young naturalist to make his now famous "Voyage Round the World" and to collect those facts and observations out of which eventually grew the theory of Natural Selection. The wonderful revolution in thought which followed the appearance of the "Origin of Species" is still fresh in our minds, but those who could remember the effects produced by the publication of "The Principles of Geology," were wont to relate that fifty years ago scientific thought and speculation received an impetus no less powerful than that of which we have witnessed the results in our own time.

The story of the life of Sir Charles Lyell is the history of "The Principles of Geology," for all Lyell's other scientific writings are either expansions of portions of that great work, or are in some way or other supplementary to it. In the account of Lyell's earlier years we trace the birth and development of the ideas so clearly embodied in this famous book, while by the records of his later years we are reminded of the untiring energy with which he collected materials to expand and illustrate those ideas in the successive editions of the work.

The volumes before us enable us for the first time to trace this interesting story in all its details, and we cannot speak too highly of the skill and judgment with which the editor has arranged the materials at her command. The book consists essentially of Lyell's own journals and letters, a few short explanatory notes on the chief events of his life being interspersed in small type and inserted between brackets, together with a few foot-notes explaining allusions or giving details about persons mentioned in the letters.

Lyell, though born in Scotland, was by descent and education an Englishman. His earlier years were spent either in the New Forest and the towns in the south of England, where he went to school, or at the home at Kinnordy, in Forfarshire, where the family usually spent the autumn. In the south of England young Lyell, whose attention had been from boyhood directed to entomology, had the opportunity of studying the Tertiary deposits of the Hampshire basin; while in Forfarshire the draining of a small loch on his father's property and the excavation of the "marl" with which it had become filled, appear to have early directed his attention to some of the important questions connected with the mode of deposition of strata and the way in which organic remains become imbedded in them—questions afterwards treated by him with such skill and ingenuity in the "Principles." Lyell's two first papers, published in the *Transactions* of the Geological Society, relate to the strata of the Hampshire Basin and the formation of these marl-deposits in the lakes of Forfarshire.

At the age of seventeen Lyell went to Oxford, and there came under the influence of the brilliant and versatile, but eccentric, Buckland. But though impressed with the eloquence and filled with admiration at the energy of his teacher, there is evidence that at a very early date Lyell's mind underwent a revolt against the bold but shallow theorisings of the Oxford professor. When a few years later Buckland published his "Reliquiæ Diluvianæ," we find the pupil not only in open opposition to the master, but actually leading the attacks of the "Fluvialists" against the great stronghold of the "Diluvialists."

Upon leaving Oxford, Lyell was destined for the bar, but he, after reading law for a short time, was obliged to desist on account of the weakness of his eyesight. Under these circumstances he repaired to Paris, where he had the opportunity of constant intercourse with Cuvier, Brongniart, Humboldt, Constant Prévost, and the other brilliant scientific thinkers who were at that period assembled in the French capital. He at the same time studied with care the Tertiaries of the Paris basin, comparing the strata and their fossils with those with which he was already so familiar in Hampshire.

Lyell had now become so thoroughly engrossed in scientific work that all idea of advancement at the bar was abandoned by him, and after going two years upon the Western Circuit, he seems to have finally relinquished law for literature and science. He first began to write in the *Quarterly Review*, having formed a close friendship with Lockhart, then editor of that journal, and, after some papers upon educational questions, he in 1827 undertook a review of Scrope's "Geology and Extinct Volcanoes of Central France." It was in this work that Lyell first showed how entirely he had adopted the principles enunciated by Hutton and Playfair, and how far he was in advance of his most eminent contemporaries, Buckland and Sedgwick in England, and Cuvier and Humboldt on the Continent.

The five years from 1825 to 1830, during which Lyell was maturing his literary style by writing for the reviews and collecting the materials for his great work, may be regarded as the turning-point of his career, and the letters written by him at this period are of the greatest interest to the historians of science. We cannot forbear from making a few extracts illustrating the nature of his work and his views at this period. On June 22, 1826, he wrote to his friend Dr. Mantell—

"I must not sport radical, as I am become a Quarterly Reviewer. You will see my article just out on 'Scientific Institutions,' by which some of my friends here think I have carried the strong works of the enemy by storm. I am now far on with a second, and hope to get it out in less than three months. I mean to help myself out of Cuvier largely, for I must write what *will be read*" (vol. p. 164).

On March 2, 1827, he writes to the same correspondent as follows:—

"I devoured Lamarck *en voyage*, as you did Sismondi, and with equal pleasure. His theories delighted me more than any novel I ever read, and much in the same way, for they address themselves to the imagination, at least of geologists who know the mighty inferences which would be deducible were they established by observations. But though I admire even his flights, and feel none of the *odium theologicum* which some modern writers in this country have visited him with, I confess I read him rather as I hear an advocate on the wrong side, to know what can be made of the case in good hands. I am glad that he has been courageous enough and logical enough to admit that his argument, if pushed as far as it must go, if worth anything, would prove that man may have come from the Ourang-Outang. But after all, what changes species may really undergo! How impossible will it be to distinguish and lay down a line, beyond which some of the so-called extinct species have never passed into recent ones. That the earth is quite as old as he supposes, has long been my creed, and I will try before six months are over to convert the readers of the *Quarterly* to that heterodox opinion" (vol. i. p. 168).

His aspirations concerning his future at that time will be understood from the following extract from a letter written to his father in the same year:—

"I find my wants diminish monthly in proportion as I am more agreeably employed, and if with the willingness to work and industry which I now have, I had any chance of earning what I require by my own exertions, I should be without a care as far as I am myself concerned. But to be willing without avail to work hard, and almost for nothing, is now the fate of many hundreds of barristers, and many millions of our labouring classes, and we must congratulate ourselves at not being among the latter. I am quite clear, from all that I have yet seen of the world, that there is most real independence in that class of society who, possessing moderate means, are engaged in literary and scientific hobbies; and that in ascending from them upwards, the feeling of independence decreases pretty nearly in the same ratio as the fortunes increase. My eyes go on tolerably, and I feel my facility of composition increases, and hope to make friends among those that a literary reputation will procure me who may assist me" (vol. i. p. 171).

Under date of February 5, 1828, he wrote to Dr. Mantell explaining his plans for the work which he had been for some time contemplating:—

"I at first intended to write 'Conversations on Geology"; it is what no doubt the booksellers, and therefore the greatest number of readers, are desirous of. My reason for abandoning this form was simply this; that I found I should not do it at all, without taking more pains than such a form would do justice to. Besides, I felt that in a subject where so much is to be reformed and struck out anew, and where one obtains new ideas and theories in the progress of one's task, where you have to controvert, and to invent an argumentation—work is required, and one like the 'Conversations on Chemistry' and others would not do. It should hardly be between the teacher and the scholar perhaps, but a dialogue like Berkeley's Alciphron, between equals. But finally I thought that when I had made up my own mind and opinions in producing another kind of book, I might then construct conversations from it. In the meantime there is a cry among the publishers for an elementary work, and I much wish you would supply it. Anything from you would be useful, for what they have now is positively bad, for such is Jamieson's 'Cuvier'" (vol. i. p. 177).

In attempting to free geological science from the trammels with which it had become involved by the efforts of well-intentioned but mischievous works, like the "Vindiciæ Geologicæ" and the "Reliquiæ Diluvianæ," Lyell undertook no light or easy task. His letters to Scrope, who had been requested by Lockhart to review the "Principles" in the pages of the *Quarterly*, show very clearly how sensible Lyell was of the difficulties by which he was beset through the nervous susceptibilities of orthodoxy. The fact that the works of Hutton and Playfair had long ago been placed in a social "Index Expurgatorius," and that Scrope's clear and admirable exposition of the Huttonian doctrines, published in his "Considerations on Volcanoes" in 1825, had altogether failed to revive interest in the ostracised works, was full of warning to Lyell. We find him writing to Scrope, while the first volume of the "Principles" was going through the press, in the following terms:—

"I was afraid to point the moral, as much as you can do in the *Q. R.*, about Moses. Perhaps I should have been tenderer about the Koran. Don't meddle much with that, if at all.

"If we don't irritate, which I fear that we may (though mere history), we shall carry all with us. If you don't triumph over them, but compliment the liberality and candour of the present age, the bishops and enlightened saints will join us in despising both the ancient and modern physico-theologians. It is just the time to strike, so rejoice that, sinner as you are, the *Q. R.* is open to you.

"If I have said more than some will like, yet I give you my word that full *half* of my history and comments was cut out, and even many facts; because either I, or Stokes, or Broderip felt that it was anticipating twenty or thirty years of the march of honest feeling to declare it undisguisedly. Nor did I dare come down to modern offenders. They themselves will be ashamed of seeing how they will look by-and-by in the pages of history, if they ever get into it, which I doubt. You see that what between Steno, Hooke, Woodward, De Luc, and others, the modern deluge systems are all borrowed. Point out to the general reader that my floods, earthquakes, &c., are all very modern, also waste of cliffs; and that I request that people will multiply, by whatever time they think man has been on the earth, the sum of this modern observed change, and not form an opinion from what history has recorded. Fifty years from this, they will furnish facts for a better volume than mine. . . ."

"I conceived the idea five or six years ago, that if ever the Mosaic geology could be set down without giving offence, it would be in an historical sketch, and you must abstract mine, in order to have as little to say as possible yourself. Let them feel it, and point the moral" (vol. i. p. 271).

On two points, as has often been pointed out, Lyell may be held to have betrayed weakness in his reasoning in the "Principles." The first of these was that he appeared to accept in the most uncompromising manner the stringent Uniformitarian views of Hutton, leaving no place even for variations in the intensity of causes now operating. In taking this line he was doubtless influenced by fear of making any dangerous concessions to his adversaries the "Diluvialists." His real feelings on the subject may be gathered from a letter in which he replies to the remonstrances of Scrope upon the subject—

"All I ask is, that at any given period of the past don't stop inquiry when puzzled by refuge to a 'beginning,' which is all one with 'another state of nature' as it appears to me. But there is no harm in your attacking me, provided you point out that it is the proof I deny, not the probability of a beginning. Mark, too, my argument, that we are called upon to say in each case, 'Which is now most probable, my ignorance of all possible effects of existing causes,' or that 'the beginning' is the cause of this puzzling phenomenon?" "It is not the beginning I look for but proofs of a *progressive* state of existence in the globe, the probability of which is *proved* by the analogy of changes in organic life" (vol. i. p. 270). See also upon the same subject his letter to Whewell in 1837 (vol. ii. p. 2).

The other question upon which Lyell's reasonings in his "Principles" betrayed weakness and inconsistency was that of the cause of the appearance from time to time of new species of plants and animals upon the earth. While stoutly maintaining the sufficiency of existing causes to account for the gradual disappearance of species by extinction, he felt himself compelled to invoke a creative power to introduce the new species as they were required. But, before we blame Lyell for this apparent weakness, we ought to remember that the work of Lamarck, the only serious attempt which had been at that time made to account for the origin of species, though brilliant and suggestive, was full of assumptions and fallacies that could not fail to betray themselves to Lyell's logical mind, and to militate against his acceptance of the theory. Lyell, moreover, saw only too clearly that the origin of man could not be treated of on different principles to that of other species of animals, and to have come into conflict with the prejudices of the day upon such a point as this, would have been to sacrifice all chance of a patient hearing for his arguments in favour of the "good cause" of which he felt himself to be the apostle. A very interesting letter written by him to Sir John Herschel in 1836, shows very clearly that Lyell had even at that early date thought deeply on the question of the origin of species by natural causes.

"In regard to the origination of new species, I am very glad to find that you think it probable that it may be carried on through the intervention of intermediate causes. I left this rather to be inferred, not thinking it worth while to offend a certain class of persons by embodying in words what would only be a speculation. But the German critics have attacked me vigorously, saying that by the impugning of the doctrine of spontaneous generation, and substituting nothing in its place, I have left them nothing but the direct and miraculous intervention of the First Cause, as often as a new species is introduced and hence I have overthrown my own doctrine of revolutions, carried on by a regular system of secondary causes. I have not wasted time in any controversies with them or others, except so far as modifying in new editions some opinions or expressions, and fortifying others, and by this means I have spared a great deal of ink-shed, and have upon the whole been very fairly treated by the critics. When I first came to the notion, which I never saw expressed elsewhere, though I have no doubt it had all been thought out before, of a succession of extinction of species, and creation of new ones, going on perpetually now, and through an indefinite period of the past, and to continue for ages to come, all in accommodation to the changes which must continue in the inanimate and habitable earth, the idea struck me as the grandest which I had ever conceived, so far as regards the attributes of the Presiding Mind. For one can in imagination summon before us a small part at least of the circumstances that must be contemplated and foreknown, before it can be decided what powers and qualities a new species must have in order to enable it to endure for a given time, and to play its part in due relation to all other beings destined to coexist with it, before it dies out. It might be necessary, perhaps, to be able to know the number by which each species would be represented in a given region 10,000 years hence, as much as for Babbage to find what would be the place of every wheel in his new calculating machine at each movement.

"It may be seen that unless some slight additional precaution be taken, the species about to be born would at a certain era be reduced to too low a number. There may be a thousand modes of insuring its duration beyond that time; one, for example, may be the rendering it more prolific, but this would perhaps make it press too hard upon other species at other times. Now if it be an insect it may be made in some of its transformations to resemble a dead stick, or a leaf, or a lichen, or a stone, so as to be somewhat less easily found by its enemies; or if this would make it too strong, an occasional variety of the species may have this advantage conferred upon it; or if this would be still too much, one sex of a certain variety. Probably there is scarcely a dash of colour on the wing or body of which the choice would be quite arbitrary, or which might not affect its duration for thousands of years. I have been told that the leaf-like expansion of the abdomen and thighs of a certain Brazilian Mantis turn from green to yellow as autumn advances, together with the leaves of the plants among which it seeks for its prey. Now if species come in in succession, such contrivances must sometimes be made, and such relations predetermined between species, as the Mantis, for example, and plants not then existing, but which it was foreseen would exist together with some particular climate at a given time. But I cannot do justice to this train of speculation in a letter, and will only say that it seems to me to offer a more beautiful subject for reasoning and reflecting on, than the notion of great batches of new species all coming in, and afterwards going out at once" (vol. i. pp. 467, 469).

It is probable that during later years Lyell receded somewhat from the position he was prepared to take up at the time when he wrote the above. The crudeness of speculation and ignorance of scientific facts which characterised the earlier editions of the "Vestiges of Creation" had in all likelihood not a little to do with this revulsion of thought, while the powerful influence of the leaders of biological thought, Edward Forbes and Louis Agassiz, always exercised in support of the idea of the permanency of species, doubtless had no little weight with Lyell, as it had with nearly all his contemporaries. How readily Lyell welcomed and embraced the views of Darwin as soon as they were published we all know, for he could not fail to see that by incorporation of the theory of natural selection into his work he was for the first time able to make it complete and consistent with itself. It is interesting to read in the volume before us the impressions made upon him by the first reading of the "Origin of Species" in 1859.

"My dear Darwin,—I have just finished your volume, and right glad I am that I did my best with Hooker to persuade you to publish it without waiting for a time, which probably could never have arrived, though you lived to the age of a hundred, when you had prepared all your facts on which you ground so many grand generalisations.

"It is a splendid case of close reasoning and long-sustained argument throughout so many pages, the condensation immense, too great perhaps for the uninitiated, but an effective and important preliminary statement, which will admit, even before your detailed proofs appear, of some occasional useful exemplifications, such as your pigeons and cirripedes, of which you make such excellent use.

"I mean that when, as I fully expect, a new edition is soon called for, you may here and there insert an actual case, to relieve the vast number of abstract propositions. So far as I am concerned, I am so well prepared to take your statements of facts for granted, that I do not think the *pièces justificatives* when published will make much difference, and I have long seen most clearly that if any concession is made, all that you claim in your concluding pages will follow.

"It is this which has made me so long hesitate, always feeling that the case of Man and his Races and of other animals, and that of plants, is one and the same, and that if a *vera causa* be admitted for one instant, of a purely unknown and imaginary one, such as the word 'creation,' all the consequences must follow" (vol. ii. p. 325.

After the first publication of the "Principles" between the years 1830 and 1833, a great part of Lyell's time and thought was given up to revising, enlarging, and re-writing portions of his book during the twelve editions through which it passed. Although many valuable corrections were made in the original work, its scope and arguments being extended, and the whole fortified with a great wealth of new illustrations, it may well be doubted whether this continual re-editing of the book was not attended with some loss in the symmetry of its arrangement and its literary excellence. In a work relating to such a rapidly-advancing science as geology, this result, much as it is to be regretted, could scarcely be avoided; but many disciples of Lyell, while they refer to the last edition as a storehouse of facts, will delight to renew their acquaintance with an old favourite by reading once more the easily flowing periods of the first edition.

We have dwelt at such length upon Lyell's relations to his great work, as illustrated in the interesting volumes before us, that we must defer to a second notice some of the other interesting topics which are suggested by their perusal.

JOHN W. JUDD

ORGANIC CHEMISTRY

Adolph Strecker's Short Text-book of Organic Chemistry. By Dr. Johannes Wislicenus, Professor of Chemistry in the University of Würzburg. Translated and edited by W. R. Hodgkinson and A. J. Greenaway. 8vo. (London: Kegan Paul, Trench and Co., 1881.)

THE new edition of Strecker's text-book by Prof. Wislicenus, published in 1874, is well known as giving a concise and comprehensive view of the state of organic chemistry at the time of its publication, and some useful additions, relating to recent discoveries, have been made by the English translators.

The classification of organic compounds in this, as in all recent works on organic chemistry, is based upon the hydrocarbons. All organic compounds of known constitution are divided into the two great groups, Fatty and Aromatic, and in each of these the saturated hydrocarbons—paraffins in the first, benzene and its homologues in the second,—are first described; next their mono-substitution derivatives: alcohols, ethers, amines, phosphines, &c.; then in succession the di-, tri-, tetra-, &c., derivatives. With regard to this matter Prof. Wislicenus says in his preface: "The most systematic arrangement would be found in the number of carbon-atoms in direct union. In each such group of equal carbon contents the paraffin would come first, next those derivatives in which only a single hydrogen-atom had been replaced, these being arranged according to the valency of the substituting element. Then would follow the di-substitution products. . . Next the tri-substituted paraffins. . . . This order of arrangement is very valuable for the study of organic chemistry, more so however for those moderately acquainted with the subject than for beginners. For the latter I think we cannot dispense with the study of homologous series, especially in the early part of a text-book. In this way alone can the clear differentiation of the various categories be made evident, depending, as they do, not so much on the accumulation of carbon-atoms, as on the nature and amount of the other elements in union." It is worth while to compare these remarks with those made by Roscoe and Schorlemmer in their lately-published "Treatise on Organic Chemistry," at p. 129 of which we read:—"Perhaps the most systematic mode of arrangement would be to commence each group (fatty and aromatic) with a discussion of the hydrocarbons, and then to follow on with a description of the series of substances obtained by the replacement of one, two, three, or more of the constituent atoms of hydrogen. Such a mode of classification, however, labours under the disadvantage that compounds which stand as a rule closely together, as, for example, the alcohols $CnH_{2n+2}O$ and the acids $C_nH_{2n}O_2$, are thus found widely separated, whilst other groups possessing but little analogy are brought into proximity. Hence it is desirable, alike for the sake of perspicuity as for the purpose of showing the genetic relationships between different bodies, to depart in many cases from such a systematic treatment, and arrange the compounds according as they are derived one from the other." It will be seen from these quotations that each author regards the arrangement adopted by the other as the most systematic, but prefers his own as best adapted to the requirements of the student.

