THURSDAY, APRIL 7, 1892.

MENDELEEFF'S PRINCIPLES OF CHEMISTRY.

The Principles of Chemistry. By D. Mendeléeff. Translated from the Russian (Fifth Edition) by George Kamensky, and edited by A. J. Greenaway. Two Vols. (London: Longmans, Green, and Co., 1891.)

ALL English-speaking chemists will cordially welcome the appearance of this book, if for no other reason than because its author in its preparation was led to the recognition of that fundamental principle of chemistry with which his name will always be associated—the principle which is embodied in what is now known as the periodic law. This fact alone would serve to stamp the book as one of the classics of chemical science. But, even apart from this circumstance, the work has very remarkable, and, indeed, exceptional, merits. Probably no scientific treatise ever more strikingly reflected the personality of its author. We have absolutely nothing like it in our language. In grasp of principles, in philosophic breadth, in copiousness of detail, in richness of speculation and suggestion, it is altogether unique among chemical manuals. Every true and earnest student of chemistry is certain to be profoundly influenced by it, even if he cannot always bring himself to subscribe to its doctrine. Of course, the facts are, for the most part, those which are common to all the larger treatises on systematic chemistry, but these are set out and marshalled in a manner wholly original. The intent and purpose of the book is to demonstrate the broad general principles on which chemistry as a science rests. This, it may be urged, is the intent and purpose of every chemical treatise. It may be so, but in many cases the philosophy is lost sight of—obscured, indeed, by the facts, just as the houses may obscure the view of the village.

In Mendeléeff's work experimental and practical data have their place, but only as means to an end, and that end is as evident on every page as it was in Dalton's immortal work. Fascinating as the book is, it must be admitted that it is by no means easy reading; and he who wishes to master its contents and to assimilate its teaching will need to gird up his mental loins. Part of the difficulty is doubtless due to the different genius of the languages, but much more depends upon the impossibility of entering into the spirit of an author, or of quickly realizing his drift and meaning, when his whole mode of thought is so very dissimilar to that which obtains among Western people. It may be that herein lies part of the peculiar charm of freshness of the work. The book of the Siberian chemist is to the ordinary run of text-books what the novels of Tolstoi or Turgenieff are to the common run of works of fiction. But there are difficulties of another kind. Probably no other book in our language—certainly no other chemical treatise—contains such an extraordinary number of footnotes. There is scarcely a page without a footnote, and some of the pages are practically little else than footnotes. The continuity of description or of argument is constantly being broken, often by a footnote extending over several pages, and frequently so diffuse and involved that, by the time the reader has disposed of it, the statement in the main text to which it had reference has been lost sight of, and must needs be picked up again. Moreover, the repeated interruption is aggravated by the circumstance that these notes are printed in "nonpareil small," which adds enormously to the physical fatigue of reading and studying the work. The author, indeed, recommends that they should be read only by the advanced student, or on a second perusal of the work; but we are afraid that no intelligent reader will follow this advice when once he has begun to dip into them. They are, in fact, like the postscripts of ladies' letters—often more important, more instructive, more suggestive, and more characteristic, than the main body of the text. But, in truth, the book is not fitted for a beginner: its proper readers are those for whom the footnotes are specially intended. It requires, too, to be read with discrimination. It was said by Davy that analogy is the fruitful parent of error, and it must be confessed that Mendeléeff's love of analogy frequently leads him to generalizations which are more ingenious and suggestive than intrinsically sound or well grounded.

The translator and the editor have, doubtless, had difficulties to contend with. They tell us that they have not considered themselves at liberty to make any alterations in the matter of the work, and they have striven to give a literal rendering of it. They have felt that, on the whole, it would be better to have some inelegance of language rather than risk the loss of the exact shade of the author's meaning. Unfortunately, in too many instances the translator and his editor have not gained in precision of meaning what they have lost in elegance of statement. Thus, for example, on p. 12 we read:

"The means of collecting and investigating gases were already known before Lavoisier's time, but he first showed the real part they [the means or the gases?] played in the processes," &c.

On p. 19 it is stated:

"By heating chalk (or else copper carbonate . . .) we obtain lime," &c.

Thus, too, on p. 47:

"In general terms water is called pure when it is clear and free from insoluble particles held in suspension and visible to the naked eye, from which it may be freed by filtration through charcoal, sand, or porous (natural or artificial) stones, and when it possesses a clean fresh taste. It depends [what depends?] on the absence of any tastable, decomposing organic matter, on the quantity of air and atmospheric gases in solution, and on the presence of mineral substances to the amount of about 300 grams per ton (or cubic metre, or, what is the same, 300 milligrams to a kilogram or litre of water), and of not more than 100 grams of organic matter."

Again, on p. 72 we read:

"Although in the majority of cases the solubility of solids increases with the temperature, yet, just as there are substances whose volume diminishes with a rise in temperature (for example, water from 0° to 4°), so there are not a few solid substances whose solubilities fall on heating. Glauber's salt, or sodium sulphate, historically forms a particularly instructive example of the case in question. If this salt be taken in an ignited state (!) (deprived of its water of crystallization), then its solubility," &c.

What, too, is the meaning of the statement on p. 83?

"Under ordinary circumstances the quantity of aqueous vapour [in the air] is much greater [than what?], but it varies with the moisture of the atmosphere."

Presumably, for "moisture" we are to read "temperature." On p. 164, in the description of the experiment of burning phosphorus in oxygen, it is recommended that "the cork closing the vessel should not fit tightly, otherwise it may fly off with the spoon." That the cork should fly off with the spoon is contrary to well-established precedent: if anything is to fly away with the spoon, it should, of course, be the dish on which the bell-jar is represented as resting. To say (p. 417) that common salt containing magnesium chloride "partially effloresces in a damp atmosphere" is opposed to fact, and was surely never so stated by Mendeléeff. Van der Waals's equation is written:

$$\left(p + \frac{a}{v^2}\right)(v - p) = R(1 - at),$$

instead of

$$\left(p + \frac{a}{v^2}\right)(v - b) = R(1 + at).$$

And on the same page we find $pr = c(1 + at)$, instead of $pv = c(1 + at)$.

Proper names are frequently wrong. Thus we have "Van der Waal" for van der Waals, "Becker" for Becher, "Brown" for Braun, "De Haen" for De Heen, "Frauenhofer" for Fraunhofer, "Personne" for Person, "Prout" for Proust, "Ray" for Rey, "Krütznach" for Kreutznach, "Wergtesgaden at Salzkammerhutte" for Berchtesgaden (which is not in the Salzkammergut).

We have taken the pains to compare the English version with the German translation of Jawein and Thillot in a number of instances where the meaning is obscure, or where statements are made which appear to be erroneous, and in no single instance is the fault to be traced to the author. We think, too, that the limitation imposed on the translator and editor by themselves has operated injuriously in another way: in cases where subsequent research should modify or supplement particular statements in the original, it was surely open to them, in the interests of knowledge, to substitute truth for error. Thus we know from the work of Winkler and Hempel the conditions under which exact determinations of oxygen by means of alkaline pyrogallol can be made; we know, too, that atmospheric ammonia and nitric acid are not by any means the main sources of the supply of nitrogen to plants; ammonium chloride is not now usually prepared by sublimation. The statement of the principle of Kjeldahl's method, given on p. 246, is inaccurate: the radicle ammonium has not been obtained, nor is the old view of the nature of the so-called "ammonium amalgam" any longer tenable, nor is there any direct proof of the existence of ammonium hydrate. Flagstone, at least in this country, is not a form of carbonate of lime: it is usually a fine-grained micaceous sandstone. The apparatus employed by Cavendish in his memorable synthesis of water in no wise resembled that described and figured on p. 167; thanks to the symbol adopted by the publishing Society which bore his name, it seems now well-nigh impossible to get rid of the belief that the pear-shaped stoppered eudiometer was devised and used by him in the course of his investigation: as a matter of fact, the explosions were made in a simple Volta tube. With respect to the illustrations in general, we think that the majority of them could well have been spared; all of them have done duty in other works, and many of them are calculated to give an erroneous impression of the thing sought to be represented. Thus the coke-tower figured on p. 443 resembles nothing actually used; Fig. 60, which is stated to be a Davy lamp, is either a Mueseler or a Clanny lamp; Fig. 47 does not illustrate the method of preparing nitric acid employed in this country, nor does Fig. 93 represent a modern blast-furnace. The only figure of a zinc-furnace given is that of the practically obsolete *per descensum* method.

We have been constrained to point out these blemishes, not in any hypercritical spirit, but solely because of our wish that Mendeléeff's great work should have been given to English and American readers in as perfect a form as possible. The blemishes, after all, are only as the spots on the sun. It is a great boon to get the book even as it is, for no thoughtful reader can fail to be quickened and animated by its fruitful and suggestive pages.

T. E. T.

THE LIGATION OF THE GREAT ARTERIES.

A Treatise on the Ligation of the Great Arteries in continuity, with Observations on the Nature, Progress, and Treatment of Aneurism. By Charles A. Ballance, M.B., M.S. Lond., F.R.C.S., and Walter Edmunds, M.A., M.C. Cantab., F.R.C.S. (London: Macmillan and Co., 1891.)

THIS work is the result of investigations carried out by the authors during the last seven years, and contains the results of long and careful study. The authors were evidently desirous of getting to the bottom of their subject, and in their endeavours to do so, have used every method of research at their disposal.

The first chapter is devoted to a short account of hæmorrhage in man, and contains valuable statistics as to the results of the ligature of the main arteries. This part will, of course, prove of the greatest interest to the pure surgeon. The second chapter contains a paragraph on the necessity of experiments on animals for the purpose of studying the mechanism of hæmorrhage; this paragraph might well have been omitted in a book written for the professional and scientific public, who are already convinced of the necessity of pathological experiments if pathology is to make any advance at all. The greater part of this chapter, however, is full of valuable facts concerning the occurrence of the disease of arteries in animals; whilst in the third chapter the structure of arteries is described, and, in this connection, the experiments made by the authors on the longitudinal tension of arteries require special mention.

Physiological occlusion and pathological obliteration of arteries are discussed in the next two chapters, and the authors point out that in physiological occlusion, Nature does not think it necessary to rupture the two inner coats of the artery, and that she does not divide the artery to reduce the longitudinal tension. In pathological obliterations, likewise, the rupture of the coats is by no means essential to occlusion, and, the coats not being ruptured, hæmorrhage does not occur.

In 1889, Messrs. Ballance and Sherrington published in the *Journal of Physiology* a valuable paper on the formation of scar-tissue, which has been practically re-copied into this book. The authors have made use for their experiment of Ziegler's method of placing glass-chambers under the skin of animals, and examining their contents at various intervals after their introduction. Messrs. Ballance and Sherrington have been unable to trace the development of the so-called plasma-cells from the ordinary cell-forms of blood and lymph, and incline towards the opinion that plasma-cells are derived from the connective-tissue elements, and ultimately develop into fibrous tissue.

I cannot help thinking that Ziegler's method is by no means satisfactory when the object in view is to study the formation of cicatricial tissue ; for in such investigations the most important point is that all the tissues to be examined should be removed intact, and examined after fixation. It is impossible to do this with glass-plates, but satisfactory results may be obtained by introducing soft material, such as filter-paper. The surrounding tissues and the paper can then be removed, and serial sections made through the whole. The examination of preparations made in this way make it doubtful whether Messrs. Ballance and Sherrington's views are correct, and would rather lead me to believe that the plasma-cells are originally derived from leucocytes.

Messrs. Ballance and Edmunds proceed to investigate the conduct and fate of the coats and of the ligature, and it is clear that they have taken immense trouble in ascertaining, by experiments on animals, how quickly a ligature becomes absorbed after being applied. Numerous and beautifully-executed plates, show the microscopical and macroscopical appearances of ligatures made of tendon, silk, floss-silk, silkworm gut, &c., at varying intervals after their application to blood-vessels in man and animals ; whilst special chapters are devoted to the ligature, of the knot, of the force used in the tying, &c. It may be noticed that the authors describe a new form of knot, which they recommend, and to which they give the name of stay-knot, whilst the old-fashioned reef-knot is entirely discarded. Moreover, the authors condemn in no uncertain terms the practice of rupturing the coats of arteries during, and the division of vessels after, ligature —points of the greatest practical importance.

The other chapters on the operation and the fate of the patient are of clinical interest chiefly ; but special mention should be made of excellent chapters on suppuration occurring after ligation, and on the pathology of hæmorrhage, as well as of the full account of the experimental investigations made by Messrs. Ballance and Edmunds. It is only right to mention that most of the experiments were made at the Brown Institution.

The book is beautifully printed, and profusely illustrated with 10 plates and 232 figures. It will be widely read by all surgeons, histologists and pathologists, and forms a most valuable addition to surgical and pathological science.

M. ARMAND RUFFER.

OUR BOOK SHELF.

Precious Stones and Gems : their History, Sources, and Characteristics. By Edwin W. Streeter, F.R.G.S., M.A.I. Fifth Edition. (London : George Bell and Sons, 1892.)

BOOKS dealing with the fascinating subject of precious stones naturally fall into three classes—mineralogical treatises, archæological essays, and works adapted for experts and commercial men. Among the last class, the well-known work above cited, which has now reached a fifth edition, takes a prominent place. The enterprise and energy of the author in seeking out and developing new sources of ornamental stones is well known, and many of the facts contained in the present volume have been collected or verified by Mr. Streeter himself, by his sons, or by their agents. The chapters, which in earlier volumes were devoted to the description of famous diamonds, and to pearls and pearl-fishing, are now omitted, these subjects having been dealt with in separate books from the author's pen, the space thus obtained being devoted to an account of the Ruby Mines of Burma, the sources of sapphire in Siam and Montana, and those of the emerald in Egypt. In all these cases Mr. Streeter's agents have taken an active part in the work of exploring the districts, and he is able to furnish much information not hitherto available to the public. While the commercial aspects of these gem-stone localities naturally receive the greatest amount of attention, it is only fair to the author to point out that much care has evidently been exercised in order to prevent the creeping in of those errors on scientific points which too often disfigure works of this class. The author acknowledges in his preface the assistance which he has received from Mr. Rudler, the Curator of the Jermyn Street Museum, in dealing with scientific questions. The new edition, like its predecessors, is admirably got up and well illustrated.

Air and Water. By Vivian B. Lewes, F.I.C., F.C.S., Professor of Chemistry, Royal Naval College, Greenwich, &c. (London : Methuen and Co., 1892.)

THIS little book is one issued in connection with a series of University Extension manuals. The author may be congratulated upon the selection of his subject, which is one of those capable of being adequately treated in a course of a dozen lectures ; and he has been no less happy in his treatment of it, for by following the historical method he has succeeded in maintaining the interest of his readers, while he fairly covers the whole ground with which an elementary treatise on this topic may be expected to deal. The story of the researches of Galileo, Torricelli, and Pascal, of Priestley, Cavendish, and Lavoisier, is followed by an admirable *résumé* of the latest achievements of chemical science, and this in turn by a clear statement of the problems involved in the maintenance of proper supplies of fresh air and pure water. The warmest votaries of other branches of science will not quarrel with our author when, in his enthusiasm, he declares chemistry to be the "most beautiful of the sciences" ; possibly, however, some may demur to the statements in the following passage : "Although the amount of oxygen present in the air amounts to 1,233,010 billions of tons, still it is only one two-millionth of the total oxygen, and had not this small fraction been left over in the creation of the world, neither animal nor vegetable life could have existed." The author must hold very decided views as to how far down extends that condition of oxidation which is so constantly found at the earth's surface.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Ornithology of the Sandwich Isles.

It is easy to make assertions which, however improbable, it is not easy to disprove. I would therefore invite Mr. Albert F. Calvert to furnish documentary evidence of those he has advanced (*suprà*, p. 485). They do not indeed materially affect what I had said; yet, for the sake of accuracy, it might be as well to know on what foundation they rest. Those who are interested in the growth of ideas will be pleased to find that Sir Joseph Banks was so far in advance of his time as, on his return from his voyage with Captain Cook, which ended in 1771, to have "several cases of birds carefully mounted and arranged according to the localities in which they were collected"; and, among them, a "group of land birds from Owhyhee"—an island which Cook did not discover until 1778—or seven years later. As these assertions alone concern the subject on which I wrote, I refrain at present from offering any remarks on the others; but your correspondent seems to have been the victim of a delusion or something worse. Alfred Newton.

I observe in Nature (p. 485) a letter from Mr. Albert F. Calvert, in which he states that certain cases of birds, which were collected by Sir Joseph Banks during his voyage in the *Endeavour* with Captain Cook, "were in the custody of the Linnean Society of London until 1863, when they formed part of their natural history sale."

This is not the fact. The birds belonging to Sir Joseph Banks were never in the possession of this Society. It is true that the Society at one time possessed the insects and shells which formed part of the Banksian collections, and these in 1863 were presented, not sold, to the Trustees of the British Museum. Had there been any birds, they would doubtless also have been presented at the same time.

Where, then, did the cases, which Mr. Albert F. Calvert says are still "carefully preserved in the museum of his ancestor Mr. John Calvert," come from? Certainly not from the Linnean Society. In view of Prof. Newton's valuable communication to Nature (March 17, pp. 465–69), it is of importance that this inquiry should be answered, and a list furnished of the species contained in these cases, the reported existence of which will agreeably surprise ornithologists.

As Mr. A. F. Calvert has disclosed a source of information likely to be useful, perhaps he may be able to answer another question.

In 1860 the late Mr. J. D. Salmon, a well-known oologist, bequeathed to the Linnean Society a valuable collection of birds' eggs. This collection was known to contain the eggs of many British birds which were then becoming scarce, and have since become still rarer, if not quite extinct as breeding species; such, for example, as the golden eagle, osprey, kite, buzzard, honey buzzard, raven, chough, dotterel; and amongst some of the rarer species not found breeding in Great Britain, the spotted eagle, gerfalcon, black kite, rough-legged buzzard, Lapp owl, &c., and, above all, an egg of the great auk (*Alca impennis*), the value of which alone would almost equal that of all the other eggs in the collection.

I am informed that on the death of Mr. Salmon this collection was intrusted by his executors, for the purpose of being catalogued, to someone known to Mr. A. F. Calvert's ancestor; and by some accident, when it came to be handed over to the Linnean Society, the egg of the great auk was (some time afterwards) found to be missing, as also the eggs of certain species above mentioned, with several others that might be named.

Possibly they may have been removed at the time for safe custody, and were forgotten to be returned when the collection was deposited in its present resting-place.

Perhaps Mr. A. F. Calvert will say whether by chance these eggs (like the Banksian birds) have been "carefully preserved in the museum of his ancestor." If so, I presume that, on proof of the bequest of the collection to the Linnean Society, and proper identification of the eggs by their numbers, initials, or other marks, Mr. A. F. Calvert would be willing (on behalf of his ancestor) to restore them to the cabinet from which they have been so long missing. J. E. Harting.

Linnean Society, Burlington House, March 28.

Poincaré's Thermodynamics.

It is clear that I was justified in attributing the gist of M. Poincaré's first letter to his not having sufficiently read my notice of his book. He has not even yet fully apprehended the bearings of that notice, as a few words will show. Far from being unable to uphold any one of my critical remarks, as M. Poincaré is pleased to hint may be the case, I reassert every one of them, and could easily add to their number.

