THURSDAY, JUNE 22, 1882

CHARLES DARWIN[1]

V.

THE effects upon Psychology of Mr. Darwin's writings have been so immense, that we shall not overstate them by saying that they are fully comparable with those which we have previously considered as having been exerted by the same writings on geology, botany, and zoology. This fact at first sight can scarcely fail to strike us as remarkable, in view of the consideration that Mr. Darwin was not only not himself a psychologist, but had little aptitude for, and perhaps less sympathy with, the technique of psychological method. The whole constitution of his mind was opposed to the subtlety of the distinctions and the mysticism of the conceptions which this technique so frequently involves; and therefore he was accustomed to regard the problems of mind in the same broad and general light that he regarded all the other problems of nature. But if at first sight we are inclined to feel surprised that, although possessing none of the special mental equipments of a psychologist, he should have produced so enormous an influence upon psychology, our surprise must vanish when we consider the matter a little more attentively. For the truth of this matter is that psychology, in being the science furthest removed from the reach of experimental means and inductive method, is the science which has longest remained in the trammels of *a priori* analysis and metaphysical thought; therefore Darwin, by casting the eye of a philosophical naturalist upon the facts, without reference to the cobwebs which the specialists had woven around them, was able to gather directly much new information as to their meaning. And the rare sagacity with which he observed and reflected upon the phenomena of mind merely as phenomena or facts of nature, led to the remarkable results which we shall presently have to consider—results which have done more than any other to unmuffle the young science of psychology from the swaddling clothes of its mediæval nursery.

The portions of Mr. Darwin's writings which refer to mental science are very limited in extent—comprising, in fact, only one chapter in the "Origin of Species," three in the "Descent of Man," and a short paper on the development of infantile intelligence. The importance of the effect produced by them is therefore rendered all the more remarkable; but in this connection it seems desirable to state that the chapters to which we have alluded represent, in an exceedingly condensed form, the result of extensive thought and reading. A year or two ago Mr. Darwin lent the present writer the original drafts of these essays, together with all the notes and memoranda which he had collected on psychological subjects during the previous forty years, and so we can testify that any one who reads these MSS. is more likely to be surprised at the amount of labour which they indicate than at the effect which has been produced by the compressed publication of its results. What strikes one most in reading the MSS. is that which also strikes one most in reading the published *résumé* that has grown out of them—namely, the honest adherence throughout to the strictly scientific, or, as the followers of Comte would say positive method of seeking and interpreting facts; speculation, hypothesis, and straw-splitting are everywhere, not so much intentionally avoided, as alien to the whole conception of the manner in which the sundry problems are to be attacked. We all know that this conception has not met with universal approval—that more than one writer, adhering to the traditional methods of psychological inquiry, has expressly joined issue upon it. But although it is an easy matter for a technical psychologist to point to an absence of technical thought, and so of a recognition of technical principles, in these parts of Mr. Darwin's writings, we are persuaded that the *exposé* only serves to reveal a beam in the eye of the technical psychologist which prevents him from seeing clearly how to remove the mote from Mr. Darwin's. In other words, although it is true that Mr. Darwin does not recognise the niceties of distinction which seem so important to what we may term the professional mind, it is no less true that in the cases to which we have alluded, the professional mind has failed in its duty of filling up for itself the technical *lacunæ* in Mr. Darwin's expositions. Such *lacunæ* no doubt occur, but they never really vitiate the integrity of the conclusions; and a trained psychologist would best fulfil his function as an under-builder, by supplying here and there the stones which the hand of the master has neglected to put in. To ourselves it always seems one of the most wonderful of the many wonderful aspects of Mr. Darwin's varied work, that by the sheer force of some exalted kind of common sense, unassisted by any special acquaintance with psychological methods, he should have been able to strike, as it were, straight down upon some of the most important truths which have ever been brought to light in the region of mental science. These we shall now proceed to consider.

The chapter in the "Origin of Species" to which we have referred, is occupied chiefly with an application of the theory of natural selection to the phenomena of instinct, and in our opinion it has done more than all other psychological writings put together to explain what instinct is, why it is, and how it came to be. Before this chapter was published, the only scientific theory concerning the origin of instincts that had been formed was the theory which regarded them as hereditary habits. Because we know that in the individual intelligent adjustments become, by frequent repetition, automatic, it was inferred that the same might be true of the species, and therefore that all instincts were to be regarded as what Lewes has aptly termed "lapsed intelligence." In this view there is, without any question, much truth, and the first thing we have to notice about Mr. Darwin's writings with reference to instinct is that they not only recognised this truth, but, by elucidating the whole subject of heredity, placed it in a much clearer light than it ever stood before. Mr. Darwin, however, carried the philosophy of the subject very much further when he argued that, in conjunction with the cause formulated as "lapsing intelligence," there was another at least as potent in the formation of instincts—namely, natural selection. His own statement of the case is so terse that we cannot do better than quote it.

"If Mozart, instead of playing the pianoforte at three

[1] Concluded from p. 147.

years with wonderfully little practice, had played a tune with no practice at all, he might truly be said to have done so instinctively. But it would be a serious error to suppose that the greater number of instincts have been acquired by habit in one generation, and then transmitted by inheritance to succeeding generations. It can be clearly shown that the most wonderful instincts with which we are acquainted, namely, those of the hive-bee and of many ants, could not possibly have been acquired by habit.[1]

"It will be universally admitted that instincts are as important as corporeal structures for the welfare of each species, under its present conditions of life. Under changed conditions of life, it is at least possible that slight modifications of instinct might be profitable to a species; and if it can be shown that instincts do vary ever so little, then I can see no difficulty in natural selection preserving and continually accumulating variations of instinct to any extent that was profitable. It is thus, I believe, that all the most complex and wonderful instincts have originated."

Briefly, then, in Mr. Darwin's view instincts may arise by lapsing intelligence, by natural selection of accidental and possibly non-intelligent variations of habit, or by both principles combined—seeing that "a little dose of judgment" is often commingled with even the most fixed (or most strongly inherited) instincts. One good test of the truth of the view as a whole is that which Mr. Darwin has himself supplied—namely, searching through the whole range of instincts to see whether any occur which are either injurious to the animals exhibiting them, or beneficial only to other animals. Now there is really no authentic case of the former, and the latter are so few in number that they may reasonably be regarded, either as rudiments of instincts once useful (so analogous to the human tail), or as still useful in some unobservable manner (so analogous to the tail of the rattlesnake). The case of aphides secreting honey-dew for the benefit of ants occurred to Mr. Darwin as one which might be adduced against his theory in this connection, and he therefore made some experiments upon the subject, which led him to conclude that "as the excretion is extremely viscid, it is no doubt a convenience to the aphides to have it removed; therefore probably they do not excrete solely for the good of the ants."

A discussion of the variability of instinct, and of the probability that variations should be inherited, leads him to consider the important case of the apparent formation of artificial instincts in our domestic dogs by continued training with selection, and also the not less important case of the effects produced upon natural instincts by the long-continued change of environment to which other of our domestic animals have been exposed. All the facts adduced as resulting from these long-continued though unintentional experiments by man, go to substantiate, in a very unmistakable manner, the theory concerning the origin and development of instincts which we are considering. The chapter concludes with a close consideration of some of the more remarkable instincts which occur in the animal kingdom, such as the parasitic instinct of the cuckoo, the slave-making instinct of ants, and the cell-making instinct of bees. A flood of light is thrown upon the latter, and the old-standing problem as to how the bees have come to make their cells in the form which requires the smallest amount of material for their construction, while affording the largest capacity for purposes of storage, is solved.

From this brief account of the chapter on "Instinct," it is evident that the new idea which it starts, and in several directions elaborates, is an idea of immense importance to psychology, and that the broad marks or general principles laid down by it afford large scope for a further filling in of numberless details by the attentive observation of facts. The phenomena of instinct, indeed, cease to be rebellious to explanation, and range themselves in orderly array under the flag of science.