The additions made to the work under consideration by the English editors belong chiefly to the aromatic group, but no mention is made of the recent investigations of Nevile and Winther, published last year in the Journal of the Chemical Society, on the Bromotoluenes, which are especially interesting on account of the light which they throw on the influence exerted by the groups or radicles which have replaced certain hydrogen-atoms in a benzene nucleus, on the position taken up by other radicles which take the place of the remaining atoms of hydrogen. In the series of paraffins there is an omission of the normal Heptane, lately discovered by Dr. Thorpe in the turpentine of *Pinus Sabiniana;* and amongst the nitroparaffins no notice is taken of the Nitrolic acids and Pseudonitroles. Under the organic compounds of boron we miss Dr. Frankland's Ammonioboric methide, $(CH_3)_3B{=}NH_3$ and Diboric ethopentethylate, $(C_2H_5O)_3B{=}B(C_2H_5)(OC_2H_5)_3$, in which boron figures as a pentad; and under guanidine there is no account of the Guanamines, $C_{n+2}H_{2n+3}N_5$, a series of bases discovered by Nencki in 1874 and 1876, and formed by the action of heat on the guanidine salts of the fatty acids.

The translation reads well, and, with the exception of a few instances of somewhat too close imitation of German forms, is expressed in good idiomatic English. There are, however, certain irregularities of nomenclature which it may be worth while to notice, partly with the view to correction in future editions, partly because the greater number of them are not peculiar to this work, but are of very frequent occurrence in our chemical literature.

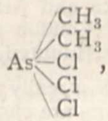
On p. 91 we read: "Carbon monoxide is the common radical of the carbonic acid derivatives, and as such is termed 'carboxyl'"; on the next page the compound CONH is called "carboxylimide"; and on p. 98 we read, "corresponding to carbo*x*yl is the radical CS, thiocarbo*ny*l." Here (and in the original) there is surely an inconsistency; if CS is thiocarbonyl, CO should certainly be called carbonyl; and such in fact is the name hitherto given to it by all writers, whereas carboxyl always denotes the group COOH. By a similar inconsistency the term *Ethyl-carbonic acid* is used on p. 353 as a synonym of propionic acid. Now most readers would probably understand by this term the compound $CO(OC_2H_5)(OH)$, *i.e.* carbonic acid having one of its hydrogen-atoms replaced by ethyl—an acid of which several salts are known—whereas propionic acid is $C_2H_5.COOH$, and its proper synonym is *ethyl-carboxylic acid*. The mistake here made arises from a too close imitation in sound of the German term "Carbonsäure," which, with the prefixes mono-, di-, tri-, was introduced by Kolbe to denote the number of carboxyl-groups, COOH, contained in an organic acid. In many instances however this term is correctly rendered; thus on pp. 557 and 561 we find the acids $C_6H_4(COOH)_2$ spoken of as *benzene-dicarboxylic acids*, though further on (p. 653) the same acids are called *phenylene-dicarbonic acids*. It seems indeed as if the two terminations were used indiscriminately.

Another irregularity of frequent occurrence in English nomenclature is the indiscriminate use of the terminations *in* and *ine*. Dr. Hofmann suggested some years ago that *ine* should be used exclusively for organic bases, and *in* for neutral bodies, such as glucosides, bitter principles, proteids, &c. This rule has been followed by some authors, and the writer of this review has taken some pains to recommend its general adoption; but the two terminations are still, by many writers, used without discrimination. As examples of this in the volume under consideration may be cited, on the one hand, gelatine, cholesterine, and on the other, chondrin, albumin, fibrin, dyslysin, &c. Now the use of special terminations for each group of compounds is very much to be desired; it is by no means an innovation, but, on the contrary, is as old as our systematic nomenclature itself; witness the well-known rule that the names of acids shall end in *ic* and *ous*, and those of the corresponding salts in *ate* and *ite*. To extend this regularity of termination to the names of all classes of compounds, especially in organic chemistry, is a main object of the rules lately issued by the Council of the Chemical Society to the Abstractors for that Society's journal, and its general adoption would certainly lead to a great improvement in our nomenclature in point of regularity.

The habit already noticed of too closely imitating foreign forms sometimes leads to awkwardness of expression in translating, as on p. 103, where it is said that "the paraffins burn easily when heated in an *oxygen-containing* atmosphere" (*in einer sauerstoffhaltenden Atmosphäre*); now it would have been quite as easy, and more in accordance with English usage, to say "in an atmosphere containing oxygen." Similar remarks may be made respecting the expression "carbon-free radical," which occurs on p. 565. It is worth some trouble to keep our language pure, and there is no more fruitful source of corruption in a language than the careless imitation of foreign words and idioms. And here I cannot avoid entering a protest against the use, in English speaking and writing, of the French words *mètre*, *décimètre*, &c., instead of their English equivalents, meter, decimeter, &c. *Meter* is a true English word, and is used both singly and in combination, as in the words barometer, thermometer, gasometer, &c., and there is therefore not the slightest occasion for interlarding our sentences with the French forms in question.

The translation affords also some instances of a very common error, viz. a confusion between the terms *substitution* and *replacement*. These words are indeed commonly regarded as synonymous, whereas they are really correlative, and the relation between them is this: *When* A *comes in and* B *goes out*, A *is substituted for* B, *and* B *is replaced*, *or displaced*, *by* A. The common error is to say "substituted," where the proper term would be "replaced." Examples both of the correct and incorrect use of these words may be found on pp. 100 and 101, *e.g.* "The hydrogen-atoms of the paraffins can be replaced . . . . by the halogen-atoms," &c.: this is correct; but a little lower down we find, "By *substitution* of only a single hydrogen-atom . . . ."; it should be by *replacement*. The same mistake occurs on the last line of p. 100; on the other hand the word "replaced" is correctly used in several places on p. 102. It would seem, therefore, that the translators regard the two words in question as synonymous.

Next with regard to notation: Many of the graphic formulæ throughout the volume are unnecessarily drawn out into long vertical columns, where they might with equal clearness have been printed horizontally; in one instance indeed seven formulæ fill up a whole page. In this, however, the English editors have simply followed the practice of the original work; but this was printed in 1874, and since that time it has been found that chemical formulæ may for the most part be printed much more concisely without any sacrifice of clearness. The formula of arsenic trichlorodimethide, for example, which is printed in the form



, might perhaps have been condensed into $(CH_3)_2AsCl_3$, without doing any great violence to the views of the author.

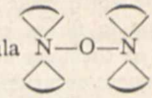
A more important matter, however, relating to notation is the habitual omission—sanctioned indeed by prevailing usage—of brackets in formulæ, where they ought to be inserted. It is of course unnecessary to insist upon the difference between $2a + b$ and $2(a + b)$, with which every schoolboy becomes familiar at a very early stage of his mathematical studies; but unfortunately it has lately become the fashion to ignore this difference in chemical formulæ, and to represent, for example, two molecules of alcohol by $2C_2H_5.OH$ instead of the proper form, $2(C_2H_5.OH)$. Now the neglect of this difference is of quite recent introduction; for in chemical books of older date it was always observed, in proof of which see Gmelin's "Handbuch der Chemie" throughout. Gmelin indeed, in the first volume of his great work (4te Auflage, 1843, p. 61, and English Edition, i. 61) lays down the law of the case as follows:—"A number placed before several symbols multiplies them all, *as far as the next + sign or comma;* or if it stands before a bracket, it multiplies all the symbols and numbers included within the brackets." This rule is consistently followed all through the "Handbuch," and, so far as I know, in most contemporary chemical writings; but lately it has fallen into disuse, and a numeral placed before a set of unbracketted symbols is supposed to multiply them all, whether separated by addition-signs (+ , .) or not. Now this last practice would be all very well if consistently followed out; but unfortunately it is not, and hence confusion arises. For example, the formula $2SO_3, H_2O$ is used, sometimes to signify $S_2O_7H_2$, that is to say, one molecule of pyrosulphuric acid, while at other times it is employed to denote $S_2H_8O_4$ or $2SO_4H_2$, *i.e.* two molecules of sulphuric acid, which latter, according to earlier usage, would have been represented by $2(SO_3, H_2O)$. Again, in the formulæ of basic salts we find such expressions as $3Fe_2O_3, SO_3$, and $2Fe_2O_3, 3SO_3$, &c., in which the co-efficient 3 or 2 is understood to multiply only the Fe_2O_3, without affecting the SO_3; these formulæ being in fact identical with $SO_3, 3Fe_2O_3$ and $3SO_3, 2Fe_2O_3$ respectively. Now it is easy to see that this varying practice in the use or omission of brackets must lead to confusion, and it is much to be desired that the rule which formerly prevailed should be restored to use.

In conclusion, I hope it will be understood that the preceding criticisms are offered solely with the view of promoting uniformity in our nomenclature and notation, and by no means in disparagement of the volume under review, which is in every way a useful and valuable addition to English chemical literature. H. WATTS

OUR BOOK SHELF

Inorganic Chemistry, Theoretical and Practical. An Elementary Text-Book. By William Jago, F.C.S., &c. (London: Longmans, Green, and Co., 1881.)

Practical Chemistry. Adapted to the First Stage of the Revised Syllabus of the Science and Art Department. By J. Howard, F.C.S., &c. (London and Glasgow: William Collins, Sons, and Co., Limited, 1881.)

THE first-named of these books is a really good text-book for laboratory use; the experiments are clearly described; most useful "laboratory hints" are given; conclusions are carefully drawn from the experimental data obtained. The methods for proving the definition of boiling point, for illustrating the manufacture of sulphuric acid, and for confirming quantitatively the equation $KClO_3 = O_3 + KCl$, are especially to be praised. The student who works through this book will have laid a good foundation on which he may afterwards build; only let him skip those parts which deal with "chemical philosophy." Why should he begin his chemical career by learning that "combining weight" is synonymous with "atomic weight" (p. 31)? Why should he trouble himself with committing to memory the "atomicity" of the most important elements as given on p. 27 of this book? Why should he draw from the statement of Avogadro's law the erroneous conclusion that "the molecules of all gases are of the same size"? Why should he deceive himself by fancying that the formula  (p. 143) gives him accurate and well-grounded information regarding the molecule of nitrous oxide? No good reason can be given for doing any of these things, therefore let the student use this book as a laboratory guide only, and he will doubtless find it a trustworthy guide.

Could Mr. Howard's chemical philosophy be separated from his directions for conducting experiments, his book might also be recommended to the student of practical chemistry.

Although this book deals with laboratory experiments, one is much tempted to think that the author does not really regard chemistry as an experimental science. He deals with the general principles of chemical science too much from a literary point of view. An instance of this method is found in the preface, where we are told that "in former editions . . . the notation of Dr. Frankland was alone used. . . . In the present edition, however, it has been thought advisable to give, in addition, the notation and formulæ used by Professors Roscoe, Williamson, Thorpe, and others." This sentence is decidedly humorous; it connects so closely phenomena which appear to the student of chemistry to have but little in common.

Authoritative statements from the text-books exert a great influence on the author of this book; witness a sentence on p. 62: "A molecule must have all its bonds engaged, that is, it cannot combine with any substance without altering the arrangement of the atoms. Hence, there must always be an even number of *bonds* in the molecule of any element or in any compound." Nitric oxide is of course formulated as N_2O_2; no hint is given that the molecular formula of this gas is NO.

The first few pages contain many excellent examples of the misuse of that much misused word "force."

LETTERS TO THE EDITOR

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

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

Primitive Traditions as to the Pleiades

MR. JUSTICE HALIBURTON'S letter of December 1 (vol. xxv. p. 100) will have been read by many as calling attention to a curious subject. As it refers especially to me, and indeed arises out of my remark on the story of the "Lost Pleiad" in Dawson's "Australian Aborigines" (NATURE, vol. xxiv. p. 530), I now write a few lines in reply. But it will not be possible to discuss properly Mr. Haliburton's ideas as to the Pleiades till he publishes them in full, with the evidence on which he grounds them. It must not be supposed that the subject has been unnoticed till now by anthropologists. That the Pleiades are an important constellation, by which seasons and years are regulated among tribes in distant parts of the world, that they are sometimes worshipped, and often festivals are held in connection with their rising, that their peculiar grouping has suggested such names as the "dancers," or "hen and chickens," and that numbers of myths have been made about them—all this has long been on record, though in a scattered way, and at any rate it is well known to students. Mr. Haliburton's letter shows that he has new information to add to the previous stock, and furthermore that he has formed a theory that the Pleiad beliefs go back to a marvellously remote period in the history of man, when these stars were, as he says, the "central sun" of the religions, calendars, myths, traditions, and symbolism of early ages. If the astronomical evidence is to support so vast a structure as this, it need hardly be said that it must go far beyond what Mr.

Haliburton mentions in his letter. But when his contemplated book is published, he may be sure of his facts being appreciated and his theories fairly dealt with. Though, as I have just said, this cannot be done here, I may be allowed one suggestion. Mr. Haliburton is good enough to speak of me as being a cautious person. May I in that capacity express a hope that verbal coincidences, when not close enough really to prove connection, may be kept out of an argument which ought to go on a more solid footing. Why should the name of the star *Alkyone* have anything to do with the name of *Alkinoos*, king of Corfu? They look indeed rather more alike in Mr. Haliburton's letter, where the latter name is misspelt with a *y*, but doubtless this is a slip of the writer or printer.

A word about my remarks on the Pleiades-myth which has led to this correspondence. The question is only a small one, belonging to comparative mythology, whether a particular Australian tale about the Pleiades, one of a dozen such known in that quarter of the world, is a genuine native myth or a spoilt version of a story borrowed from the white men. I doubted its being genuine, because it says that the lost one of the seven was the queen or chiefess. This is hardly according to nature, for we should expect the star supposed to have gone away to be one of the insignificant ones of the group, not such a bright one as a story-teller would call the queen. Of the many Englishmen who have heard of the "Lost Pleiad" it is curious how few know the probable explanation of the classic tale, as a nature-myth derived from the difficulty of making out more than six stars with the naked eye. It has been suggested by some that there may have been a loss of brilliancy in one of the smaller stars of the group since ancient times. If any of your astronomical readers think there is anything whatever in this supposition, it would be interesting to have their judgment on it.

EDWARD B. TYLOR

Fumifugium

IN justice to Evelyn it ought, I think, to be made known that Mr. Shaw Lefevre was entirely wrong in stating at the opening ceremony of the Smoke Abatement Exhibition that "Evelyn proposed as a remedy for the smoke of which he complained, that the use of coal should be prohibited in the City and neighbourhood of London." "Fumifugium" (which was printed in 1661, and not in 1644) is of course extremely rare, and even the editor of the reprint which was issued in 1772, and is now rare, calls the original "this very scarce tract," so that the way in which the blunder in question has been repeated, is perhaps not to be wondered at.

As a matter of fact Evelyn only mentions the idea of supplanting coal by wood to call it "madnesse," and he then goes on to say: "But the *Remedy*, which I would propose has nothing in it of this difficulty, requiring only the Removal of such *Trades* as are manifest *Nuisances* to the City, which I would have placed at further distances, especially such as in their Works and Fournaces use great quantities of *Sea-Coale*, the sole and only cause of those prodigious Clouds of *Smoake* which so universally and so fatally infest the *Aer*, and would in no city of *Europe* be permitted, where men had either respect to Health or Ornament," thus recognising the two points of view so well represented by the cooperation of the National Health and Kyrle Societies. "I propose, therefore," he continues, "that by an *Act* of this present *Parliament*, this infernal *Nuisance* be reformed; enjoyning that all those *Works* be removed five or six miles distant from *London*, below the River of *Thames*, &c., &c." Although this has been done to a considerable extent, we may, I think, on a foggy day, agree with Evelyn when he says that "the City of *London* resembles the face rather of *Mount Ætna*, the *Court of Vulcan*, *Stromboli*, or the suburbs of *Hell*, than an assembly of rational creatures and the imperial seat of our incomparable *Monarch*."

W. H. CORFIELD

Jamaica Petrel

THIS bird, known locally as the "Blue Mountain Duck" or "Booby Duck," appears in a carefully compiled list of the birds of Jamaica, by Prof. A. Newton and his brother, the Hon. Ed. Newton, Colonial Secretary of Jamaica, published in the "Jamaica Handbook, 1881, p. 117, as follows:—"PROCELLARIIDÆ—*Œstrelata jamaicensis*, Bancroft, Jamaica Petrel. *Procellaria jamaicensis*, Bancroft, *Zool. Journ.* v. p. 81; Blue Mountain Duck, Gosse, "Birds of Jamaica," p. 437 (Hill); *Pterodroma caribbæa*, Carte, *P.Z.S.* 1866, p. 93, Pl. x." During certain seasons of the year it is remarkable that this sea-bird should be found in holes under trees and in burrows on the Cinchona plantations and in the unfrequented woods of the Blue Mountain range, at elevations from 6000 feet to 7000 feet. The natural inference was that the birds make their nests on these places. But, although careful search has been made during the last two years, and a reward offered for nests, eggs, or any signs of nidification, nothing whatever has been found in that direction. It is therefore very probable that the birds use these holes and burrows simply as resting-places during the day, from whence they sally forth at night to their feeding-grounds at sea. The latter is distant only, as the crow flies, about twelve or fourteen miles. The birds are found in their burrows chiefly during the months of November, December, January, and March. Sometimes two lie in one hole, and dogs easily find them; but it has been noticed that the birds are always full grown, and with no apparent nest. I have been led to send you these remarks in the hope that possibly some of your readers with a wider knowledge of the habits of petrels might be able to give some clue as to the locality and general character of their nesting-places.