Let us begin with the particular item of my criticism which M. Poincaré persists in regarding as the most important. My words were:—"in his assumed capacity [of pure analyst] he quite naturally looks with indifference, if not with absolute contempt, on the work of the lowly experimenter." As an illustration of this I instanced M. Poincaré's ignoration of the thermo-electric researches of Sir W. Thomson, Magnus, &c. Then I quoted (in full) two of his remarks on the "Thomson effect." In the first of these he used the very peculiar phrase

"Sir W. Thomson *admet* qu'il existe une force &c.":—

and in the second he said

"si l'effet Thomson a pu être mis en évidence par l'expérience, on n'a pu jusqu'ici *constater l'existence* des forces electromotrices qui lui donnent naissance."

To these he has, in his recent letters, added other like statements. Now, as I understand the matter, Lord Kelvin *proved* (which, as I take it, means a good deal more than might be implied by "constater") the existence of the electromotive force which depends on the so-called "Thomson effect" (giving also thereby the means of measuring its amount) by showing that the Peltier electromotive force does not in general fully account for the observed current in a thermo-electric circuit, and may even be directly opposed to it; while no other source of electromotive force can exist save the gradation of temperature in one or both of the metals. He then proceeded, by experiment, to measure the amount of the "Thomson effect" for unit current in various metals, unequally heated. When the passages above quoted from M. Poincaré's work are compared with the facts just stated, my comments on them will be seen to be fully justified.

It is necessary to add that I made *no reference whatever* to M. Poincaré's distinctions between "true" and "apparent" electromotive force:—simply because I regard these, along with many other celebrated terms such as "disgregation" &c., as mere empty names employed to conceal our present ignorance.

As to the three chief objections I made to the work of M. Poincaré, every one (author, critic, or onlooker) is entitled to form and express his opinion. I need not restate mine, though I continue to adhere to every word of it:—but I may make the following additional remarks on these objections severally.

1. What sort of title to completeness can be claimed for a Treatise, on Thermodynamics, in which no mention is made of the grand principle of Dissipation of Energy; nor of Thermodynamic Motivity, "that possession the waste of which is called Dissipation"?

2. With regard to the measurement of Absolute Temperature, what I *did* say was that the experiments of Joule and Thomson, which justified them in basing it on Carnot's Function, were not mentioned by M. Poincaré *in this connection*. The omission by M. Poincaré of the italicized words makes an absolutely vital change in the meaning of my statement; and enables him to make what, (at first sight only), appears to be an answer to it.

3. As regards the foundation of the Second Law, it is unfortunately clear that M. Poincaré and I must continue to differ:—so that further discussion of this point would be unprofitable. For I presume that M. Poincaré has not formed his opinion without careful study of all that Clerk-Maxwell said on the point:—so that even a perusal of Lord Kelvin's latest paper (*Fortnightly Review*, March 1892) is not likely to induce him to change it. P. G. T.

26/3/92.

M. Poincaré and Maxwell.

In his recent treatise on "Électricité et Optique," M. Poincaré professes to give a description of Maxwell's theories of electromagnetic actions. M. Poincaré appears to think that Mossotti's theory is consistent with and differs but little from Maxwell's. On this Maxwell says (§62):—"The theory of direct action at a distance is mathematically identical with that of action by means of a medium . . . provided suitable hypotheses be introduced when any difficulty occurs. Thus Mossotti has deduced the mathematical theory of dielectrics from the ordinary theory of attraction." Maxwell anyway repudiated Mossotti's theory. M. Poincaré introduces a "fluide inducteur" as the name of a thing displaced in the dielectric, when what Maxwell calls electric displacement occurs. This is all very well. It is anyway not inconsistent with Maxwell, even though Maxwell says distinctly that he does not know what the change of structure is like which he calls electric displacement. It might be a bending or twisting or lots of things, but M. Poincaré is partially justified in fixing the idea thus. He calls this "fluide inducteur" elastic, though at the same time he calls it incompressible. It is not quite clear what "fluide" means here. M. Poincaré certainly observes that the elasticity of the "fluide inducteur" is quite different from that of material bodies, and in fact acknowledges that it is such as can hardly be fairly attributed to an incompressible fluid. Indeed, how can an incompressible fluid be elastic at all? There must be *something* besides the fluid; there must be some structure fixed in space which offers an elastic reaction to the fluid when driven past it, or else there must be the two liquids he objects to that are driven past one another. It is hardly a fair representation to talk of *an elastic incompressible fluid*, and then to invent difficulties, when the phenomena could not confessedly be represented by any such thing, but only by a fluid with some other mechanism superadded.

M. Poincaré's statement, "La méthode précédente n'est pas la seule que l'on puisse employer pour déduire de la théorie de Maxwell les lois de la distribution électrique," coupled with his further statement of "une autre méthode . . . sans supposer l'existence de ce fluide," seems at variance with his implication that this elastic incompressible fluid is part of or involved in Maxwell's theory.

This leads to the question of how far Mossotti's theory can fairly be considered as a substitution for or as a development of Maxwell's. It does not in any real sense get over action at a distance. There are the horrid old electrical charges acting upon one another across a space full of some non-conducting medium. This is practically no advance as far as a theory of electrical action is concerned. It is an advance no doubt as far as the behaviour of the medium is concerned, inasmuch as it enables a time propagation through space to occur; but as a theory of electric action it is a distinctly retrograde step on Maxwell's scientific position that he did not know what was the structure of the ether.

M. Poincaré proceeds to criticize Faraday's theory of the stresses in the dielectric, which he attributes to Maxwell. He begins by suggesting that the forces should have been explicable by the elasticity of the inductive fluid, in the same way as mechanical forces are due to the elasticity of matter. He has in this quite forgotten that what he calls the elasticity of this fluid, is not a bit like the elasticity of any matter, and would require either a second fluid, which he rejects, or some structure other than the fluid, to explain its properties. Granting such an additional structure, then the elastic energy of the medium, fluid and structure combined, *does* exactly explain the motions of conductors. Nobody has explained exactly *how* conductors differ from non-conducting space in structure, and can or do move, and this is not a bit clearer on Mossotti's hypothesis than on any other, not even when the non-conducting diaphragms are made infinitely thin. Maxwell long ago pointed out that no linear system of stress could leave a medium in equilibrium and move bodies immersed in it; and yet M. Poincaré criticizes Faraday's system because it is not linear; and this after remarking himself that the elasticity postulated already was not a bit like that of matter. All that is necessary is some assumption as to the connection between the conducting matter and the dielectric, for the "fluide inducteur" by hypothesis has elastic properties that make it the seat of the right amount of potential energy; and all that can possibly be necessary is to connect the matter with it in such a way that the energy of the medium lost when the conductor moves is given up to the conductor. M. Poincaré has again omitted to remember that the peculiar elasticity of the "fluide inducteur" necessitates some structure with which it is connected, and the Faraday stress may be in this structure, and due to its connection with the "fluide inducteur," and not at all due to another fluid with peculiar properties. If the stresses are due to the connections of the "fluide inducteur" there is no great difficulty in supposing them proportional to the squares of the displacements of the "fluide inducteur," just as the increased tension of a stretched horizontal string due to a small weight at its centre is proportional to the square of this weight. In fact, a suggested model working upon this sort of principle has been published as illustrating this very point, and Dr. Lodge's model ethers, in the first part of his "Modern Views," are all of this kind.

M. Poincaré proceeds to find "une difficulté plus grave." He creates this by assuming that the energy of the medium is all due to the work done by these mechanical stresses deforming it. This is a most gratuitous assumption. Take the case of the stretched string with the weight on it. The increased energy of the system is not due only to the work done by the *increased* tension. At last he confesses, however, that if the energy in the dielectric be kinetic and not potential these difficulties would disappear. "Mais on ne peut encore adopter cette interprétation de la pensée de Maxwell sans se heurter à de grandes difficultés." And why? Merely because Maxwell afterwards calls the electric energy potential while he calls the magnetic energy kinetic. Has M. Poincaré forgotten that potential energy may in any case be the kinetic energy of an associated system? or can he not imagine two modes of motion of the same medium? Anyway, if the potential energy may be the kinetic energy of an associated system, and if M. Poincaré's difficulties are inapplicable to a kinetic explanation of the phenomena, it seems impossible but that they are really inapplicable to a potential system if this system be judiciously devised. It is just here that M. Poincaré fails. He revels in elastic fluids, and yet he continually harps upon the same difficulty—namely, "How can an incompressible liquid be elastic at all?"—and instead of once for all solving this by acknowledging that there must be some structure, he reverts to it as if it were a new difficulty whenever he comes across its consequences.

As a mere mathematical work the book is admirable and clear, if a little prolix. GEO. FRAS. FITZGERALD.

Trinity College, Dublin, March 24.

Prof. Burnside's Paper on the Partition of Energy, R.S.E., July 1887.

IN his criticism on a paper of mine on the partition of energy in a set of non-homogeneous spheres (NATURE, March 31, p. 512), Mr. Watson says that the conclusions are vitiated owing to my having omitted to introduce the frequency factor of collisions before proceeding to take the averages This is not exactly accurate, since *a* frequency factor is introduced, viz. the relative speed of the centres of inertia of the impinging spheres parallel to the line of impact.

In the spring of 1888 Prof. Boltzmann published a criticism of the same paper in the *Sitzungsberichte* of the Vienna Academy, in which he contended that the correct frequency factor should be the relative speed of the points of impact of the spheres parallel to the line of impact; and in which he showed that the result of averaging with this frequency factor is to make the mean rotational energy equal to the mean energy of translation. Had I been entirely satisfied at the time of the cogency of Prof. Boltzmann's reasoning, I should, of course, have published a short note calling attention to the correction he proposed to make; and I regret now that this was not done, as it would have prevented the waste of a certain amount of valuable time and trouble. W. BURNSIDE.

Royal Naval College, Greenwich, April 1.

DR. WATSON has shown in his letter to NATURE of March 31 (p. 512) how the general methods of Maxwell and Boltzmann may be applied to the particular problem discussed by Prof. Burnside. He has also pointed out an error in Burnside's reasoning—namely, the non-introduction of the factor $u - U + c\varpi$, whereby Burnside's conclusions at variance with the Maxwell-Boltzmann law of partition of energy are vitiated.

You may, perhaps, allow me space to point out a little more precisely in what, as appears to me, the error consists. Burnside has to find the average value of the expression—

$$(u - U + c\varpi)\{2\varpi - c(K + k)(u - U)\}$$

(see p. 503). Now, we may take averages in two ways:

(1) as above noticed for all collisions by introducing the factor $u - U + c\varpi$ to denote frequency of collision; or (2) for all spheres as they exist at a given instant. Now, Prof. Burnside has calculated the average rotation energy by method (2), which gives $2A\overline{\omega^2} = \frac{1}{k}$. But when he comes to the translation energy, he takes the result $\overline{(u - U)^2} = \frac{2}{h}$ from Prof. Tait, not observing, I think, that that result is given by Tait as the average for all collisions per unit time. And then, equating two inconsistent things, he gets the conclusion $\frac{1}{k} = \frac{2}{h}$, or the mean energy of rotation is twice that of translation.

To be consistent, he should have given the mean of $(u - U)^2$ for all pairs of spheres—that is, $\overline{u^2} + \overline{U^2} = \frac{1}{h}$. And so his result should have been $\frac{1}{k} = \frac{1}{h}$, agreeing with Maxwell.

It may be interesting to see what would have been the result of introducing the factor $u - U + c\varpi$ to denote frequency of collision. The expression whose average is required would then be—

$$(u - U + c\varpi)^2\{2\varpi - c(K + k)(u - U)\}.$$

As the frequency factor $u - U + c\varpi$ must be always positive, we must integrate between the limits $U = +\infty$ to $U = -\infty$, and $u = +\infty$ to $u = U - c\varpi$. After integration, we reject odd powers of ϖ.

I have worked it out to the first power of c, rejecting c^2, &c. We have, in that case, to evaluate—

$$\int_{-\infty}^{\infty}\int_{U-c\varpi}^{\infty}\{2\varpi(u-U)^2+4c\varpi^2(u-U) - c(K+k)(u-U)^3\}\epsilon^{-hu^2}\epsilon^{-hU^2}\,du\,dU.$$

The first term gives zero. The second term gives $\frac{4c\varpi^2}{h}$, and the third term gives $\frac{2c(K+k)}{h^2}$. From which, on substitution, and integrating for ω, Ω, &c., from ∞ to $-\infty$, we easily obtain $k_1 = k_2 = \&c. = h$.

To extend the process to cases in which c^2, &c., cannot be neglected, would be difficult. But I think the *onus probandi* now lies on the other side.

S. H. Burbury.

Double Orange.

On a blood orange being cut open by my little daughter yesterday, a small orange was found inside, which, although no larger than a hazel-nut, was yet perfect in form and colour. It showed no point of difference, other than that of size, as compared with the parent orange, and there was nothing in the appearance of the uncut fruit suggestive of the miniature of itself carried within. My sole right to write upon this subject is one you have always recognized in your journal, viz. that to record an interesting fact.

Gerald B. Francis.

Katrine, Surbiton.

METALS AT HIGH TEMPERATURES.[1]

I PROPOSE this evening to consider, first, the methods of measuring high temperatures, and, second, to describe certain effects they produce on metals.

Geber, writing in the eighth century, gives directions for obtaining high temperatures, but points to the difficulties that arise in practice, "because fire is not a thing which can be measured, '*sed quoniam non est res ignis, quæ mensurari possit.*'"[2] It is not sufficient to attain temperatures that are known to be high; it is necessary, for the purpose of modern investigation, to measure them with accuracy; and few of the early chemists in this country did more in affording a basis for the study of metals at high temperatures than Robert Boyle, the application of whose well-known law to solutions of metals in each other has been made evident by recent work. The 30th of December last was the third centenary of his death; it is well, therefore, that this lecture should begin with a tribute to his memory. He suggested improvements in the ordinary mercurial thermometer,[1] constructed what would appear to be the first air thermometer with an index; and although he did not do much for thermometry at high temperatures, he appears to have been struck by what must have been a quaint device for regulating high temperatures, for he points out that "the great mechanic, Cornelius Drebel[2] made an automatous musical instrument and a furnace which he could regulate to any degree of heat by means of the same instrument." He indicates various degrees of intensity of heat by reference to the colour of a glowing mass of fuel, and says that,[3] "tho' we vulgarly say in English, 'a thing is red hot,' to express a superlative degree of heat, yet, at the forges and furnaces of artificers, by a white heat they understand a further degree of ignition than by a red one." It is not a little strange that for three centuries after his death the same vague expressions have constantly been used in describing high temperatures.

A great step in advance was made in 1701 by Sir Isaac Newton,[4] who applied the law of cooling to the measurement of temperatures beyond the range of the mercurial thermometer, and in the notes which accompany his "Scala graduum caloris" he showed that he knew that the freezing-point of lead differs slightly from its melting-point.

Eighty years later, Josiah Wedgewood (1782),[5] aided by one of my predecessors, Mr. Alchorne, Assay Master of the Mint, determined a few melting-points of metals, and, in communicating a description of his "thermometer for measuring the higher degrees of heat" to the Royal Society, we find him, one thousand years after Geber had said that "fire cannot be measured," still lamenting the want of suitable instruments, saying: "How much it is to be wished that the authors [to whom he refers] had been able to convey to us a measure of the heat made use of in their valuable processes; . . . a red heat, a bright red, and a white heat are," Wedgewood adds, "indeterminate expressions, and even though the three stages are sufficiently distinct from each other, they are of too great latitude, and pass into each other by numerous gradations which can neither be expressed in words nor discriminated by the eye." Another ninety years brings us to the last time that the measurements of high temperatures formed the subject of a Friday evening discourse in this Institution. On March 1, 1872, the late Sir William Siemens addressed you on the measurement of "heat by electricity";[6] and, speaking of the mercurial thermometer, said: "When we ascend the scale of intensity we soon approach a point at which mercury boils, and from that point upwards we are left without a reliable guide, and the result is that we find, in scientific books on chemical processes, statements to the effect that such and such a reaction takes place at a 'dull red, such another at a 'bright red,' or a 'cherry red,' or a 'white heat'—expressions which remind one," he adds, "of the days of alchemy rather than of chemical science at the present day."

1 A Lecture delivered at the Royal Institution by Prof. W. C. Roberts-Austen, C.B., F.R.S., on Friday evening, February 5, 1892.

2 From the edition of his "Summa Perfectionis Magisterii," p. 28, published in Venice, 1542.

1 Boyle's Works, Shaw's edition, vol. i. p. 575, 1738.

2 Cornelius van Drebel, 1572-1634, *loc. cit.* vol. iii. p. 38, 1738.

3 *Loc. cit.*, vol. ii p. 28.

4 Phil. Trans. Roy. Soc., vol. xxii. p. 824.

5 *Ibid.* vol. lxxii. p. 305.

6 Roy. Inst. Proc., vol. vi. p. 438, 1872.

It is not a little singular that the same lament should have been uttered, with so long an interval between, by two prominent technical men, and it suggests that but little experimental work had been done in the meantime with a view to the measurement of high temperatures. This is, however, far from being the case. A vast amount of work was done by physicists and metallurgists whose chief masters were "indefatigable labour, the closest inspection, and hands that were not afraid of the blackness of charcoal"; and their more noteworthy efforts were based on the employment of the air thermometer, in which the expansion of air replaces the expansion of the mercury in the ordinary thermometer, the bulb being of some fire-resisting material.[1] For this purpose, Princep (1827) used a bulb of gold, Pouillet (1836) one of platinum, and finally, Deville and Troost, in a truly splendid series of investigations, adopted bulbs of porcelain, with iodine vapour as the elastic fluid. They ultimately reverted to the use of air.

You will remember that old mercurial thermometers had much information, supposed to be useful, engraven on their scales, and such statements as "water freezes," "water boils," "blood heat," "fever heat," "summer heat," were considered indispensable. It is by exposure to known temperatures that a thermoscope can be converted into a pyrometer for measuring intense heat; and the air or gas thermometer has, in the hands of Deville and Troost, rendered excellent service by enabling such gradations to be effected. The gas thermometer is not, in itself, a handy appliance, for it requires much subsidiary apparatus, and elaborate corrections of various kinds have to be introduced into the numerical data it affords; but it has given many fixed temperatures—such as melting-points and boiling-points of elements, and of compounds—which may safely be made use of in graduating pyrometers. For very high temperatures, 900° C. and over, we rely on the excellent work of M. Violle[2] on the specific heats of platinum, silver, gold, palladium, and iridium, which have enabled the melting-points of the respective metals to be calculated.

The determinations of temperatures between 300° and 1000°, which are now generally accepted, also rest upon data accumulated by the aid of the air thermometer, which has thus enabled the graduation to be effected of instruments widely differing from it, that can be trusted to give rapid and accurate indications in daily use. I can only bring before you two of the many kinds which have been devised; they are, however, by far the best that are available, and for the determination of temperatures up to the melting-point of platinum, leave little to be desired—

(1) A pyrometer which depends on the increase in the resistance of a heated conductor through which a divided electrical current is passing; and

(2) One in which the strength of an electric current, generated by the heating of a thermo-junction, is used as a measure of the heat applied to the thermo-junction.