But not less important than the chapter on "Instinct" are the chapters in the "Descent of Man" on the mental powers of man as compared with those of the lower animals, on the moral sense, and on the development of both during primæval and civilised times. Our estimate of the value of these chapters is so high that we gladly endorse the opinion of the late Prof. Clifford—who was no mean judge upon such matters—when he writes of them as presenting to his mind "the simplest, and clearest, and most profound philosophy that was ever written upon this subject." As the three chapters together cover only 80 pages, it seems needless to render an abstract of them, so we shall only observe that although it is easy to show in them, as Mr. Mivart and others have shown, a want of appreciation of technical terms, and even of Aristotelian ideas, nowhere in the whole range of Mr. Darwin's writings is his immense power of judicious generalisation more conspicuously shown. So much is this the case that in studying these chapters we have ourselves always felt glad that Mr. Darwin was not the specialist in psychology which some of his critics seem to suppose that he ought to have been if he presumed to shake their science to its base; had he been such a specialist the great sweep of his thought might have been hindered by comparatively immaterial details.

Of the three chapters which we are considering, the most important is the one on the moral sense. As he himself says:—

"This great question (the origin of the moral sense) has been discussed by many writers of consummate ability; and my only excuse for touching upon it, is the impossibility of here passing it over; and because, so far as I know, no one has approached it exclusively from the side of natural history. The investigation possesses, also, some independent interest, as an attempt to see how far the study of the lower animals throws light on one of the highest psychical faculties of man."

The result of this investigation and study has been to give, if not a new point of departure to the science of ethics, at least a completely new conception as to the origin of the faculties with which that science has to deal; and without attempting to discuss the objections which have been raised against the doctrine, or to enumerate the points of contact between this doctrine and older ethical theories—to neither of which undertakings would our present space be adapted—we may say in general that, as in the case of instinct, so in that of conscience, we feel persuaded that Mr. Darwin's genius has been the first to bring within the grasp of human understanding

[1] Because the individuals which exhibit them, being neuters, can never have progeny. It is indeed surprising, as Mr. Darwin further on observes, that no one previously "advanced this demonstrative case of neuter insects against the well-known doctrine of inherited habit as advanced by Lamarck."

large classes of phenomena which had been previously wholly unintelligible.

"The Expression of the Emotions in Man and Animals" is an essay which may be more suitably mentioned in the present article than in any of the preceding. The work is a highly interesting one, not only on account of its philosophical theories, but also as an extensive accumulation of facts. "The three chief principles" enunciated by the former are: (1) "the principle of serviceable associated habits"; (2) "the principle of antithesis"; and (3) "the principle of actions due to the constitution of the Nervous System, independently from the first of the Will, and independently to a certain extent of Habit." It is shown that the first of these principles leads to the performance of actions expressive of emotions because "certain complex actions are of direct or indirect service under certain states of mind, in order to relieve or gratify certain sensations, desires, &c.; and whenever the same state of mind is induced, however feebly, there is a tendency through the force of habit and association for the same movements to be performed, though they may not then be of the least use." The second principle arises because, "when a directly opposite state of mind is induced, there is a strong and involuntary tendency to the performance of movements of a directly opposite nature, though these are of no use; and such movements are in some cases highly expressive." And the third principle occurs because "when the sensorium is strongly excited, nerve-force is generated in excess, and is transmitted in certain definite directions, depending on the connection of the nerve-cells, and partly on habit." All these principles are more or less well substantiated by large bodies of facts, and although the essay, from the nature of its subject-matter, is necessarily not of so transforming a character in psychology as those which we have already considered, and although we may doubt whether it gives a full explanation of every display of expressive movement, we think there can be no reasonable question that the three principles above quoted are shown to be true principles, and therefore that the essay is completely successful within the scope of its purposes.

Lastly, we have to allude to the brief paper published in *Mind* on the psychogenesis of a child. These notes were not published till long after they were taken, so that Mr. Darwin was the first observer, by many years, in a department of psychology which—owing chiefly to the attention which his other writings have directed to the phenomena of evolution—is now being very fully explored. The observations relate entirely to matters of fact, and display the same qualities of thoughtfulness and accuracy which are so conspicuous in all his other work.

On the whole, then, we must say that Mr. Darwin has left as broad and deep a mark upon Psychology as he has upon Geology, Botany, and Zoology. Groups of facts which previously seemed to be separate, are now seen to be bound together in the most intimate manner; and some of what must be regarded as the first principles of the science, hitherto unsuspected, have been brought to light. No longer is it enough to say that such and such actions are the result of instinct, and so beyond the reach of explanation; for now the very thing to be explained is the character and origin of the instinct—the causes which led to its development, its continuance, its precision, and its use. No longer is it enough to consider the instincts manifested by an animal, or group of animals, as an isolated body of phenomena, devoid of any scientific meaning because standing out of relation to any known causes; for now the whole scientific import of instincts as manifested by one animal depends on the degree in which they are connected by general principles of causation with the instincts that are manifested by other animals. And not only in respect of instincts, but also in respect of intelligence, the science of comparative psychology may be said for the first time really to have begun with the discovery of the general causes in question; while from the simplest reflex actions, up to the most recondite processes of reason and the most imperious dictates of conscience, we are able to trace a continuity of development. A revelation of truth so extensive as this in the department of science which, in most nearly touching the personality of man, is of most importance for man to explore, cannot fail to justify the anticipations of the revealer, who in referring to psychology, could "in the future see open fields for far more important researches" than those relating to geology and biology. If the proper study of mankind is man, Mr. Darwin has done more than any other human being to further the most desirable kind of learning, for it is through him that humanity in our generation has first been able to begin its response to the precept of antiquity —*know thyself*.

The series of brief articles whereby we have endeavoured to take a sort of bird's eye view of Mr. Darwin's great and many labours have now drawn to a close. But we cannot finish this very rudimentary sketch of his work without alluding once more to what was said in the opening paragraphs of the series, and which cannot be more terseley repeated than in Mr. Darwin's own words there quoted with reference to Prof. Henslow:—"Reflecting over his character with gratitude and reverence, his moral attributes rise, as they should do in the highest character, in pre-eminence over his intellect."

In the gratitude and reverence which we feel in a measure never to be expressed, we sometimes regret that the ill-health which led to his seclusion prevented the extraordinary beauty of his character from being more generally known by personal intercourse. True it is that the world has shown in a wonderful degree a just appreciation of this character, so that many thousands in many nations who had never even seen the man heard that Charles Darwin was dead with a shock like that which follows such an announcement in the case of a well-loved friend; still it seems almost sad that when such an exalted character has lived, it should only have been to so comparatively few of us that the last farewell over the open grave at Westminster implied a severance of feelings which had never been formed before, and which, while ever living among the most hallowed lights of memory, we know too well can never be formed again. But to those of us who have now to mourn so unspeakable a loss, it is some consolation to think, while much that was sweetest and much that was noblest in our lives has ended in that death, his great life and finished work still stand before our view; and in regarding them we may almost bring our hearts to cry—Not for him, but for ourselves, we weep.

IMITATION CHEESE

IN NATURE, vol. xxv. p. 269, we gave an account of the mode of manufacture of "butterine," a compound containing about 90 per cent. of a mixture of animal fats known as "oleomargarine."

"Butterine," as the name would imply, is an imitation butter which is largely imported into this country from America and from Holland. "Oleomargarine" is principally made in the United States by the patented process of Mége-Mouries, which consists in heating disintegrated suet to a temperature of 120° F., when a clear yellow oil is (to borrow a term of the metallurgists) "liquated out." This is allowed to solidify, and "refined" by subjecting it to pressure at a temperature of about 90° F. "Oleomargarine" is converted into "butterine" by adding about 10 per cent. of milk to it and churning the mixture, colouring the product with annetto, and rolling it in ice to "set" it. Some idea of the development of this industry may be gleaned from the fact that Mr. Nimmo, the chief of the United States Statistical Department, reported that the export of oleomargarine for the year ending June 30, 1880, was close upon 19,000,000 pounds. And this is probably under-estimated, as it is certain that considerable quantities of "butterine" passed through the Customs under the designation of butter. It is not very easy to get data which are altogether trustworthy; but looking to the rate of increase furnished by the statistics of previous years, it is probable that the present export of oleomargarine from America is not less than from 25,000,000 to 30,000,000 lbs. per annum. Oleomargarine was the subject of some discussion in the House of Commons during the last session, and the matter was again brought up a few nights ago in the form of some interrogatories addressed to the President of the Board of Trade.