D. MORRIS

Botanical Department, Jamaica, November 14

Biology in Schools

MANY eminent biologists seem to think that there are insuperable difficulties in the way of sound biological instruction in public schools. Possibly my experience in this connection may be of interest. I began to teach biology some ten years ago. Two years' experience satisfied me "that the power of repeating a classification of animals with all the appropriate definitions has any thing to do with genuine knowledge is one of the commonest and most mischievous delusions of both students and their examiners." For the third year I prepared a series of laboratory notes sufficient for the dissection of a few plants and animals. Since the publication of Huxley and Martin's admirable text-book of biology we have used that as a laboratory guide. Through the liberality of the School Board we are provided with eight of Beck's students' microscopes. We begin with the study of the torula; we then take in succession the following organisms:—Protococcus, amœba, bacteria, mould, stone-wort, ferns, flowering plants, infusorian fresh-water polyp, clam lobster, and frog. We devote to laboratory work one hour daily for seven months. At the end of the course come morphological and physiological generalisations. Our classes number about eighty, and are divided into working sections of sixteen each. The average age of the students is sixteen years, rather more than half of them being girls. I have found the students eager and enthusiastic, and a large majority of them regret the untimely end of their study of biology. To enter college a lad needs between four and five years' work in Latin, and, if a scientific student, about five weeks in botany. Most of our high schools accept this estimate of the value of a scientific training, and only do the little that is necessary for the pass examination.

GEO. W. PECKHAM

Biological Laboratory, Milwaukee High School,
Milwaukee, Wisconsin

A Natural Ant Trap

LAST June I was staying at Husum, in the Lærdal Valley, Norway, and observed on the almost precipitous sides of the valley facing the south, immediately behind the station-house, a considerable number of the red German catchfly (*Lychnis viscaria*). The plants were growing luxuriantly at an altitude of some 1000 feet above the bed of the river, and were just then showing a gorgeous array of blossoms. On plucking some of the flowers I became aware of a most unpleasant stickiness around the stems; in some instances the glutinous secretion being powerful enough to support the whole weight of the stem when I inverted and opened my hand. Thereupon I carefully examined more than a hundred plants, and was somewhat surprised at finding, on quite 95 per cent., either the dead bodies of a large species of ant, or individuals in all stages of dying. Some flowering stems had only one dead or dying ant upon each; others had two; others three; whilst others again had as many as seven or eight. Some ants had, as it were, simply lain down in the glutinous matter and succumbed without further struggling. The heads of others, firmly imbedded in the treacherous stuff, with the rest of the body stiffened and suspended in mid-air, testified to violent and prolonged resistance. Some ants again had the body arched up, as if to avoid contact with the stem, and the legs only were fatally caught.

As is well known, the glutinous or sticky tracts lie around the stem directly beneath the nodes, and are about half an inch or more in depth. These glutinous zones are absent from the nodes, which are lower down on the stalk. But a darkening of the colour, just similar to what one sees below the sticky nodes, suggests the probability of these non-sticky nodes having been sticky at some former time.

I can find no reference in the ordinary books to the fact that ants visit, and die upon, this plant. In Smith's "English Botany," 1800, however, occurs the following remarkable account of *Lychnis viscaria*:—"Stem straight, about a foot high, simple, angular, leafy, dark brown, and clammy under each joint, by which insects are plentifully caught, as in several other plants of the pink or campion tribe, for what purpose no one has yet ascertained; probably their decaying bodies form an air which is salutary to vegetable life." As I do not quite understand the author's meaning in the latter part of his remarks I naturally forbear criticising the statement, and mention it here merely to show the opinion of a botanist on the subject eighty-one years ago.

On each flowering stem there are from two to four sticky nodes. I found that the majority of the deaths had occurred in the first zone of stickiness; fewer in the second, and still fewer at the higher nodes. Those ants therefore which gained the summit of their ambition would be pre-eminently strong and lusty, for to have arrived at the top of the plant among the flowers, they must have waded through morasses, each of which was sufficient to cause the death of many of their comrades. I found very few ants at the summit of the flowering stalks, and those that I did find there alive showed, from their want of vigour and restlessness, that they had been severely tried by the ordeals through which they had passed. The plant was growing in very rocky soil, each specimen quite isolated from any surrounding vegetation; so that I am satisfied that no ants, on the plants I examined, could have gained the summits by adventitious aids.

Time and the want of proper apparatus prevented my making some experiments I wished to have tried, and as I do not know when again I shall be able to pursue this most interesting investigation, your kind insertion of this may perhaps induce some of your readers to pursue the subject further. These are amongst the questions which have occurred to me:—(1) Is there any attraction in the glutinous secretion, or does the attraction lie in the flowers? I saw no ant-hills or nests anywhere in the neighbourhood of the flowers, and my impression at the time was that the ants had come a long distance. I scrutinisingly examined the ground, and, to my astonishment, found that almost the only ants on the spot were upon the plants. (2) How is it, if these sticky zones are simply to prevent ants and other small walking animals from getting to the flowers, that they do not occur at the lower part of the stem as well as higher up? (3) What injury, if any, do ants cause to this plant? (4) Is it likely that the plant derives direct benefit from the deaths that take place upon it? Is there, in short, any digestive action in the glutinous secretion, and any absorptive power in those portions of the stem where it is found?

I brought home some specimens showing the dead bodies of ants stuck to the flower stalks, and these were exhibited at the last meeting of the Linnean Society. I shall be happy to show them to any who are interested in the subject. J. HARRIS STONE

11, Sheffield Gardens, Kensington, December 2

Solar, Gas-Flame, and Electric-Light Spectra

IN answer to Mr. J. Hopkins Walters' inquiry contained in NATURE, vol. xxv. p. 103, the spectroscope declares that all these three spectra have for their base a continuous strip or band of light; in the case of gas-flame (the bright part) crossed by the sodium lines only; in that of the sun by the well-known Frauenhofer dark lines; and of the electric (arc) light by the bright lines of carbon. The illuminating power of each of these sources of light is thus shown to be due to the incandescence of their several solid and gaseous constituents, concerning which a volume might be written. The relative effect of the sun's bright golden glare, the gas-flame's duller yellow tint, and the electric-light's moon-like whiteness, on the optic nerve; have not, as far as I am aware, been yet made the subject of special research. Popular opinion assigns injurious results to the whiter light. Mr. Walters will find in "Photographed Spectra," on Pl. xv., Fig. 4, and the extra plate, the solar spectrum, and on Pl. v., Figs. 3 and 4, the spectrum of the electric arc between carbon points specially prepared to insure purity. In Dr. Marshall Watt's "Index of Spectra" the spectrum of the blue base of candle-flame is represented by the graphical diagram and description, Carbon I. The illuminating portion of a gas-flame presents in the spectroscope the appearance of a dull sun spectrum without the dark lines. J. RAND CAPRON

Guildown, December 3

Tele-dynamics and the Accumulation of Energy—their Application to the Channel Tunnel

A REMARKABLE opportunity is now presented to electrical and mechanical engineers of applying to eminent practical service the recent discoveries and advances made in relation to the accumulation and transmission of energy in the form of electricity. I allude to the construction and working of the Channel Tunnel Railway. Of course the direct application of steam-power to the work of boring is out of the question. The power employed in boring Mont Cenis and St. Gothard was transmitted by compressed air through metal tubes, but this is a very costly, wasteful, and in some respects inconvenient process; and this cost and waste increases in a very high ratio to the distance of transmission. Since those works were executed an immense advance has been made in the practice of transmitting energy by electric current, and particularly in storing that energy; and I predict that if the tunnel is ever completed (which I do not doubt) it will be by means of electrical agency. An eminent civil engineer, who had invented a boring-machine which he considered of great promise for that work, told me more than a year ago that Dr. Siemens assured him that he would undertake to transmit 50 per cent. of the initial power by electric current half way through the tunnel; and by this time he would most probably give a much larger percentage. An eminent French authority promises from sixteen to twenty horse-power by a single current over a distance of from ten to fifty kilometres. If these statements are founded on fact your readers will at once realise the applicability and potency of the agent. Then there must of necessity be an immense quantity of material to be carried to and fro. The electrical railways of Berlin, Brussels, and Paris have left no question open as to the easiest and most economical means of propelling the trollies; and by using several conductors as many trolly trains in succession could be run as there would be conductors. It would be premature to discuss now the subject of working this railway, but it is certain that electricity will be the agent, and there is very little doubt that the twenty miles of level tunnel way will be worked by energy generated and stored by the train itself in its descent from the land level to the tunnel level. An examination of this question in detail would be incompatible with your space and purpose. I will simply say that a train of 100 tons descending a gradient of 1 in 100 for five miles would start with a potential force of nearly 60,000,000 foot pounds, a very small portion of which would be expended in useful work. Let the surplus of this be applied not to destroying the rails by brakes and conversion into useless heat, but by revolving generators and storing the product to be used in again turning the generators (now motors) for propelling the train. I do not say that the train could be lifted up the five miles at the other end by this stored energy: the engineers may be intrusted with that duty. E. WALKER

Tottenham

JOHNSTON LAVIS.—Your paper wants beginning and title. Please send.

DANTE AND THE SOUTHERN CROSS.—A correspondent inquires where Dante could have learned about the Southern Cross, to which there is evident allusion in the first canto of the "Purgatorio."

JAMAICA

OF all the West Indian Colonies appertaining to the British Crown, that of the Island of Jamaica can claim to be the largest in area, the most numerous in population, and the wealthiest in revenue. Within half a century of being the oldest of the English possessions in the new world—Barbadoes was settled in 1605; Jamaica was capitulated in 1655—it has, though with many vicissitudes, been the most successful, and it has always shown strong signs of a healthy life, in that it has recovered promptly and well from its periods of misfortune. The extreme length of this fertile island is about 144 miles, while its greatest width is 49, and its least width 21 miles. Its surface is extremely mountainous, attaining a maximum in the Blue Mountain Peak of some 7360 feet. Of its superficial area of 4139 square miles only about 646 are flat, consisting of marl, alluvium, and swamps. It possesses numerous rivers and springs, and a fertile soil. Its total of population in 1861 was 441,264; in 1871, 506,154.

A Handbook to Jamaica, compiled from official and other records, has lately been published at the Government printing establishment at Kingston. It has been most judiciously edited by two members of the Jamaica Civil Service, and forms a volume of 450 pages, which deserves to be known to all interested in our colonies. Passing over the first two parts of the volume, which contain matter of chiefly local interest, the third part contains a chronological history of the island, with an account of its various parishes, its mountain ranges, lakes, and rivers, and an excellent sketch of its mineral resources, from which it would appear that the natural resources of the island have not as yet been satisfactorily explored. The fourth part is devoted to the consideration of the meteorology and climate, and of the birds, fishes, and insects of the isle.

Mr. Maxwell Hall is to be congratulated, that, after some opposition and under great difficulties, he has succeeded in some measure in establishing a system of daily weather reports, which are sent daily to the local press for publication. As the result of the reduction of a series of observations on the rainfall in different stations, and extending over periods of from five to fifteen years, Mr. Hall has been able to make out a certain systematic distribution of the rainfall over the island. It would thus appear that, while the May and October rains are everywhere strongly marked, the northern part of the island has winter rains in November, December, and January, the southern part has summer rains in August and September; and it would appear from the tables given that each part is further divided by the amount of the rainfall. Thus the north-eastern has the greatest rainfall; the west central comes next; the northern division third, and the southern has the least annual rainfall. Some such distribution, Mr. Hall thinks, was also existing at the time Sir Hans Sloane wrote his "Natural History of Jamaica" (about 200 years ago), and although he sees in the records of Sloane a change in the rainfall, yet he believes this to be not a constant change, such as might indicate a continually diminishing rainfall, but a variable change, probably systematic and periodic. On the question of the influence of forests on the rainfall, he decides that woods and forests are chiefly beneficial in reducing the range of temperature, and in maintaining the moisture of the ground, thereby preserving a constant supply of water for the springs and rivers. It may be noted that the central and uncultivated parts of Jamaica are still densely wooded, thereby aiding the constant river supply. Jamaica has two rainy and two dry seasons. The rainy seasons are in May and in October, lasting about two months, the intervening periods being dry. The climate may generally be described as a sedative one.

The Catalogue of the Birds of Jamaica is compiled by the well-known ornithologists Alfred and Edward Newton, the latter Colonial Secretary. Forty-three birds are enumerated as presumably peculiar to the island, that is to say, not known to have been found elsewhere. The list of the fishes is large. The river chub (*Labrax mucronatus*) is described as a "surpassingly delicious fish." Though fish abound in the seas, and each district has a sea-frontage, yet the yearly importation of cured "fish stuffs" of different kinds amounts in value to 200,000*l.* a year. To help and remedy this state of things the Jamaica Institute has offered a series of prizes for preserved fish.

The fifth and sixth parts treat of the economic botany of the island. The Government Surveyor, in reporting on the timber supply, estimates that there are at present about 800,000 acres of timber-producing forest in the island; that out of this there might be cut each year—without permanent injury—400 feet to the acre, say 320,000,000 as an annual supply; of this large amount only some 3,500,000 a year are actually cut for building purposes. About 166,000*l.* worth of fine timber was exported in 1880, but a large quantity of lumber and shingles is imported. This state of things Mr. Harrison accounts for by the difficulty of getting the timber out of the mountain fastnesses where it grows. He does not seem to agree with Mr. Hall on the subject of the change in the rainfall, for he declares that he has ascertained beyond doubt that forests exercise a great influence on it, that where the forests have been destroyed the rainfall has diminished, and he alludes to springs becoming dried up, and rivers that have ceased to flow. A very interesting list of some fifty of the woods of Jamaica, their qualities and the uses they are generally put to, is appended to this report. The island would appear to be a paradise for the fern collector, over 450 species being enumerated. Within a radius of five miles, taking Morce's Gap as a centre, over 200 species are to be found. The orchids are not so numerous, only 135 being named.

Of the main crops of the island, sugar still heads the list, the value of that exported in 1880 being 768,792*l.* The value of the coffee raised in the island in the same year is calculated at 381,595*l.* The coffee of the Blue Mountains is celebrated for its superiority, but a good marketable article is grown throughout the island. In a most valuable report by Mr. Morris, the present Director of Public Gardens strongly urges the propagation of the Liberian coffee, which was introduced in 1874. From the fact that this coffee will grow on the plains, where the preliminary expenses in the acquisition and clearing of land are lower than on the hills, where labour, too, is cheaper and more abundant, and where the difficulties and expenses of labour would be avoided, Liberian coffee possesses advantages not only over the Arabian coffee, but over almost any cultivation requiring the same capital and attention. Among the minor crops, that of the fruit crop is steadily and remarkably increasing in value, from 10,000*l.* in 1834 to 51,000*l.* in 1880. Jamaica tobacco is finding its way into the market. In the German markets—considered the most important for leaf-tobacco—Jamaica tobacco is well thought of, and in price ranks next to Havanna leaf, and since 1879 the consumption of Jamaica cigars in England has spread in an extensive manner. The cocoanut export, from a value of 3,357*l.* in 1870, has risen to 20,525*l.* in 1880. Ginger, pimento, and cacao are all successfully grown. The introduction of cinchona cultivation into Jamaica through a liberal supply of seeds sent in 1861 by Sir W. J. Hooker, promises to be a great success. For the year 1879-80 the quantity of bark shipped was 27,399 lbs., which realised the net sum of 5146*l.* From an elaborate report by Mr. Morris we take the following:—The plantations are estimated to cover nearly 400 acres; owing, however, to the practice of wide planting, the actual area occupied by regularly-planted trees is probably only a half of this. The advantages of close planting are undoubted. The climate of Jamaica would seem to be peculiarly well adapted for the successful cultivation of one or other of the various species of cinchona, at all elevations, from about 2500 feet to the Blue Mountain Peak itself. Thus *Cinchona succirubra* flourishes in the parish of Manchester at an elevation of

2700 feet, with a rainfall of about 120 inches and a mean annual temperature of 70° Fahr. This is perhaps the lowest elevation for the more valuable cinchonas at the Government plantations; the same species flourishes at 5000 feet, with an annual rainfall of 136 inches and a mean annual temperature of 60° F. The trees at this elevation do not seed freely, and it may be taken as the highest at which it would be advisable to cultivate the red bark in Jamaica. The range of cultivation for the valuable crown bark (*Cinchona officinalis*) is between 4500 and 6300 feet of elevation. It may here be convenient to refer to the department—that of Public Gardens and Plantations—which was newly organised in 1879. This department has under its control the Botanic Gardens at Castleton and Bath, the Park at Kingston, the Cocoanut Plantation at Kingston Harbour, and the Hope and Cinchona Plantation. The staff is directed by Mr. Daniel Morris, M.A., who had been assistant at the Ceylon Botanic Gardens. To an island dependent as Jamaica is for its prosperity on the produce of its soil, the importance of such a department is undoubted, and we trust that the new director will receive all due encouragement in developing the botanical treasures of the place.

The concluding parts of this most interesting handbook are devoted to the political constitution and parochial boards of the island; to the details of the various departments and colleges; to the statistics of population, crime, &c.; to the laws of quarantine, &c.; together forming a most useful volume of reference. There seems little doubt that if the capabilities of Jamaica were better known, it would attract the attention of settlers. There are surely as great attractions in bark or coffee-growing as in wool-growing, and Jamaica is nearer to us than the Australian colonies, and, with due precautions, as healthy a climate to live in.

OUR WINTER REFUGES—THE SOUTH OF ENGLAND[1]

II.

As regards temperature and rainfall the South of England, from Dover to Portland, presents a unique and well-marked winter climate, quite distinct from that of any other tract of the British Islands. The tract in question embraces the comparatively narrow belt, varying in width from two to ten miles, stretching between these two places and backed on the north by the sheltering range of the South Downs.

The rainfall in the east of England, from the Humber to Ramsgate, varies in the average annual amounts from 22 to 25½ inches; but on reaching Dover it rapidly rises to 30 inches, and from this point westward to Portland the rainfall varies only from 28 to 30 inches, the amounts differing within these narrow limits according to the flatness or boldness of the coast, and the character of the country in the immediate neighbourhood. To the west of Portland, along the coast, the rainfall rises considerably, and after passing Prawle Point, more rapidly to 44 inches at Penzance. Further, on striking inland from the coast towards and up the slopes of the Downs, the annual amounts increase to about 34 or 36 inches, on the high grounds separating the valley of the Thames from the lands sloping south to the channel; and from this ridge northwards it gradually falls to about 25 inches round London. Thus the Downs, as regards the rainfall and the winds, have important bearings on the meteorology of the south of England.

Equally decided are the temperature characteristics which mark off, climatically, these districts of England from each other. We may accept January as fairly representing the temperature peculiarities of the winter months. In this month the mean temperature of the whole of the eastern slope of Great Britain, from Wick to Dover, varies only from about 37°·5 to 38°·5, the temperature of the coasts being a little higher than that of the interior. But on arriving at Dover we encounter a January mean temperature of 40°·0, and from this point westward there is a steady increase, first slow as far as Worthing, where the mean is 40°·4, and then more rapid to Bournemouth, where the mean is 41°·2. On advancing inland upon the Downs, temperature falls much more rapidly than what is due to mere height, and this fall is continued in proceeding northwards towards London, the mean of which is 2°·5 and 1°·5 lower than that of Bournemouth and Brighton respectively. West of Portland the increase of temperature is still more rapid, the mean being 42°·9 at Torquay, 44°·6 at Falmouth, and 46°·2 in the Scilly Isles, the last temperature being the mean of London in the beginning of April.