The principle of the electrical resistance pyrometer was indicated by Sir William Siemens ("Collected Papers," vol. ii. "Electricity," p. 84, 1889) in a letter addressed to Dr. Tyndall, dated December 1860, and the nature of the instrument may be made clear by the accompanying diagram, Fig. 1. A divided current passes from the battery B, to a platinum wire, C, coiled round a clay cylinder, and to a resistance coil, R. At the ordinary temperature the resistance of the platinum coil is balanced by the standard resistance R. If, however, the platinum coil be heated, its resistance will be increased, and, this increase of resistance, which can be measured in various ways, indicates the temperature of the coil C. The coil itself may be adequately protected and exposed to temperatures which have been determined by the air thermometer; the deflection of a suitable (differential) galvanometer, G, will then indicate temperatures directly. For instance, the temperature at which zinc boils has been accurately fixed at 940° C., and if the coil is heated in the vapour of boiling zinc, the angle through which the galvanometer mirror is deflected marks the temperature of 940° C.

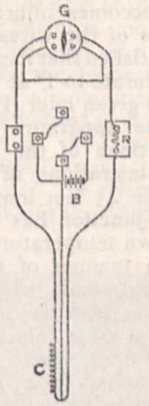

FIG. 1.

The Report of a British Association Committee showed in 1874 that the instrument is liable to changes of zero, but Mr. H. L. Callendar has recently (1887) restored confidence in the method which had been shaken by the Committee. He has proved that if sufficiently pure platinum wire be used, and if the wire be carefully annealed and protected from strain and contamination,[1] resistance pyrometers may be made practically free from changes of zero even when used at temperatures as high as 1000° C. He attributes the changes of zero to which the Siemens pyrometers are liable to the action on the wire of the clay cylinder on which it is wound, and of the iron tube in which it is inclosed. As the result of his experiments he has introduced certain modifications, which render the instrument not only trustworthy but very sensitive. He winds the platinum wire on a thin plate of mica, and incloses it in a doubly glazed tube of hard porcelain. He uses the zero method of measuring the resistance; but for these and other details of manipulation his own very interesting papers must be consulted. I will only add that I have had the pleasure of working with him in the Mint Laboratory, and I am satisfied that at temperatures about 1000° the *comparative* results afforded by his method are accurate to the tenth of a degree, a result which would certainly have been deemed impossible a year or two ago.[2]

The necessity for working with small volumes of fused metals, into which the tube of Callendar's pyrometer could not be plunged, has led me to prefer to adopt a method that would be classified under the second heading I have given. A very small thermo-junction may, in fact, be employed in such cases. The use of thermo-junctions for measuring high temperatures appears to have been sug-

[1] See the excellent bibliography given by C. Barus, Bull. Geological Survey, U.S.A., No. 54, 1889.

[2] *Comptes rendus*, vol. lxxxix. p. 702, 1879; vol. xcii. p. 866, 1881.

[1] Phil. Trans. Roy. Soc. vol. 178 (1887). A, pp. 161-230, and vol. 182 (1891), A, pp. 119-157; *Phil. Mag.* vol. xxxii. July 1891, p. 104, and vol. xxxiii. Feb. 1892, p. 220.

[2] As this statement has been received with some surprise, it may be as well to state briefly how this degree of accuracy and sensitiveness is attained. The resistance-box is compensated for changes of temperature, and changes of resistance in the wires leading to the pyrometer are automatically eliminated. The resistance itself is measured by a modification of the well-known Carey-Foster method. The balancing resistance of the Wheatstone bridge employed, is composed partly of resistance coils and partly of a bridge-wire along which a contact key slides. The resistance of a centimetre of this wire is made to correspond to the increase of resistance of the pyrometer produced by a rise of 1° C. The galvanometer can easily be made sensitive to one-hundreth of a centimetre of this bridge-wire, so that one-tenth of a centimetre, which corresponds to one-tenth of a degree, can, of course, be measured with certainty.

gested in 1826 by Becquerel, and adopted by Pouillet in 1836,[1] who advocates the use of iron in conjunction with platinum; but of all the varied combinations of metals and alloys which have been tried from time to time, that proposedby H. Le Chatelier possesses many advantages, on which I have elsewhere dwelt.[2] It consists of a platinum wire twisted at its end with a wire of platinum alloyed with 10 per cent. of rhodium. Such a couple may be used for some time without change of zero, and if the junction becomes injured it may be cut off, and the severed ends of the wires may be twisted together again. I am satisfied that it can afford comparative results which are accurate to 1° at temperatures of over 1000°. The diagrams given later (Figs. 4, 5, and 6) show the disposition of the apparatus. The spot of light indicating the deflections of the galvanometer needle is caused, for the illustrations of this lecture, to fall onto a graduated scale 45 feet long on the wall of the theatre. The thermo-junction has been calibrated with the aid of certain known temperatures, and the long scale is inscribed after the manner of the old thermometer scales, with certain fixed points, which are, of course, far higher than those it was possible to indicate by the expansion of mercury in a glass tube. [These fixed points were: "water boils" (100°), "lead melts" (326°), "zinc boils" (940°), "gold melts" (1045°), "palladium melts" (1500°), "platinum melts" (1775°). On heating the thermo-junction to bright redness in a Bunsen flame, the spot of light moved rapidly to the point marked "zinc boils."] For laboratory experiments the scale is a short transparent one, rigidly fixed in relation to the galvanometer.

In leading up to the experiments which follow, in the course of which metals will be exposed to high temperatures, I would remind you that if an ordinary thermometer be plunged into water which is gradually losing its heat to a cold environment, the mercury will fall until the water begins to freeze, but directly this happens the mercury remains stationary until all the water is frozen; so that if the rate of fall be measured with a chronograph, there will be a steady fall to the freezing-point of water, then a long arrest, followed by a renewed fall. If these readings be plotted, a well-known time-temperature curve will be obtained. Exactly the same effect is produced when a fluid metal "freezes," and before proceeding further it may be well to determine experimentally the freezing-point of gold. Beneath this little mass of pure gold, A (Fig. 2), a thermo-junction, B, is protected by a very thin

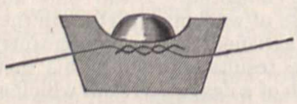

FIG. 2.

layer of clay from the metal. The oxyhydrogen flame is made to play on the gold, there is a rapid movement of the spot of light over almost 25 feet of the scale, there is a diminution in the rate of rise near the point marked 1045°, the melting-point of gold, and then, when the metal becomes fluid, the temperature rapidly rises as more heat is given to the little mass. The source of heat is now removed, the temperature falls, there is an arrest just at 1045° C., the freezing-point of gold, and then the spot of light resumes its course as the gold cools down to the temperature of the room. The melting-point and freezing-point of palladium, 1500° C., were then shown in the same way. It should be observed, however, that when a small fragment of palladium is fused in the naked flame of the oxyhydrogen blow-pipe, hydrogen appears to be absorbed by the metal; and this absorption of gas lowers the freezing-point materially, and makes it far less steady than when a fresh piece of metal, cut from a large mass, is fused for the first time.

When the spot of light is allowed to fall on a sensitized plate in a suitable camera,[1] the time-temperature curve traced on a moving plate will be of the form shown in Fig. 3.

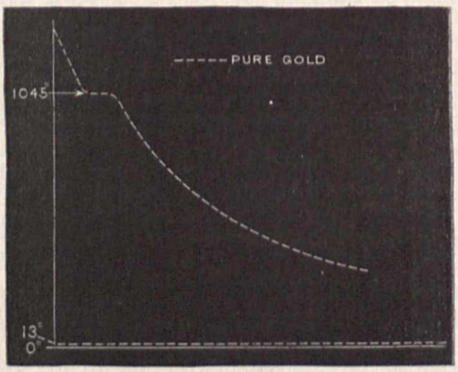

FIG. 3.

It may be useful to show the method by which these autographic curves are obtained: the following diagram, Fig. 4, is therefore added.

The arrangement consists in inclosing a galvanometer of the Deprez and d'Arsonval type in a large camera; a fixed mirror, F, being placed below the movable mirror, M, of the galvanometer, so that the light, from the lime cylinder L, reflected in the mirror H, passes to both mirrors, F and M, and is reflected in the direction of a fine horizontal slit, A B, behind which a sensitized photographic plate, C, is drawn vertically past the slit, by means of gearing, D, driven by clockwork. The ray from the fixed mirror is interrupted periodically by the vane E, and a beaded datum line is given, which enables any irregularity in the advance of the plate to be detected.

The amount of divergence from its datum line of the spot of light reflected by the movable mirror at any given moment bears a relation (which can readily be found by calibration) to the temperature to which the thermo-junction X is heated, and the variations of temperature are recorded by a curve which is the resultant of the upward movement of the plate and the horizontal movement of the spot of light. A crucible, *c*, which may be filled with molten metal, is provided with a tubulure, T, for the insertion of the thermo-junction. The crucible is suspended by wires in a double jacket of tin plate, *a b*.

It will have been evident that the thermo-junction of platinum and platinum-rhodium could not be used for measuring temperatures higher than the melting-point of the platinum of which it is made. Metals with higher fusion-points than platinum are, however, available; thus iridium will only just melt in the flame produced by the combustion of pure and dry hydrogen and oxygen. By the kindness of Mr. Edward Matthey, a thin rod of iridium has been prepared with much labour, and it can be used as a thermo-junction with a similar rod of iridium alloyed with 10 per cent. of platinum. The junction may be readily melted in the electric arc, and by this means a temperature may be registered which careful laboratory experiments show to be close to 2000°, and this agrees with the estimate of the melting-point of iridium which Violle[2] deduced from calorimetric experiments. [This experiment was shown, a different scale being employed for the screen, as the thermo-electric constants of the iridium, and iridium-platinum couple, are different from those of the platinum and rhodium one previously used.]

It is interesting to remember that within a year in this Institution temperatures ranging from − 200° to + 2000°

[1] *Comptes rendus*, vol. iii. p. 782, 1836
[2] British Association Lecture, NATURE, vol. xli., 1889, pp. 11–32; Report Inst. Mech. Eng., Oct. 1891, p. 543.

[1] Proc. Roy. Soc., vol. xlix., 1891, p. 347.
[2] *Loc. cit.*

have been mapped out, the lower temperature by Prof. Dewar in his memorable Faraday Lecture: the higher point is now measured in public for the first time.

How difficult it is for us to realize what this range of temperature really means, for we have but little power of appreciating temperatures beyond those we can conveniently bear. We, perhaps, know the meaning of extreme cold better than great heat, but even the vivid imagery of Dante, who might have been expected to afford some guidance, gives us singularly little help. I think in depicting the terror of torture inflicted by extreme cold he succeeds better than when he describes the suffering of those who are exposed to flames. His words (Canto xxxiii.)—

"Blue, pinched, and shrined in ice the spirits stood"—

mark the highest suffering drawn in the "Inferno." It is, however, probable that my failure to appreciate the descriptive powers of Dante may be the result of resentment, for I read with regret that he consigns to the tenth chasm of Hell, not only the coiner who

"falsified
The metal with the Baptist's form impressed,"[1]

but also an honest metallurgist, Cappoccio of Sienna, who,

"by the power
Of alchemy, . . . aped creative Nature by his subtle art";

and, I think, deserved a better fate.

We are now in a position to consider certain other effects of high temperature on metals. Many years ago, my colleague Mr. Lockyer, and I, conducted an investigation on the spectra of the vapours of certain metals[2] at the highest temperatures we could produce, with the aid of the oxyhydrogen flame. We distilled silver, zinc, cadmium, and volatilized iron and other metals, from a lime crucible, and caused their vapours to pass into a horizontal tube of strongly-heated lime. By these experiments we satisfied ourselves that the molecular structure of metals is gradually simplified as higher temperatures are employed; and we came to the conclusion that each molecular simplification is marked by a distinctive spectrum, and that there is also an intimate connection between the facility with which the final stage is reached, the group to which the element belongs, and the place which it occupies in the solar atmosphere. At the highest temperature of the oxyhydrogen flame, molecules of metals are simplified, but their constituent *atoms* remain unchanged. Mr. Lockyer has, however, since done far more: he has shown that the intense heat of the sun carries the process of molecular simplification much further; and, if we compare the complicated spectra of the vapours of metals produced by the highest temperatures available here with the very simple spectra of the same metals as they exist in the hottest part of the sun's atmosphere, it is difficult to resist the conclusion that the *atom* of the chemist has itself been changed. My own belief is that these "atoms" are changed, and that iron, as it exists in the sun, is not the vapour of iron as we know it upon earth. We will not dwell in this lecture on the effects of very high temperatures on metals, but rather on the influence of comparatively low temperatures—that is, below whiteness—in changing the number and arrangement of the atoms in metallic molecules. A profound change must occur when the viscous form of sulphur passes spontaneously at the ordinary temperature into the yellow crystalline variety, but the change is accompanied by but little thermal disturbance. In the case of metals there is also abundant evidence that molecular change may take place at low temperatures. Take the fusible alloy of bismuth, lead, and tin, which bears Newton's name, and contains—

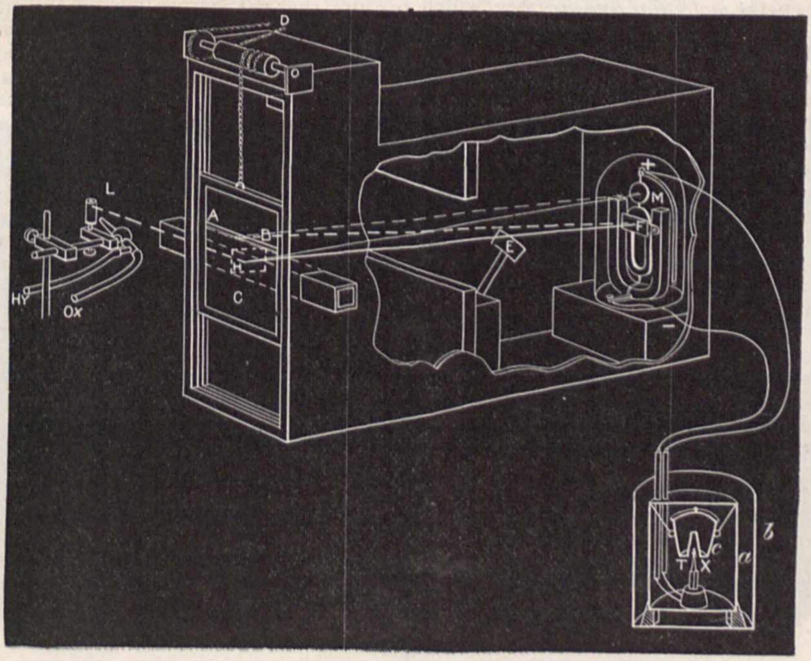

FIG. 4.

Bismuth	50·00
Lead	31·25
Tin	18·75
	100·00

It fuses at 90°; it may be cast round a thermo-junction, and plunged in water and cooled thoroughly until the observer is certain that the mass has returned to the atmospheric temperature: take it out of the water, dry it

[1] The golden florin of Florence.
[2] Proc. Roy. Soc., vol. xxiii. p. 344, 1875.

rapidly, and in a few moments it will become too hot to hold. The "fracture" of the metal is totally different before and after the molecular change, which is the cause of this evolution of heat, has taken place. The change, moreover, takes place in the solid metal, and is not due to the release of the latent heat of fusion. The mass, solid as it appears to be, must be the scene of an internal struggle between the molecules in the effort to attain a state of equilibrium, and this conflict is but a type of the action that takes place in many metals and alloys which are of vast industrial importance.

Time will only permit me to deal with three cases of the action of high temperatures on atoms and molecules of metals. In the first case, the *arrangement* of the atoms in the molecule of a metal, iron, is disturbed, and the result is of great industrial importance. In the second case, the atoms of a metal, gold, appear to *combine* with those of another metal; and the result, while it is mainly of interest in connection with the history of science, has nevertheless an important bearing upon art. The third case relates to the *molecular bombardment* which takes place when a small quantity of metal is dissolved in a mass of a metallic solvent, and is of interest in connection with modern views both as to osmotic pressure and solution generally.

(1) The pyrometric couple is inserted in the centre of a little mass of steel, which is being slowly raised to a bright red heat; when the flame is withdrawn, the spot of light will return towards the zero end of the scale, falling slowly until a temperature of 655° is reached, and then there will be an abrupt and prolonged arrest. The metal has never been near its melting-point, and the evolution of heat must be due to a molecular change in the solid metal. In the case of this particular sample of steel, the evolution of heat is mainly the result of a change in the relation between the carbon and the iron; but by laboratory experiments and careful chronographic records, Osmond has shown that, in the case of certain varieties of steel, it can be demonstrated that what here appears as a single change, attended by an evolution of heat, is really an exceedingly complex one. I have shown that it occurs in the purest iron the chemists can prepare by electrolysis, and I agree with Osmond in believing that the change which occurs in pure iron at 855° is a molecular one, independent of the presence of impurity If the mass of steel (Fig. 5, *a*) be heated again and allowed to

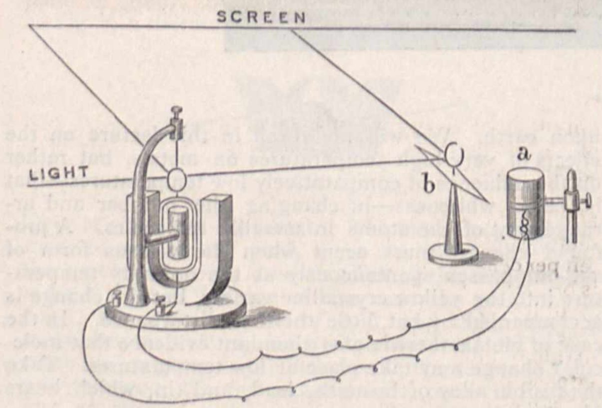

FIG. 5.

cool, you will observe that the point of "recalescence" appears to be that at which the iron regains its magnetic property;[1] for a magnetized needle, *b*, is attracted at the moment the arrest of the spot of light on the pyrometer scale marks the temperature at which the change occurs, and at that precise moment a second spot of light from a mirror, mounted on the magnetic needle, will rapidly move away from its zero. I have elsewhere[1] dwelt on the importance of the molecular change in iron and steel, and can now only summarize the significant facts.

It is unnecessary to point to the extreme industrial importance of the property steel possesses, of becoming hard when it is quenched from redness in a fluid which will abstract its heat with more or less rapidity.

The changes which take place at 855° and 650° have to be arrested, as it were, by rapidly cooling the mass of steel; and if this is done, the steel will be more or less hard according to the rapidity with which the progress of the molecular change has been stopped. It is, however, useless to attempt to harden steel if the temperature of the mass has fallen below 650°. In "oil hardening" or cooling a large mass of steel, like the "A" tube of a gun, which may be 30 feet long, great care should be taken to insure that the temperature of the mass is as uniform as possible; for, if part of the mass is hotter than 650°, while part is colder, the oil will really be cooling a mass of steel which is itself passing through various stages of complex molecular change, and the operation of "hardening" arrests, as it were, the atoms in the midst of a conflict incidental to their attempt to group themselves into one or other of the molecular modifications of iron. By cooling a mass of steel which is not at uniform temperature, stresses of great complexity and intensity are set up, stresses that may greatly reduce the effective strength of the gun.[2] The result is told in failures, by which many lives have been sacrificed; but I need hardly say that the Director-General of Ordnance is fully sensible of the national importance of studying the behaviour of iron and steel at high temperatures, and, at Dr. Anderson's suggestion, the Institution of Mechanical Engineers appointed a Committee, and have intrusted me with a large portion of the inquiry.