It appears that oleomargarine has recently taken a new departure, and that its use is no longer confined to the manufacture of "butter substitute," or "butterine." Our ingenious friends on the other side of the Atlantic have discovered that a mixture of oleomargarine and blue skim milk make what Mr. H. M. Jenkins, the Secretary of the Royal Agricultural Society of England describes as an excellent imitation of "American Cheddar." Indeed, so excellent is the imitation, that competent judges in the City and elsewhere informed Mr. Jenkins that unless they had been told, they could not have distinguished the oleomargarine cheese from ordinary American cheese, and that they valued it from 52*s*. to 56*s*. per cwt. wholesale, and from 8*d*. to 9*d*. per lb. retail. American cheese of presumably legitimate origin is of course a well-established article of importation into this country; the estimated value of the cheese we import is upwards of 5,000,000*l*., of which at least one-half is credited to the United States; and "American Cheddar," "American Cheshire," "American Stilton," &c., are well-recognised terms among retail provision dealers. We are now to have "Imitation American Cheddar," "Imitation American Stilton"—Stilton and Cheddar, in fact, several times removed; and Mr. Jenkins tells us that a very extensive trade will be shortly established in these articles, provided that their quality proves to be sufficiently good for the English market. Samples of these imitation cheeses have been examined by Dr. Voelcker, the chemist to the Royal Agricultural Society, who pronounces them to be perfectly wholesome articles of food. If the "oleomargarine" which enters into their composition be obtained from healthy fat or suet, there is, of course, no reason to doubt their wholesomeness or their alimentary value. Provided that the proportion of skim milk and oleomargarine be properly adjusted, the composition of the imitation cheese will differ but slightly from that of the best English made cheese, and will, so far as its nutritive value goes, be probably preferable to the ordinary skim milk cheeses of this country, or even to the more esteemed varieties of Gruyère and Parmesan. "Imitation factory cheese," as Americans call the new produce, will, of course, be almost entirely wanting in the characteristic fats of milk, such as butyrin, caprin, and caproin, to the decomposition of which the ripening and flavour of good cheese is mainly due. As is well known, hard solid cheese, in which the proportion of fat is comparatively low, ripens but slowly, and unless artificially flavoured, as in the case of Parmesan, acquires little or no piquancy. On the other hand, a rich cheese rapidly loses the acid reaction which it has when new; the casein and the fat suffer change, and the fatty acids thus formed combine with the products of the decomposition of the nitrogenous constituents giving rise to compounds, which, when accompanied by a due proportion of the green mould of *Aspergillus glaucus*, or the red mould of *Sporendonema casei*, afford the piquant flavour and aroma of the more valuable varieties of cheese. Such cheeses, however, soon run into putrefactive decay; they become strongly alkaline, and may even give rise to poisonous products. The fats of "oleomargarine" consist mainly of olein, stearine and their congeners, and are much more stable compounds; hence the cheese in which these bodies function will ripen comparatively slowly, and would of themselves never acquire the flavour of such rich cheeses as Stilton or Double Gloucester. Still art can do much, and he would be rash who would attempt to set a limit to American ingenuity; but we may at least hope that *Aspergillus* and *Sporendonema* may prove to be beyond the reach of the imitative power of the Transatlantic cheese-merchant. T. E. THORPE

THE IRRAWADDI RIVER

Report on the Irrawaddi River. Part I. Hydrography of the Irrawaddi River. Part II. Hydrology of the Irrawaddi River. Part III. Hydraulics of the Irrawaddi. Part IV. Hydraulic Works connected with the Nawoon River. Parts I. and II. (in one vol.), 195 pp.; Part III., 227 pp.; Part IV., 151 pp. fol. By R. Gordon, Esq., M.I.C.E., &c. (Rangoon, 1879-80.)

THIS is a valuable Monograph on the Irrawaddi River by the Executive Engineer of the great Embankment Works of the Irrawaddi Delta, the well-known experimenter and writer on river hydraulics. The mode of publication does not do justice to the great labour and research shown in so large a work (573 pp. folio). It is apparently a Government Report, written in the period 1877-80, and printed at the Government Secretariat Press, Rangoon, in 1879-80. Great allowances must always be made for the difficulties in proper correction of the proofs of a work published at a small native press at a great distance from the author. Still the misprints in this work are quite unusually numerous, sometimes *three* in a single line of French or German, sometimes *four* in a single page of ordinary matter; this throws some doubt on the accuracy of the printed Tables (which cover about 130 pp. folio). The complete Report must have included about 29 plates (constant reference being made to them), but only three are published; the absence of these plates makes it often difficult, sometimes impossible to follow the author's argument. Again, the want of uniform transliteration of proper names causes difficulty in identifying unfamiliar places, the same place being often spelt in two or more ways (*e.g.* Shoaygheen, Shwaygheen, Shwégyeen, &c.). There is a too frequent use of local words (*e.g. choung* = river, *eng* = lake, &c.), and also of odd un-English words (*e.g.* divagation, prescinding, &c.). These are, however, trifling drawbacks compared with the fact that the work is one of great value, combining the results of unusual knowledge of the literature about the Irrawaddi with probably unique practical knowledge of the Irrawaddi Delta.

The work contains three very distinct subjects: 1, the question of the sources of the Irrawaddi (Parts I. and II.); 2, the hydraulic works on the Irrawaddi (Part III.) and Nawoon Rivers (Part IV.); 3, the theory of the flow of water in rivers (Parts III. and IV.).

Sources of the Irrawaddi.—Parts I. and II. form a monograph on the vexed question of the lower course of the great Thibetan River (Tsanpou or Sanpo); its upper course from west to east within the heart of the Himálya mountains has long been roughly known (by travellers' reports), but its lower course beyond the Himálya is still strangely obscure. The Indian Survey maps have long shown the Sanpo as continuous with the Brahmaputra. But the author adopts the view of the great French geographer, D'Anville (*circa* 1730), that the Sanpo is the upper course of the Irrawaddi. He discusses at great length the general features of the Thibetan plateau and of the Brahmaputra and Irrawaddi valleys, especially as to the distribution of mountain and valley, and as to rainfall and river discharges. The chief argument is that just above the *débouchure* of the highest known large affluent (the Mogoung), a little above Bhámo, at a distance of 800 miles from the sea, the Irrawaddi is still an immense river 1000 yards wide and with a flood discharge of over 1,000,000 cubic feet per second, and therefore requiring a large drainage area above Bhámo. Now within 100 miles above Bhámo, the five great rivers—Brahmaputra, Irrawaddi, Salween, Mekhong, and Yang-tse-kiang—are known to be contained within a narrow strip of 200 miles width; from this it would seem that the sources of the Irrawaddi must be very distant (from the want of numerous large affluents). By collating the various travellers' accounts of the Sanpo, it is shown that they are consistent with its being continuous with the Irrawaddi.

Most of this appears to have been written in 1877. But in 1877-78 the Indian Survey Department conducted some special investigations on the question; their explorers traced the Sanpo downwards to within about 100 miles of the nearest regular survey party then at work on the affluents of the Brahmaputra near the limits of British territory; this gap of 100 miles was left a *terra incognita*, so that the question was still open to conjecture. After some discussion of this later work, it is shown rather to favour the author's earlier writings.