Hence if the invalid requires a winter climate characterised by the combined qualities of mildness and dryness, such a climate must be sought for on the shores of the Channel from Dover to Portland. In the south-west a much higher temperature may be had, but the climate is there damper, and raw weather is of much more frequent occurrence; and in the eastern counties the climate is as dry, or rather drier, but the temperature of the air is from 2°·0 to 3°·0 colder.

The south coast possesses another climatic advantage of no small importance. The prevailing winds in the south during the winter months are west-south-westerly, and thus the winds which blow over the Isle of Wight pass on in the direction of London. Now we have seen that in passing from the Isle of Wight to London the mean temperature gradually falls 3°·0—the depression being due to the more rapid rate at which the land, as compared with the sea, is cooled down by terrestrial radiation. From this steady and continued lowering of the temperature of the south-westerly winds as they advance inland from the coast, it follows that haze and cloud are formed with greater frequency and of greater denseness as the winds successively advance into the colder districts. Hence the skies of the south coast are clearer and brighter than in the valley of the Thames—a consideration of the highest climatic significance in the cure of many diseases.

The generally light and porous character of the soil and subsoil along the south coast is a strong recommendation in favour of the sanataria of that region; because, as it affords a ready escape for the rain, the roads are quickly dry, and out-door exercise may be safely indulged in shortly after the rain has ceased. The generally bold character of the coast and sloping character of the surface is also advantageous as offering facilities for carrying out an effective system of drainage.

We have referred to the Downs as affording more or less shelter to the south coast from northerly winds, and to the Undercliff as a protection to Ventnor from north-east, north, north-west, and west winds (NATURE, vol. xxv. p. 33). Indeed the chief source of the advantages possessed by one of these watering-places of the south over another is the degree of protection it holds out from the deleterious effects of the easterly and north-easterly winds, and in some degree also to its distance from those parts of the Continent from which the east wind blows. Of the strictly local peculiarities which give one place a decided preference over another is the extent to which the district is planted with well-grown trees, by which the force of the winds, particularly east winds, is broken up and dissipated. In this respect the firs which have been planted in and around Bournemouth strongly recommend this sanatarium to the invalid, since, if fair overhead, he can almost always take outdoor exercise along the walks and promenades which are so completely sheltered by these evergreens. Bournemouth has the additional advantage of being to some extent protected from the full violence

[1] Continued from p. 34.

of the south-west winds by the South Downs of Dorsetshire, and also, though in a less degree, from the east winds by the Isle of Wight.

Since the averages here used are all for the twenty-four years ending with 1880 for the temperature, and for the twenty-one years ending with 1880 for the rainfall, the figures for these two chief elements of climate are strictly comparable throughout. The result is that all strong statements sometimes made in favour of local climatologies, such as the rainfall of Ventnor being as small as that of London, entirely disappear. Such differences could easily be found by the results of different terms of years suited to the purpose, being selected for the particular places whose climatologies are compared. All such comparisons, however, are not merely worthless, they are misleading.

It is, however, now indubitably shown that the south coast of England, from Dover to Portland, enjoys the best winter climate anywhere to be found in the British Islands as respects the two important qualities of mildness and dryness combined, and it is highly probable that the climate of the same tract has clearer, brighter skies, and consequently more sunshine, than elsewhere in these islands. In view of the results of Buchan and Mitchell's investigation into the weather and health of London (NATURE, vol. xxiv. pp. 143 and 173) it is evident that it is to the South of England the invalid who suffers from bronchitis, pneumonia, or other throat diseases, must look for the climate best suited in the treatment of his case, and that it is to the same climate, owing to its clearer air, brighter skies, and more frequent sunshine, that those suffering from nervous and mental diseases should look as more likely to give them the relief they are in search of.

TORNADOES, WHIRLWINDS, WATERSPOUTS, AND HAILSTORMS

I.

WHILE identical with and resembling cyclones in not a few of their leading characteristics, tornadoes and whirlwinds are yet in several all-important respects widely and radically different. The largest tornadoes are of so small dimensions when compared with the smallest cyclones as to point to a difference so decided that admits of no shading of the one class of phenomena into the other. Again, cyclones occur at all hours of the day, whereas whirlwinds and tornadoes are all but restricted to the warmer hours of the day, and perhaps altogether to the time of the day when the sun is above the horizon. Further, and intimately connected with the above, cyclones take place under conditions which imply unequal densities at the same heights of the atmosphere, whether these be due to inequalities in the geographical distribution of temperature or humidity; but whirlwinds occur where the air is unusually warm or moist for the time, and where, consequently, temperature and humidity diminish with height at an abnormally rapid rate. To put it otherwise, cyclones are phenomena consequent on a disturbance of the equilibrium of the atmosphere considered horizontally, but tornadoes, on the other hand, have their origin in a vertical disturbance of atmospheric equilibrium.

Hence whirlwinds are of occasional occurrence nearly everywhere, penetrating into regions where cyclones are altogether unknown; and even tornadoes, which are the most violent and destructive manifestations of the whirlwind, are phenomena either of rare or of frequent occurrence in nearly all climates.

Among the most remarkable of the tornado-swept tracts of the globe are certain portions of the United States of America; and to the examination of these the Meteorological Service of the States has given special attention by a systematic, careful, and minute observation of their attendant phenomena and their destructive effects. The results of these inquiries have been for some years recorded with great, but by no means too great, fulness and elaborateness in the annual meteorological reports of the Chief Signal Officer. Much has been done of late years, as our readers are aware, by observation and discussions of observations, to throw light on these atmospheric meteors; and in this connection we have the greatest pleasure in referring to Prof. Ferrel's recently published "Cyclones, Tornadoes, and Waterspouts," Part II., the portion of which, bearing on tornadoes and whirlwinds, is the most successfully handled part of that very suggestive work, and indeed presents the best theory of whirlwinds yet propounded.

Tornadoes, whirlwinds, and waterspouts are essentially the same, differing from each other only in their dimensions, their intensity, or in the degree in which the moisture is condensed into visible vapour; while the extraordinary downfalls of hail or rain, constituting the hailstorm and rainstorm, are simply the manner and degree of the precipitation. In the waterspout the main features of whirlwinds are best seen, owing to the degree, more or less complete, in which the vapour has been condensed into visible cloud through the whole length of the meteor.

Figs. 1, 2, and 3 represent different forms of the waterspout. In Fig. 1 is seen the black cloud covering the sky, from which a projection is let down from the cloud in the form of an inverted cone as at A, which continues to increase and extend downwards. The surface of the sea at D immediately beneath is soon seen to be stirred, and quickly thrown into a state of violent agitation. At this stage the whirling movement which originated in the clouds has extended downwards to the sea, and is doubtless continuous throughout, though the portion of the whirling column from A downward is not yet present to the eye by the condensation into cloud of its contained moisture. The cone at A continues to lengthen downwards, and ultimately reaches to the earth's surface as shown at B and C, and by the waterspouts of Figs. 2 and 3. As the whirling movements of the aërial column of the waterspout become more intensely developed, the increasing rapidity of the gyrations brings about increased rarefaction of the air within, with the inevitable result of increased condensation into cloud downward. The protrusion from the clouds and extension toward the surface of the sea of the waterspout is thus not due to the descent of vapour from the clouds, but to the visible condensation of the vapour of the spirally ascending air-currents arising from an increasing rarefaction due to the accelerated rate of the gyrations, the condensation being similar to that of the cloud seen in exhausting an air-pump.

The onward progressive motion of tornadoes and whirlwinds varies greatly, and is probably in all cases that of the general movement of that portion of the earth's atmosphere in which they are embedded and form a part. Tornadoes sometimes rage with destructive violence on heights and hill tops, while intervening valleys remain untouched, thus showing that they occasionally occur at comparatively small elevations, but do not reach down to the surface of the earth. It also sometimes happens that the tornado in its onward course rises for a brief interval above the surface and again descends. As soon as the rapidity of the gyrations of the column become diminished, the rarefaction of the air of the column and the condensation of the vapour are correspondingly lessened, and thereafter the waterspout gradually breaks up and disappears.

Under each of the waterspouts in Figs. 1, 2, and 3 the surface of the sea is seen to be more or less heaped up as well as in violent commotion, indicating that atmospheric pressure immediately under the gyrating column is less than it is all round. On land, when a tornado passes directly over a closed building, many instances have occurred when the whole building, walls and roof, has been thrown outward with great violence, the wreckage presenting the appearance of a sudden explosion, proving that the pressure outside the building was instantaneously and largely diminished, and the building wrecked by the expansion of the air within. It is in this way that the tornado works no inconsiderable part of its most dreadful havoc in the destruction of human life.

During the storm of 1703, the greatest recorded in British history, it was observed that the roofs of many houses on the lee side of the buildings were wrecked as if by an explosion within. The destruction in this case was caused by the extreme rarefaction produced on the lee side of buildings by the mere mechanical action through friction of the terrific wind which swept past them. The

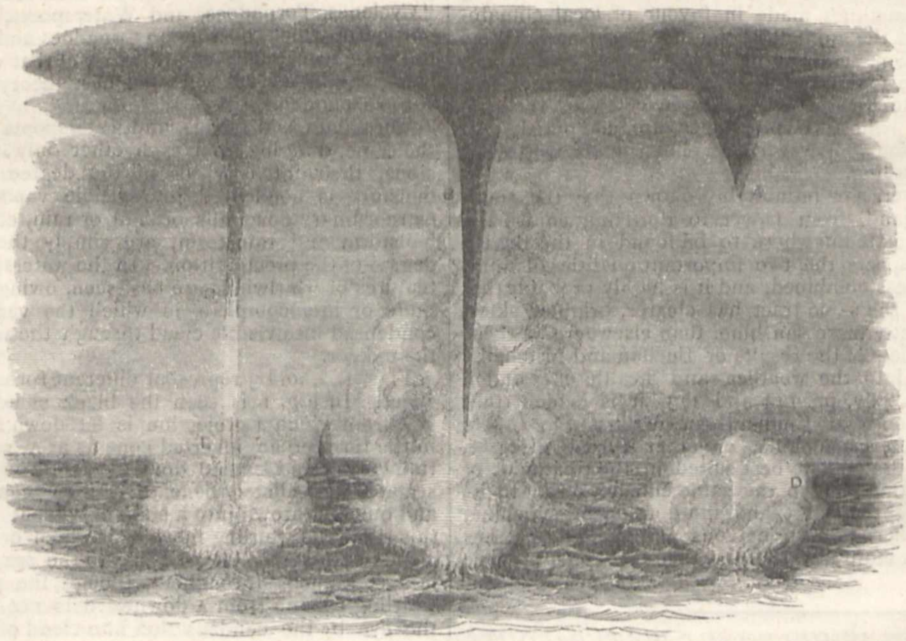


FIG. 1.

records of tornadoes abound in illustrations of houses and other structures thus reduced to hopeless wrecks.

It is probable that the wind sometimes reaches a force in tornadoes exceeding what is ever reached in cyclones. During the tornado which occurred in Ohio on February 4, 1842, large buildings were lifted entire from their foundations, carried several rods through the air, and then

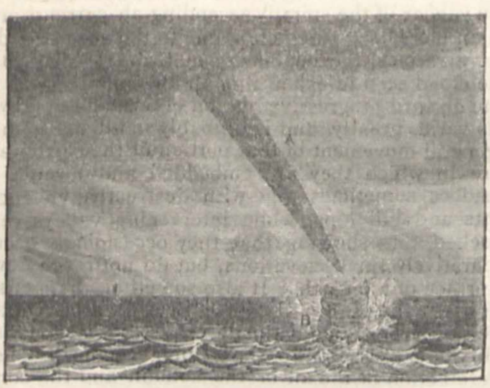


FIG. 2.

dashed to pieces, some of the fragments being transported a distance of seven or eight miles; and large oaks nearly seven feet in girth were snapped across like reeds. This tornado sped on its course at the rate of thirty-four miles an hour, and at one place it did its fearful work in the brief space of a minute. During the tornado which swept over Mount Carmel, Illinois, June 4, 1877, the spire, vane, and gilded ball of the Methodist Church were carried fifteen miles to the north-eastward. On this incident Prof. Ferrel remarks that the ascending currents which could keep this structure suspended in the air for at least fifteen or twenty minutes must have had an enormous velocity.

The usual position of the gyrating columns of whirl-

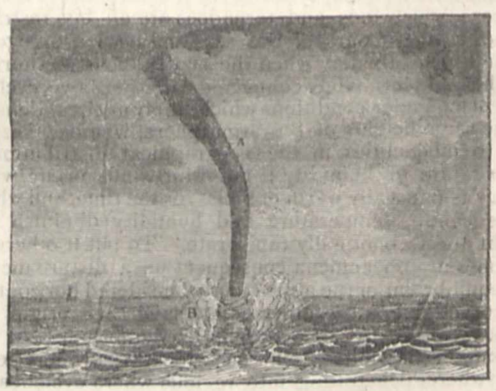


FIG. 3.

winds is vertical, as seen in Fig. 1. Among other positions the column assumes a slanting direction as in Fig. 2, and a curved form as in Fig. 3. It is probable that to these latter forms many of those stationary or slowly moving dangerous squalls are to be referred that spring up with unexpected suddenness so frequently in such regions as the western lochs and islands of Scotland—

these sudden squalls which lash into a tempest of waves what is but a mere patch or narrow lane of sea, while all round remains like a sheet of glass, the squall being only the lowermost part of the gyrating column of a slanting whirlwind. Nothing is more surprising to the landsman who encounters one of these squalls for the first time than to see a mere bit of sea lashed into a tempest by say an east wind in which no sail can live, while but a short way to leeward other vessels are seen either under a good-going breeze or in calm water, altogether untouched by the tempest, which seems to blow directly to them, but which strangely never reaches them.

In examining cyclones, phenomena occasionally present themselves which strongly suggest the idea that they include within their circuit, as an independent meteor, the whirlwind or the tornado, the phenomena in question being most frequently met with in those cyclones which present, in close continuity, masses of air differing very widely from each other in temperature and humidity. Of such cyclones the great storm of October 14 last appears to be one. On that occasion the changes of temperature and humidity were sharp and sudden, particularly from the Grampians to the Cheviots, the great fall occurring when the wind changed to northward. As we have already stated (NATURE, vol. xxiv. p. 585), off the Berwickshire coast the darkness accompanying the changes of wind, temperature, and humidity was denser and more threatening than elsewhere, and almost simultaneously with the approach of these changes, a hurricane, or rather tornado, broke out with a devouring energy which bore everything before it. The tornado-character of the storm off Eyemouth is shown by the accounts of some of the survivors, who describe the wind as blowing straight down from the sky with an impetuosity so vehement and overmastering that the sea for some extent was beaten down flat into a stretch of seething foam, in which many boats sank as if driven down beneath the foam by the wind, while outside this tract the waves seemed to be driven up to a height absolutely appalling, which in their turn engulfed many of the boats yet remaining. Similar seas, with level wastes of seething foam, bounded immediately by waves of a height and threatening aspect never before witnessed, were encountered by several well-appointed steamers out in the middle of the North Sea during this storm, thus confirming the observations of the Eyemouth fishermen. These facts seem to point to one or perhaps more tornadoes of no inconsiderable dimensions, with slanting columns, the terrific force of the gyrations of whose lower extremities played no inconspicuous part in the devastation wrought during the continuance of this memorable storm.

(*To be continued.*)

SIR DAVID BREWSTER'S SCIENTIFIC WORK

BUT thirteen years ago there passed away from the roll of living scientific worthies one whose name will ever hold a high place for the variety and scope of the researches carried out with untiring zeal through a long and useful life. Since our last number the centenary of Sir David Brewster's birth has been commemorated in Edinburgh, and the occurrence forms a fitting opportunity to review briefly his multifarious work in the light of the science of to-day. Sir David Brewster was born in 1781. He must therefore have been twenty-five years of age at the date when his first published scientific memoir, entitled "Remarks on Achromatic Eyepieces" (published in *Nicholson's Journal*), saw the light. Until 1867 he continued actively to pursue scientific researches. Whilst his literary works are of themselves amply sufficient to cause the name of Brewster to be handed down to posterity, the long list of four hundred original memoirs which appears in his name in the Royal Society's Catalogue shows with what unremitting ardour the fire of discovery burned within his breast.

In the domain of Physical Optics Brewster was an eager and successful worker: and his industry was rewarded by a series of brilliant experimental discoveries. The genius of Young, the keen perception and quick acumen of Malus, and the trained intellect of Arago had been concentrated on this hitherto neglected department of science. But Brewster, who cannot be said to have possessed, either by birth or education, the powers of any of these investigators, discovered more than all of them put together, and by diligent observation unravelled complicated phenomena which baffled their powers.

In 1812, having heard of Malus's celebrated discovery of the polarisation of light by reflection, he took up the study of polarisation, and in the course of the next two years advanced our knowledge in various directions. He discovered the property of the agate to give a single polarised image; the polarisation of the rainbow; the polarisation tints in thin plates of crystal; the so-called depolarising power of mineral and other substances; and the partial polarisation produced by metals.

These discoveries he followed up immediately by several of equal interest. He observed the double system of elliptical rings of colour in topaz, and subsequently investigated the appearances presented by other crystals, both monaxial and biaxial in convergent polarised light. He not only discovered but determined the law of the partial polarisation effected by transmitting light obliquely through a bundle of thin plates of mica or glass. Meantime he was actively prosecuting literary work. His "New Philosophical Instruments," published in 1813, contained a great deal of matter new in the science of optics, the results of original research. Hitherto in tables of the refractive index of bodies diamond had stood at the head, and ice at the foot of the list. But Brewster showed that realgar and chromate of lead exceed the diamond in refractive power, whilst fluorspar, cyolite, and tabashear fall below ice both in refractive and in dispersive power.