In the next experiment, Fig. 6, a bar (*a*) of steel, ½ inch

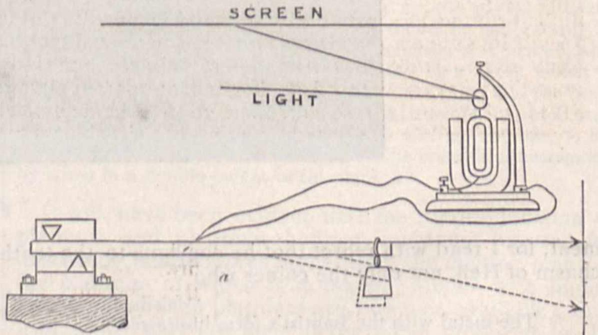

FIG. 6.

in section and 18 inches long, was heated to bright redness and fixed firmly at one end; a weight of about 2 pounds is rapidly hung to the free end, a light pointer is added to magnify the motion of the bar, and the thermo-junction is rapidly introduced into a small hole drilled in what is arranged to be the hottest part of the bar. The bar is not softest at a red heat; it remains perfectly rigid until it has cooled down to dull redness, and the temperature, as measured by the spot of light from the galvanometer, shows that "recalescence" has occurred. At that mo-

[1] The temperature at which these molecular changes take place in iron and steel was first demonstrated to an audience in my Newcastle lecture, 1889; but my friend Prof. Reinold, of the Royal Naval College, first arranged an experiment for lecture purposes which showed the magnetic change simultaneously with the thermal one.

[1] Report to the Institution of Mechanical Engineers, Proceedings, 1891, p. 543.

[2] "Internal Stresses in Cast Iron and Steel," by Nicholas Kalakoutsky, 1888.

ment of molecular weakness in the bar, the weight has power to bend it, and the pointer falls. By such experiments the exact temperature at which the metal becomes weak, in different varieties of steel, can readily be determined.

(2) Evidence will now be given in support of the second case it was proposed to treat, and it will be shown that at high temperatures the atoms of metals may truly combine with each other; in fact, taking gold as a basis for the experiments, compounds may be formed which would, had they been known centuries ago, have strangely affected the history of science. When the alchemists subjected the metals to high temperatures, their efforts were mainly directed to the discovery of some substance that would either change base metals to the colour of gold, or would give them the brilliancy of silver. The mediæval chemists believed that there were two distinct substances that would effect this, "one for the white" and another "for the red." Many of their writings might be quoted in support of this view, but a reference to Geber, who wrote in the eighth century, will be sufficient. He pointed out that the transmuting agent "has a tincture of itself so clear and splendid, white or red, clean and incombustible, stable and fixed, that fire cannot prevail against it; . . . and a property of the medicine is to give a splendid colour, white or intensely citrine," to metals to which it is added.

That was the effect expected from the transmuting agent, but do not think that the attempt to produce gold arose entirely from the love of gain. The colour of gold and purple impressed men strangely, and the search for the transmuting agent was most eagerly pursued in times when people lived for art, in a dream of colour. The effort to find the secret of the tint of gold is due to the same impulse which made the French in the thirteenth century manifest a keen "sensitiveness to luminous splendour and intensity of hue," so that, as Sir Frederic Leighton tells us, "a stained glass window, by Cousin, was limpid with hues of amethyst, sapphire, and topaz, and fair as a May morning." The chemists were able to stain glass ruby and purple with gold: why should they not impart the same glories to metals? I could not hope to interest you in what follows, did I not call artists to my aid; and many will remember the glowing words Mr. Ruskin uses,[1] calling purple a "liquid prism and stream of opal," reminding us of the crimson and purple of the poppy, the scarlet and orange of fire and the dawn. No wonder he chides us with turning the lamp of Athena into the safety-lamp of the miner, and with getting our purple from coal instead of, as of old, from the murex of the sea; "and thus grotesquely," he says, "we have had forced on us the doubt that held the old world between blackness and fire, and have completed the shadow and the fear of it by giving to a degraded form of modern purple a name from battle—'Magenta.'"

You will remember that Faraday showed that gold, when finely divided, is brilliantly coloured scarlet and purple. Here is a solution of chloride of gold. Add a little dissolved phosphorus, and the gold is precipated in an extremely fine state of division, which tinges the solution crimson, but if you try to remove this suspended gold you will only gain a brownish mud. However, I will give you the secret by which anyone who possesses a blow-pipe, a bead of gold, and a fragment of one of the most widely-diffused metals, aluminium, may stain gold purple through and through. But if you add aluminium to molten gold, you obtain many things, as this coloured diagram and series of specimens show. [This diagram cannot be reproduced without colour.]

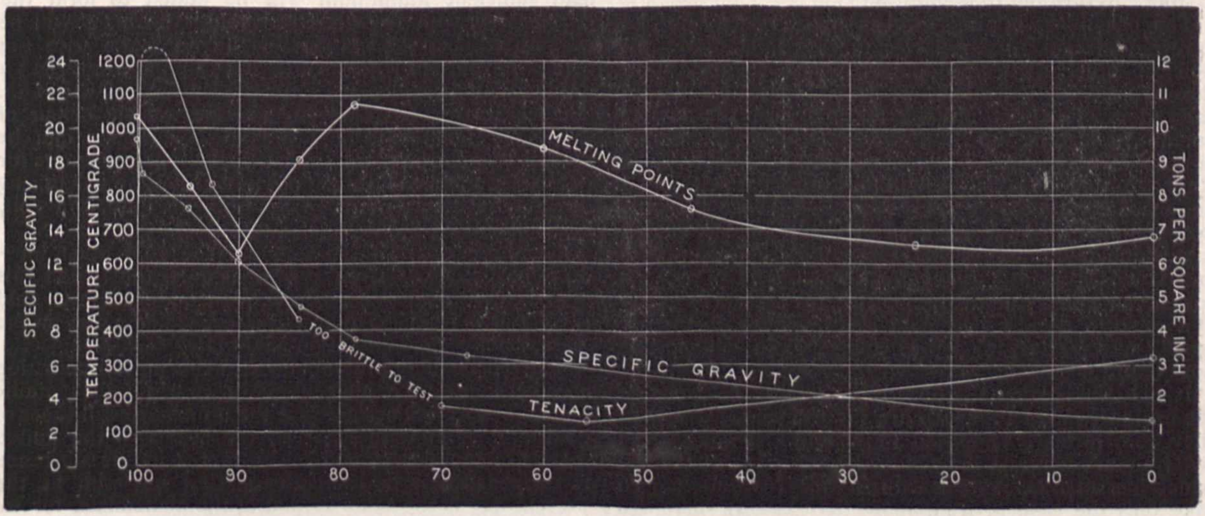

FIG. 7.

The series of specimens showed that as the proportion of aluminium is increased, the golden colour of the precious metal is lessened, and when an alloy is formed with about ten per cent of aluminium, the fractured surface of the mass is brilliantly white: from this point forwards, as aluminium is added, the tint deepens, until flecks of pink appear, and when seventy-eight parts of gold are added to twenty-two of aluminium a splendid purple is obtained, in which intense ruby-coloured opaque crystals may readily be recognized. Then, as the quantity of aluminium is still further increased, the alloys lose their colour, and pass to the dull grey hue of aluminium itself. Perhaps the most remarkable point about the purple alloy is its melting-point, which I have shown to be many degrees higher than that of gold itself.[1] See diagram, Fig. 7, in which curves of several constants of these alloys are given. This fact affords strong evidence that the alloy $AuAl_2$ is a true compound, having analogies to the sulphides, for in every other series of alloys the melting-points of all the members of the series are

[1] "The Queen of the Air," ed. 1887, p. 129; *Times*, December 11, 1891.

[1] Proc. Roy Soc., vol. l., 1891, p. 367.

lower than that of the least fusible constituent. There is one other fact of much interest connected with this alloy. When it is treated with dilute hydrochloric acid, chloride of aluminium is formed, and gold is released in a singularly voluminous form. The heat of formation of the gold-aluminium alloy has not been determined, but hydrochloric acid, which will not attack gold, will readily split up this compound, of which more than three-fourths is gold; the compound, in fact, behaves like a distinct metal, having special heats of oxidation and chlorination of its own.

(3) Lastly, we come to the question of solutions of metals in each other. One very remarkable instance of the behaviour of metals at high temperatures reveals the fact that the presence of a small amount of a metal in a mass of another lowers the freezing-point of the mass. In the industrial world this has long been known. Cellini tells us, for instance, that when the bronze for his great figure of Perseus, at Florence, was running out of the furnace, it suddenly showed signs of setting, and he therefore threw pewter plates and dishes into the ducts through which the metal had to pass—"a thing," he says, "never before done." The fluidity of the metal was immediately increased, and he found every part of the casting "to turn out to admiration."

The excellent work of Heycock and Neville,[1] on the lowering of the freezing-points of metals, by the addition of other metals, should, I would suggest, form the subject of a lecture in this Institution at an early day. I cannot attempt to deal with the matter here. In leading up to these questions of solution, as applied to metals, I would remind you that Lord Rayleigh told us a few evenings since that it was by no means certain that a gas rushing into a vacuous globe ever completely fills it, as there may still be tiny spaces into which "odd molecules" fail to find room to vibrate in. If it is difficult for a gas to entirely fill a vacuous space, you would think it impossible for a small quantity of a metal to rapidly permeate a fluid mass of another metal; nevertheless, so far as analysis can detect, this does happen.

It may be incidentally observed that the relations of the ordinary gases to metals are far more intimate than they were formerly supposed to be, and this was proved by Graham's work on the absorption of gases by metals, which has often been dealt with in this Institution. To take only the case of iron, more than twenty years ago Sir Lowthian Bell showed that carbonic oxide can carry away iron, which is released when the temperature is raised. Ludwig Mond and Langer have since isolated most interesting compounds of iron and carbonic oxide. But to return to the solution of metals in metals.

The method of taking autographic curves of the cooling of masses of metal has already been indicated in Fig. 4,[2] and they ought to enable much information to be gained as to what is taking place throughout the mass. Such curves should render it possible to ascertain which of the rival theories as to the nature of solution, as applied to salts, is supported by the behaviour of a metal dissolved in a metal. When, for instance, a little aluminium dissolves in gold, is the analogue of a hydride formed, and, if so, is the curve of freezing-points of a series of aluminium-gold alloys a continuous one? On the other hand, does the theory advocated by van 't Hoff, Arrhenius, and Ostwald gain support, and do the molecules of the dissolved metals act independently of the solvent—that is, does *osmotic* pressure come into play? It will be remembered that the law which regulates osmotic pressure has exactly the same form as Boyle's law—that is, the pressure is proportional to the density of the gas or of the solution. Is the view of Arrhenius correct—that, if a solution be very dilute, the molecules of the dissolved substance are dissociated, act independently of each other, and behave like a perfect gas?

1 Chem. Soc. Journ., vol. lv., 1889, p. 666; vol. lvii., 1890, pp. 376, 656; vol. lix., 1891, p. 936.
2 Proc. Roy. Soc., vol. xlix. p. 347, 1891.

It will require years of patient work before these questions can be answered; but it appears certain, from the admirable experiments of Heycock and Neville,[1] to which reference has already been made, that, taking metals with low melting-points (such as tin or lead) as solvents, the lowering of the freezing-point of the solvent is really due to the bombardment exerted by the molecules of the dissolved metals.

I have extended this investigation by employing as a solvent a mass of fluid gold, which has a high melting-point, and is not liable to oxidation, and the results confirm those obtained by Heycock and Neville.

There is yet one other question: When metals are added in small quantities to a metallic mass, may the solvent remain inert? Here is a mass of 1000 grammes of lead, and to it 15 grammes of gold, or 1·6 atoms for every 100 atoms of lead, will now be added. It could be shown that the gold is readily dissolved, and remains dissolved, even if the lead be solidified. Now, to the fluid lead sufficient aluminium will be added to form the purple alloy with the dissolved gold; the mass will be well stirred, but the aluminium will not unite with the lead; it will nevertheless find out the gold, and, after uniting with it, will carry it to the surface of the bath. Thence it can be removed, and the purple colour of the alloy identified, or the gold it contains can be revealed by the method Prof. Hartley[2] has given us for detecting the presence of gold in an alloy by volatilizing the alloy in a torrent of sparks from an induction coil, and condensing the vapour on mica.

The union of the aluminium and the gold must, however, be peculiar. Crookes[3] has shown that when this alloy is used as an electrode in a vacuum tube, the gold is volatilized from the alloy and deposited as a film on the glass, leaving the aluminium behind.

The purple alloy presents us with the most interesting case yet known of a molecule built up of purely metallic atoms, but we are certain that the atoms are still those of gold and aluminium—that is, the atoms of the united metals remain unchanged. The interest in this substance is deepened if it be remembered that our aim at the present day is the same as that of the alchemists, for we are striving, as they did, to attack and change the chemist's *atoms* themselves. We seek, as truly as they, to effect the transmutations, which, as Boyle said, would "be none the less real for not being gainful," and employ high temperatures in the hope of simplifying the molecular structure of metals. We no longer consider gold to be the "sum of perfection," but still retain the belief expressed by Geber, eleven hundred years ago, that, "if we would change metals, we must needs use excess of heat." A poet also appears to have felt this, for George Herbert writes in the seventeenth century—

"I know . . . what the stars conspire,
What willing Nature speaks, what forced by fire";

thus comparing the ordinary response of Nature to the investigator with the evidence he elicits from her by heat.

By fusing gold, and staining it "the purple of the dawn," a new interest has been given to the metal which the alchemists always connected with the sun; and for further proof that metallic atoms may be changed, we must turn to the sun itself, as to a great metallurgical centre, where "all the elements shall melt with fervent heat."

1 *Loc. cit.*
2 Proc. Roy. Soc., vol. xlvi., 1889, p. 88.
3 Proc. Roy. Soc., vol. l., 1891, p. 88.

ON INSECT COLOURS.[1]

II.

NOW it is necessary to explain the "reversion effects" of red, so frequently alluded to. I am tempted to give a detailed account of the experiments made in this connection, but the length to which this article has already run warns me that I must be very brief indeed: and I will therefore content myself with giving simply the broad results.[2] All reds and pinks (always omitting the last four in the table), are turned orange or yellow instantly by acids. When nitric acid is used, this effect is *permanent*; and whether the yellowed wing be dried, or washed, the yellow is immovable. I have kept such wings for five or six months, and they were as yellow as possible at the end of that time. In the case of all other acids,[3] the *yellow is permanent only so long as the wing is actually acid:* directly the acid is removed, the original red returns; and thus a wing may be alternately yellowed and "reverted" time after time. This reversion to the original red may be produced either by long exposure to the air, allowing the last traces of acid to drain off; or instantly by neutralizing the residual acid with a drop of ammonia, *or by copious washing*. It must therefore be understood that, with the exception of those cases in which nitric acid has been used, the permanency of the artificial yellow is *entirely dependent upon the presence of acid:* remove the acid, and the yellow vanishes. Accordingly, I have suggested the following explanation. Let us denote the molecule of red pigment by X; when any acid, except nitric, is added, I assume that this forms with X a so-called molecular compound: for instance, on treating with hydrochloric acid, we should get the hydrochloride of X, viz. $X.(H.Cl)_n$; and it is evident that these hydrochlorides, hydrosulphates, &c., of X are yellow, although the original X is red. To all these facts, of course, there are ample analogies known to chemistry. Next, for the resuscitation of red. We must suppose—what is certainly to all appearance very clear—that these molecular compounds are very unstable; an easily understandable fact; and that consequently the addition of even excess of water is sufficient to decompose them, removing the acid molecule, and thus restoring the pigment X to its original condition. Far more rapidly does this resuscitation occur if a drop of ammonia be used, this at once combining with the acid and liberating the X molecule. In the case of resuscitation produced by slow air drying, the action apparently would be in some cases due to gradual evaporation, or to some process of oxidation—anyhow producing dissociation of the molecular salt of X. Finally, in the case of nitric acid, it is clear that this acid does not form a molecular compound, but, as we might expect, exercises a permanently destructive action on the original pigment. Admitting that red has been developed from yellow, it is not surprising that it may be easily reconverted permanently into yellow by such a reagent as nitric acid. Before quitting this topic, I may point out that the cyanide reaction of the yellows is very suggestive indeed as to the kind of process by which the red pigment is developed from yellow.

Now, as to the last four species noted in the table. In these, I believe, the red is not developed from yellow at all, but from its close analogue, chestnut. Up till very recently, I supposed *V. atalanta* to be the only representative of such development, and was rather surprised that yellow should so commonly develop into red, and chestnut so rarely. But recently I have found that *Anartia amalthea* is exactly identical in behaviour with *V. atalanta*, whilst *Heliconius amaryllis* seems half-way between these species and the normal reds, but nearer to the former. The evidence on which this conclusion as to the nature of the red in *V. atalanta* was founded is as follows. The red of *atalanta* does *not* change to yellow, but to the brown or *chestnut normally present* in *V. cardui*, or to a more colourless tint. The change is not similar to that of red to yellow, but is a solution effect: consequently *no reversion effect* can be obtained; and this alone is almost decisive. It seems to me especially interesting that this experimental conclusion as to the nature of *atalanta* red is entirely corroborated by totally independent evidence from the entomological side, since the connection of *V. atalanta* and *cardui* is exceedingly close, and there are transition forms between them.[1]

And now we come to the last colour—chestnut—for which a very brief account will suffice, in addition to the details already given in the table, and the incidental remarks made during the discussion of yellow. It must be understood, then, that the constitution of chestnut appears to be very close indeed to that of yellow: like as in yellow, we can distinguish several stages of solubility, although deepening colour still less implies decreasing solubility even than it does in yellow—a conclusion which will be borne out by an examination of the table. Like yellow, chestnut may develop into red, as has already been explained; and the brilliant copper colour of *Lycæna phlæas* and *virgaurea* appears to occupy, both in its extreme solubility and its relation to the main line of development of the chestnut pigment, a position exactly analogous to that occupied by the orange of *E. cardamines* among the yellows. The only further remark that I have to make with reference to this colour concerns *V. io* and *V. antiopa*, which I have specially marked as notable examples. In these species the rich chocolate colour is very soluble, but leaves a *black* wing instead of a white. If chestnut had been developed from a white pigment, this would have been a grave difficulty; but it perfectly accords with the view that the pigment has been developed, not from any such white pigment, but in a previously unpigmented, usually white, wing; in these species it has been developed in a previously black wing. I have always considered the behaviour of these two species to considerably support my views of the nature of the chestnut pigment, and indirectly of the yellow.[2]

The main heads of the preceding pages may be very briefly summarized as follows. Blacks and whites are not pigment but absorption and reflection colours respectively. The great majority of blues are also physical colours—chiefly, if not entirely, interference colours; and it is doubtful if there be any pigment blues at all. Some greens are also physical colours, very similar to the blues; the character of another group is somewhat ambiguous, although probably these, too, are physical. A third group, is pigmental, and probably derived from yellow. All reds are pigmental, being developed chiefly from yellow, but in a few cases from chestnut; the former are characterized by the reversion effect. The great majority of yellows are pigmental, of various degrees of solubility or insolubility; but a few cannot at present be decisively pronounced either physical or pigmental, and the same remark applies to the chestnuts.[3]

In concluding this summary of my work, I must point out that it is not put forward as in any sense of the word final, even so far as it goes, but merely as a basis of systematic inquiry, in various directions. Up to the present, almost nothing at all has been known about the behaviour or character of these colours; now I will dare

[1] Continued from p. 517.