Besides the main (geographical) argument there is much interesting matter in these two parts on the geology, meteorology, and some minor features of the Sanpo and Irrawaddi basins. The want of a good detailed map is much felt here in attempting to follow the geographical argument.

Hydraulic Works.—Parts III. and IV. deal chiefly with the (engineering question of) Embankment Works in the Delta of the Irrawaddi, intended for reclaiming the rich alluvial land and for shutting out flood water. In a practical sense this is much the most important part of the Report; but in the absence of the plates it is impossible to follow the great detail given. Still there is much of general interest admitting of some notice here.

Firstly, it is explained that most of the easily cultivable land in British Burma having been already taken up, the country—though apparently thinly populated—is actually well populated over the only good land; and that, to prevent over population, what is now really wanted is more land. In this view the reclamation of good land acquires great importance. This work in some way resembles that in the Mississippi Delta, with the important difference that the latter is a rich country with ample funds for the prosecution of large works, whilst Burma is a poor country without adequate means for the same. Indeed, the history of the works as herein set forth is throughout one of insufficient provision of funds for their rapid prosecution, and sometimes even for their proper repair; this was very disadvantageous, as of all works the timely repair of an embankment is perhaps the most urgent, as its breach may be simply disastrous.

In early days high floods on the Irrawaddi seem to have been rare; at any rate the floods of late years (1868, 1871, 1875, 1877, 1879) have all risen considerably above the highest supposed possible from local inquiry in 1862. It seems possible that this is due partly to the gradual destruction of the forests above, which causes the rainfall to be more violent while it lasts and also favours its rapid descent to the main stream, and partly to the erection of the embankments themselves which confine the floods to the main river.

A very curious instance is noticed that the 1875 and 1879 floods were *foretold* by the Burmese astrologers.

Flow of Water.—The uncertainty of hydraulic knowledge nowadays is well illustrated by the various opinions of successive engineers on the rise that would ensue in the river consequent on embanking it on both sides throughout the Delta. It is said that Col. Stoddard reported in 1869 that the rise would not exceed the average of one foot, whilst the professional adviser of Government considered that it might amount to 3 or 4 feet at Henzahda, and the author himself considered from 7 to 12 feet a probable rise. It is obvious that these results cannot be said to be any better than conjectural; their discrepancy showing that the formulæ in use for such sort of calculation were (as too often happens) inapplicable to the case in hand. The Government naturally declined to sanction the project.

The author then undertook an extensive series of direct discharge-measurements of the river nearly on the lines of the Mississippi work, viz. by direct velocity-measurements at numerous points of certain selected sites. Besides the practical value of these as necessary data for the embankment projects, the details may be of great use in the study of the flow of water. And indeed this forms the most interesting portion of the work in a scientific sense, being a mass of original experiment on the flow in a mighty river. Much credit is due to the author for the zeal with which he had these experiments carried on for several years, in the face of great difficulties and discouragements. The experiments are discussed only so far as necessary to explain the application of the results (chiefly discharge-measurements) to the embankment projects. A further special report upon the experiments themselves is promised, which should be of great value.

The velocity-measurements appear to have been entirely made with the "double-float," whereof the surface-float was a wood disc 6"×6"×1" joined by a cord $^1/_{16}$" thick, of various lengths, to a cylindric wood sub-float 6"×6"×12" loaded with clay, and sunk to various depths from 1 to 24 mètres. At moderate depths this instrument would be pretty efficient; unfortunately the efficiency of all double-floats decreases with the depth of immersion, and at the greatest depth of 24 mètres this one must have been very inefficient; for—supposing even that the sub-float retained its most favourable (the upright) position—the relative areas of connector and sub-float exposed to direct current-action would be as 73 to 100, and to lateral current-action as 52 to 100; so that the observed velocity of the instrument was certainly not that of the current at 24 mètres depth (as it is taken to be). Notwithstanding the inherent objections to the double-float, there seems to be as yet no better instrument available for mighty rivers.

Two sorts of velocity-measurements were undertaken, viz. (1) at one mètre depth at many points (from 30 to 60) across the channel; and (2) at every mètre of depth upon selected verticals in the channel. The latter were considered the more important. This sort of work must necessarily have been very tedious in flood-seasons on a mighty river; at such times only ten complete series could be done daily; altogether about 10,000 such series were done. This is a collection of experimental data quite unique in river hydraulics, of which the author may justly be proud. From these data, together with the cross-section figure, the discharges were computed; the mode of computation seems to have been as good as the data admit of.

The mode of presenting the results is open to some objection—*e.g.* many of the velocities are carried to *four* decimals of feet per second, a degree of accuracy quite unattainable; again the discharges are given in *several* different forms, viz. in cubic feet per second, in cubic mètres, and also in tons and in "mètre-tons" per day, per month, and per year, and in some tables the unit is not stated. The author points out that one cubic mètre of water weighs about 55 ÷ 56ths of a ton, so that the two measures (cubic mètres and tons) may be used indifferently with an error of less than 2 per cent., and he emphasises this coincidence by the use of a new term, "mètre-ton"; but by all modern usage this term means either the "moment of one ton of pressure at one foot leverage," or the "work done in raising one ton weight through one foot height," so that this new usage is inconvenient.

As to the theory of running water some novel views are brought forward. It is stated that, speaking broadly, two theories of flow in open channels have existed; viz. that previous to Du Buat's time the motion had been supposed due solely to pressure, and since his time has been supposed due solely to surface-slope, so that the earlier formulæ involve pressure, and the later surface slope. The author himself is the advocate of a "new theory," viz. that the motion is due to both pressure and surface-slope; his arguments appear to be chiefly two, viz. (1) that formulæ involving only one of these elements all fail under varying conditions; (2) that in many cases the ratio of the deep-seated velocities to those near the surface (which is usually <1) rises with increase of depth of the stream, and may sometimes even exceed unity in very deep streams. These views can hardly be admitted. Firstly, as to causality, surface-slope is really only a *property* of running water, not a *cause* of motion. All change of motion is due to and is evidence of the action of some *unbalanced* force (or pressure). In the case of running water the unbalanced active force (effective in forward motion) is the part of the earth's attraction *not directly* balanced by the normal resistances (ultimately of the margin), *i.e.* the resolved part thereof parallel to the motion, the *measure* of which is $g\rho \sin i$, and actually enters into all modern hydraulic formulæ in various equivalent forms, *e.g.* as $(p-p')$, $(dp \div dx) . \delta x$, $g\rho(h-h')$, $g\rho \sin i$, $g\rho S$, &c.; it cannot therefore be said that pressure is excluded from modern formulæ (although, after substituting numerical values for $g\rho$, the evidence of it is apparently lost). In the argument it seems also to be implied that the increase of pressure due to increase of depth should cause increase of velocity, but the fact is that increase of pressure does not of itself affect motion at all, unless the increase be (at least in part) unbalanced.

An interesting series of discharge-measurements was made at three sites in concert, viz. at Saiktha, near the head of the Delta, and at Zaloon and Thapangyo, which are situate on the two largest Delta streams. There appear to be only some minor local *affluents* into and *effluents* out of the space between the upper and lower sites. It would seem therefore that the discharge-measurements at the upper and at the two lower sites together should be nearly equal; this affords a valuable test of the consistence of the results. The field work seems to have been done at *each* of the three sites on seventy-three days in 1872-73, thus giving seventy-three pretty complete results; other eighty-three days' results are also given, but these are partly interpolated, and therefore of less value. The discrepancies are sometimes very large, ranging from a *gain* of 27 per cent. to a *loss* of 15 per cent. in the daily results; most of them (97 out of 154) are on the side of gain. After making allowance for the utmost possible supply from the minor affluents between the sites (by adding in the whole rainfall all over their drainage-basins) the residual discrepancy is attributed to the "storage power" of the river area (about 305 square miles) between the sites. While there is no doubt some (temporary) "storage power," it seems more likely that most of the discrepancy is due to real error in the results themselves, the fact being that only *very rough* approximation can be expected in the discharge-measurement of mighty rivers in flood. This collection of river discharge-measurements being made in such a way as to test each other is almost unique, most published results being isolated results incapable of test.