During these and the subsequent years the disturbed relations between Great Britain and France prevented the workers in science on opposite sides of the Channel from learning what progress was being made, with the result that many of Brewster's discoveries were independently made by others. Thus Malus anticipated Brewster in the discovery of the "depolarising" effect of mica films, of the partial polarisation of metals, and of the polarisation effected by bundles of thin plates, though he missed the law of the last phenomenon. Arago also anticipated Brewster in finding the colours of thin crystal plates in polarised light. In 1814 and 1815 Brewster discovered a new relation of polarised light, namely, that existing between the ray and the state of mechanical strain of the body through which it passed. He observed that heated glass exhibited coloured tints in polarised light, and that Rupert's drops did the same. Subsequently he produced both double refraction and chromatic polarisation in soft and indurated jellies, in horn, and in a variety of animal and vegetable bodies, particularly in the crystalline lens of the eyes of animals, whose structure is thereby revealed. The most important of these early researches was undoubtedly the law connecting the angle of maximum polarisation by reflection with the refractive index of the body. The difficulty of establishing such a law was in Brewster's case enhanced by the circumstance that his mind was not a mathematical one. With a skill that rose superior to the defects of apparatus, and with an unflagging patience at which one can only marvel, he scrutinised with minute care every fragment of mineral in the cabinets of his scientific acquaintances. By this means he constructed tables of refractive and dispersive powers and of the polarising angles of the various reflecting surfaces. And from these two sets of data he brought out the discovery of the tangent law now always identified with his name. But there were other occasions when some mere mathematician stepped in to take up the elaborate facts which Brewster had elaborated, and from them in a few minutes to deduce a law for which he took the credit of discovery. "It seems to us," writes Prof. Tait in an article which appeared in the *Scotsman* shortly after Brewster's decease, "that sufficient allowance has not been made for the natural irritation which such treatment was certain to cause, especially in a high-souled and single-minded man incapable of treating others as he felt himself treated. His biographer will have a painful, but a necessary and salutary, task to perform in gibbeting such thankless parasites. Many a much-praised scientific article—volume even—may be found where the facts are taken mainly from Brewster, though his name is barely mentioned. He was driven by such treatment into frequent disputes about priority, and in general he was successful, though often before the final settlement of the question the obnoxious paper had found its way to a non-scientific public, and even to foreign journals. It is always a difficult matter to determine what is the proper course for a philosopher under such circumstances. Few have the calmness to rely upon the almost invariably just decision of posterity; and most of those who do so go unrecognised to their graves."

In 1816 Brewster announced his discovery of the cause of the colours playing over the surface of mother-of-pearl, and of the possibility of transferring them to casts taken in wax, isinglass, and fusible metals. He also investigated the images and fringes of colour visible in some natural specimens of calc-spar, and turned his attention, though this time with only incomplete results, to the production of tints by multiple reflexions at the surfaces of polished metals. When in 1830 he returned to the subject, there resulted a remarkable memoir on Elliptical Polarisation, which appeared in the *Philosophical Transactions*. In 1817 he discovered the whole class of biaxial crystals, and triumphantly deduced the law of their action on light, thereby solving the difficulties which had perplexed Biot and Arago. He even sketched out a relation between the primitive forms of crystallisation of minerals and their optical behaviour.

His attention was next directed to the question of the absorption of light. In this department of science he made many observations. He was the first to observe in any systematic way the effects of absorption upon the prismatic spectrum. He reinvestigated the solar lines discovered by Wollaston and Fraunhofer, and observed even a larger number of them than the latter had detected. He made the important observation that many of these lines are due to absorption by the earth's atmosphere; and in one of the latest of his contributions to science—a joint paper by himself and Dr. J. Hall Gladstone, which appears in the *Philosophical Transactions* for 1860—he returned to the subject with unabated vigour and unsurpassed perspicacity of thought. He also discovered the power possessed by nitrous oxide gas to produce absorption lines, and he noted the great and extraordinary increase in their number and density when the gas is heated. "The power of heat alone to render a gas which is almost colourless as red as blood, without decomposing it, is in itself a most singular result; and my surprise was greatly increased when I afterwards succeeded in rendering the same pale nitrous gas so absolutely black by heat, that not a ray of the brightest summer sun was capable of penetrating it." Indeed he seemed to be here on the very verge of the discoveries on the spectroscopic significance of the width and frequency of the absorption lines which have been made by Mr. Lockyer, M. Janssen, and others during the present decade. In 1831 Brewster published his "New Analysis of Solar Light," the new analysis being nothing else than the operation of looking at the solar spectrum through coloured absorbent media. It was this series of experiments which led him to conceive the theory of the three primary colours which he so resolutely maintained against all opponents till his dying day. Through his red glass he could see light through a considerable range of the visible spectrum, and therefore, he concluded, there is some red in all parts, but with different degrees of brightness. The yellow and the blue were, he held, also distributed, each with a maximum of its own, throughout the range of the whole light. He believed that he had proved the conversion of blue rays into violet ones by viewing them through an absorbent medium. "We must remember," says Prof. Tait, by way of apology for the persistence with which Brewster clung to his pet theory, "that he trusted to an eyesight that had rarely deceived him—an eyesight once so perfect that he is one of but a very few who have *seen* the extraordinary ultra-red rays which he was the first to discover as visible light."

One of his researches connected the subject of absorption with his work on polarisation. He investigated the property known as *dichroism* possessed by a great number of coloured crystals, tourmaline, Brazilian topaz, and others, a property which has lately given rise to several important investigations by physicists in Germany and in England. He showed how the absorbed tints are altered by heating, and here he anticipated a point in the electromagnetic theory of light which was then of course quite undreamed of.

To enumerate the whole of Brewster's researches would occupy so many columns that only a few of the more prominent must now be adverted to. Optical illusions of sundry kinds, fluid cavities in crystals, polarisation of the sky, phosphorescence, fluorescence, photography, the optical properties of agate, opal, and labradorite, the magic mirror of Japan, and the theory of binocular vision, all claimed their notice and formed the bases of many careful researches. The experimental researches of Brewster in optics are in fact paralleled only by those of Brewster's great contemporary Faraday in electricity.

Brewster was the inventor of several well-known optical instruments. The *kaleidoscope*, which was brought out in 1816, created such a furore that 30,000 were sold in a few days. His *monochromatic* lamp appeared in 1823. In 1849-50 he brought out his lenticular *stereoscope* (an improvement upon Wheatstone's reflecting stereoscope of 1838), and a binocular camera, for use in producing stereoscopic pictures. Still more important, though far less widely known, was his discovery of the application of lenses and combinations of lenses to lighthouses. This was in 1812; in 1820 he was urging the adoption of his system on those in authority—two years before Fresnel, who usually gets the credit of this application, had begun his work.

His objections to the undulatory theory of light endured to the last, when he stood almost alone in his refusing explicit adherence to the theory. Trained himself in another school of thought, and accustomed through long years to the Newtonian theory, it is not remarkable that in the absence of mathematical predilections the mathematical intricacies of the fabric woven by Fresnel had little charm for him. And if we find it hard to realise the slowness of minds like Brewster's to receive the undulatory theory as an established truth, we may perhaps find no inapt parallelism in the repugnance felt even amongst some of the "crowned heads of science" at the present day towards entertaining the still more modern electromagnetic theory of light in which the undulatory theory is fast being swallowed up piecemeal.

Brewster's literary activity was simply extraordinary. He brought out the "Edinburgh Encyclopædia" between the years 1808 and 1830, writing many of the articles himself. To the seventh and eighth editions of the "Encyclopædia Britannica" he contributed the articles on Electricity, Magnetism, Microscope, Optics, Stereoscope, and Voltaic Electricity. No fewer than seventy-five articles in the *North British Review* are from his pen. From the year 1819 he was, along with Jameson, editor of the *Edinburgh Philosophical Journal*, in which so many of his researches saw the light. His "Letters on Natural Magic," his "More Worlds than One," his treatise on "Optics," his "Martyrs of Science," and his "Life of Sir Isaac Newton," all testify to an unremitting activity rarely equalled. He was made Principal of the University of St. Andrews in 1838, a post which he relinquished only in 1859 to succeed to the Principalship of the University of Edinburgh. As one of the founders of the British Association in 1831, no less than as a distinguished representative of science, he received the honour of knighthood.

A man who could unite so many varied qualifications in himself, who, besides adding so richly to the total of exact knowledge, could do so much to diffuse that knowledge to succeeding generations, who could write not only with the calm decision of a philosopher, but with the vivid imagination of a poet and even with the fervour of a preacher, must indeed be acknowledged to be a remarkable figure in the age in which he lived. His position in the physical sciences, standing as he did between the old generation of workers and the new, is not very easy to define. But his memory will doubtless descend to posterity in connection with numerous departments of the science of optics, in which his work remains to testify to his place amongst the men of science of whom Great Britain has just reason to be proud.

NOTES

THE Lord President of the Privy Council has appointed Prof. Archibald Geikie, F.R.S., to be Director-General of the Geological Surveys of the United Kingdom, and Director of the Museum of Economic Geology, Jermyn Street, in succession to Sir Andrew C. Ramsay, F.R.S., who retires from the public service towards the end of the present year.

MONDAY night was an enthusiastic Arctic night at the Geographical Society. The first paper was by Mr. C. R. Markham, on the important discoveries made by the *Rodgers* in and around Wrangel Land, and on the proposal that England should lend a hand to search for the missing *Jeannette*, and that a Government expedition should be sent out to look for Leigh Smith. Lieut. Hovgaard of the *Vega* also read an Arctic paper, detailing his plan for a *Jeannette* search from about Cape Chelyuskin as a basis; while an instructive paper was sent by the Dutch Commodore Janssens, on the ice-conditions in Barents Sea, and the probable position of Mr. Leigh Smith in the *Eira*. Of course Mr. Markham's energetic enthusiasm was infectious, and everybody seemed to agree that it would be disgraceful to England not to send out search expeditions. Sir George Nares and Sir Allan Young spoke, but it cannot be said that they threw much light on the problem; the good-natured Sir Allan took much trouble to say he knew nothing about these seas, and therefore he thought an expedition should be sent out for the *Eira*. Mr. Grant, the well-known Arctic photographer, told his experiences on the ice of the Barents with the Dutch and with Mr. Leigh Smith during the last four years, and he seems to think, what every one else thinks, and what is evident, that Mr. Leigh Smith is locked up in the ice somewhere. But all the speakers on Monday night evaded the main point, which was clearly stated in Mr. Eaton's letter in last week's NATURE (p. 123). Mr. Eaton declares that Mr. Smith went out with the deliberate intention of wintering, and that he has now provisions to last two years, Of course, in cases of doubt, it is well to take the worst possible view. But there seems to be a conflict of evidence. Mr. Eaton, than whom no one ought to know better, positively states that the *Eira* is provided as we have indicated; while on the other side there are vague and inconsistent statements. Were we convinced of the real danger of the *Eira's* situation, we should heartily support a relief expedition; but in this case there seems to be no doubt. The matter may be safely left in the hands of the Admiralty, who will doubtless look at the situation all round, and take care that they do not commit themselves, at the most, to more than a mere search, in conjunction, we should suggest, with relatives and friends. But on consideration of all the evidence, it may not be thought sufficient to warrant Government intervention. We were pleased to learn that the object of the Dutch in sending out expeditions year after year to these seas is to obtain a thorough knowledge of the movements of the ice before they venture to send out a fully-equipped expedition to force its way northwards; this is thoroughly scientific in its method.

A BALLOON accident, which we fear may turn out unfortunate, occurred in the South of England last Saturday. Capt. James Templer, Mr. Walter Powell, M.P., and Mr. Agg Gardner, ascended at Bath on Saturday at 1.55 p.m. for the purpose of taking the temperature of the air, and the amount of snow in the air, for the Meteorological Office. Capt. Templer, in a letter to Mr. R. H. Scott, describes what followed: "I cleared the snow clouds at 4000 feet altitude; the temperature of these clouds was 28°, and the wet-bulb thermometer read 26°. At 4200 feet we passed over Wells, the time being 2h. 50m. At this height I worked over Glastonbury; the temperature now rose to 41°, and the sky was perfectly clear. I passed then between Somerton and Langport, and I here found that I was in a N. ½ W. current. I asked Mr. Powell to send the balloon up to 6000 feet to ascertain the temperature of a small bank of cirrus. I found this temperature to be 31°, and then I asked him to place me at 2000 feet altitude, to regain the N. ½ W. current, and we then came in view of Crewkerne. I now kept at a low altitude until I reached Beaminster. Mr. Powell here observed that we were going at thirty miles an hour, and here we first heard the roar of the sea. The balloon suddenly rose to 4000 feet; at this time I said to Mr. Powell, "Go down to within 100 feet of the earth, and ascertain our exact position." We coasted along close to the ground until we reached Symondsbury. I here called to a man and asked him how far the distance was to Bridport, and he said about a mile. I asked Mr. Powell to prepare to 'take in,' our pace now increasing to thirty-five miles an hour. To avoid the little village of Neape Mr. Powell threw out some ballast. This took us to 1500 feet elevation, and we had still two miles to get in. I opened the valve and descended, about 150 yards short of the cliff. The balloon on touching the ground dragged a few feet, and I rolled out of the car with the valve line in my hand. This caused the balloon to ascend about 8 feet, when Mr. Gardner dropped off, and unfortunately broke his leg. I found that the rope was being pulled through my hands, and I called to Mr. Powell, who was standing in the car, to come down the line. He took hold of the line, and in a few more seconds the line was torn through my hands. The balloon rose rapidly. Mr. Powell waved his hands to me, and I took his compass bearings, and found that he was going in a S. ½ E. direction." Capt. Templer lost no time in getting into a steamer at Weymouth and searching the Channel in the most likely direction, but without result. Up to the present nothing has been heard of Mr. Powell, and the worst is to be feared. This accident is certainly to be regretted, more especially as the expedition was in the interests of science. Still in spite of the accident the Meteorological Council are to be congratulated upon the endeavour to get at the correct facts of the air.

IN the *Comptes Rendus* for December 5, 1881, p. 936, there appears a paper animadverting on the meteorological stations it has been proposed to establish in the neighbourhood of the

North Pole, which paper, according as it is looked at, is provocative either of amusement or amazement. It is amusing to read that it is all the way from the equator that these cirrus clouds travel, giving us Europeans, by systems of vorticose movements let down from their lofty heights, our cyclones, our rains, our thunderstorms, our hail, and even our snow; that, towards the elucidation of the great problems of the movements of the atmosphere in their bearings on climate aad weather, the observations made forty years ago by Lottin, Martius, Bravais, &c., in the Arctic regions, are quite sufficient for the purpose. The additional data to be expected from the Arctic network of stations now proposed to be established at Bossekop, Jan Mayen, Navaja Zemlia, Spitzbergen, &c., being quite insignificant; and that the French meteorologists in agreeing to establish, as their contribution to this extensive research into the movements of the atmosphere, a station near Cape Horn, supposed, as assumed by M. Faye, that this station near the antarctic circle, would assist them in framing weather forecasts for France. It is amazing to see it quietly assumed that the fishermen and sailors on the French coasts have no practical, or at least personal interest in the storms which sweep across the British Islands and Scandinavia; and to read the explicit statement that in the interests of the seaports of France, and of science itself, what is above all things needed is the organising of a first-class meteorological station (*une grande station météorologique*) in the Azores. With regard to this, French sailors and fishermen may be thankful that other counsels rule the action of those who are entrusted with the preparation of weather forecasts for their country and with the investigation of those laws, a knowledge of which will enhance the value of this branch of practical meteorology.

IN his inaugural address at the opening of the Session of the Sanitary Institute, Dr. Alfred Carpenter dwelt upon the necessity of such an organisation, as proved by the lamentable ignorance of the mere elements of sanitary science shown by many of the candidates for the diploma of the Institute, most of them already official guides of health and other bodies. It seems strange to be told by Dr. Carpenter that there is a feeling of antagonism to the Institute in the Social Science Association. The former is the practical outcome of the latter, and the Association ought therefore to rejoice that its teachings have borne such desirable fruit. The Institute is certainly doing much good, and there seems to be no doubt that by its action and by other means, a beginning has been made in this country of a thorough sanitary reform.

AN instructive case of injury from lightning, on a gentleman's estate near Geneva, is recorded by M. Colladon (*Arch. des Sciences*, September 15). The lightning first struck a tall poplar standing near an iron-wire fence; thence the fluid passed to an elm standing close to the fence on the other side, damaged three main branches of this, and wounded the trunk on the fence side, down to a point opposite the top wire of the fence. The course was then along this wire, but only, it appears, in one direction, viz. towards an iron gate a little way off, under which passed the pipe which supplied gas to the house. The wire, a double one, was fused in some parts. After damaging the gate the current found its way to the gaspipe (making a hole in the ground), and passed along this to the house, injuring no part of the pipe-system of that, but only a piece of ornamental rose-work containing iron wire in the ceiling of the drawing-room over the lustre. Thence it passed to earth by the iron pipes and wires on a balcony outside the room. Several bushes near the poplar and fence were affected (coloured brown), and the plate on the collar of a dog which was attached to a wire between two shrubs, and had been heard to howl at the time, had disappeared. The extended character of the discharge and the influence of wires seem to be salient points in this case. M. Colladon advises making the parts of telegraphic or telephonic wires that pass near a house double or triple the mean thickness, so as to diminish the chances of lateral discharge.

M. PLATEAU has studied the phenomena of the bursting of bubbles. When a bubble bursts it disappears almost instantaneously, leaving behind it a multitude of small liquid drops. The order of the phenomena is really as follows:—The bubble begins to burst at one point, the film rolling away in a circle around the opening, and its edge becoming a rapidly-enlarging liquid ring. This ring draws itself together into segmental portions, which ultimately become small spherules. At the same time the contraction of the rest of the bubble causes a rush of air through the aperture, and blows off the spherules into the air with a kind of small explosion. The phenomena are best observed by blowing a bubble of glyceric solution upon an iron wire ring, and then bursting it at the top by touching it with a needle whose point has been dipped in oil.

THE conduct of competitive examinations in China seems to be farther from perfection than might be expected in the case of such an ancient institution. The *Peking Gazette* contains a memorial from one of the censors complaining that the matsheds which are erected at the entrance to the examination hall in the capital to issue tickets of admission to competitors are frequently overturned by the rush of applicants, that an unseemly crowding and snatching of tickets from the officials take place, and that candidates break the rule prohibiting them from leaving the compartments in which they are isolated during the examination. They are allowed, he says, to fetch their food themselves (examinations in China last from thirty-six hours to three days at a stretch) from the kitchens, and they meet and converse freely. Prepared essays, the memorialist fears, are passed in from outside during these hours by the student's friends. Again, when the lists of successful candidates are posted up, a tumultuous crowd assembles outside the gates; bands of the unsuccessful ones obstruct the progress of the chief examiner, employing threats and entreaties to prevail on him to alter the lists. The censor also protests against the length of time frequently taken before the results of an examination are known. The Chinese examiners, however, have an excuse for this which our own Civil Service Commissioners have not, viz., the number of students examined; at the triennial provincial examination held in Canton in 1879 there were 10,160 candidates for 82 degrees!