[2] A full account of these experiments will be found in the *Entomologist*, xxiii. pp. 39–40 and 53–59.

[3] I have used hydrochloric, sulphuric, acetic, phosphoric, hydrofluosilicic, and oxalic acids, in these experiments.

[1] There is similar evidence in the case of *Anartia amalthea*. Two specimens of this were sent me for experiment. One was marked with a chestnut band, and one with a scarlet. This scarlet was at once changed into the chesnut normally present in the other.

[2] I am disappointed at having as yet come across no yellow species analogous to *V. io*. But in this connection I may call attention to the behaviour of the *green* species of *Cidaria*, which are changed to a brownish-grey. It is possible that these greens may be descended from yellow developed on an originally dark wing.

[3] Cp. the instances of *Vanessa io* and *antiopa*.

to hope that at least a basis of operations has been found. May I also venture to ask that any other investigators who may not have already been working on this subject will do me the favour of allowing me for a time to continue my researches alone—so far, that is, as concerns the Lepidoptera, both *imagines* and larvæ—for I am planning various lines of research suggested by my previous work. As to the various other orders of insects, I shall be delighted if other workers who may have opportunities, that I have not, of obtaining abundant material, will take up the work, and determine how far my conclusions will hold for these other orders also.

Tables of Results.[1]

Initial of group.	Name of species.	Natural colour.	Effect of reagents.
R.	*Apatura iris*	Rich purple velvet glow	Chiefly unaltered; gloss destroyed by KHO.
,,	*Trepsichrois linnæi*		The glow much dulled.
,,	*Hypolimnas bolina*		The glow much dulled or unaltered.
,,	*Hypolimnas salamis*	Violet	Dull steel blue.
,,	*Papilio*—six species	Bright and pale blues.	Bronze; leaf brown; or steel blue; &c.
,,	*Morpho menelaus*	Brilliant blue	Less brilliant.
,,	*Papilio machaon*	Dullish blue	Greyish.
,,	*Limenitis sibylla*	Do.	Do.
S.	*Smerinthus ocellatus*	Do.	Greyish or unaltered.
N.	*Catocala fraxini*	Dull blue	No marked alteration.
R.	*Vanessa*—four species	Blue	Paler or unaltered; destroyed in *V. antiopa*.
,,	*Lycæna*—five species	Various blues	Usually slate colour; occasionally *greenish*.
R.	*Parthenos gambrisius* (upper surface)	Metallic green	Purplish bronze or blackish.
,,	*Hesperia* sp.	Dark metallic green	Do.
B.	*Urania fulgens*	Do.	Do.
R.	*Papilio polyctor*	Bronze yellow green	Brownish or blackish.
,,	*Parthenos gambrisius* (below)	Sage green	Dun brown.
,,	*Limenitis procris*	Do.	Dun grey.
S.	*Ino statices*, &c.	Glittering green	Bronze brown.
R.	*Argynnis paphia*, &c.	Deep foliage-green	Do.
,,	*Thecla rubi*	Almost arsenic green	Brown, *like upper surface*.
,,	*Papilio*—four species	Leaf green	Whitish; white; or yellow.
,,	*Eromia argia*	Very pale greenish	Paler or whitish.
N.	*Halias prasinana*	Green	Whitish; *ochre yellow* in one case.
,,	*M. orion*; and *D. aprilina*	Do.	White.
G.	*Larentia*, &c.—several species	Do.	White; occasionally yellowish white.
,,	*Cidaria miata*, &c.	Do.	Brownish grey.
T.	*Tortrix viridana*	Do.	Whitish.
R.	*Euchloe cardamines*	Pseudo-green	Black.
S.	*Smerinthus*, &c.—several species	Pink *	Yellow or yellowish.
B.	*Deiopeia bella*	Do. *	Yellow.
,,	*Actias luna*	Do. *	Do.
,,	*Attacus cynthia*	Do.	Colourless or faded.
,,	*Hepialus humuli*	Do.	Faint orange.
G.	*Hyria auroraria*	Do.	Yellowish or whitish.
,,	*Aristelia rubricata*	Do.	Dunnish colour.
R.	*Parnassius apollo*	Red *	Orange.
,,	*Delias hierte* and *eucharis*	Red (*)	Yellow: *then white*.
,,	*Papilio*—various species	Crimson, scarlet, &c. *	Yellow, orange, &c.
S.	*Zygæna filipendulæ*	Red *	Orange.
B.	*Arctia caia*	Do. *	Do.
,,	*Euchelia jacobeæ*	Do. *	Do.
N.	*Catocala nupti*	Do. *	Do.
,,	*Xanthia silago*	Reddish	Yellow.
R.	*Papilio vertumnus* and *constolochia*	Pale red *	Pale ochrish or bread colour.
,,	*Heliconius amaryllis*	Scarlet	Brown: finally white.
,,	*Anartia amalthea*	Do.	Chestnut.
,,	*Vanessa atalanta*	Do.	A "*cardui*" brown.

Initial of group.	Name of species.	Natural colour.	Effect of reagents.
R.	*Papilio machaon*	Palish yellow	White.
,,	*Heliconius amaryllis*	Very pale yellow	Do.
,,	*Catopsilia catilla* ♀	Light sulphur	Do.
,,	*Vanessa antiopa*	Dull palish yellow	Whitish.
,,	*Charaxes athamas*	Light sulphur No †	White or towards whitish.
,,	*Tenias nise* and *vabella*	Lemon yellow †	Pure white.
,,	*Delias eucharis* and *hierte*	Yellow	Do.
,,	*Hebomoia glaucippe*	Deep orange	Perfectly transparent.
,,	*Colias edusa*	Rich yellow †	Pure white.
,,	*Gonepteryx cleopatra*	Bright brimstone	Practically white.
B.	*Hazis* sp.	Deep yellow No †	Unaffected or nearly white.
R.	*Euchloe cardamines*	Rich orange	White.
,,	*Gonepteryx cleopatra*	Do.	Through brimstone to white.
,,	*Gonepteryx rhamni*	Bright brimstone †	Towards whitish.
,,	*Colias hyale*	Pale brimstone †	Lighter or fairly white.
,,	*Polyommatus alexis*	Orange	Scarcely affected or towards whitish.
B.	*Hepialus humuli*	Dullish yellow	Sickly yellowish; nearly transparent.
G.	*Rumia cratoegata*	Pale brimstone No †	Towards whitish.
,,	*Camptogramma bilineata*	Dull orange No †	Towards or whitish.
,,	*Hyria auroraria*	Dull yellow	Whitish or white.
,,	*Abraxas grossulariata*	Palish orange	Towards white or white.
,,	*Venilia maculata*	Yellow	Unaffected, or paler, or white.
,,	*Angerona prunaria*	Brownish orange No †	Dulled or unaffected.
R.	*Papilio asterias*	Orange yellow	Usually unaltered.
,,	*Ornithoptera darsius*	Crocus gold	Practically unaltered; but some probably dissolved.
N.	*Eudryas grata*	Yellow	Transparent or rather faded.
B.	*Tithonea haumoni*	Do. No †	Much paler, or towards whitish.

The following yellow or orange-yellow species are almost or entirely unaffected:—

(R.) *Papilio thoas, polyxenes*, and *philenor*; (B.) *Deiopeia bella. Callimorpha hera* (no †), *Arctia villica, Citherona regalia*; (N.) *Xanthia silago, Triphæna pronuba* (no †), *Heliaca tenebrata*.

Initial of group.	Name of species.	Natural colour.	Effect of reagents.
R.	*Vanessa io* and *antiopa*	Chocolate	Blackish. N.B.
,,	*V. urticæ*	Rich chestnutish	Whitish.
,,	*Argynnis paphia* and *selene*	Rich chestnut	Whitish; faded; or nothing.
,,	*Danais chrysippus* and *hegesippus*	Do.	Very faded; or whitish grey.
,,	*Athyma nefta*	Dull chestnut	White or transparent.
,,	*Cænonympha pamphila*	Pale chestnut	Whitish or white.
,,	*Hesperia sylvanus*	Chestnut	Faded or white.
,,	*Lycæna phlæas* and *virgaurea*	Brilliant copper	Much faded or whitish.
,,	*Vanessa cardui*	Chestnut	Faded or whitish.
,,	*Epinephile tithonus* and *janira*	Do.	Faded more or less.
,,	*Satyrus megæra*	Yellow chestnut	Little faded only.
,,	*Atella phalanta*	Rich chestnut	Faded or very faded indeed.
,,		Rich chestnut	Very faded indeed.
,,	*Athyma penus*	Yellow chestnut	Dulled.
G.	*Selenia illustraria*	Deep chestnut and chocolate	Scarcely affected.

The following species are chiefly or wholly unaffected:—

(S.) *Philampelas athemon, Darapsa myron*; (R.) *Dione passifloræ*; (B). *Orgyia antiqua, Bombyx quercus*; (N.) *Orthosia macilenta, Mamestra oleracea*; (G.) *Cidaria suffumata, Coremia ferrugata* and *minutata*.

[1] These tables afford only a very condensed summary of results; for fuller details *vide Entomologist*. The initials R., B., N., G., S., T. in first column signify respectively Rhopalocera, Bombyces, Noctuæ, Geometræ, Sphingidæ, and Tortrices. The asterisk (*) against various red species signifies "reversion effect," and the mark † against certain yellow species that the "cyanide effect" has been obtained; similarly, No † that no cyanide effect can be obtained with that species.

F. H. Perry Coste.

EXAMINATION OF THE STANDARDS OF MEASURE AND WEIGHT IMMURED IN THE HOUSES OF PARLIAMENT.

A FORMAL examination of the standards of measure and weight which are immured in the Houses of Parliament was made on Saturday last, of which some account may possibly interest our readers.

In the "New Palace at Westminster" there were deposited in the year 1853 a copy of the "Imperial standard of the yard measure" and also a copy of the "Imperial pound weight." In the same year similar copies of the Imperial standards were also deposited with the Royal Society, at the Royal Observatory, and at the Royal Mint respectively. Such copies of the standards were deposited in accordance with the recommendations of a Commission appointed in 1843 to superintend the construction of new Parliamentary standards of length and weight intended to replace the original Imperial standards which were destroyed by the fire at the old House of Commons in 1834. The new Imperial standards were subsequently legally recognized by the Act 18 and 19 Vict., c. 72 (1855), and more recently by the Weights and Measures Act of 1878.

The Act of 1878 requires an inter-comparison of the copies of the Imperial standards which are deposited with the Royal Society, and at the Royal Observatory, and the Royal Mint, to be made once in every ten years; and such inter-comparison has been recently duly made under the directions of the Board of Trade. It appears also to be practically necessary that the standards immured at the Houses of Parliament should be examined at certain intervals, examinations having been made in the years 1865 and 1872; and the examination which was made on Saturday last in the presence of the Speaker, the President of the Board of Trade, and other representative authorities, was therefore the first that has been made for the past twenty years.

As yet we can make no reference to natural elements for the values represented by such standards as those above referred to. The earth's dimensions (as the ten-millionth of a meridian), or a physical quantity (as the length of the seconds pendulum), cannot at present be fixed or redetermined with sufficient accuracy for metrological purposes; and we have still therefore to rely on the length and weight of certain arbitrary or material standards placed in the custody of selected authorities. As all such material standards—whether made of iridio-platinum, quartz, gold, or other accepted materials—are liable to alteration by time or circumstance, it becomes the duty of the custodians of such standards to assure themselves from time to time that their standards are unaltered, so far as any intercomparison of material standards may afford evidence of their constancy; and such was the object of the examination on Saturday last, when the Board of Trade had their standards compared with those immured at the Houses of Parliament.

The history of these Parliamentary standards may be found in the classical contributions of the late Astronomer-Royal, and Prof. W. H. Miller, to the Philosophical Transactions of the Royal Society (Parts III. for 1856 and 1857); a history that has largely developed scientific inquiry in such direction, as may be instanced particularly by the creation and work of the International Committee of Weights and Measures at Paris.

On Saturday the examination of the immured standards was conducted by the officers of the Standards Department of the Board of Trade, who, for the purposes of the examination, had provided a microscopic-comparator and a balance of precision. The comparator had in it nothing new, and indicated differences of length approaching to 0·00001 inch, excepting that it was portable, so that the comparison of the immured standard yard with the Board of Trade standard might be then made at the Houses of Parliament without the risk of removal from the building. It was, indeed, a condition of the examination that the immured standards should not be removed from the custody of the Clerk of the House. The balance used indicated differences of weight approaching 0·0005 grain, although, unlike other balances of precision, this balance was inclosed in a closely-fitting copper case, so that disturbance by currents of air might be avoided as far as possible whilst the examination was being made. The mode of comparison of the yard measure was that adopted by Baily and Sheepshanks in 1843-48, Gauss's method of weighing being followed; and the temperature and atmospheric pressure were indicated by instruments verified at the Kew Observatory.

The immured standard yard, like the Imperial yard, was found to be a bronze bar about 38 inches in length, marked "Copper 16 oz., tin 2½, zinc 1. Mr. Baily's metal. No. 4 standard yard, at 61°·98 Fahrenheit. Cast in 1845. Troughton and Simms, London"; the length of the yard, or of 36 inches, being determined by a straight line or distance between two fine lines marked on gold studs or plugs which were inserted at the bottom of two holes or wells at about half an inch below the surface of the bar. The bar was found to be supported on bronze rollers, placed under it in such manner as best to avoid flexure of the bar.

The immured pound weight, like the Imperial pound, was found to be a cylinder of platinum about 1·35 inch in height and 1·15 inch in diameter, having a density of 21·1516; and being (in 1856) 0·00314 grain lighter than the Imperial standard deposited with the Board of Trade.

Both standards were placed in mahogany boxes; the pound weight being wrapped in Swedish filtering paper, and inclosed in a silver-gilt case, which was further inclosed in a solid bronze box. The mahogany boxes were inclosed in a leaden case, which was re-inclosed in a sealed oak case.

Although the actual result of the examination on Saturday could not then be made known to those present, it was stated that the immured standards were found to be in the same condition as when they were previously examined in 1872, and were to all intents and purposes unchanged since their original deposit in 1853. The official report of the Board of Trade, which will be shortly issued, will state the full particulars of the examination.

After the comparisons of the standards had been completed, the immured standards were replaced within the oak case, which was then replaced in an inclosure or cavity prepared for it in a recess under a blank window on the right-hand side of the second landing leading from the lower waiting hall up to the Commons Committee-rooms; the rabbet of the inclosure was then covered with lime putty, the front stone being inserted and driven into close contact with the rabbet so covered, liquid plaster of Paris being poured in so as to fill all the joints of the front stone.

We are glad to see from a paper recently laid before both Houses of Parliament that the Board of Trade also possess authoritative copies (*prototypes nationaux*) of the international standards of the metre and kilogramme; and that metric weights and measures—now also of the highest importance in this country—may be accurately verified by comparison with such standards.

NOTES.

MR. BALFOUR is expected to make a statement in the House of Commons this evening about the Royal Commission to which the question of a Teaching University in London is to be referred. He hopes to be able to give the terms of the reference as well as a complete list of the members of the Commission.

The honour of knighthood has been conferred on Dr. George Buchanan, F.R.S., on his retirement from the post of Medical Officer to the Local Government Board.

The friends and admirers of the late Mr. H. W. Bates, F.R.S., propose to give substantial expression to their regard for his character. A fund is to be raised for presentation to his widow. Any communications relating to the matter should be addressed to S. W. Silver, 3 York Gate, Regent's Park, N.W.

A branch of the Royal Scottish Geographical Society has been established in London. At the first meeting, which was held on Monday, Prof. James Bryce delivered an address on "The Migrations of the Races of Men considered historically." The Marquis of Lothian, President of the London branch, occupied the chair. He said they had no intention of competing, or in any sense of vying with, the Royal Geographical Society. The Scottish Society had branches in Glasgow, Dundee, and Aberdeen; and it had been felt that another might be appropriately formed for the benefit of members in London.

The Committee appointed to consider the question of grants to University Colleges in Great Britain have issued their Report. The principle on which they consider that the distribution of the grant to the Colleges now sharing it should be made for the remainder of the quinquennial period is as follows: (1) they award a grant to each College, varying according to the nature and extent of its University work; (2) a grant for every professor or other teacher receiving more than £250 per annum; (3) a percentage on the College income from all sources. A table is printed, giving the present grants, which the Committee wish to be continued; and the grants which they wish to be added.

The rebuilding of the College of Agriculture, Downton, rendered necessary by the destructive fire of last year, has now been so far completed that the premises will be ready for occupation and use next term.

The Director of the new Imperial German Zoological Station at Heligoland will be Dr. Heincke, of Oldenburg, a recognized authority on fish and fisheries. As his first assistant he will have Dr. Clemens Hartlaub (son of Dr. G. Hartlaub, the well-known ornithologist of Bremen), who will take charge of the scientific branch of the establishment. Since the death of Dr. Philip Carpenter, Dr. Clemens Hartlaub has become one of our leading authorities on starfishes. He has just published in the *Nova Acta* of the Imperial Leopoldino-Caroline Academy, an elaborate memoir on the Comatulidæ collected by Prof. Brock in the Moluccan Seas and deposited in the Göttingen Museum. In the course of this article nine species of the genera *Antedon* and *Actinometra* are described as new to science.

On March 26, the members of the Geologists' Association, assembled at the house of Mr. W. H. Hudleston, F.R.S., President of the Geological Society, in order to inspect the handsome private museum he has attached to his residence. The occasion was rendered particularly interesting by the fact that the Council of the Association had decided to take this opportunity of presenting to Mr. Hudleston an illuminated address expressing its sense of the helpful interest he had always shown in the work of the Association. Among those present were many former Presidents and officers of the Association, who now rank as leaders of geological science. Although at least one hundred persons had been concerned in the arrangement of the testimonial, the secret was so well kept that the presentation came as a complete surprise to its intended recipient. The signatories of the address had been selected to represent all grades of past and present workers of the Association. In making the presentation, the present President of the Geologists' Association, Rev. Prof. J. F. Blake, after suitably referring to Mr. Hudleston's eminent services to geological science, expressed the particular pleasure he felt that it should have fallen to his share to hand a testimonial so richly deserved to his old colleague and fellow-worker. Mr. Hudleston, in the course of a well-chosen reply, referred to the curious coincidence that of the authors of the joint work of Blake and Hudleston many years ago, the one was President of the Geologists' Association and the other of the Geological Society during the same year. The contents of Mr. Hudleston's museum, now in course of arrangement, excited considerable interest, particularly the minerals, many of the choicest specimens of which are from the private collection of the late Prof. J. Tennant, and also the extensive series of British Jurassic Gasteropoda collected by the author for the monograph now in course of publication by the Palæontographical Society.