ALLAN CUNNINGHAM

OUR BOOK SHELF

The Botanical Atlas; a Guide to the Practical Study of Plants. By D. McAlpine, F.C.S. (Edinburgh: W. and A. K. Johnston, 1882.)

THE above is the title of a publication appearing in monthly parts, each containing, in the words of the prospectus, "four beautifully coloured plates and descriptive letterpress." Part I. deals with common representatives of the natural orders *Caryophyllaceæ*, *Cruciferæ*, *Fumariaceæ*, *Geraniaceæ*, and *Labiatæ*.

We are perplexed as to the intentions of the author of this work, which is advertised as designed "for the use of medical schools and Universities." If the "Botanical Atlas" is intended to supply candidates for certain elementary examinations with the facts absolutely necessary for a "pass" certificate, it seems fair to expect accuracy in the drawings of common objects.

The author, however, appears to think otherwise; not only are there gross inaccuracies in the execution of the conspicuous figures, but the types are ill-chosen and imperfectly referred to.

In illustration of this may be noted Fig. 2, on Pl. xiv., professing to represent a vertical section of the common wallflower; the reference "long stamen," points to the anther of a short one, and the words "short stamen," are referred to a green band, which might be imagined as intended for filament, petal, or sepal, and seems to do duty for all three.

In the figures standing for other vertical sections of flowers—*e.g.* Fig. 3, Pl. xvi., and Figs. 2 and 3, Pl. xxiii.—no one can avoid noticing the mysterious vagueness in the lower portions of the drawings; the same remark applies to the sections on Pl. xv. Is the author undetermined as to the relations of the parts composing the andrœcium and gynœcium, or does he expect students, for whom elaborately-coloured drawings of sepals and petals have been prepared, to discover the forms and relations of the smaller essential organs without aid—or, rather, in spite of the misleading caricatures here placed like pitfalls in his path?

Similar faults are apparent in the diagrammatic plans of the flowers, and one wonders at the ingenuity displayed in going so far out of the way to prepare imperfect and inaccurate drawings of common objects.

Among other equally ingenious misrepresentations may be named Figs. 9 and 10, Pl. xv.—the marvellous streaks in a somewhat oval frame (Fig. 11, Pl. xvi.) supposed to represent a longitudinal section of the seed of *Geranium*, the incomprehensible *stigma* in Fig. 8, Pl. xxiii., with reference to which we cannot agree with the author when he says: "The figures will show the arrangement of the parts better than any description."

Passing over such errors as Nostoe, Hydrodictyon—possibly printer's mistakes—and the questionable mixtures of Latin and English names, we may notice one or two specimens of description appended to these gaily-tinted plates. We are told, without further remark: "The form and arrangement of the different parts (Fumitory) are evidently suggestive of some purpose." Also, the description of the wallflower commences: "Wallflower is a universal favourite, no less from its beautiful colour than from its sweet smell," and then passes on to a highly condensed and imperfect synopsis. We are told that the "Campion" "also smells in the evening in order to guide and attract insects," and that in Herb Robert "the stem forks a deal, and is very brittle at the joints."

Such drawings and writing speak for themselves. We can only express the hope that if the other parts are published, more attention will be paid to accurate delineation and exhaustive description, and less to merely gaudy colouring. So far, the "Botanical Atlas" must be considered as but a very inefficient and faulty "guide to the practical study of plants." W.

LETTERS TO THE EDITOR

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

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

The Magnetic Storms of 1882, April

THE Astronomer Royal having received from Mr. Charles Carpmael, superintendent of the Meteorological Office at Toronto, Canada, copies of the Toronto records of the double magnetic storm of April last, has had them compared generally with the Greenwich records. Some results of this comparison I am desired by him to communicate to you for insertion, if you think proper, in the columns of NATURE.

The records comprise traces of the changes of magnetic declination, horizontal force, and vertical force. The commencement of disturbance on April 16 was sudden, in all elements, as was also the renewal of disturbance on April 19. Measuring out the times, both for Greenwich and Toronto, the following results are found:—

Element.	Toronto time of commencement of disturbance. h. m.	Corresponding Greenwich time. h. m.	Greenwich time of commencement of disturbance. h. m.
Declination ...	April 16, 6 17 ...	16, 11 35 ...	April 16, 11 31
Hor. Force ...	,, 6 15 ...	,, 11 33 ...	,, 11 31
Ver. Force ...	,, 6 16 ...	,, 11 34 ...	,, 11 35
Means		April 16, 11 34 ...	April 16, 11 32
Declination ...	April 19, 10 15 ...	19, 15 33 ...	April 19, 15 34
Hor. Force ...	,, 10 16 ...	,, 15 34 ...	,, 15 34
Ver. Force ...	,, 10 16 ...	,, 15 34 ...	,, 15 37
Means		April 19, 15 34 ...	April 19, 15 35

The times are mean solar. In the first case the mean of the Greenwich times is but 2m. earlier than that of the reduced Toronto times; in the second case 1m. later, indicating no real difference. This confirms, what has been before observed, that in times of storm the commencement of disturbance at different places appears to be simultaneous.

As regards the variations registered during the progress of the storm there does not seem to be any very close correspondence between the records of the two places excepting in one particular, the occurrence of a very remarkable decrease of horizontal force, soon after the first outbreak on April 16, which continued for some hours, and is a striking feature in both records.

WILLIAM ELLIS

Royal Observatory, Greenwich, June 16

Earthquakes in China

PLUSIEURS secousses de tremblement de terre ressenties en Chine pendant l'année 1881 ont été rapportées dans les journaux de Shanghai, de Hongkong et de l'étranger. Voici sur deux d'entr'eux quelques circonstances toutes particulières qui sont venues à ma connaissance et dont on n'a pas parlé.

Le 20 Juillet, un peu après 9h du soir, une secousse assez forte ébranla la ville de Tchong-kin, capitale de la Province de Sze-tchuen, longitude 104° E. de Paris; immédiatement après, écrit un Missionnaire, la ville fur couverte d'une brume tellement dense qu'on ne voyait pas à 10 pieds devant soi; de plus une odeur de soufre très sensible se repandit partout. On prit tout d'abord cette brume et cette odeur pour un indice d'incendie;

mais on le chercha en vain. La terreur était d'autant plus grande dans la ville que le peuple, à cette époque même, multipliait les superstitions, et les sacrifices en l'honneur des divinités protectrices contre les incendies. Aussi le grand mandarin de Tchong-kin avait prohibé la vente et l'usage des allumettes chimiques importées par les étrangers (yang ha) et celui du pétrole (yang icon) qui avaient naguère occasionée une terrible conflagration.

Voici une note de Mr. Harding, l'ingénieur chargé d'elever un phare à la pointe sud de l'île Formose.—Le 11 Décembre 1881, vers 4h du matin, trois chocs distincts de tremblement de terre furent ressentis." The first, of which the motion was oscillating, and which was the most severe, lasted about 3 to 4 seconds; then an interval of about 10 seconds followed by the second shock—an interval of 1 or 2 seconds, followed by the third shock—direction from S.S.W. to N.N.E. These shocks, which were of great severity, were also felt in Tai-wan-foo and Takow (environ 75 et 52 milles an nord nord-ouest du cap sud). The water at South Cape rose 16 feet, causing great destruction to the cargo boats moored near the beach, and was accompanied by a heavy southerly swell. The water which at spring tides only rises 3 feet 8 inches, had resumed its ordinary level at 7 a.m.

On March 19, 1882, at about 5 p m., a slight shock of earthquake (toujours au cap sud), with a gentle oscillating motion from south to north. Duration of shock about 3 seconds."

J'ai pensé que ces curieux faits intéresseraient les lecteurs de NATURE.

Le Directeur de l'Observatoire,

MARC DECHEVRENS, S.J.