THE death is announced of Mr. William Bramsen, a Danish gentleman whose acquirements in Japanese scholarship were extensive. During a residence of more than twelve years in Japan Mr. Bramsen devoted his leisure to a study of the language, chronology, and numismatics of the country. His principal work is "Japanese Chronology," published in 1880, the only complete treatise on the subject which has ever been written. In this laborious work Mr. Bramsen has given the exact day of the month and year corresponding to the Japanese dates for the past thousand years. He has further explained the complicated systems by which the Japanese and Chinese reckoned time, and has thrown out the suggestion that in the early periods of Japanese and Chinese history the year included the time between the equinoxes, and did not correspond to our year. This idea he has supported with much learning, and should it on further examination turn out to be correct, it will revolutionise our notions of the antiquity of the Chinese and Japanese peoples. During the past year Mr. Bramsen was engaged in producing in parts a beautiful work on Japanese numismatics. Only the first part, dealing with recent copper coins, had been published. A few weeks ago he read a paper on the subject before the Numismatic Society of London. His collection of Japanese coins was the most complete private collection in existence, and was, we believe, valued by himself at about 2000*l*. He was not much past thirty at the time of his death. It was his intention to return to Japan so soon as he was called to the English bar. The main characteristics of his work—which was but an earnest of what might have been expected from him had he been spared—were thoroughness and care. It will be difficult to fill the important, and in some sense peculiar, position which he occupied in the field of Japanese scholarship. He was a member of the Royal Asiatic Society, and of numerous native and foreign societies in Japan.

THE Brighton Health Congress and Domestic and Scientific Exhibition are being held this week. The Exhibition, over which Lord Chichester presided, was opened on Monday. The Congress, over which Dr. Richardson, F.R.S., presided, was opened on Tuesday evening by the delivery of his inaugural address. Dr. Richardson spoke on the "Seed-time of Health," pointing out the perils that beset youth in the present condition of hygienic education, and empirical and unscientific practice. The sittings of the Congress will be continued until to-morrow. The Congress is composed of three sections. The first, presided over by Mr. Edwin Chadwick, C.B., relates to the Health of Towns; the second, presided over by Mr. J. R. Hollond, M.A., M.P., relates to Food; and the third, presided over by Dr. Alfred Carpenter, C.S.S., relates to Domestic Health. A large number of important papers are down for reading and discussion, including, amongst others, essays on slaughter-house reform, by Mr. H. T. Lester, B.A.; food-plant improvement, by Major Hallett; sanitation in decoration, by Mr. Robert Edis, F.S.A.; food preservation by cold, by Mr. T. B. Lightfoot; recreation spaces in large towns, by Dr. Fussell; bread reform, by Miss Yates; cheap food and longevity, by Dr. Drysdale; rational feeding, by Mr. Wynter Blyth; diet in public institutions, by Dr. Whittle; home sanitation, by Mr. H. H. Collins; a comparison of English and foreign watering places, by Mr. H. S. Mitchell, M.A.; health lessons in schools, by Mr. Charles Cassal, B.A.; clothing and health, by Mrs. E. M. King; and papers by Sir Antonio Brady, Dr. Browning, Edward Easton, C.E., Ellice Clarke, C.E., Messrs. Stephens, E. Bailey Denton, and others. Yesterday the Mayor and Mayoress held a reception in the Pavilion. To-day, Dr. Taaffe, Medical Officer of Health for Brighton, delivers a public lecture. On Saturday, excursions will be made to various places of interest in and about Brighton, and in the evening of Saturday, the proceedings of the Congress will be brought to a close by a lecture to the working classes from Mr. Brudenell Carter, F.R.C.S., on the subject of Eyesight. The Exhibition is on a most extensive scale, including objects relating to food; domestic, labour-saving, and educational appliances; house sanitation; industrial dwellings; lighting, electrical and kindred inventions; decorative art, such as photography, painting on china; horology; and a very extensive loan collection of great value from the South Kensington Department.

AT the meeting of the Sanitary Institute on December 7, Dr. Alfred Carpenter in the chair, the adjudicator, Dr. W. Farr, F.R.S., and Dr. Richardson, F.R.S., reported that the Wyatt-Edgell prize of 200*l*. for an essay on the Range of Hereditary Tendencies in Health and Disease was awarded by them to the essay bearing the motto "The subtlety of nature far exceeds the subtlety of reason." On the sealed envelope accompanying the essay being opened the chairman announced the author to be George Gaskoin, of 7, Westbourne Park. The prize will be presented by the Rev. E. Wyatt Edgell at the next ordinary meeting, February 8. The inaugural address was delivered by Dr. A. Carpenter, vice chairman of the Council.

ALL the members of the Royal Commission on Technical Instruction have returned to England. The chairman, Mr. Samuelson, M.P., remained at Paris for some days in order to obtain additional information on the general policy of the Department of Public Instruction. The selection of the members of the Commission, on account of their acquaintance with different branches of the inquiry, has proved very useful, Dr. Roscoe having been able to devote his attention more particularly to chemical technology, Mr. Philip Magnus to school organisation, and Mr. Slagg, M.P., Mr. Woodall, M.P., and Mr. Swire Smith to the bearing of technical instruction on the branches of industry with which they are familiar. It is proposed to take the evidence of experts in this country in February, and to visit Germany, Switzerland, and Belgium in the spring.

M. PASTEUR has been elected to one of the vacant seats in the French Academy.

PROF. FLOWER has just been appointed by the President and Council of the Royal Society a trustee of Sir John Soane's Museum in the vacancy occasioned by the death of Sir Philip de Malpas Grey Egerton, M.P.

WE have received from Messrs. De la Rue and Co. their pocket and desk diaries for 1882, together with beautiful cards and almost microscopic registers for use during the coming year. If possible all these are more beautiful examples of the printer's art than those produced in past years, and especially interesting from NATURE's point of view, at all events in the fact that the amount of scientific facts packed into the closely-printed page is greater than ever. The mechanical equivalent of heat, the present magnetic elements, the mean distance of the sun, and such like data, are all to be found in their proper place, while the astronomical portion is so full that the amateur astronomer will be spared many references to his *Nautical Almanac*.

ON Friday last took place the first distribution of prizes and certificates to the successful students in the various schools connected with the City and Guilds of London Institute. The Report showed the rapid increase of candidates at the examinations of the Institute, and Sir F. Bramwell gave an address, explaining what was meant by technical education, and the great benefit which must accrue to the various industries by the application to them of the scientific principles on which they were based.

SOME severe shocks of earthquake, accompanied by loud detonations, are reported by the *Valais Gazette* to have occurred at Sion and Sierre on Sunday the 4th inst.

WE learn on good authority that M. Cochéry is preparing a project for the protection of cables and the general regulation of telegraphy. It will be laid before the French Chamber of Deputies after the end of the recess, which will begin in a very few days.

A "SOLAR" locomotive has been placed on the French Northern Railway. It is so called owing to an electric light which is placed in front and fed by the engine itself, and intended to illuminate the way for a long distance in front.

A GEOGRAPHICAL and Natural History Exhibition has recently been opened at Gotha. It will close on the 20th inst.

THE additions to the Zoological Society's Gardens during the past week include a Pomatorhine Skua (*Stercorarius pomatorhinus*), British, presented by Mr. George H. Baxter; two Kestrels (*Tinnunculus alaudarius*), British, presented by Mr. F. Usher; a Horrid Rattlesnake (*Crotalus horridus*) from Brazil, presented by Dr. A. Stradling, C.M.Z.S.; a Dwarf Chameleon (*Chamæleo pumilus*) from South Africa, presented by Major Hunt; a Common Jay (*Garrulus glandarius*), British, presented by Mr. J. Young; two Cape Crowned Cranes (*Balearica chrysopelargus*) from South Africa, a Giant Toad (*Bufo agua*) from Brazil deposited; a Red Kangaroo (*Macropus rufus*), two Mocassin Snakes (*Tropidonotus fasciatus*), born in the Gardens.

SOLAR PHYSICS[1]

I.

I HAVE to address you in this course of lectures on what we know of the infra-red end of the spectrum and its relation to solar physics. I will commence by asking a question, and endeavour to answer it in such a way as will, I hope, be understood. The question I propound is, How do we know that there are any rays below the red rays of the spectrum? In answering the question I would beg you to remember that every body in motion possesses what we call energy, or a capacity for doing work, be the motion a wave motion or a direct motion. Let us take one or two examples of waves: first, that of water, which is familiar to us. I need scarcely point out that a wave of the sea is capable of doing an immense amount of work, not to say mischief; there is no doubt, then, that it is capable of doing work, and this we may take as the true definition of energy, existing in a body, viz. the capacity of doing work. Whence, then, does a wave derive its energy? Perhaps we may have to travel many miles from the place where we find our wave. Travelling to the origin of the waves, we shall no doubt find that a wind has generated them, and in reality it is the energy possessed by the wind which is carried by the waves to the distant shore. The energy possessed by the wind has not been directly expended on our coast, but when transmitted by the waves this same energy is applied in different manner, and by this difference in application it becomes effective. We all know, for instance, that a child may ring a church bell if he give a pull at the right intervals of time, and so, by timing the impact of waves correctly, it is possible for them to do work which in any other way would be impossible. Another example of the energy of waves is the tuning-fork, as in the experiment which Mr. Lockyer showed you. You will recollect that he demonstrated that if one tuning-fork was brought near another of the same pitch the second took up the vibration of the air. The tuning-fork which was struck, or bowed, generated waves in the air carrying some part of the energy of the vibrating prongs to be expended on the second tuning-fork, and as this tuning-fork vibrated in the same period as the first one, each blow of the air-waves was essentially well-timed, and the fork was thus set in motion. You will also recollect that a a fork not of the same pitch—that is, not sounding the same note—was unable to cause vibration in the second fork; and this was simply because the energy was applied at wrong intervals of time. In the case of the tuning-fork, then, the air is the medium through which this energy was conveyed.

With light we have the same kind of motion in the luminiferous ether: the motions of the molecules swinging in the source of light may, for the sake of illustration, be looked upon as composed of an infinite number of tuning-forks, the ether, instead of the air, carrying their energy in all directions. How can the energy in the ether show itself? In the first place it must meet with some obstruction, and secondly that obstruction must be capable of vibrating with it, and thus damp or destroy the waves. The destruction of the wave motion in the ether is known as the absorption, and thus we see that where there is absorption there work of some kind must be done. The work, then, that light can perform is this. [When I say light, I say it with a definite object. It has been said that it is nonsense to talk about dark light; but it is no more nonsense to talk about dark light than to talk of a white violet, a yellow rose, and so on. Therefore, I prefer to call the whole ether vibrations with which we are acquainted, light, until we get a more authoritative definition.] The work that light may perform then is this, it may cause certain appliances in our eye to vibrate (and perhaps also cause chemical decomposition on the colouring matter of some membrane which is placed near the retina), which gives us the sensation of vision. Secondly, it may cause the molecules of the material body on which it falls to vibrate more freely than they do when in a normal state of vibration, and thus raise the temperature of the body. (It must be recollected that physicists suppose the molecules of all matters to be in active vibration, and a rise of temperature simply means an increase of those molecular motions). In the third place it may cause the atoms which compose the molecules to vibrate more energetically than they do under ordinary circumstances, and cause one or more of the atoms to swing off, as it were, and thus create a new molecule; in other words, cause a dissociation of the molecule. We may sum up our definition by saying that the presence of light can be known by three distinct kinds of work. It may be known by its causing the sensation of vision; it may be known by a rise in temperature of the body on which it falls; and it may be also known by the chemical action which it induces. I think, then, we have an answer to the question which I propounded, How can we tell that there are rays which exist below the visible red of the spectrum? If they exist, they must be shown by a rise in the temperature of any body which may absorb those rays when placed in their path, or by their chemical effect. That they do not give rise to the sensation of vision I need scarcely say.

The dark rays were discovered in the years 1800 and 1801 by Sir William Herschel, who was investigating the solar surface with a telescope. Finding that the heat sent to the eye was unbearable, he wished to obtain some medium to cut off those particular rays which gave the heating effect. In order to do that he undertook a series of investigations of the spectrum, in what we should now call perhaps a rough kind of way, in a manner which I will show you on the screen. A beam of light was passed through a prism fixed horizontally against a slit in a wall, being bent so that the spectrum fell upon a table beneath, on which he ruled lines marking the boundaries of the colours. On a sloping board turning on castors he placed three thermometers in a line, two of which he caused to lie within the spectrum, the third remaining outside it. He then noted the height of the mercury in all three of the thermometers, and thus compared the two in the spectrum with that lying beyond it (I may say that the diameters of the bulbs of the two thermometers in the spectrum, which is rather an important point, were one-eighth of an inch and half an inch respectively). Not only did Sir William Herschel use thermometers, but he also used the principle of absorption to increase their indications, for he blackened those thermometers with China black. He found he got a greater effect by using lampblack than by using the bare bulbs of the thermometers. He commenced by placing his two thermometers in the violet, and he found he got a certain rise of mercury. Having made a scale in accordance with the ruled lines on his table, he set up at the point indicating the violet an ordinate also to scale, showing the number of degrees of rise in the thermometer at that particular point. Then in the indigo he set up another ordinate indicating the degrees of rise there, and so on at all these different points; so that he was able to construct, as it were, a mountain of the heat effect due to the spectrum in all parts (Fig. 1). Having gone in this way over the

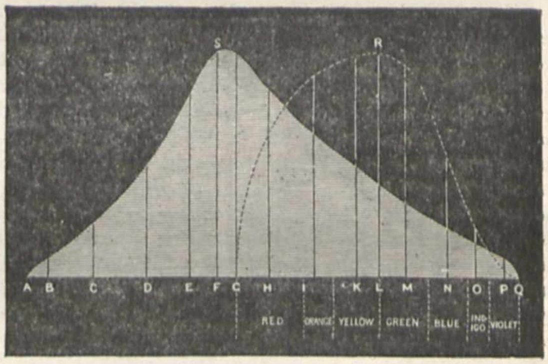


FIG. 1.

whole of the visible solar spectrum, he found there was a rise in the two thermometers, as he approached the red from the violet. (It must be recollected that before his time there was no knowledge of any rays which existed below the red). He therefore ruled lines on his table beyond the red, and having reached the limit of the luminous spectrum, he shifted his thermometers beyond, and found that they rose even higher than in the red. This led him to continue the experiment, and he found by going a long way beyond the red he still got a slight trace of rise in the mercury of his thermometers. By this means he was able to construct his well-known curve (which answers to a curve of energy) in the very simple manner shown in Fig. 1. I shall have to refer to this curve in another lecture, and I want you to fully bear in mind that the heights of every part of this curve answered to a comparative measure of the energy of the particular parts of the spectrum—

[1] Lecture delivered on May 25, 1881, at the Lecture Theatre, South Kensington Museum, by Capt. Abney, R.E., F.R.S.

in other words, to the comparative heating effect of different parts of the solar spectrum. Thus you see that Sir William Herschel, by the use of thermometers, was able to discover that there were rays existing below the red, and this he did by the second method, viz. by noting the rise of temperature in the absorbent body, lampblack, placed in the path of the rays. There are other modes of showing a rise in temperature in lampblack, amongst others by the thermopile, and to that I shall have to refer more at length in a subsequent lecture, and therefore I pass on at once to the chemical effect of the different rays of the spectrum.

In order to show you how we can arrive at a knowledge of the existence of energy in the different parts of the spectrum by their doing chemical work, I must take you through one or two very simple experiments, experiments which I dare say you may have heard of before, but the sight of which perhaps may impress your minds more than if you merely read of them. The first experiment will show the effect upon chloride of silver. I have in this frame a piece of paper impregnated with chloride of silver, and behind this screen, which I have had erected to save your eyes, is a Siemens electric lamp, which gives out a very intense light indeed. A lens placed at a proper distance from the points will cause an image of them to fall on this paper. After a few seconds' exposure you see that where the image fell we shall have a darkening effect; in fact we have an image of the arc printed on the chloride of silver paper. Now this paper apparently to all casual observation is white; what rays of the spectrum are they then that cause this blackening effect? By and by we shall have to find out to what this blackening is due. I have here some chloride of silver in collodion, and with this I think we shall be able to see what rays they are which affect the chloride of silver. In front of the slit of this spectroscopic arrangement we place this chloride of silver, and you will remark that the image of the slit cast by the prism on yonder white wall is of a lemon colour, and that the violet of the spectrum is much subdued in intensity. The true colour of chloride of silver in that particular state in which it exists in the paper then is that which absorbs the violet, and from what has been said on the principle of work being done where absorption takes place, we should expect to find that it is the violet rays which discolour that chloride of silver, and not the yellow rays, which, as we saw, passed freely through it. We will prove that. I have here a blue glass, and we will see whether we can print through the blue glass in the same satisfactory manner that we can when the light is unshaded. Placing a yellow glass in front of the light, first you see that there is no action whatever—the paper remains perfectly white. Now, taking the blue glass, and trying to print through it, you see we have a print of the arc—not perhaps quite so deep as where the unshaded light acted on the paper, but still sufficiently so to show that the blue and violet light are effective. The proposition enunciated then in this case is correct, that the rays active in the dissociation of chloride of silver are the rays it absorbs. But we may consult the spectrum still further, and, by placing such a piece of paper directly in it, and allowing it to print, we shall find that the chloride of silver is only attacked by the blue and violet rays, and not at all by the yellow. Such, you see, is the case in the photograph before you. There is an invisible part of the spectrum beyond the violet by which the silver chloride is even more affected, but that is a region of the spectrum with which I will not trouble you, as it is beyond the scope of my lectures. It will be seen then that we have a specimen of the chemical decomposition of a solid body. Now I should like to show you what is the cause of that darkening; namely, how the chloride of silver is changed. I am obliged to use chemical symbols, because they are short, but I will try to explain them. In ordinary chemistry the chloride of silver is designated AgCl, Ag meaning one atom of silver, and Cl meaning one atom of chlorine, which are joined together. But in order to explain phenomena which are met with in photography, silver really requires two atoms of chlorine to be combined with it to form chloride of silver; that is silver is a *diad* element. This I have expressed by this symbol which I have here, $Ag''Cl_2$; that is two equivalents of chlorine are obliged to be combined with one equivalent of silver. I will give the reason why the old formula is not perfectly correct: when you have light acting on chloride of silver, work of some description is done amongst the atoms of the molecules forming it, and we have one atom of chlorine thrown off by the vibrations of the blue part of the spectrum, and the new molecule $Ag''Cl$ is what is formed, which will grasp any other unsatisfied atom or molecule which may come in its way. In chloride of silver, then, we have an example of the decomposition of a solid body by the action of light. I have here two bottles, both containing ethyl iodide, a body which we will say roughly is composed of ethyl and iodine combined together; the action of light on this is to cause the iodine to separate from the ethyl, and the iodine liberated colours the liquid, as we have it in one bottle. The other bottle is ethyl iodide unaltered by light. So that you see we are obliged to shield this liquid from the light in order to prevent it from decomposing into ethyl and iodine. Here we have decomposition of a liquid by light.