Mr. J. P. Barrett, chief of the department of electricity in connection with the Chicago Exhibition, has issued a pamphlet containing all the information that can be needed to enable intending exhibitors to proceed intelligently. He will be glad to give special information to any one who may want it, and invites correspondence.

A body called the Scientific Alliance was recently organized at New York. It consists of six societies engaged in the promotion of research, and two others will probably soon be added. The six societies are the New York Academy of Sciences, the Torrey Botanical Club, the New York Microscopical Society, the Linnean Society of New York, the New York Mineralogical Club, and the New York Mathematical Society. According to *Science*, these societies do not in any way sink their individuality or surrender any part of the management of their own affairs. Their union is merely in the way of co-operation for the advancement of science, and for mutual encouragement, carried out through a central representative body, known as the Council, having advisory powers only. The Council is made up of the president and two other delegates from each society. A monthly bulletin is issued under the authority of the Council, announcing the proposed proceedings of all the societies, and a copy of this bulletin is sent to every member. The bulletin contains an invitation to the members to attend any of the meetings. An annual directory is issued, and it is proposed that there shall also be an annual report on the work done. *Science* says that the brief experience of the Alliance has convinced the members that still closer union is necessary, and this feeling has led to a movement for the securing of a permanent building as a home for all the societies. It is hoped that a building may be erected in a central part of New York, "large enough to afford each society rooms for its ordinary meetings, for its library and collections, as well as facilities for research, and also to contain a lecture hall, capable of seating twelve hundred people, to be used by all the societies in their public work."

We have referred in the astronomical column to the astronomical observations recorded in the "Washington Observations for 1887." We will here briefly summarize the contents of this volume with regard to the other observations there tabulated. Appendix I. contains a report upon some of the magnetic observatories of Europe, which was made by C. C. Marsh, who was commissioned to pay special attention to the instruments, buildings, methods of observing, and the question of the reduction of the observations. In this report, which was considerably cut short, owing to the author having to proceed to sea on his return, much interesting and valuable material has been collected which should be consulted by all those who are connected with the taking of such observations, and with the construction of magnetic observatories. The plates which accompany the text show plans of heating and ventilating the Pavlovsk and Potsdam Observatories, the cellar and ground plans of the latter, and details of instruments used in other Observatories. In the second and third appendices will be found all the magnetic and meteorological observations made in the years 1890 and 1883–87 respectively: these are brought together in a way that will be found most convenient for reference, while several plates showing the mean diurnal variations of some of the magnetic elements have been added. All the above-mentioned observations have been reduced in the usual way, and the results obtained are here tabulated.

ON Thursday and Friday, last week, a tornado passed over the North-Western States of America and caused enormous damage and great loss of life, in some cases whole towns being devastated. It is said to have been the most far-reaching and destructive storm ever known to have occurred in these regions. Texas, Kansas, Missouri, Iowa, and Nebraska suffered most.

IN the Annual Report for 1892 of the Berlin branch of the German Meteorological Society, Prof. G. Hellmann gives an account of his continued experiments on the effects of exposure on rainfall records, and on the determination of the distance apart that rain-gauges should be erected in order to obtain an accurate account of the rainfall of any district. Simple as the question appears, the experiments, which have been carried on for seven years, have not sufficed to give a definite answer. Very considerable differences are found in the amounts recorded at stations comparatively close to each other. This result is partly owing to the effect of wind, especially in the case of snow. The following are the most important conclusions derived from the experiments:—(1) The more a rain-gauge is exposed to the wind, under otherwise similar circumstances, the less rainfall it records, and the higher a gauge is placed above the ground, the less rain it catches, as the disturbing influence of the wind is greater than on the surface of the ground. But if properly protected from the wind, a gauge will give useful results in an elevated position. The usual instructions to erect the gauge as openly as possible are therefore incorrect. (2) Even in a flat country, differences of 5 per cent. occur in different months, at stations a quarter of a mile apart; in stormy weather, especially during thunderstorms, the difference may amount to 100 per cent. The amounts recorded at neighbouring stations agree better together in spring and autumn, and also in relatively wet years. Further experiments are needed, if possible by means of anemometers erected at the same level as the rain-gauges, to determine more accurately the effect of wind on both rainfall and snow.

IN connection with the celebration of the fourth centenary of the discovery of America by Columbus, the Italian Botanical Society invites the attendance of botanists of all countries at a Botanical International Congress, to be held at Genoa, from the 4th to the 11th of September. In addition to the meeting for scientific purposes, there will be excursions on the shores of the Mediterranean and in the Maritime Alps; and during the same time will also take place the inauguration of the new Botanical Institute built and presented to the University of Genoa by the munificence of Mr. Thomas Hanbury, of La Mortola, and the opening of an Exhibition of Horticulture. All communications should be addressed to Prof. Penzig, of the University of Genoa.

HARVARD UNIVERSITY is indebted to the munificence of Prof. George L. Goodale, the Director of the Botanic Garden at Cambridge, Mass., for a remarkable development of the botanical establishment of the University during the last ten years. It has acquired a large fire-proof Museum, to contain not only its collections, but its lecture-rooms and laboratories; has added greatly to its collections and its library; and has also obtained larger permanent funds for its support.

IN a report on the Great Skua in Shetland during the season of 1891, printed in the new number (the second) of the *Annals of Scottish Natural History*, Mr. W. E. Clarke says that the attention which was called to the persecution of the Great Skua, at the close of the disastrous breeding season of 1890, was the means of doing much good. It aroused and secured the interest and influence of ornithologists and others on behalf of the bird's future welfare and its preservation as an indigenous British species. He notes that the number of Skuas resorting to Foula annually during the summer may be estimated at not less than 120 individuals. Of these, two-thirds are to be reckoned as breeding birds.

MESSRS. MACMILLAN AND CO. have issued a second edition of Mr. A. R. Wallace's well-known "Island Life, or the Phenomena and Causes of Insular Faunas and Floras." The work has been carefully revised throughout, and, owing to the great increase to our knowledge of the natural history of some of the islands during the last twelve years, considerable additions and alterations have been required.

THE paper on the opium question, recently read by Mr. G. H. M. Batten before the Society of Arts, is printed in the current number of the Society's journal. It is followed by a report of the animated discussion to which it gave rise, and by statements which would have been submitted to the meeting by various gentlemen if there had been time. Mr. Batten cites the opinion of a number of "independent persons of high character and reputation," to the effect that "the daily use of opium in moderation is not only harmless but of positive benefit, and frequently even a necessity of life"; and that "this moderate use is the rule, and excess the exception." Persons who have arrived at an opposite conclusion have had an experience, he thinks, almost entirely confined to towns and the sea coast. "They knew little or nothing of the millions of the healthy, industrious population in the interior of the country to whom the use of opium is as common, as moderate, and as beneficial as that of beer is to the people of England."

A WRITER in *Nature Notes*, calling attention to "the iniquity of rooting up wild flowers to sell them to English dealers," says he could name a district in the Basses Pyrénées, where not a single wild daffodil is now to be found. The flower was once abundant there, but an English resident chose to bargain with a well-known dealer, to furnish him with roots; and this has been attended by grave injustice to France.

MR. G. C. GREEN records in *Nature Notes* for April a curious reminiscence with regard to a pair of jackdaws kept by him at Modbury Vicarage, South Devon, about twenty years ago. They had been taken from the nest, and during the first summer their wings were slightly clipped. After this their wings were allowed to grow, and they lived at full liberty in the garden. They were perfectly tame, and would come at call and feed out of the hand, would come into the house, and in the morning knock at the windows to ask for some breakfast. In the spring they used to fly away and join their wild companions, make their nests, and rear a family; but when this was over they came back to the garden again, fed from the hand, and were as tame as ever. But the curious thing was, that after one or two seasons they brought another jackdaw with them, presumably the young of one of them, which was just as tame as themselves, although nothing had ever been done to tame it, so that it was impossible to tell which were the original favourites, and which was the new one. Moreover, when after a few years one of these jackdaws was accidentally killed, another was brought by the other two.

Mr. W. W. Smith, writing to the new number of the *Entomologist* from Ashburton, New Zealand, says that he has for twelve years successfully used hellebore as an insecticide. It is used annually by many orchardists in the South Island for destroying the larvæ of *Tenthredo* (*Selandria*) *cerasi*. Mr. Smith uses it in the proportion of half an ounce to a bucket of water. When he notices the newly-hatched larvæ on the leaves, he carefully and effectually syringes the trees with the solution, choosing a calm day for doing so. The larvæ are equally common on the cherry-, plum-, and pear-trees, and rapidly destroy the foliage if they are not checked or destroyed. One good syringing suffices. When the trees are syringed early, the imago sawfly is prevented from laying eggs further on the foliage, and by this course much labour is avoided. He does not go over the trees syringing a second time with pure water, as the particles of powder left adhering to the foliage are invariably washed off by rains before any of the fruit ripens.

Mr. J. W. Fewkes contributes to the January number of the *American Naturalist*, just received, an interesting paper on the ceremonial circuit of the cardinal points among the Tusayan Indians. During the progress of the secret ceremonials which are performed in the Kib-vas or Estufas at Hual-pi, and other pueblos of the old province of Tusayan, it is customary for a priest to pass on the north side of the fire-place as he approaches the altar, and on the south as he passes from the altar to the ladder. This custom is conscientiously followed by the older priests, especially when taking part in important ceremonials; and Mr. Fewkes has seen novices, and even old priests, corrected and sent back when they had violated this simple Kib-va custom. The four directions do not correspond with the true cardinal points. The so-called Kwi-ni-wi-ke of the Hopi is neither the magnetic nor the polar north, but about north-west, or 45° west of north, and the other points vary in the same ratio. Mr. Fewkes thinks that a ready explanation of this is found in the orientation of the Kib-vas, which, in turn, depends on the extension of the mesa upon which Hual-pi is situated—or, speaking more accurately, as he says in a note, on the direction of the lines of fissure of the rock of which the mesa is built up. The ceremonial circuit is constantly followed in the preparation of so-called medicine. When a priest pours the liquid of which it is made into the terraced rectangular bowl, preparatory to placing the other ingredients in it, he pours the fluid first on the north side, then on the west, then on the south, then on the east side of the bowl. The ceremonial circuit is followed in connection with many other observances noted by Mr. Fewkes. He also remarks that the following colours correspond to the four cardinal points (bearing in mind that the Hopi north is really north-west): north, yellow; west, blue (represented ceremonially by malachite green); south, red; east, white. The priest of the antelope assemblage, in making the sand mosaic picture a few days before the snake dance, first makes the yellow border, then the green, then the red, then the white. The north line of the yellow is followed by the west of the same colour, then the south, then the east. The same sequence occurs when he outlines and makes the body of the semicircular clouds in the centre of the mosaic (dry painting). The lightning serpents of the four colours are made in the same order of the colours. It is interesting to note, as Mr. Fewkes says, that the ceremonial circuit is opposite that of the sun in its daily course in the sky. He thinks it is probably more than a coincidence that it is the same circuit which the snake and antelope priests take when they move about the place, and the latter carry the snakes in their mouths.

Last year Dr. J. T. Rothrock received from the American Philosophical Society a grant of 300 dollars to defray part of the expenses of a trip to the West Indies. The object was the collecting of photographs and information which could be utilized in the preparation and delivery of the annual lectures popularly known as "the Michaux forestry course." About 150 good negatives were obtained, and there are about 75 satisfactory illustrations of the trees, physical geography, and topography of the islands visited. The trip lasted three months. Dr. Rothrock was particularly struck by the contrast between the Bahamas and Jamaica. In the course of some interesting observations printed in the latest instalment of the Proceedings of the American Philosophical Society, he points out that the Bahamas are low and show no considerable elevation, while Jamaica reaches a maximum altitude of 7360 feet above the sea-level. The soil of the Bahamas is scanty, and consequently cultivation entails fertilization. That of Jamaica is of great depth, and its continued productiveness is evidence of a vast natural fertility. The flora of the Bahamas shows marked resemblance to that of Florida. The flora of Jamaica is essentially tropical, save at such altitudes as suit plants of cooler regions. In such places are found the common chickweed (*Stellaria media*), the white clover (*Trifolium repens*), associated with plants from the cooler parts of southern regions. The mangrove (*Rhizophora mangle*), common to the tropical seas around the globe, attains in Jamaica (compared with that in Florida and in the Bahamas) a surprising height. Near Port Morant are large jungles, where the trees attain a height of at least 60 feet. Dr. Rothrock calls attention to possible tannin production from the mangrove. No tree in North America, he says, at all approaches the mangrove in the percentage of tannin it contains. That the mangrove should have remained so long unutilized is due to the difficulty of obtaining its tannin free from colouring matter. Dr. Rothrock thinks that in the near future, owing to exhaustion of other tannin-producing trees, the arts will be forced to draw upon the mangrove, even if an improved chemistry is not able to free it from this objectionable colour. The natives obtain a red-brown dye from the bark by simply steeping it in water.

Mr. T. Southwell, Norwich, records in the April number of the *Zoologist* that he was lately informed, by Mr. D. C. Burlingham, of the occurrence of a male Greenland shark, (*Læmargus borealis*), which measured 14 feet 2 inches in length and weighed 1¼ tons, at Lynn, on the 21st of January last. It was found stranded on a sand-bank on the east side of the Bulldog Channel, and was brought up to Lynn by a fishing-smack, being still alive when Mr. Burlingham saw it. It was subsequently exhibited at Cambridge, and its owner intended to take it to Huntingdon, Peterborough, and elsewhere. This species is of rare occurrence on the Norfolk and Suffolk coast, and the present example is only the fourth of which Mr. Southwell has notes.

Dr. E. Ráthay states that the galls of *Cynips calycis*, produced on *Quercus pedunculata*, attract, by their viscid secretion, a number of small ants, which he believes to be advantageous to the tree, in killing quantities of caterpillars and other insects which are its natural enemies. He illustrates the value of this protection by the statement that the inhabitants of a single ants' nest may destroy in a single day upwards of 100,000 insects.

In the *Bullettino* of the Italian Botanical Society, Signor F. Pasquale proposes a new theory of the morphology of the carpel in flowering plants, founded on an extended observation of the course of the vascular bundles. According to him, a carpel is not a single modified leaf, but is the result of the concrescence of three, less often of two, leaves, which take part in the formation and in the nutrition of the ovules and of the seeds. The carpel is therefore a *triphyllome*, of which one leaf (the inferior one) is sterile, and the other two (superior) are fertile; and between these there is an intimate fusion, with complete anastomosis of the vascular bundles. Each fertile leaf is composed of a membranous portion, the *placental hemiphyll*, and an *ovular hemiphyll*, which is entirely transformed into the ovules with their funicles, together with the style and stigma. The placental hemiphyll also takes part in the formation of the pericarp and septa. The ovules originate from the whole of the ovular hemiphyll, and not merely from the carpellary margins or teeth.

WE notice the appearance of a very useful work, in Russian, by Prof. Samokvasoff, on Russian prehistoric antiquities, under the title of "Foundations of a Chronological Classification of Antiquities, and Catalogue." As seen from the title, the work consists of two parts: a catalogue of the very rich collection of the Russian Professor, partly illustrated, and a general description of the various epochs which may be distinguished in the relics of the past on the territory of Russia. He has no difficulty in showing that the Slavonians of the first centuries of our era were by no means mere savages. The burial places of that period, usually situated close to the earthen forts, some of which must have required the work of a considerable population, contain hundreds and thousands of graves, so that it is certain that the Slavonians of that period were living in large societies, and had their fortified towns. The same burial customs prevailed over large areas, but the treasures now unearthed from various graves show that differences of wealth and social position existed at that time as well. Considerable amounts of Greek, Roman, and Arabian gold and silver coins were found in the graves, the metal alone of the coins found in some graves attaining, at its present prices, the value of several hundred pounds; while numbers of objects of art, of Greek, Roman, Byzantine, and Arabian origin, are proofs of the brisk foreign trade which took place at that time. The graves of the pagan Slavonians contain flax, woollen, silk, and gold-embroidered tissues; ornaments in gold, silver, bronze, and bone; iron weapons and parts of armament; gold, silver, bronze, iron, and clay vessels, and so on; while the sickles and the grains of wheat, oat, and barley which were found in the graves of South Russia, together with small idols and other objects devoted to pagan worship, are proofs of agriculture having been carried on during the pagan epoch.

Two new liquids containing fluorine have been synthesized by M. Meslans. They are halogen derivatives of glycerin, and were obtained by allowing allyl fluoride, a gaseous substance recently described by M. Meslans, to react with chlorine and bromine. Allyl fluoride, C_3H_5F, is readily prepared by the gradual addition of allyl iodide to dry silver fluoride. It is a colourless gas of peculiar odour, which burns with a luminous flame upon ignition, with liberation of vapour of hydrofluoric acid. When a jet from which chlorine is escaping is brought into a vessel filled with allyl fluoride, combination at once ensues, and drops of a colourless liquid commence to be deposited upon the walls of the vessel. In order to obtain the liquid in greater quantity a large flask is employed, through the caoutchouc stopper of which pass two tubes, one delivering chlorine and the other allyl fluoride. Considerable heat is developed during the act of combination, hence the flask is immersed in a bath of cold water. A slight excess of chlorine is maintained during the reaction, and the liquid which rapidly collects is consequently coloured green; but when sufficient has been accumulated the supply of chlorine is first arrested in order that the excess of that gas, which produces the green coloration, shall be converted to the colourless liquid by the still-issuing allyl fluoride and the liquid thus decolorized. The colourless mobile liquid so obtained is then submitted to distillation, when practically the whole passes over into the receiver between 122° and 123°. If the synthetical preparation is conducted volumetrically, it is found that equal volumes of allyl fluoride and chlorine unite; the resulting liquid therefore presumably possesses the composition $C_3H_5FCl_2$, an assumption confirmed by a determination of vapour density which yielded the number 4·50, the vapour density calculated from this formula being 4·51. The compound is indeed a derivative of glycerin, its constitution being probably $CH_2Cl—CHCl—CH_2F$, and may be termed dichloro-fluorhydrin.

THE second new compound is analogous to the one just described, and resembles it very closely in properties. It is obtained by the direct union of bromine with allyl fluoride. If a few drops of bromine are allowed to fall into a vessel filled with allyl fluoride, the latter is rapidly absorbed with considerable rise of temperature, the red colour of the bromine simultaneously disappearing. To prepare the liquid in quantity, allyl fluoride is allowed to stream slowly into a quantity of bromine contained in a cooled flask, the operation being continued until the red colour of the liquid has entirely disappeared. The colourless liquid thus obtained distils without decomposition at 162°–163°. The data afforded by determinations of the bromine content and the vapour density point to the formula $C_3H_5FBr_2$. Both the liquids above described appear to be very stable compounds, for even during their distillation the glass vessels containing them exhibit no signs of etching. They are miscible with ether, and readily soluble in absolute alcohol, but they are almost perfectly insoluble in water. They possess pleasant odours, somewhat reminding one of chloroform, and are sweet but burning in taste. They are incombustible, but at a high temperature the vapours burn with liberation of hydrofluoric and hydrochloric or hydrobromic acids.