Observatoire de Zi-ka-wei, près Shanghai, Chine, Avril 25

P.S.—Une grave perturbation magnétique a été enregistrée à Zi-ka-wei le 17 Avril dernier. Elle débuta brusquement à 7h. 36m. du matin (temps moyen de Zi-ka-wei, longitude 8h. 5m. 50s. de Gr.) par une augmentation de la composante horizontale de l'intensité et une diminution de la déclinaison. Vers 8h. un mouvement inverse commença pour se continuer avec de larges ondulations mêlées de saccades brusques et nombreuses jusqu'á 2h. 22m. de l'après midi, moment où la déclinaison atteignit son maximum. Entre le minimum, qui fut enregistré quelques minutes après le début de la perturbation, et ce maximum de 2h. 22m. la déclinaison a varié de 21'·1, valeur considérable à Zi-ka-wei. La composante horizontale eut son minimum d'intensité (apparent à cause de la variation de température) d'abord á 4h. 20m. du soir, puis encore à 7h. 20m. du soir. On peut se rendre compte de l'énorme baisse enregistrée en cette occasion, en sachant que le 19, par exemple, où la variation de la composante fut normale, la courbe photographiée à une amplitude totale de 15 millim, tandis que le 17 pendant la perturbation la variation totale s'éleva à 76 millim. entre 7h. 36m. du matin et 4h. 20m. du soir. Les grandes ondulations se dessinèrent pendant le minimum d'intensité entre 2h. du soir et 10h. du soir. Vers 11h. 30m. du soir la composante se releva très-brusquement, oscilla encore 3 ou 4 fois ce vers 3h. du matin 18 la perturbation était finie. A cette augmentation brusque d'intensité correspondit une diminution non moins rapide de la déclinaison, suivie aussitôt après d'une forte augmentation qui mit aussi fin à la perturbation de cette boussole.

Pendant tout ce temps l'aimant de la composante verticale oscilla constamment mais dans de très-petites limites; il n'y eut que deux ondulations qui se dessinèrent assez nettement, l'une entre 8¼ et 8½ du soir l'autre à 11.30.

Le 20 Avril, nouvelle perturbation aussi intéressante commençant avec une soudaineté et une violence extraordinaire à 11h. 40m. du matin par une énorme diminution de la composante horizontale, suivie de sauts ou d'ondulations amples et assez rapides. Inutile de dire que la déclinaison a varié proportionellement et en sens inverse. Le maximum de déclinaison fut enregistré à 3h. 43m. de l'après-midi; entre le minimum normal de 9h. et ce maximum de l'après-midi la variation à été de 13'·2 seulement. La perturbation prit fin à 2h. 20m. du matin le 21, quoique la déclinaison continuât à être irrégulière dans la journée.

A cette double perturbation magnétique ont correspondu des troubles profonds dans toutes les lignes télégraphiques, marines ou terrestes, de l'extrême Orient, de Singapore et Manille jusqu'à Tientsin. Les moments ou les courants perturbateurs furent observés furent surtout, le 17, entre 10h. et midi (Nagasaki-Shanghai Shanghai-Hongkong), à midi 50m. (Hongkong-Amoy-Shanghai), à ce moment la correspondance entre Hongkong, Manille, et Singapore entiérement interrompue; entre 2h. 5m. et 2h. 20m. Shanghai Amoy). Le 20, à midi (Shanghai-Nagasaki).

Tout cela indique pour l'Europe de belles aurore boréales. Ici rien.

Non-Electric Incandescent Lamps

IT is I believe well known that a method of obtaining light by means of incandescent platinum was patented by A. Cruckshanks in 1839. The following extracts from his specification (No. 8141) will, I think, show that there is no essential difference between the lamp devised by Prof. Regnard (NATURE, vol. xxvi. p. 108) and the invention of the patentee, which is described as follows:—

"In order to increase the light obtained from substances that are rich in carbon and to obtain light from gases and vapours that do not contain the proportions of carbon necessary to produce a bright flame, I construct a cage of fine platina wire gauze or network of the form of the flame and just so much smaller than the flame, that it may be entirely immersed in the outer portion of it, where it will soon become intensely ignited."

Further on it is stated that the platinum (covered with lime) may be heated by jets of the "vapour of inflammable liquids mixed with atmospheric air," and the patentee says that "the most advantageous method in practice" of obtaining the mixture "is to pass a current of air through such liquid."

The use of incandescent platinum as a source of light was again patented in 1849 by Gillard, and put in practice at Narbonne and some other small towns in France, but after a fair trial the experiment was abandoned (see King's "Coal Gas," vol. i. p. 53).

F. M. SEXTON

61, Barrington Road, S.W., June 12

Conservation of Solar Energy

THE views of Dr. C. William Siemens suggest a consideration of the influence of solar rotation upon the æthereal atmosphere, at various distances from sun's centre.

Laplace's limit of equal rotary and planetary velocity is at $36·35\ r_o$, r_o, being sun's semidiameter. The centrifugal force of rotation at that limit would be 1321·3 times as great as at sun's surface, while the centripetal force of gravitation is only $\frac{1}{1321·3}$ as great. The lately-published photographs of the solar eclipse indicate an atmospheric oblateness which may be due to the equilibrating tendencies of the two opposing forces.

If the æthereal disturbances from this source are not sufficient to account for luminous and thermal vibrations, we may look next to the velocity which the subsiding particles would acquire in falling from the equatorial limit to the solar poles. If there were no resistance, this velocity would be

$$\left(\frac{35·35}{36·35} \times 2gr\right)^{\frac{1}{2}} = 376·8 \text{ miles per second.}$$

Any diminution of this velocity by resistance would be converted into heat.

If we apply Coulomb's formula of torsional elasticity, $f = \frac{\pi a^2 W}{2gt}$, to solar rotation, W may represent sun's mass, a the coefficient of the radius of torsion, f the coefficient of torsion, g gravitating acceleration at sun's equatorial surface, t time of oscillation when the force of torsion is removed, or time of a solar half-rotation. Then

$$f = \frac{m}{2} = \frac{W}{2} \cdot \frac{\pi^2 a^2 r_o}{gt^2}; \therefore \pi a^2 r_o = gt^2 = \pi^2 l.$$

But g', or the projectile velocity that is represented by sun's rotary oscillation, is *the velocity of light*; gt^2 is the modulus of light at sun's equatorial surface; $a^2 r_o$ is the theoretical length of a pendulum, at sun's surface, which would oscillate once in each half rotation; ar_o is the length of an equatorial radius rotating with sun, and having the superficial orbital velocity, $\sqrt{gr}$, at its emote extremity.

PLINY EARLE CHASE

May 27

The Function of the Ears in the Perception of Direction

UNFORTUNATELY, through the bungling of my late agents, I am unable to refer to NATURE, vol. xxiv. p. 499, as quoted by

Mr. S. E. Peal, I may, therefore, be communicating stale information; but as it is the result of personal experience, what I have to relate may be of some use as confirmatory of statements of others. Mr. Peal would not be able accurately to estimate direction, unless the sense of hearing—the capacity to receive sound—was precisely equal and similar in each ear. A greater sensibility in one than in the other, would incline him to the right, or the left, as the case might be.

Few people know that they may be partially deaf on one side, and yet not perceive it, just as some are right- or left-eyed, without knowing it. A good test is a watch slid along a two-foot rule, the end of which touches the cheek. The watch is moved away, inch by inch, till the ticking is no longer audible—if the distance is the same on both sides, the sensibility of each ear is of course equal.

I shall not forget my horror when my medical man, applying this test, showed that while with my right ear I could distinguish the ticks far beyond the 2 feet, my left ear was impervious to the sound until within an inch or two of the watch. Hearing on that side has now totally ceased, and the result is *I have not the slightest idea of the direction of sound*. I can hear certain sounds (for I am becoming deaf on the right side also), but to whichever side I incline my right ear, from thence do I fancy the sound to come.