I will now endeavour to show you the decomposition, or dissociation, which is perhaps a better term, of the molecules of a gas, and its combination with something else. I have here a jar of chlorine, and I think you will see by holding it in the rays of the spectrum that we have certainly the violet cut off, and a good deal of the blue. Therefore, if we find any work can take place within this chlorine it must be by those rays which are absorbed or cut off. I have here a jar of hydrogen which is perfectly colourless, and were I to put that in the spectrum I should teach you nothing, because the whole of the rays would pass through it. If then I have a mixture of hydrogen and chlorine together, and allow light to act upon them, it is quite evident that the only matter which can be acted upon is the chlorine. Now, in these small glass bulbs which are covered with yellow paper, are equal volumes of chlorine and hydrogen. When chlorine and hydrogen are combined together we have what is known as hydrochloric acid, a gas of the same tenuity as a mixture of the two. It has been found by practical experiment that if you have an intense source of light acting upon chlorine and hydrogen a combination between these two at once takes place, and we have the hydrochloric acid formed with a violent explosion—not enough to do any harm, but one which will make the room echo. Now our conception of the matter is this, that no hydrogen atom can exist by itself; that there must be two atoms to form hydrogen molecules; and that there must be at least two atoms of chlorine to form a chlorine molecule, perhaps more. Anyhow, you cannot have less than two atoms to form one molecule of chlorine, or less than two of hydrogen to form a molecule of hydrogen. If then we have these two mixed together, and cause light to act upon them, what is the physical result? The physical result is that the atoms of chlorine will swing violently apart from one another, they will be dissociated, and in their swing will catch up one of the atoms of hydrogen, and hydrochloric gas will be formed. Now you saw that the mixture of chlorine and hydrogen, or rather that the chlorine itself cut off the blue; in other words, it was the blue light which would have any chemical effect upon the mixture. I will now get Mr. Greening to allow the Siemens light to act through red glass, and you will see, I think, that there will be no effect whatever. Now, directly he takes it away there is a violent combination of the two with an explosion. To show that it is the blue light that is the effective light we will cause those two gases to explode by means of white light filtered through blue glass. I may say that the arrangement is very simple. We have the Siemens light; a lens brings the rays from that Siemens light to a focus on the centre of the bulb, and the vibration of the ether proceeding from those points causes those two molecules to combine in the way that you saw just now. We will put the blue and yellow glasses together first, and start the Siemens machine; when we draw away the yellow glass we have the same result as before.

For photographic purposes silver salts are the most convenient; if, however, we had to wait until the silver salts visibly darkened before we obtained a photograph, we should have to wait much longer than it is in the experience of all that we have to do. I will try and explain what happens when a very short exposure is given to a silver salt. For certain reasons silver chloride is not used, but we have recourse to silver iodide or silver bromide, and in some cases where both are used together a better result is obtained. What happens to these salts by a short exposure? What happens to silver chloride when acted upon by light? You will remember I told you that (with our notation for silver) after light had acted on silver chloride we had one atom of silver combined with one atom of chlorine. In the same way, if we replace the chlorine by iodine we have sub-iodide of silver formed. The visible image and the image impressed by a very short exposure are identical except in the quantity of matter altered. We will suppose, for instance, according to the modern theory that each of these molecules are charged with electricity, with one kind of electricity. These sub-molecules, the sub-iodide and the sub-chloride, are unsaturated compounds, and are ready to join hands with any body for which they have the least affinity. Suppose then we have a solution of a silver salt, say of silver nitrate, and that we introduce some body which will precipitate that silver in a metallic state, and suppose again that one atom of silver as it is precipitated is charged with one kind of electricity, it is quite within the range of probability that the silver atom, as it is precipitated in the solution, may be attracted by the sub-iodide of silver when charged with another kind of electricity, and so form a new molecule as it were. This built-up molecule will not be fully satisfied, but probably have an excess of the opposite kind of electricity, with which the sub-iodide was charged, and the silver atoms which were oppositely charged to those first attracted would in their turn be attracted, and so on until the image is, as it were, built up on the small quantity of sub-iodide first formed by the action of light. In the case of silver bromide we have the same thing happening. We have sub-bromide of silver formed, which is represented by one atom of silver, and one of bromine, $Ag''Br$, and it is quite within the range of possibility that another body may be brought in contact with that, which, being charged with electricity opposite in polarity to that with which this molecule is charged, may attract away, as it were, the bromine, and thus leave the metallic silver itself behind. This is what happens really in the case of what we call alkaline development. In the first case we had acid development, or an image built up by the deposition of silver from silver nitrate, and in the other abstraction of bromine from the small quantity of sub-bromide of silver which is formed by the action of light. The image so built up, however, would scarcely be apparent, since the metallic silver thus formed would be inappreciable to the eye. Another phenomenon seems, however, to present itself, and that is, that the atoms of metallic silver, and of the unaltered bromide of silver, are oppositely charged with electricity, and combine to form fresh sub-bromide of silver; these new molecules of sub-bromide are reduced, and so the action goes on till an image is built up, each molecule of sub-bromide originally formed by the action of light forming a nucleus for the reduction. If instead of forming the iodide and bromide of silver in collodion films, as is usually the case now, we form iodide and bromide of silver on a metallic silver plate by allowing iodine and bromine vapour to have access to it, and if, after exposure to light, we allowed mercury vapour to act upon it, then the same kind of action would take place, the condensed mercury vapour would be attracted to those points which had been acted upon by light. That is the earliest form of photography, and was known as daguerrotype.

I now propose to show you a practical demonstration of the two methods of development of which I have shown you the outlines by diagrams. First, I will ask you to notice the part of the spectrum which the silver iodide plate cuts off. I have placed such a plate in front of the slit of the lamp, and you will see the blue is cut off. Iodide like chloride cuts off the blue rays, but with more intensity than the chloride. Where there is absorption there alone can work be done, the blue rays are therefore most effective in altering iodide of silver. I have a photographic spectrum apparatus placed in position. We have the Siemens light as before, a collimating lens (about which I need not enter into details), a prism through which the light has to pass from the slit, and here we have a lens and an ordinary camera. I propose to place a plate coated with silver iodide in the spectrum of the arc, and another coated with bromide, and then develop them if possible on the screen before you by the two methods of development. The first plate we expose is an ordinary wet plate, *i.e.* we have a collodion film which covers the plate, and this collodion film contains iodide of silver, and it is moistened with nitrate of silver. We will expose that to the spectrum for a second. I have in front of the slit at present a solution of bichromate of potash, which cuts off the blue, and therefore the light passing through would have no effect on the plate. I withdraw the front of the slide, and give a very short exposure. Now we will take a cell, F, containing what we call a developing solution, that is, something which will precipitate metallic silver from the soluble nitrate of silver; we will place it upon the stand of the lantern, B, and by means of the lens G we shall see a reversed image of part of the cell on the screen. I next place a piece of yellow glass, E, in front of the lantern lens. I take the plate and simply immerse it in the solution, and by degrees you will see a blackening take place. I am afraid the film is a little too intense—it is now coming out more rapidly and more vigorously; and here we have the image of the blue end of the spectrum perfectly developed. I will get my assistant to take charge of this, and in the meanwhile I will throw on the screen another spectrum which was taken in a similar manner.

Now we will try the other mode of development, which I hope will be more visible to the audience than the one we have tried. We will use the Siemens light again to form the spectrum. We will keep the yellow solution in front to cut off the blue rays whilst focussing it, and then I am perfectly safe. Then we will remove the yellow solution, and give a very short exposure, and we will develop by the alkaline process. We will use the same apparatus as before for developing. We have here a cell containing the liquid which has a great affinity for bromine, and I have no doubt we shall find that the solution will take away the bromine and leave metallic silver behind. It will probably be rather slower than the other in appearing. The plate is now placed in the cell, and we see the lines of the spectrum are appearing, and finally the image is fully out. I now withdraw the yellow glass.

You may ask how it is I can afford to let the light fall on the plate without causing a further deposition. The fact is, this solution

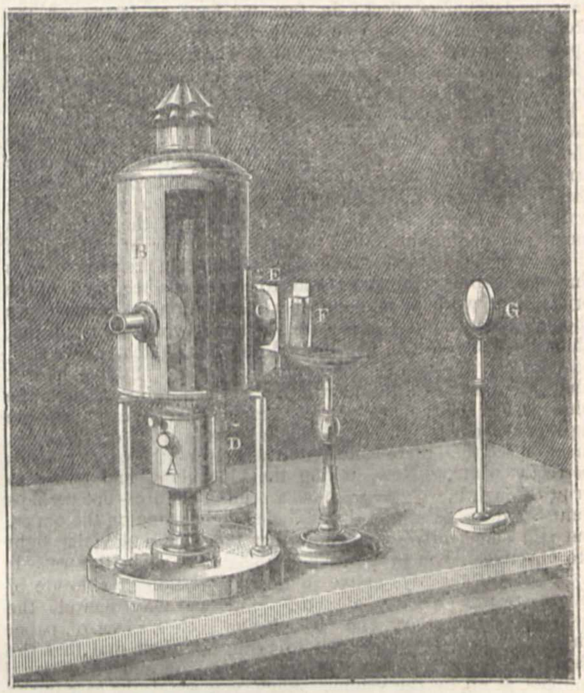


FIG. 2.

itself is coloured red,[1] and therefore the light passing through it has no effect on the bromide. I daresay by and by, when those pictures are thrown on the screen, you will be able to see what kind of spectrum we have got. You will remember, then, that we have in these methods of development which I have endeavoured to bring before you two kinds of chemical action—one a physical action of attraction exercised by sub-iodide of silver, the other a chemical one by the taking away of bromine from the silver salt.

The first mention that we have of a photographic spectrum in the red or below the red was in the year 1839, when Dr. Draper published a paper saying that he had been able to observe certain lines in the solar spectrum below the limit of the red. I propose to show you on the screen a copy of his photograph, and explain how it was taken. Dr. Draper proceeded in this way. He took a daguerreotype plate, exposed it to light first, then exposed it to the spectrum. In Dr. Draper's spectrum we have the whole of the blue and violet part of the spectrum delineated, but in part of the green and all of the yellow there is a blank, but be-

[1] The plate was developed by ferrous oxalate.

low these the lines in the red A and B in the red are shown; but still lower down there are three lines delineated, which he calls α, β, γ. Recollect the way he proceeded. He exposed the daguerrotype to light, and he allowed the spectrum to fall upon it. What was the meaning of that? The meaning of that was he altered the iodide of silver to begin with. I have here a piece of iodide of silver paper. One half has been exposed to light, and the other has not been exposed to light at all, and you will see the difference in colour. One has a greenish brown tint, and the other a decided yellow. The yellow you saw cut off the blue; the brown tint, if you put that in the spectrum, would allow the yellow to pass, but would cut off not only the blue but also some of the red too. How then do we explain this action? It is true he found that these lines, A, B, α, β and γ, had impressed themselves, but he found they were what we call reversed lines, that is to say, what ought to be black before were white, and what ought to be white before were black. How can this be explained? The spectrum was the same, the iodide of silver plate was the same. All was the same except the previous exposure to light. Now, this remained unexplained for a long time. It was supposed there was a certain antagonism between the rays, that is to say, that the red rays were able to undo the work which the blue had done, and that the yellow remained neutral. But was this an explanation? There can be no such thing as antagonism of energy in this matter, and therefore it required some further explanation.

Fig. 3.

In investigating the subject it fell to my lot to try experiments on the constitution of the photographic image, and my experiments led me to find that what was supposed to be antagonism of rays in the red part of the spectrum to those of the blue was nothing else but another chemical action which was called into play.

I have here a negative of a line drawing. I place a collodion plate containing iodide of silver in contact with it, but before I develop it I will place half of it in a solution of peroxide of hydrogen, which is an oxidising solution, and on developing I find only half the image has appeared, viz. the part which was not placed in the oxygen peroxide; to adopt the old explanation, there is an antagonism of the peroxide of hydrogen to the action of light! This gave a clue to Dr. Draper's reversal of the image which he got in the red rays of the spectrum. I will show you what it occurred to me did happen. Dr. Draper's plate after it had been exposed to light contained molecules of sub-iodide of silver, and when he allowed the spectrum to play upon it the sub-iodide of silver was anxious to obtain anything it could to satisfy itself, and so took up oxygen from the air and formed an oxy-iodide of silver, Ag″I, combined with oxygen, and that oxide of silver was totally incapable of development. Why? Simply because its energy of attraction was satisfied; there was nothing to attract the mercury condensed from the mercury vapour by which his image was developed. This then might be an explanation of Draper's photograph which should be capable of proof, and in order to prove it I will show the way I proceeded. I will take a plate of iodide of silver and expose it to light before you; then I propose to immerse it in a cell containing an oxidising solution of very delicate peroxide of hydrogen, which, as we know, is a very strong oxidiser, in fact one which will give up oxygen very freely to anything brought in contact with it. I have an arrangement by which I can do that, consisting of a dark slide in which I can put the cell and the silver plate. *Here* you see the prepared plate in its normal state. The electric light is allowed to act upon it, and of course the iodide of silver will be reduced to sub-iodide. I shall next allow the spectrum to play on the plate whilst immersed in this oxidising solution, and see if we cannot get the same results that Draper did in his reversed spectrum. If the theory that I give of the production of Draper's oxidising photograph is correct, then the red light ought to aid the oxidation of the photograph, or rather of the subiodide, forming that oxyiodide of silver which I mentioned to you. I will leave the bichromate cell in front of the slit in order that you may see the blue rays have nothing at all to do with the matter. I will give rather a longer exposure than I did before, and in that way I think I shall be able to get a result. I will give it to my assistant to develop, and then throw it on the screen as soon as he has finished with it. Now what shall we expect to find in this photograph? If the red rays help the oxidation of the iodide we shall expect to find that where the red rays are active no development whatever will have taken place, and as a fact that is really what we find. This method of Draper's, of photographing the ultra-red part of the spectrum, is exceedingly inconvenient, as it requires a long exposure, and, I may say, is unsatisfactory, because it gives very bad definition. You have to use a very open slit in order to get it.

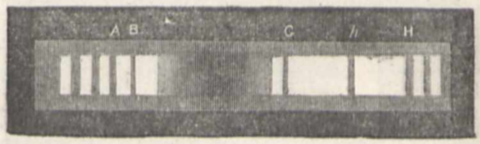


Fig. 4.

I now throw on the screen a portion of the *solar* spectrum taken in the way I have just carried out before you. In it you will see a facsimile almost of Draper's photograph. The question arises, what would happen suppose the plate was immersed in a deoxidising solution such as potassium nitrite? We have the answer at hand in the shape of a photograph so exposed. The reversal, you see, is entirely absent.

Now the question comes, Is it possible to show the existence of the ultra-red rays of the spectrum by visible means? in other words, by the exposure of any surface to the light? I think I can show you that it is possible by Balmain's phosphorescent paint. If I expose this to the light of the electric lamp we shall find that of course it will become very luminous indeed. Now when I expose this luminous surface to the action of the spectrum something ought to happen, perhaps which will give us an idea of Draper's photographs. I will try, and then I must pass round this luminous plate, because you will not be able to see it at a distance. I bring the light to a focus on the slit of the spectroscope, and place the surface of the paint, which is still luminous, in the spectrum for a short period, and now I will pass it round, and you will be able to see the phenomena; first of all a bright patch, and then a black patch beyond. The bright patch is caused by the blue rays; the black patch beyond is caused by the ultra-red rays, the red rays and the yellow rays; in other words, these rays have the property of quenching the vibrations of the phosphorescent particles, so that you see we have a means of showing visibly the existence of the ultra-red rays of the spectrum.

Our knowledge of the value of the photography of the spectrum, as regards its most refrangible portion, was very limited indeed until Mr. Lockyer took up the subject of spectrum photography with earnestness. In the year 1872 at Chatham we also began our researches in this matter, and we hoped that what we had found to be so immensely valuable in the violet and the blue regions of the spectrum—we might also be able to accomplish for the red and ultra-red rays of the spectrum as well. In the year 1872 Vogel made an important announcement, which, if it had proved everything one could have wished, would have left no need for further experimentation. He said if you took a bromide of silver plate or iodide of silver plate and covered it with a dye, that in the spectrum, where the dye absorbed, there a photographic action, although beyond the usual boundary of photographic action, would be seen. We followed up this very carefully, hoping to find some dye by which we might be able to photograph the ultra-red rays of the spectrum. Had I known as much then as I do now I should not have followed any such chimerical idea. But what Vogel stated was perfectly correct, viz. that in that regions of the spectrum which certain dyes absorbed, a photographic action would take place. Suppose I take a plate prepared with some silver salt, and flow over it a dye, and then expose it to the spectrum; I find where the dye absorbed there a photographic image was formed. What was the meaning of this? This required investigation as well. The first dye that was taken up was that of cyanin blue. I have here a plate covered with cyanin blue, and when this plate was placed in the spectrum it was found that it bleached in the yellow, No. II. Fig. 5. Now what was the meaning of that bleaching in the yellow? Let us consult the absorption spectrum of cyanin blue, to see whether it absorbs in that particular part of the spectrum; for if it absorbs in that particular part there work must be done as I have already shown you. I will throw the spectrum on the screen, and then introduce a solution of cyanin blue in front of the slit of the lantern, No. I. Fig. 5. We do this, and it will be seen that there is great absorption in the yellow, so that that particular portion of spectrum bleached the cyanin blue which the cyanin blue cut off. So that work and absorption went hand in hand: when the action was investigated more closely, it was found

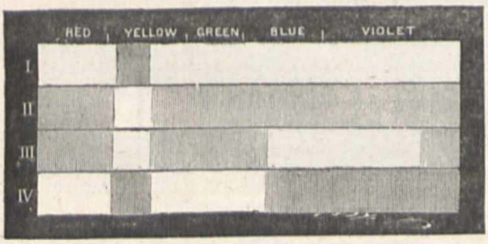


FIG. 5.—I., absorption spectrum of cyanin blue; II., bleaching effect of spectrum on paper stained with cyanin blue; III., bleaching effect of spectrum on a silver bromide film stained with cyanin blue; IV., photographic impression of spectrum prepared as in III. The shaded parts show metallic silver on development.

that the work performed was an oxidation; in other words, that bleaching took place through the effect of oxidation. So much for cyanin blue, *per se;* when, however, we took a plate covered with bromo-iodide of silver in collodion, and dyed with cyanin blue, exposing it for some time to the spectrum, it was found not only that the cyanin was bleached in the yellow, but it was also bleached in the blue, No. III. Fig. 5. It should be remembered that the only factor of difference was in the first case we had cyanin blue, in the collodion by itself, and in this last the dye and iodide and bromide of silver. What then was the explanation of this? That required a further investigation, and I think, perhaps, I shall be able to show you what really did happen. I will take that same cell of cyanin blue used before, and place it in front of the slit of the lantern, and we have the cyanin absorption spectrum on the screen. Now I have told you that when bromide of silver or iodide of silver is exposed to light, one atom of iodine or bromine is given off, and if the exposure be prolonged the amount is measurable, therefore it is possible that the bleaching action in the blue might be due to the action of bromine, and if so, bromine ought to be able to bleach cyanin blue. If we take bromine water and drop it into the cell, I think you will find that the whole spectrum will appear again in its usual brilliancy; we drop the bromine water in, and the whole spectrum does appear on the screen. Our question then is answered. The bromine liberated from the bromide of silver by the action of light when the dyed film was placed in the spectrum, was able to bleach it in the blue part of the spectrum in the same way that the oxygen in the air was able to bleach it under the influence of the yellow rays.