THE additions to the Zoological Society's Gardens during the past week include a Guinea Baboon (*Cynocephalus sphinx* ♀), a Bateleur Eagle (*Helotarsus ecaudatus*), a Puff Adder (*Vipera arietans*) from South Africa, presented by Mr. Keith Anstruther; a Japanese Deer (*Cervus sika* ♀) from Japan, presented by Sir Douglas Brook, Bart.; a Wedge-tailed Eagle (*Aquila audax*) from Australia, presented by Miss Carr; a Tawny Owl (*Syrnium aluco*), European, presented by Mr. E. A. Rocheda; a Puff Adder (*Vipera arietans*) from South Africa, presented by Mr. D. Wilson; two Common Vipers (*Vipera berus*), British, presented by Mr. W. H. B. Pain; a Shielded Eryx (*Eryx thebaicus*) from North Africa, deposited; four Topela Finches (*Munia topela*) from China, a Black-necked Swan (*Cygnus nigricollis*) from Antarctic America, purchased.

OUR ASTRONOMICAL COLUMN.

THE RELATIVE MOTION OF 61 CYGNI.—The large proper motion of 61 Cygni, combined with its remarkable duplex character, renders it an object of great interest. Doubts have been expressed, however, as to whether the two components are really connected by a bond of mutual attraction, and it has been assumed that they will gradually separate and traverse widely different paths in space. Prof. A. Hall has brought together all the observations which have been made of the position-angle and distance of the star since 1825, and has investigated them with a view of settling this question (*Astronomical Journal*, No. 258). The result is in favour of the physical connection of the two stars, but all that can be said of the period of revolution is that it is very long. The mass of the brighter star appears to be 3·4 times that of the companion.

THE TEMPERATURE OF THE SUN.—Numerous attempts have been made to determine the sun's temperature, and the results obtained range from 1500° to 5,000,000°. The enormous differences that exist between the different estimates result from the fact that different laws have been assumed to represent the rate of radiation. M. H. Le Chatelier communicated the latest contribution to the subject at the meeting of the Paris Academy of March 28. His experiments show that the intensity of the radiations emitted by an incandescent body of which the emissive power is unity is expressed by the formula—

$$I = 10^{6\cdot7}\,T\,\frac{3210}{T}.$$

The temperatures employed range from 680° to 1770°, and these, with the observed intensity of radiation, have been used to plot a curve. By extending the curve and measuring the intensity of the radiation from the sun, an estimation of 7600° as the effective solar temperature is obtained. The term effective temperature is used to express that temperature which a body having an emissive power equal to unity should possess, in order to send out radiations of the same intensity as the sun. The real temperature of the photosphere is higher than 7600°, because its radiations are absorbed by the cooler solar atmosphere, and it may be, also, because the emissive power of the sun is less than unity.

Comet Swift, March 6.—The following ephemeris for this comet is given in *Astronomische Nachrichten*, No. 3082, for 12h. Berlin mean time:—

1892.		R.A.		Decl.
		h. m. s.		° ′ ″
April 8	...	21 16 41	...	+0 33·6
„ 9	...	21 20 18	...	1 34·2
„ 10	...	21 23 54	...	2 34·3
„ 11	...	21 27 28	...	3 33·8
„ 12	...	21 31 0	...	4 32·7
„ 13	...	21 34 30	...	5 30·9
„ 14	...	21 37 59	...	6 28·4
„ 15	...	21 41 25	...	7 25·2

Wolf's Comet, 1891 II.—In *Astronomische Nachrichten*, No. 3082, an ephemeris for this comet is given by Herr Dr. Thraen, of which the following is an extract (12h. Berlin mean time):—

1892.		R.A.		Decl.
		h. m. s.		° ′ ″
April 8	...	5 53 19·13	...	−0 12 43·2
„ 9	...	5 54 49·39	...	−0 7 21·8
„ 10	...	5 56 19·84	...	−0 2 7·1
„ 11	...	5 57 50·48	...	+0 3 0·9
„ 12	...	5 59 21·29	...	+0 8 2·1
„ 13	...	6 0 52·27	...	+0 12 56·6
„ 14	...	6 2 23·43	...	+0 17 44·3
„ 15	...	6 3 54·78	...	+0 22 25·1

Periodic Perturbations of the Four Inner Planets.—In the astronomical papers which are prepared for the use of the American Ephemeris and Nautical Almanac (vol. iii., part v.), most valuable computations of the periodic perturbations of the longitudes and radii vectores of the four inner planets of the first order as to masses are contributed. Prof. Newcomb, under whose directions these computations were made, tells us in the introductory note that in the preparation of the fundamental data for the new tables, all the coefficients, which are included in the expressions for the general perturbations, were redetermined: the values obtained for them agreed well with those obtained by Leverrier, and prove that their accuracy is placed beyond doubt.

To eliminate any errors that might have been made, duplicate computations were undertaken, and the results of them both are given in the final expressions for the perturbations in longitude. It may be stated that the complete theory is not here published, the secular variations, perturbations of the latitude, and those of long period in the longitude, not being printed, owing to their unfinished state.

N.P.D.'s observed with Greenwich and Washington Transit Circles.—Prof. Newcomb, under whose direction these computations were made, gives in vol. ii. part vi. of the same series of papers just referred to an interesting discussion on the differences that have been found in these observations. Those made with the Greenwich circle cover a period of thirty-six years, from 1851–87, while the Washington observations are included in the years 1866–86. The author has a firm basis here, on which he can rely, for in the former series the same methods of reduction and observation were in use for this entire period without interruption. He inquires first of all into the conclusions which can be gathered from the stability of the instrument, from both direct and reflection observations, and finds that the R–D corrections are mainly due to flexure. The constant of refraction and the possible periodic error due to those in the graduation of the circle are then dealt with, together with corrections for reductions to the equinox during the years 1851–56. The hypothesis of the secular change in the latitude is here considered as too improbable for acceptance with our present data, so that the apparent variations are here supposed to be due to the changes in the instrument or habits of the observers. In the section on the latitude of the Royal Observatory, he finds that the co-latitude derivable from observations of the four polar stars during the period 1877–86 is greater by 0″·31 than that derivable from observations of all circumpolar stars. At the conclusion of this investigation he gives a table showing the corrections to the north polar distances, derived annually from the observations with the Greenwich transit circle, to reduce them to the instrumental standard of the present paper, and the Pulkowa refractions.

Washington Observations, 1887.—All the observations which were made during the year 1887 at the United States Naval Observatory are included in this volume. The introduction, besides giving the report of the Superintendent on the state of the Observatory generally, contains all the detailed information relative to the methods of computing the observations made with the transit circle, meridian transit instrument, and the 26-inch and 9·6-inch equatorials. The principal work of the transit circle during this period has been upon the sun, moon, and planets, and miscellaneous stars. These last, included stars of the American Ephemeris for clock corrections, &c.; stars whose occultations were observed at this Observatory, and by the various American parties that observed the 1874 transit of Venus; those selected for standard stars in the formation of the catalogue made from 1846–49; and stars of the B.A.C. between 120° 0′ and 131° 10′ N.P.D. that have not been observed three times in R.A. and declination at Washington. The meridian transit instrument was devoted to the determination of the errors of the standard mean time clock in connection with the transmission of time, and 1645 transits were taken. The clock's rate was found satisfactory, its variations following closely those of the barometer. The 26-inch and 9·6-inch equatorials have been also used, the former for observations of double stars and small stars in the Pleiades, the latter for comets. Besides these, many other magnetic and meteorological observations are recorded, but a brief account of them will be found in the notes.

FERTILIZATION OF THE CASUARINACEÆ.

Few recent articles in botanical literature can compare in interest and importance with that contributed by Dr. Melchior Treub to the tenth volume of the *Annales du Jardin Botanique de Buitenzorg*, "On the *Casuarinaceæ*, and their Position in the Natural System." The startling announcement is made of the occurrence of a mode of fertilization of the ovule essentially different from that which takes place in other flowering plants.

The species of the genus *Casuarina*, which alone make up the order, are about twenty-three in number, and are trees, nearly all natives of Australia, where they are known as "beef-wood trees," characterized by their jointed, almost leafless branches. From the catkin-like inflorescence of very imperfect flowers, they are generally placed among Incompletæ or Monochlamydeæ, near to Myricaceæ and Juglandaceæ. The female flower is composed of two carpels, without either calyx or corolla, and has at the base an ovarian cavity, in which are formed (in *C. suberosa*) the two ovules with parietal placentation, but connected from the first with its summit by cords of cellulose. Corresponding to the style in most plants, is an axial mass of tissue which M. Treub calls the stylar cylinder, surrounded by a peripheral region containing tracheides, and terminating in two elongated stigmas. The two ovules are unequal in size, and coalesce in their growth by their placental portions; the connection between them and the base of the stylar column is called the bridge; they are also connected with the base of the ovarian cavity by their funicles.

The processes which take place within the ovule up to the time of the formation of the embryo-sac are very different from those hitherto observed in Angiosperms. Several large hypodermal cells, the archespore-cells, at the summit of the nucellus, divide tangentially; and two of the cells thus produced towards the inner side, the primordial mother-cells, divide further, giving rise to a thick cylinder of large cells occupying the centre of the nucellus, the sporogenous tissue, surrounded by flattened cells corresponding to the "Tapetenzellen" of Goebel. The cells of the sporogenous tissue are equivalent to the mother-cells of the embryo-sac in other Angiosperms. These cells divide transversely into large megaspores (macrospores); the small inactive cells become absorbed. In *C. glauca* and *Rumphiana* tracheides are formed, analogous to the elaters of the Hepaticæ; their function is uncertain. The megaspores, or embryo-sacs, of which there are usually from sixteen to twenty, lengthen in the direction of the chalaza, some of them sometimes penetrating and forming "tails" between the elements of the fibrovascular bundle of the funicle. The sister-cells of the embryo-sacs, instead of being absorbed at an early period, as in other Angiosperms, disappear only much later. The megaspores which develop fully divide at the end into two or three cells, which are in most cases naked, and result from the division of a single cell. In the great majority of cases only a single megaspore in each nucellus has these terminal or sexual cells furnished with cell-walls; this is the future embryo-sac. The oosphere is always formed from the sexual cell which has the thickest wall. No antipodals are formed.

Only a single ovule is ever fertilized, and the pollen-grain which fecundates it advances towards the embryo-sac in a way entirely different from anything that occurs in other Phanerogams. The pollen-tube does not enter the ovarian cavity; it descends the stylary cylinder, crosses the bridge and the tissue which unites the ovule with the wall of the ovary, and arrives at the fibrovascular bundle which leads to the chalaza, where it produces two short branches, then traverses the chalaza, and enters the ovule by means of the "tail" of a sterile megaspore, and continues its course towards the embryo-sac. Towards the middle of the nucellus it contracts, tapers off, and ruptures, the terminal fecundating portion becoming separated from the rest of the pollen-tube. This portion, which has a thickened wall, and contains distinct protoplasm, never enters the micropyle or the embryo-sac, but becomes firmly attached to the wall of the latter, at a spot variable in position, but always at some distance from the sexual apparatus. Dr. Treub has not, at present, been able to detect in this portion a definite nucleus, or to follow the actual process of fecundation. During the development of the embryo-sac, numerous endosperm-nuclei are formed, and subsequently the embryo makes its appearance. The mode of development of the embryo does not differ from that which occurs in other Dicotyledons.

The peculiar processes which accompany the act of fecundation, and the presence of a large number of megaspores, each containing a sexual apparatus, induce Dr. Treub to regard the Casuarinaceæ as a distinct group of Angiosperms, of equal rank with the Monocotyledones and Dicotyledones together, and he proposes the following primary classification of Phanerogams:—

I. GYMNOSPERMS.
II. ANGIOSPERMS.
  A. CHALAZOGAMS (Casuarinaceæ).
  B. POROGAMS.
    1. *Monocotyledones.*
    2. *Dicotyledones.*

The Chalazogams are not intermediate between Gymnosperms and Angiosperms, but occupy an isolated and inferior position among the latter, somewhat analogous to that of *Lycopodium* among Vascular Cryptogams. The paper is illustrated by 21 fine plates. A. W. B.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—*Endowment of Original Research.*—The following notice has been received by the Vice-Chancellor:—A gentleman has established a Scholarship of £100, tenable for one year, for the encouragement of original research. The Scholar will be selected by a Committee composed of Dr. George Thin, Surgeon-General Cornish, and Prof. A. Winter-Blyth. The conditions of the Scholarship are, that the research be on a subject requiring for its elucidation both chemical and bacteriological methods, and the subject will be selected by the Committee of Selection. With the concurrence of the Scholar, the work is to be done in the laboratories of the College of State Medicine, 101 Great Russell Street, W.C., and the Scholar will have to devote his whole time to the work. Application to be made to Surgeon-General Cornish, on or before April 18, 1892.

In a Convocation held on April 5, it was decreed (the Council of the Royal Geographical Society having offered a further sum of £150 a year, to be met by an equal sum from the University, for the payment of a Reader in Geography during the next five years) that the offer be accepted, and that the thanks of the University should be conveyed to the Council of the Royal Geographical Society for their liberal offer.

The programme of the fifth summer meeting of University Extension and other students, to be held in Oxford in July and August 1892, has been issued, and in its general character resembles that of last year. The inaugural lecture will be delivered by Mr. John Addington Symonds (if his health permits) on Friday, July 29, at 8.30 p.m. The meeting will, as in former years, be divided into two parts, viz. from July 29 to August 9, and from August 10 to August 26. In Natural Science the following arrangements have been made:—

In Chemistry: a course of eighteen days' practical instruction in the University laboratory, limited to 100 students, conducted by Messrs. J. E. Marsh and A. D. Hall of Balliol College.

In Geology: a special course of fourteen days' practical instruction, with field work provided, if at least 40 students offer themselves.

In Botany: in addition to lectures on primroses and their relations, it is proposed to arrange, for a class of not less than 40 students, a three weeks' course of practical instruction.

In Biology: to the same minimum number of students is offered a special course of lectures and demonstrations in the physiological laboratory, to form an introduction to the study of life, and especially of nervous organisms.

Courses of lectures and instruction on Astronomy, Mechanics, Sound, Light and Heat, Electricity, Physiography, and Hygiene can be arranged.

It is also announced that there will be no summer meeting in 1893, as during August in that year the Examination Schools will be in the hands of workpeople.

ST. ANDREWS.—*Summer Session.*—A course of lectures in zoology and botany, qualifying for graduation, will commence on May 2, the former by Prof. Prince, the latter by Mr. Robertson, the University Lecturer on Botany. These are open to students of either sex.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 31.—"Aberration Problems: a Discussion concerning the Connection between Ether and Matter, and the Motion of the Ether near the Earth." By Oliver Lodge, F.R.S., Professor of Physics, University College, Liverpool.

The paper begins by recognizing the distinction between ether in free space and ether as modified by transparent matter, and points out that the modified ether, or at least the modification, necessarily travels with the matter. The well-known hypothesis of Fresnel is discussed and re-stated in modern form.

Of its two parts, one has been verified by the experiment of Fizeau, the other has not yet been verified. Its two parts are, (1) that inside transparent matter the velocity of light is affected by the motion of that matter, and (2) that immediately outside moving matter there is no such effect. The author proceeds to examine into the truth of this second part, (1) by discussing what is already known, (2) by fresh experiment.

The phenomena resulting from motion are four, viz.:—

(1) Changes in direction, observed by telescope and called aberration.

(2) Change in frequency, observed by spectroscope and called Doppler effect.

(3) Change in time of journey, observed by lag of phase or shift of interference bands.

(4) Change in intensity, observed by energy received by thermopile.

After a discussion of the effects of motion in general, which differ according as projectiles or waves are contemplated, the case of a fixed source in a moving medium is considered; then of a moving source in a fixed medium; then the case of medium alone moving past source and receiver; and, finally, of the receiver only moving.

It is found that the medium alone moving causes no change in direction, no change in frequency, no detectable lag of phase, and probably no change of intensity; and hence arises the difficulty of ascertaining whether the general body of the ether is moving relatively to the earth or not.

A clear distinction has to be drawn, however, between the effect of general motion of the medium as a whole, and motion of parts of the medium, as when dense matter is artificially moved. The latter kind of motion may produce many effects which the former cannot.

A summary of this part of the discussion is as follows :—

Source alone moving produces a real and apparent change of colour; a real but not apparent error in direction; no lag of phase, except that appropriate to altered wave-length; a change of intensity corresponding to different wave-lengths.

Medium alone moving, or source and receiver moving together, gives no change of colour; no change of direction; a real lag of phase, but undetectable without control over the medium; a change of intensity corresponding to different distances but compensated by change of radiating power.

Receiver alone moving gives an apparent change of colour; an apparent change of direction; no change of phase, except that appropriate to extra virtual speed of light; change of intensity corresponding to different virtual velocity of light.

The probable absence of a first order effect of any kind, due to ethereal drift or relative motion between earth and ether, makes it necessary to attend to second order effects.

The principle of least time is applied, after the manner of Lorentz, to define a ray rigorously, and to display the effect of existence or non-existence of a velocity potential. Fresnel's law is seen to be equivalent to extending the velocity potential throughout all transparent matter.

It is shown that a ray traversing space or transparent substances will retain its shape, whatever the motion of the medium, so long as that motion is irrotational, and that in that case the apparent direction of objects depends simply on motion of observer; but, on the other hand, that if the earth drags with it some of the ether in its neighbourhood, stellar rays will be curved, and astronomical aberration will be a function of latitude and time of day.

The experiment of Boscovich, Airy, and Hoek, as to the effect of filling a telescope-tube with water, does not discriminate between these theories. For if the ether is entirely non-viscous and has a velocity potential, stellar rays continue straight, in spite of change of medium (or at oblique incidence are refracted in the simple manner), and there will be no fresh effect due to change of medium; while, if, on the contrary, the ether is all carried along near the earth, then it is stationary in a telescope tube, whether that be filled with water or air, and likewise no effect is to be expected. In the case of a viscous ether, all the difficulty of aberration must be attacked in the upper layers above the earth; all the bending is over by the time the surface is reached. It is difficult to see how an ethereal drift will not tend to cause an aberration in the wrong direction.

Of the experiments hitherto made by Arago, Babinet, Maxwell, Mascart, Hoek, and perhaps others, though all necessary to be tried, not one really discriminates between the rival hypotheses. All are consistent either with absolute quiescence of ether near moving bodies, or with relative quiescence near the earth's surface. They may be said, perhaps, to be inconsistent with any intermediate position.

Two others, however, do appear to discriminate, viz. an old and difficult polarization experiment of Fizeau (*Ann. de Chim. et de Phys*, 1859), which has not been repeated since, and the recent famous experiment of Michelson (*Phil. Mag.*, 1887) with rays made to interfere after traversing and retraversing paths at right angles.