To me, with my passion for ornithology, it is a terrible deprivation. In my youth my senses were intensely acute. I could instantly detect and proceed to the faintest note uttered by a bird in the forest. Now the loudest call only puzzles me the more. But I was a long time before I found this out. I fancied more than once that the bird I was pursuing had powers of ventriloquism; then that there were two or more, calling from different directions. As soon, however, as I found, as before stated, that I was deaf on one side, I began experimenting on myself, and quickly arrived at the conclusion that in order to estimate direction, both ears needed to be equally sensitive.

A curious instance of correct judgment as to direction and distance of sound is given by one of the South African explorers, Green, I think. He had been much annoyed by a lion which roared round his camp. Taking his rifle and some of his native followers, he went outside in the direction of the sound. Halting, they listened intently, and at the next roar caught the exact direction, and judged the animal to be at (I think) about 300 yards. Carefully levelling his rifle, he fired, and had the satisfaction of hearing the well-known "thud" of the bullet, and a change of note from the midnight serenader. Next morning showed traces of blood, and following up the track they found, and despatched the wounded beast.

Colour-blindness is represented in the other sense, by an inability to distinguish certain sounds. That this is caused by partial deafness, I am now pretty well certain. My father-in-law, who never knew he was deaf, never heard the chirrup of the cricket, and I now find I cannot do so. Only last night my wife observed "what a noise the crickets are making," to me there was unbroken silence as regarded outward sounds. I only heard the continuous "buzzing" that sounds in my head, augmented by the regular "thud thud" of my pulse.

I have a clock, the hours of which strike on a sweet-toned, metallic bell. If my right ear is turned towards it, at a moderate distance, I hear the ringing note; if turned away, I am only conscious of the "burr" of the works, and a dull "thud, thud," denoting the stroke. Why do I hear the "burr," which is not so loud, or clear, as the metallic "ting"? That wave of sound seems to pass by and not affect me; it is like the sharp note of the cricket.

The cause why certain sounds are inaudible to certain ears is a subject well worthy of investigation, as bearing on the placing of sentries or outposts at night, in time of war; also for sportsmen hunting large dangerous game. E. L. LAYARD

British Consulate, Noumea, New Caledonia, April 7

Jamaica Petrel

MR. D. MORRIS asks (NATURE, vol. xxv. p. 151) for some clue to the locality and general character of the nesting-places of petrels. As I fail to find any reply in your pages up to January 19, I venture to send my mite by way of response to Mr. Morris.

Most of the petrels (*Æstrelata*), the Storm Petrels (*Thalassidroma*) and the Sheerwaters (*Puffinus*) breed in holes in the ground, excavated by themselves; sometimes on small islands, at other times on high mountains, at considerable distances from the sea. Their movements to and fro are almost always performed at night, and as they are capable of a very rapid flight, a distance of fifteen or twenty miles is quickly traversed.

Here, in New Caledonia, the well-known *Æstrelata mollis* breeds on the summit of Mont Mou (about 4000 feet) in January-March, in great numbers, laying one white egg, as usual. I am informed that in some places the ground is honeycombed with their burrows. I am also told that during the non-breeding season numbers come to roost in their old holes.

The larger, *Æ. rostrata*, Peall, nests in similar places, but at a much less elevation, on the Island of Uen, the most southern portion of New Caledonia, and hardly divided from it by the celebrated Wodin passage.

Other species are said to frequent other mountains in the interior, but I have no personal knowledge of them.

In Fiji I obtained *Puffinus ungax*, which bred far away in the mountainous interior, and there are other true petrels which do the same.

Vast numbers of various petrels and shearwaters are found in these seas, and I fancy all burrow, more or less, in the earth, to lay their eggs. Of the "Great Grey Petrel" (*Adamastor cinerea*) Capt. Hutton says, that it "burrows horizontally into the wet, peaty earth" (of Kerguelen's land) "from two to eighteen feet."

That the "Jamaica Petrel" resorts to the Blue Mountain range of Jamaica, for the purposes of breeding, I have not the smallest doubt, and if the holes are examined at the right time of year, I feel sure eggs will be found. The birds probably do use the holes as resting places, during certain periods of the year. They do not, however, *lay their eggs at sea!* and sooner or later Mr. Morris may be sure of finding eggs, though he may not find much of a nest.

The breeding of the Mutton Bird (*Puffinus brevicaudus*) on many of the Australian islands has been often described. Its burrows render walking positively dangerous. If Mr. Morris can refer to Gould's "Birds of Australia," he will find much information on this head. E. L. LAYARD

British Consulate, Noumea, April 10

THE REGNARD INCANDESCENT LAMP.—In reply to several correspondents who find a difficulty in the use of petroleum for this lamp, we think they may be more successful with benzoline.

DOUBLE STARS [1]

II.

WE are in possession of numerous methods of computing double star orbits. Sir John Herschel gave one of the first solutions of this problem, and his method has been used more than any other up to this, and so far from becoming obsolete, it is yearly gaining ground at the cost of the methods that have been proposed elsewhere. It starts with the construction of the orbit, which the companion appears to describe round the main star. It is clear that as the planes of the orbits may be inclined in every direction in space, we see only the projection of the real orbits on the heavens, but this, as well as every other projection of an ellipse on a plane surface, is another ellipse, though the main star does no longer appear situated in the focus. Five points determine an ellipse, if we therefore possess five complete observations, we can determine the apparent ellipse. Now the observations are not perfectly accurate, but the calculus of probabilities furnishes us with means to ascertain the most probable ellipse from a great number of observations, to which different weight may be attributed, according to their reliability, as far as known. But at Herschel's time, though the angles had been fairly observed, the measurement of these minute distances was still in its infancy. He, in consequence, threw them away, and computed distances by aid of the Keplerian law referred to above, from the angular velocities, concluded from a comparison of observations separated by moderate intervals. He improved the angles in the following way:—On a paper neatly divided into squares, he lays down a

[1] Continued from p. 155.

point for every observed angle of position, the epoch in years and decimals being measured as an abscissa along the horizontal lines, and the angle in degrees as an ordinate along the vertical ones. A series of points are thus obtained, which, if the observations were exact, would necessarily admit of a regular curve being drawn through them, whose nature is of course determined by the laws of elliptic motion, and one of whose essential characters is to have within those limits of the abscissa, which correspond to a whole period of revolution (that is, to a difference of 360 units in the ordinates), in some cases two, in some four, points of contrary flexure, but never more than the latter, nor fewer than the former, and to have, moreover, in all its points, a peculiarly graceful and flowing outline. The errors of observation, however, prevent the drawing of such a curve through all the points. It must be drawn with a free but careful hand, not through, but among the points, and so that it shall deviate less from every point, according as it is more or less reliable. Now after Herschel's time the accuracy of the observed distances has wonderfully improved, and we are therefore able to draw another curve representing the distances as ordinates, which then ought to agree with those deduced from the angles, and the angles ought to agree with those deducible by aid of integral calculus from the distances. The curves must be varied till they thus mutually support each other, and then we may construct any number of points of the apparent orbit by reading off the angles and distances for the corresponding epochs on the curves, and if we find the arc described sufficiently extensive, the apparent ellipse is simply drawn as nearly as possible through them. From the apparent orbit the elements of the real orbit, described in space, are then determined. These are seven in number :—

a. The major semi-axis, expressed in seconds of arc, *i.e.* the angle under which their mean mutual distance would appear if placed perpendicular to the line of vision, *i.e.* the straight line joining us with the star.

ϵ. The eccentricity of the real ellipse.

γ. The inclination of the plane of the real orbit to the plane perpendicular upon the line of vision.

Ω. The node, *i.e.* the angle of position of the line in which the plane of the orbit intersects the plane perpendicular upon the line of vision.

π. The longitude of the projected peri-astron, *i.e.* the angle of position of the companion at the epoch of its actual nearest approach to the main star.

T. The epoch, when the nearest approach in space occurs.

P. The period of revolution, *i.e.* the time it takes the companion to complete an entire revolution round the main star.