Now I will show you what the action of oxidised matter on silver is. Here we have a glass plate on which was written "May 25th" with an oxidised solution of albumen. This was coated with a collodion film containing bromide and iodide of silver, and developed in the usual way. You will see that where the oxidisable matter is placed, there we have a deposition of silver upon those particular portions. Apply this to the spectrum developed on a plate stained with cyanin blue; where it is bleached in the yellow, the oxidised dye will cause a deposit of silver to be formed,[1] whilst where the blue rays have acted we shall have a deposition of silver due to ordinary development, as already explained. I throw upon the screen a spectrum showing this. The film of collodion containing the silver salts was dyed, and then the bromide of silver dissolved away. You see we have a bleaching in the yellow and also a bleaching in the blue, one being due to the oxidising action of the yellow rays on the plate, the other due to the action of the bromine upon the dye itself. Next I will show you a photograph (No. IV. Fig. 5) of the spectrum taken on such a dyed plate. We have the part impressed by the blue rays, and a deposition of silver as before, and also we have the yellow where there is another strong deposition of silver. For convenience' sake I have photographed the absorption spectrum of cyanine and placed it below the spectrum photographed on the silver stained with cyanin. You will thus see that the band in the yellow impressed on the latter plate corresponds exactly with the absorption of the cyanin blue itself.

Carrying the investigation a little further, it was found that the same took place with eosin. I have an eosin solution here, and here is an absorption-spectrum of eosin which cuts off a great deal of the green—we have the yellow, but the green is cut out and the blue is damped. The green is the principal portion which is absorbed; in other words, the work which has to be done on the dye will be done in that part of the spectrum. In the photograph of the spectrum of eosin taken with bromide of silver dyed with eosin, you see as a result that we have the plate impressed by the blue rays, and also the plate impressed by the green. The deposition of silver on the two parts is due to different causes: that in the green is due to the work done on the dye; the work was not done on the silver directly, but on the dye first. That on the blue was due to the work done on the silver bromide itself. I may say that all dyes which I have found useful in the photographic sense are what we call fugitive dyes; in other words, dyes which fade in the light. Ladies are perfectly well acquainted with the fact that some dyes will not stand well; those which fade most rapidly give the best results in spectrum photography.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Clothworkers' Exhibition for proficiency in Physical Science, tenable for three years by an unattached student at Oxford or Cambridge, has been awarded to J. Davies. The next Clothworkers' Exhibition will be awarded for Physical Science by means of the examination for certificates to be held next July by the Oxford and Cambridge Schools Examination Board. Candidates must be non-collegiate students of one term's standing at Oxford or Cambridge, or non-residents who are prepared to enter as such.

The oral and practical examinations in the second part of the Natural Sciences Tripos concluded on Monday last (12th).

Prof. Stuart has now thirty-eight pupils in mechanism and engineering, and more space and new machinery are needed to meet their growing requirements. A new room measuring thirty-six by twenty-five feet is asked for, with motive power, a heavy lathe, a slotting machine and larger forge. Messrs. Greenwood and Batley will present a slotting machine when there is a place to put it in. The building will cost 225*l.*

Mr. D. MacAlister, Fellow and Medical Lecturer of St. John's College, will lecture on Methods of Physical Diagnosis three times a week next term, beginning February 2.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, December 3.—Sir John Lubbock, Bart., president, in the chair.—Mr. J. Harris Stone exhibited specimens of the dried plant and made remarks on *Lychnis viscaria* as a

[1] It has been objected by Dr. Vogel that the bleaching action requires time to effect it, and that the phenomenon is visible after a short exposure. The simple answer to the question is, When does the bleaching commence? The merest trace of reduced dye would act as a nucleus for development, as does the merest immeasurable trace of subiodide or bromide.

trap for ants. He pointed out that three or four glutinous or sticky rings are situate immediately underneath the nodes in the flowering stalks. Ants climbing the stems are arrested and die in numbers at the sticky zones, and few reach the flower. In Norway last summer he had observed as many as 95 per cent. of the plants with dead and dying ants thereon; and he therefore submits whether the zones are a protection to the flowers? the ants noxious? or that their dead bodies ultimately serve as pabulum for the plant? Dr. T. S. Cobbold exhibited diseased roots of *Stephanotus* which he had received from Dr. Masters. They swarmed with myriads of nematode worms and were also covered with minute Acari. He referred the worms to the genus *Leptodera*, and stated that thirty-three years back he discovered similar parasites in the shrivelled leaves of Gloxinias.—Dr. Maxwell Masters read a note on the foliation and ramification of *Buddleia auriculata*. He shows from a study of the mode of development and other considerations that the leafy auricles between the petioles represent leaves of a whorl, so that the verticil consists of two perfect and two imperfect leaves. An additional link between Loganiaceæ and Rubiaceæ is thus afforded. Further details were given concerning the multiple axillary buds in this plant and the supra-axillary shoots. Some of the peculiarities alluded to are usually explained on the hypothesis of fusion, but the author shows that in this, as in many similar cases, the appearances are due to an arrest of development, in consequence of which parts that should become free in course of growth remain inseparate, and in some cases are "uplifted" with the axis as it lengthens, and are thus removed from their normal position.—Prof. Owen read a paper on the homology of the conario-hypophysial tract, or the so-called pineal and pituitary glands. He propounds the view that it is the modified homologue of the mouth and gullet of Invertebrates; that the sub-œsophageal ganglia or ganglionic masses or neural cords constitute the centres whence are derived and caudally continued the homologues of the Vertebrate myelon.—Mr. McLachlan communicated a paper on the Neuroptera of Madeira and the Canary Islands. Prompted by the researches of the Rev. A. E. Eaton in November and December, 1880. The author gives a *résumé* of all that had been published on the subject, and a tabular statement of the species found in the islands, indicating those known also to exist in Europe. In all about 53 species had been noticed from the islands, of which 19 are known inhabitants of the European continent, and 4 African; 37 species had been found in Madeira, 31 in the Canaries, 16 being common to both. The paper concluded with a detailed account of the species, including descriptions of several new ones.—The following gentlemen were balloted for, and elected Fellows of the Society:—Capt. P. Greene, G. S. Jenman, W. Landau, E. G. Warnford Lock, Rev. T. P. H. Sturges, Lieut.-Col. C. Swinhoe, G. C. Walton, C. S. Wilkinson, G. S. V. Wills, and Rev. Geo. Wilson.

Mathematical Society, December 8.—Mr. S. Roberts, F.R.S., president, in the chair.—Mr. G. H. Stuart, M.A., late Fellow of Emmanuel College, Cambridge, was elected a Member, and Miss C. A. Scott was admitted into the Society.—The following papers were read:—On the polar planes of four quadrics, Mr. W. Spottiswoode, Pres. R.S.—On some forms of cubic determinants, Mr. R. F. Scott.—On the flow of a viscous fluid through a pipe or channel, Prof. Greenhill.—The covariant which is the complete locus of the vertex of the involution pencil of tangents to a cubic, Mr. J. J. Walker.

Chemical Society, December 1.—Prof. Roscoe, president, in the chair.—The following papers were read:—Researches on the laws of substitution in the naphthalene series, Part ii., by Dr. Armstrong and Mr. Graham. The product of the action of cold sulphuric acid on β naphthol proves not to be identical with the isomeric sulphonic acid of Rumpf, but to be β naphthyl-sulphonate. The same substance may be obtained pure by the action of sulphochloride on β naphthol. By studying the reactions of this body the authors prove that bromine and the sulpho group do not assume the same position in the body when the sulphate is treated with bromine and sulphochloride respectively, and express the opinion that modifications of the OH group appear to lead to important modifications of the laws of substitution. A third and a fourth isomeric naphthalene-disulphonic acid have been obtained.—On benzylphenol and its derivatives, by E. H. Rennie. The author has obtained a monosulphonic acid and its salts in a crystalline condition. He has investigated the action of nitric acid and of bromine on the salts. He believes benzolphenol to be a para derivative.—Note on the action of ethyli chlorocarbonate on benzene in the presence of aluminic chloride, by E. H. Rennie.—On peppermint camphor and some of its derivatives, by R. W. Atkinson and H. Yoshida. The authors have studied the action of bichromate on this camphor. Menthone is produced; from its reactions the authors conclude that the relation between menthol and menthone is similar to that between borneol and camphor. They have examined the physical properties of these derivatives, and give the probable constitution of these bodies.—On the production of oxalic acid from paraffin oil, by J. Galletly and J. S. Thomson. The authors have acted on paraffin oil from shale, with nitric acid, and find that oxalic acid is produced.

Physical Society, November 26.—Prof. W. G. Adams in the chair.—Mr. C. Vernon Boys read a paper on integrating apparatus. After referring to his original "cart" machine for integrating, described at a former meeting of the Society, he showed how he had been led to construct the new machine exhibited, in which a cylinder is caused to reciprocate longitudinally in contact with a disk, and give the integral by its rotation. Integrators were of three kinds: (1) radius machines, (2) cosine machines, (3) tangent machines. Sliding friction and inertia render the first two kinds unsuitable where there are delicate forces or rapid variations in the function to be integrated. Tangent machines depend on pure rolling, and the inertia and friction are inappreciable. They are therefore more practical than the other sort. It is to this class that Mr. Boys' machines belong. The author then described a theoretical tangent integrator depending on the mutual rolling of two smoke rings, and showed how the steering of a bicycle or wheelbarrow could be applied to integrate directly with a cylinder either the quotient or product of two functions. If the tangent wheel is turned through a right angle at starting, the machine will integrate a reciprocal, or it can be made to integrate functions by an inverse process. If instead of a cylinder some other surface of revolution is employed as an integrating surface, then special integrations can be effected. He showed a polar planimeter, in which the integrating surface is a sphere. A special use of these integrators is for finding the total work done by a fluid pressure-reciprocating engine. The difference of pressure on the two sides of the piston determines the tangent of the inclination of the tangent wheel which runs on the integrating cylinder, while the motion of the latter is made to keep time with that of the piston. In this case the number of revolutions of the cylinder measures the total amount of work done by the engine. The disk cylinder integrator may also be applied to find the total amount of work transmitted by shafting or belting from one part of a factory to another. An electric current meter may be made by giving inclination to the disk, which is for this purpose made exceedingly small and delicate, by means of a heavy magnetic needle deflected by the current. This, like Edison's, is a direction meter, but a meter in which no regard is paid to the direction of the current can be made by help of an iron armature of such a shape that the force with which it is attracted to fill the space between the poles of an electromagnet is inversely as its displacement, and then, by resisting this motion by a spring or a pendulum, the movement is proportional to the current, and a tangent wheel actuated by this movement causes the reciprocating cylinder on which it runs to integrate the current strength. Mr. Boys exhibited two such electric-energy meters, that is, machines which integrate the product of the current strength by the difference of potential between two points with respect to time. In these the main current is made to pass through a pair of concentric solenoids, and in the annular space between these is hung a solenoid, the upper half of which is wound in the opposite direction to the lower half. By the use of what Mr. Boys calls "induction traps" of iron, the magnetic force is confined to a small portion of the suspended solenoid, and by this means the force is independent of the position. The solenoid is hung to one end of a beam, and its motion is resisted by a pendulum weight, by which the energy meters may be regulated like clocks to give standard measure. The beam carries the tangent wheels, and the rotation of the cylinder gives the energy expended in foot pounds or other measure. The use of an equal number of turns in opposite directions on the movable solenoid causes the instrument to be uninfluenced by external magnetic forces. Mr. Boys showed on the screen an image of an electric arc, and by its side was a spot of light whose position indicated the energy and showed every flicker of the light and fluctuation of current in the arc. He showed on the screen that if the poles are brought too near the energy expended is less, though the current is stronger, and that if the poles are too far apart, though the electromotive force is greater, the energy is less; so that the apparatus may be made to find the distance at which the greatest energy, and so the greatest heat and light, may be produced.—At the conclusion of the paper Prof. W. G. Adams and Prof. G. C. Foster could not refrain from expressing their high admiration of the ingenious and able manner in which Mr. Boys had developed the subject.

December 10.—Prof. W. G. Adams in the chair.—New members: Lieut. C. E. Gladstone, R.N., Lieut. C. Gauntlett Dicken, R.N., Mr. Walter George Woolcombe, B.A., F.L.S., Rev. Prof. Sircomb, Mr. Arthur Clayden, M.A.—Prof. Adams said that it had been thought advisable to invite the members to view the Smoke Abatement Exhibition now opened, and the meeting was adjourned for that purpose; Prof. Chandler Roberts acting as guide.—The next meeting of the Society will be held on January 28.

PARIS

Academy of Sciences, December 5.—M. Wurtz in the chair.—The first volume of the works of Cauchy, the first of a new edition of the works of Niels-Henrik Abel, and the scientific MSS. of Chasles, were presented and commented upon.—General survey map of France, by M. Perrier. This is a work of the geographical service of the War Department, and indicates, by curves, the orography of the country.—Meridian observations of small planets and of comet *b* of 1881 at Paris Observatory, during the third quarter of 1881, by M. Mouchez.—On the theory of chain-shot, by M. Resal.—On some applications of the theory of elliptic functions, by M. Hermite.—Chemical studies on the skeleton of plants, by MM. Fremy and Urbain. The substances forming the skeleton are chiefly pectose and its derivatives, cellulosic substances in their different isomeric states, cutose, and vasculose. The results of analyses of stems, roots, leaves, petals, fruits, and seeds, also of the tissues of champignons, are given. It is thought they may be useful to botanists for classification, for physiological studies, for study of manures, and fossil fuel, &c., and that they have also industrial uses.—Summary account of a zoological exploration in the Atlantic, in the *Travailleur*, by M. Alph. Milne-Edwards. This exploration was chiefly off the coasts of Spain and Portugal, in August. In one case dredging was done at the great depth of 5100 m., where numerous animals were met with, small indeed, but of high groups (annelids, crustaceans, &c.); the temperature was $+ 3^{\circ}\cdot5$. Near the Spanish coast, and beyond 1000 m. depth, numerous polypiers, mostly unknown, showed marvellous development at some points. Among other "finds" were three very rare sharks (at 1200m.), which seem never to leave the depths, a Norwegian Pontophile, which had been thought peculiar to northern seas, a new Pontophile, a gigantic Pycnogonidian, ten new genera of Bryozoa, magnificent corals of the genera *Lophohelia* and *Amphihelia*, a remarkable *Asteria* representing a new genus, an organism got at 1145m., which may belong to the group of Infusoria, and (there also) a fine *Eugliphus* like *Difflugies* of fresh water.—On certain meteorological stations it is proposed to establish in the neighbourhood of the North Pole, by M. Faye. He thinks little of the project. Its authors consider that the ice of polar regions is *perhaps* the regulator of our climates; but modern science shows the regulator to be rather in the vast equatorial zone, whence storms travel over the two hemispheres. After expounding his conception of these phenomena, Mr. Faye points out that France would do better to organise a meteorological station in the Azores rather than at Cape Horn.—On the theory of linear differential equations of the second order, by M. Brioschi.—Deposit of metallic layers of different colours by electricity, by M. Weil. Using a single copper bath, he can cover steel or brass *e.g.* with bright layers of such and such a colour at will, by means of different suboxides of copper, whose chemical nature is not yet determined. The same bath will produce the whole series of colours according to the manner of exposing the pieces. The effect is not one of thin plates.—Observations in 1881 on phylloxera and on the means of defence adopted, by M. Boiteau.—Observations of solar spots and faculæ at the Observatory of the Roman College during the third quarter of 1881, by M. Tacchini. An exceptional *maximum* of spots occurred in July (as predicted). The solar activity has gone on increasing, with special periods of greater frequency of spots, nearly corresponding, as before, to a half-solar rotation. The faculæ show a marked maximum in September.—On the spectrum of Encke's comet, by M. Tacchini. This, observed on November 8, had the three carbon bands (the central brightest) shaded off on the violet side; the weak continuous spectrum of the nucleus forming a uniform straight line across the bands.—On Wendell's comet (*g* 1881), by M. Tacchini.—Rectification and addition to a previous note on the curve of the solar spots, by M. Duponchel. He predicts that the next sun-spot maximum will not be before 1890 (others say 1882), perhaps 1888, but more probably 1892.—On the curves defined by differential equations, by M. Poincaré.—Distribution of energy by electricity, by M. Deprez.—On the determination of the ohm; reply to M. Brillouin, by M. Lippmann.—Variations of the resistance of electric machines with their velocity, by M. Lacoine. He shows reason for thinking these variations are explained by those of contact between the movable commutator and the springs in friction.—Determination of the illuminating power of simple radiations, by MM. Crova and Lagarde. Sunlight and a Carcel lamp were compared. Part of the spectrum is isolated with a slit, and a Nicol turned till striæ cease to be perceptible. With the lamp the maximum corresponds to the radiation 592; with the sun, to 564.—On the velocity of cooling of gases at high temperatures, by MM. Maillard and Le Chatelier.—Combination of hydrogen with oxygen under the influence of electric *effluves*, by MM. Deherain and Maquenne. The state of humidity of the surfaces between which the *effluve* is produced affects profoundly the nature of the discharge, both as to external aspect, and to its action on the gases.—On the titrage of œnoline and œnotannin in wines, by M. Jean.—Meteorological observations during a balloon ascent on October 20, 1881, by MM. Duté, Poitevin, and Du Havel. These relate chiefly to formation of clouds.—Observation of palpebral reflex in chloroformic anæsthesia, by M. Berger.—On the convulsing action of morphine in mammalia, by MM. Grasset and Amblard. All researches on the antagonism of various medicaments to morphine should be revised, the substances opposing the soporific effects, and those opposing the excito-motor effects of the alkaloid, being studied separately.—Researches on the history of generation in insects, by M. Jobert.—On the post-embryonal development of diptera, by M. Viallanes.—Researches on the action of digestive juices of cephalopoda on amylaceous matters, by M. Bourquelot. The liver and pancreas produce or contain a ferment which has no action on raw fecula, but which changes hydrated starch into sugar.—On the diamantiferous beds of Minas Geraës, Brazil, by M. Gorecix.—M. De Lesseps gave some information with regard to the scheme for piercing the Isthmus of Corinth.

VIENNA

Imperial Academy of Sciences, December 1.—V. Burg in the chair.—The following papers were read:—R. Andreasch, on some further examples of syntheses of the sulphydantoins by means of thioglycollic acid.—Anton Tomaschek, on the power of movement of pollen-bags and pollen-tubes.—W. F. Loebisch and Dr. A. Looss, on the action of carbonic oxide gas on monopotassium glycerate.—On the preparation of dipotassium glycerate, by the same.—T. Hann, on the monthly and yearly oscillations of temperature in Austro-Hungary.

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