The conclusions deducible from these two experiments are antagonistic. Fizeau's appears to uphold absolute rest of ether; Michelson's upholds relative rest, *i.e.* drag by the earth.

The author now attempts a direct experiment as to the effect of moving matter on the velocity of light in its neighbourhood; assuming that a positive or negative result with regard to the effect of motion on the velocity of light will be accepted as equivalent to a positive or negative result with respect to the motion of the ether.

He gives a detailed account of the experiment, the result of which is to show that such a mass as a pair of circular saws clamped together does not whirl the ether between the plates to any appreciable amount, not so much, for instance, as a 1/500th part of their speed. He concludes, therefore, that the ether is not appreciably viscous. But, nevertheless, it may perhaps be argued that enormous masses may act upon it gravitationally, straining it so as perhaps to produce the same sort of effect as if they dragged it with them. He proposes to try the effect of a larger mass. Also to see if, when subject to a strong magnetic field, ether can be dragged by matter.

The aberrational effect of slabs of moving transparent matter is considered, also the effect of a differently refractive medium.

Motion of medium, though incompetent to produce any aberrational or Doppler effect, is shown to be able to slightly modify them if otherwise produced.

The Doppler effect is then entered into. The question is discussed as to what the deviation produced by a prism or a grating really depends on: whether on frequency or wave-length. It is shown that whereas the effect of a grating must be independent of its motion and depend on wave-length alone, yet that the effect observed with a moving grating by a moving observer depends on frequency, because the motion of the observer superposes an aberrational effect on the true effect of the grating. This suggests a means of discriminating motion of source from motion of observer; in other words, of detecting absolute motion through ether; but the smallness of the difference is not hopeful.

Michelson's experiment is then discussed in detail, as a case of normal reflection from a moving mirror or from a mirror in a drifting medium. No error in its theory is discovered.

The subjects of change of phase, of energy, of reflection in a moving medium, work done on a moving mirror, and the laws of reflection and refraction as modified by motion, are considered.

It is found that the law of reflection is not really obeyed in a relatively moving medium, though to an observer stationary with respect to the mirror it appears to be obeyed, so far as the first order of aberration magnitude is concerned; but that there is a residual discrepancy involving even powers of aberration magnitude, of an amount possibly capable of being detected by very delicate observation.

The following statements are made and justified :—

(1) The planes of incidence and reflection are always the same.

(2) The angles of incidence and reflection, measured between ray and normal to surface, usually differ.

(3) If the mirror is stationary and medium moving, they differ by a quantity depending on the square of aberration magnitude, *i.e.* by 1 part in 100,000,000; and a stationary telescope, if delicate enough, might show the effect.

(4) If the medium is moving and mirror stationary, the angles differ by a quantity depending on the first power of aberration magnitude (1 part in 10,000), but a telescope moving with the mirror will not be able to observe it; for the commonplace aberration caused by motion of receiver will obliterate the odd powers and leave only the even ones; the same as in case (3).

(5) As regards the angles which the incident and reflected *waves* make with the surface, they differ in case (3) by a first order magnitude, in case (4) by a second order magnitude.

(6) At grazing incidence the ordinary laws are accurately obeyed. At normal incidence the error is a maximum.

(7) The ordinary laws are obeyed when the direction of drift is either tangential or normal to the mirror, and is disobeyed most when the drift is at 45°.

(8) In general, the shape of the incident wave is not precisely preserved after reflection in a moving medium. To a parallel beam the mirror acts as if slightly tilted; to a conical beam as if slightly curved. But either effect, as observable in the result, is almost hopelessly small.

(9) Similar statements are true for refraction, assuming Fresnel's law.

The possibility of obtaining first order effects from general ethereal motion by means of electrical observations is considered.

Chemical Society, March 17.—Dr. W. J. Russell, F.R.S. Vice-President, in the chair.—The following papers were read:—A study of the conditions which determine combination between the cyanides of zinc and mercury, and of the composition and properties of the resulting double salt, by W. R. Dunstan. When a solution of zinc sulphate is added to one of mercuric potassium cyanide, $HgK_2(CN)_4$, or when mercuric chloride is added to a solution of zinc potassium cyanide, $ZnK_2(CN)_4$, a white precipitate is formed, which has been stated, on the authority of Gmelin, to consist of a double cyanide of zinc and mercury of the formula $ZnHg(CN)_4$. This, however, is not the case. The maximum amount of mercuric cyanide that can be retained by the precipitate is only 38·5 per cent., and is dependent on the amount of water present during precipitation as well as on the proportions in which the salts interact. When washed with cold water the precipitate loses a large proportion, though not all, of the mercuric cyanide contained in it. Boiling water and cold potassium iodide solution extract the mercuric cyanide more readily. Experiments have been made in which the relative masses of the interacting substances were varied, these experiments prove that a true compound of the two cyanides is formed, and suffers decomposition to a greater or less extent, depending on the amount of water present. An examination of the curves plotted from these results leads to the inference that the double salt is a tetrazincic monomercuridecacyanide, $Zn_4Hg(CN)_{10}$.—A lecture experiment to illustrate the phenomena of coal-dust explosions, by T. E. Thorpe. The author describes an apparatus by means of which the phenomena of a coal-dust explosion, resulting either from a local explosion of fire-damp or by the direct action of a blown-out shot, may be illustrated. The apparatus consists of a long narrow wooden box having an explosion chamber at one end; a thin layer of fine coal-dust or lycopodium powder is spread along the bottom of the box. On firing a mixture of coal-gas and air in the explosion chamber, the explosive wave sweeps along the box with increasing strength until it shoots out at the open end of the apparatus. By observations made with this apparatus the author finds that there is no evidence of a diminution of pressure along the sides of the space through which the flame rushes, and he is of opinion that there is no experimental proof of the validity of the "suction theory," which assumes that in consequence of this alleged diminution of pressure, occluded fire-damp is drawn out from the coal, and contributes to the violence of the explosion.—The production of the ketone, 1 : 2 : 4 acetylorthoxylene from camphor by the action of sulphuric acid and zinc chloride, by H. E. Armstrong and F. S. Kipping. The authors have previously stated that they have separated a ketone of the composition $C_9H_{12}O$ from the crude product of the action of sulphuric acid on camphor. On treatment with bromine the ketone yields a compound which readily decomposes, giving a monobromo-derivative, $C_9H_{11}BrO$, melting at 63°–64°. When oxidized with dilute nitric acid, the ketone yields two acids, separable by means of chloroform. One of these proves to be paraxylic acid, viz. 1 : 2 : 4 dimethylbenzoic acid, whilst the other is xylidinic or 1 : 2 : 4 methylisophthalic acid. The ketone is therefore 1 : 2 : 4 acetylorthoxylene, a compound which Claus has synthesized from acetic chloride and orthoxylene in presence of aluminium chloride.—Platinum tetrachloride, by W. Pullinger. The author has obtained platinum tetrachloride by heating hydrated hydrogen platinic chloride in a current of dry hydrogen chloride at 163° for fifteen hours. When thus prepared, it is a very soluble, but not deliquescent, substance.—Note on a new acid from camphoric acid, by W. H. Perkin, Jun. When warmed with sulphuric acid, camphoric acid is converted into sulphocamphoric acid, with loss of water and carbon monoxide, $C_{10}H_{16}O_4 + H_2SO_4 = C_9H_{16}SO_6 + CO + H_2O$. Kachler found that, when fused with potash, sulphocamphoric acid yields a crystalline substance, $C_9H_{12}O_2$, melting at 148°, which is apparently not an acid. The author in repeating Kachler's experiments, but sulphonating at 100° instead of at 65°, obtained a well-characterized monobasic acid, $C_9H_{12}O_2$, isomeric with this substance and melting at 108°. It would appear from these results that the acid obtained by sulphonating camphoric acid at 100° is isomeric with ordinary sulphocamphoric acid.—The specific rotatory and cupric reducing power of invert sugar and of dextrose obtained from cane sugar by means of invertase, by J. O'Sullivan. The author describes experiments in which the hydrolysis of cane sugar was effected by means of invertase instead of by means of acid. The specific rotatory power of invert sugar obtained by means of invertase, which has no action on lævulose, is $[\alpha]_j = -24°·5$, and that of the dextrose prepared from such invert sugar is $[\alpha]_j = 57°$. The apparent specific rotatory power of lævulose calculated from these numbers is $[\alpha]_j = -106°$, or $[\alpha]_D = -93°·8$, a value agreeing with that generally accepted.—Ethyldimethylamidobenzene, by W. R. Hodgkinson and L. Limpach. This amine is prepared by heating paraxylidine hydrochloride with ethyl alcohol at 250°–300°. It is purified from diethyldimethylamidobenzene by crystallization of the sulphates. The sulphate of the latter substance is the more soluble. The formyl and acetyl derivatives of the amine are described.—Action of nitric acid on oxanilide and its analogues, by A. G. Perkin. The author finds that oxanilide and its analogues are readily converted by nitration into the higher nitro-derivatives, thus differing from acetanilide and similar compounds, which yield dinitro-derivatives only with great difficulty.

Royal Meteorological Society, March 16.—Dr. C. Theodore Williams, the President, delivered an address on the value of meteorological instruments in the selection of health resorts. He drew attention to thermometers, maximum and minimum, as the foundation-stone on which medical climatology rests, and instanced effects of extreme cold or of heat on the human organism. The direct rays of the sun are of the greatest importance, and in health resorts should be utilized to the full—in fact, only climates where during the winter months even a delicate person can lie or sit for several hours a day basking in the sunshine are to be recommended for most complaints, and the various forms of sunshine-recorders are used to aid the medical adviser in the choice of such health stations. After referring to the value of rain gauges, hygrometers, and barometers, Dr. Williams stated that many health resorts owe their reputation almost solely to their shelter from cold winds; for instance, the advantage in climate which Hyères and Mentone enjoy over Marseilles is chiefly due to their being more sheltered from the Mistral, or north-west wind, the scourge of the lower valley of the Rhone from Valence to Avignon. He went on to describe the climate of the Riviera, illustrating it by lantern slides from recent photographs, including views of Hyères, Costabelle, Cannes, Nice, Mentone, San Remo, &c., and he showed the three principal causes of the warm winter of this region to be (1) the southern latitude, (2) the protection from cold winds by mountain ranges, and (3) the equalizing and warming influence of the Mediterranean Sea, which, being practically tideless, is always equally potent, not varying with hour and season. Dr. Williams mentioned the weak points of the south of France climate, with its blustering Mistral, its occasional cold Bise, its moist Scirocco wind; but summed up the Riviera winter climate as being, as a whole, clear, bright, and dry, with fog and mist practically unknown, with a winter temperature from 8° to 10° higher than England though subject to considerable nocturnal radiation, with about half the number of rainy days, and four or five times the number of bright ones, which we can boast of, with cold winds and cold weather, without which it would lose its health-giving effect.—After the delivery of this address the meeting was adjourned in order to allow the Fellows and their friends an opportunity to inspect the Exhibition of Instruments relating to climatology, which had been arranged in the rooms of the Institution of Civil Engineers, 25 Great George Street. The Meteorological Office showed a set of instruments necessary for the equipment of a climatological station, viz. Stevenson thermometer screen, fitted with dry bulb, wet bulb, maximum and minimum thermometers, and also a rain gauge. Thermometers were also shown for ascertaining the temperature on the ground, under the ground, and at a distance, as well as for recording temperature continuously. Various forms of sunshine-recorders were exhibited, as well as a number of actinometers and solar radiation instruments for ascertaining the heating effect of the solar rays. The Exhibition included a large and interesting collection of hygrometers, also several rain-gauges and other instruments. Among the curiosities was a piece of plate glass, which was "starred" during a thunderstorm on August 21, 1879; this was not broken, but it has a number of wavy hair-like lines. The Exhibition contained a large number of beautiful photographs of clouds, lightning, and snow scenes, as well as of the damage done by the destructive tornado at Lawrence, Mass., U.S.A. The Exhibition remained open until Tuesday, the 22nd ult.

Anthropological Institute, March 22.—Francis Galton, F.R.S., Vice-President, in the chair.—Mr. Theodore Bent read a paper on the finds at the Great Zimbabwe ruins. The outer wall of the semicircular temple on the hill is decorated by a number of birds perched on long soapstone pedestals, all of which appear to be intended to represent the same bird, probably a vulture. Two of the birds, similar in character and slightly varying from the others, are represented as perched on zones or cesti, and there seems to be a similar class of symbolism connecting them all. Mr. Bent is of opinion that these birds represent the Assyrian Astarte or Venus—the female element in creation. In the centre of the temple stood an altar, into the stones of which were inserted a large number of soapstone objects, which afforded ample evidence of the existence of phallic worship in this place. Within the sacred inclosure are two solid round towers, the largest of which is 34 feet in height and has a girth of 53 feet. Before them is a raised platform, presumably for sacrifice, and the wall behind them is decorated with large standing monoliths. Some of the fragments of pottery found are very good, and give evidence of a highly developed artistic skill. Close underneath the temple stood a gold-smelting furnace, made of very hard cement of powdered granite, with a chimney of the same material, and the quantity of rejected quartz found hard by proved that these ruins had formed the fortress for the protection of a gold-producing people. The ruins and the things in them are not in any way connected with any known African race; the objects of art and of special cult are foreign altogether to the country, and neither the date of construction nor the race of the builders can now be determined with accuracy; but the evidence in favour of this race being one of the many tribes of Arabia is very strong, and all the facts point to a remote antiquity.

PARIS.

Academy of Sciences, March 28.—M. d'Abbadie in the chair.—Note on a theorem on the calculation of probabilities, by M. J. Bertrand.—On the periodic variations of latitude, according to a letter from M. Helmert to the members of the Permanent Commission of the International Geodetic Association, by M. Faye. (See Our Astronomical Column.)—On the approximate theoretical calculation of the delivery from an orifice in a thin wall, by M. J. Boussinesq.—On the population of the five continents of the earth, by M. Émile Levasseur. A comparison of M. Levasseur's estimations with those given by others shows that the differences are greater for Africa, Asia, Oceania, and America, than for Europe. This is what would be expected. M. Levasseur's numbers are as follows:—

	Area, in millions of square kilometres.		Population, in millions.
Europe	10·0		360
Africa	30·5		153
Asia	42·2		824
Oceania	11·1		38
North America	23·7	42·4	88
South America	18·7		34
Total	136·2		1497

—Note on a theory on the production of various vegetable galls, by M. A. Laboulbène.—Mechanical laws of atmospheric circulation; surfaces of equal density; squalls; secondary and general circulations, by M. Le Goarant de Tromelin.—Observations of Swift's comet (Rochester, March 6, 1892) and of the minor planet discovered by Wolf on March 18, made with the East Tower equatorial of Paris Observatory, by Mdlle. D. Klumpke. Observations for position were made on March 17, 21, 23, and 24.—Observations of Swift's comet made at Toulouse Observatory, by M. B. Baillaud. Observations for position were made on March 16, 18, 19, 21, and 25.—Observations of Wolf's periodic comet made with the great telescope of Toulouse Observatory, by MM. E. Cosserat and F. Rossard. Dates of observations for position: November 28, December 1, 4, 21, 22, 26, and 31.—On plane *réseaux* having equal invariants, by M. G. Kœnigs.—On congruences of which the mean surface is a plane, by M. C. Guichard.—On the existence of integrals in differential systems, by M. Riquier.—An electro-ballistic chronograph, by M. W. Schmidt.—On the radiations of incandescent bodies and the optical measure of high temperatures, by M. J. Violle. By estimating the intensities of the lines at C and D in the radiations of a piece of platinum, the author has determined the temperature of the metal. His results agree very well with those obtained by M. Le Chatelier up to 1500°.—On the temperature of the sun, by M. H. Le Chatelier. (See Our Astronomical Column.)—Application of the theory of lines of force to the demonstration of an electrostratic theorem, by M. L. de la Rive.—On electro-capillary phenomena, by M. A. Berget.—On a safety-lamp for use with coal gas, by M. F. Parmentier. The author records some experiments on the action of platinum wires and crucibles in cooling flames below the temperature necessary for the combustion of the gases.—Action of potassium fluoride on anhydrous chlorides; preparation of anhydrous fluorides of nickel and potassium, and of cobalt and potassium, by M. C. Poulenc. The compounds prepared have the composition $NiKF_3$ and $CoKF_3$. Full descriptions are given of the mode of preparation and the properties of the new substances.—On the fixation of iodine by starch, by M. G. Rouvier.—On the estimation of fluorine, by M. Ad. Carnot.—On the aldehydes and acetone bromides which result from the action of bromine on alcohols of the fatty group, by M. A. Étard.—On propylamines and some of their derivatives, by M. F. Chancel.—On some reactions of isomeric amido-benzoic acids, by M. Oechsner de Coninck.—Study of the velocity of decomposition of diazo-compounds, by MM. J. Hausser and P. Th. Muller.—On two fluorhydrines of glycerine, by M. Maurice Meslans. (See Notes.)—On the mode of union of rings of the abdomen (zigzag articulation) of Hymenoptera, by M. G. Carlet.—On the embryonic development of the Galatheida of the genus Diptychus, by M. E. L. Bouvier.—On the histology of the pituitary gland, by M. G. Saint-Remy.—On the blue colouring matter in the blood of Crustacea, by M. F. Heim.—On a new marine Rhizopod (*Pontomyxa flava*, g. et sp. n.), by M. E. Topsent.—The streptonary nervous system of Heteropods, by M. Paul Pelseneer.—Observations on *l'anthracnose maculée*, by M. Louis Mangin.—On the artificial culture of Diatomaceæ, by M. P. Miquel.—On the crystalline rocks of Chablais, by M. Michel-Lévy.—The Saint-Béat marble, its age and stratigraphical relations, by M. Caralp.—On some minimum perceptible quantities of certain odours, by M. Jacques Passy.—Difference in the functions exercised on the bladder by the afferent nerves of the hypogastric plexus, by M. Lannegrace.—On the Martinique cyclone of August 18, 1891, by M. G. Landes.—Magnetic disturbances and seismic phenomena, by M. Émile Rivière.

BOOKS and PAMPHLETS RECEIVED.

BOOKS.—Index of Meteorological Observations in the United States (Washington).—Essex Institute Historical Collections, vol. xxvii. (Salem, Mass.)—A New Course of Experimental Chemistry: J. Castell-Evans (Murby).—Souvenir of Shakespeare's King Henry the Eighth (*Black and White*).—Deutsches Meteorologisches Jahrbuch für 1890 (Hamburg).—Island Life, 2nd edition: A. R. Wallace (Macmillan).—A Naturalist in the Transvaal: W. L. Distant (Porter).—The Clyde Sea Area: Dr. H. R. Mill (Williams and Norgate).—Live Stock: Prof. J. Wrightson (Cassell).—The Great Earthquake in Japan, 1891: J. Milne and W. K. Burton (Stanford).

PAMPHLETS.—Azimut Assoluto del Segnale Trigonometrico di Monte Vesco sull' orizzonte di Torino: F. Porro (Torino).—Ergebnisse der Meteorologischen Beobachtungen im Systeme der Deutsche Seewarte für das Lustrum 1886–90 (Hamburg).

CONTENTS.