It is impossible to say what part of the orbit is inclined towards us, and what is removed from us,[1] we cannot therefore, distinguish between an ascending and a descending node.

Both before and after Herschel's investigations, several methods of calculating double-star orbits have been proposed. Savary, at Arago's request, was the first who gave an analytical determination of an orbit from five points. He also proposed a method, subsequently improved by Encke, for calculating the seven elements of the real orbit from four complete observations. These furnish eight co-ordinates corresponding to known epochs, and as only seven quantities, the elements are sought, the problem is over-determined. It has to be solved with different values of one of the data, in order that all the other data may be represented. The case is, at the time it generally was possible to obtain but three complete positions from the discussion of modern observations, while the fourth, depending upon Herschel's measures, gives seldom even an approximate value of the distance. After that, a longer series of angles had become available. Klinkerfues proposed to determine at once the seven elements from six observed angles, and at least one distance. The dimensions of the orbit could evidently not be obtained without the aid of observed dimensions, though all the other elements can be derived from the angles. It sometimes occurs that the companion moves in an orbit, the edge of which is turned towards us. The inclination is then about ninety degrees, and all the angles are nearly equal to, or half a circumference different from the longitude of the node. We are then obliged to make more extensive use of the distances. Several analytical methods have been adopted to this contingency, which has actually occurred in a few cases. It seldom happens that an orbit founded upon a few positions, even if these are distributed over a great part of the arc described, is the most accurate that could possibly be deduced from all the observations at our disposal. It is therefore advisable to finally correct the elements according to the rules of the calculus of probabilities, so that the errors ultimately left behind in the representation of the observed places, may, taking into account their relative accuracy and their different peculiarities, be as small as possible. In this country, Hind, one of the greatest authorities on anything relating to double stars, has made most extensive calculations of this nature.

The number of those double stars whose orbits can be computed is limited to between thirty and forty. The time during which micrometric measures have been made is so short, that one but seldom can make anything like a correct guess of what the whole orbit may turn out to be like, when the period is above three hundred years ; and even so, the peri-astron passage must happen to fall within a hundred years from now ; that is, if the eccentricity is not unusually small. The eccentricity of these orbits is generally large, and it appears to be larger the greater the period and the greater the axis is. Now in the cases—by far the most frequent—where the measures do not embrace a larger part of the orbit, we represent the motion of the companion by formulæ, deduced in accordance with the proportionality of the areas with the times. These formulæ suffice to foretell the co-ordinates for some few years to come, and they are interesting in other respects, too. Thus, if the distance of a double star from the solar system were known to us, we would also know the dimensions of the orbit in miles, and then we could calculate its mass by aid of Kepler's laws. Now their distances from us are not known, but if we assume the mass to be on an average *e.g.* about three times the mass of the sun, then we obtain—if we make the further assumption that the distance actually measured in seconds of arc between the components is in an average of a very great number of stars equal to their mean distance from each other—from the period of revolution (concluded from the variation of the angle) through the inverse calculation their distance from us. Now these hypotheses can only be used in discussing the results of an average of a great number of systems that have certain characters in common, *e.g.* brightness, colour, or distance. I am at present engaged with such researches, from which I hope to arrive at interesting results.

Most of the double stars, that so far have been found to revolve, are close and more or less difficult objects. Few of them are more than six and a half seconds of arc asunder. The components are in most cases of about the same brightness, but the companion is, on the whole, smaller, the greater the distance.

There are two kinds of revolving double stars. The first of them consists of bodies whose colours are strictly identical, whereas the second consists of bodies whose colour is generally complementary. The principal star in both cases is white or yellow—white stars preponderating in the first case, yellows in the second. In systems of

[1] This could be ascertained if we could at any time observe the speed in miles per second with which the companion approaches to or recedes from us in the line of vision. Spectroscopists are making some progress in similar researches, but their apparatus are not as yet sufficient for our purpose.

the latter kind, the companion is generally bluish, and the number of blue companions increases rapidly with the distance, so that the close pairs are generally of the same, more or less white colour, and not very different in magnitude, whereas the wide pairs are of complementary colours, and the companion much fainter than the primary. We see then that the brightness is, on the whole, more different, the more different the colour is—a circumstance first pointed out by Struve. Now it is well known that the colour of a solid or liquid cooling body passes from white through yellow to red, and these are the colours in which the single stars, without exception, appear to us, whereas the blue and purple stars are found only as small companions to brighter stars. Holden, in Washington, has suggested that these colours are due to absorptive atmospheres—an opinion, the decision of which we leave to spectroscopists. Pickering, in Cambridge, U.S., has shown, from the absence of polarisation, that they do not shine with reflected light.

The last remarks remind us of the planets, that, no doubt, revolve round those distant suns, and derive from them their light and heat. When we reflect upon the complicated nature of the orbits, which the planets and comets describe round our sun, we get some idea of how remarkable must be the nature of those curves that planets describe round double suns, compared to which the motion of our moon is simple, and easily understood. Perhaps the same comets that disappear to our gaze, leaving the sphere of attraction of the sun, are attracted, and for a time become members of those wonderful systems. Our mathematics do not suffice for solving the problems that are thus suggested, but it is not unlikely that, unless situated very close to one or other of the suns—so close that the other appears not much larger than a star, though comparable in brightness to the nearest sun—the planets are whirled out in the cold space by the gravitational influence of the other sun, as very likely many a minor planet was ejected from the solar system under the influence of Jupiter. What must be the nature of those worlds illuminated by two different suns, one yellow and another purple? Now rises the one, and all is clothed in yellow, now the other, and illuminated from complementary sources, every object appears in its natural colour. Then sets the yellow sun, and what must be the diversity of the effects as it approaches the horizon! And behold nature puts on a purple mantle. Then also that sun sets, and in the darkness of night, though there is seldom night where there are two suns, the starry heavens are seen much the same there as here, except perhaps for moons reflecting light from the differently coloured suns. But stars that seem large to us are hardly visible there, while our sun is perceived in the telescopes of the mysterious beings that inhabit those strange globes as only a faint star, and metaphysicians there prove from *a priori* considerations to their attentive pupils, that no life could bask in the feeble glare of a single sun—how all would famish but for the opposite effects of the two suns. And no doubt! life there is heightened, and the wonders of nature are unravelled by aid of and under the influence of the energy of several suns, more highly developed science, seeing how glorious is the creation on this poor orb, that is kept alive by but one. Not only the play of colour must there be more varied than here, but phenomena of which we have not the faintest idea, must be produced also through the action of heat, electricity, and magnetism.[1] Indeed, upon subjects like these, science gives no information, and we may therefore give our fancy free reins. Oh! that we did possess the power of appreciating these things like the divine Milton, when in truly prophetic strain he wrote:—

"Other suns, perhaps,
With their attendant moons, thou wilt descry,
Communicating male and female light,
Which two great sexes animate the world,
Stored in each orb, perhaps, with some that live;
For such vast room in nature unpossess'd,
By living soul, desert and desolate,
Only to shine, yet scarce to contribute
Each orb a glimpse of light, conveyed so far
Down to this habitable, which returns
Light back to them, is obvious to dispute."

W. DOBERCK

[1] The tides upon the planets caused by the two suns must be very interesting. Of course they also produce tides upon each other, and their spots present no doubt most peculiar features. It is not unlikely that in some cases the phenomena presented by new as well as by some variable stars are to be explained as the effect of tides caused by darker companions.

ON THE PHOTOGRAPHIC SPECTRUM OF COMET (WELLS) I., 1882[1]

ON May 31 I obtained a photograph of the spectrum of this comet, with an exposure of one hour and a quarter. On the same plate I took a spectrum of α Ursæ majoris for comparison. The comet's spectrum on the plate consists of a strong continuous spectrum extending from about F to a little beyond H. I am not able to distinguish any of the Fraunhofer lines in this continuous spectrum. The slit was rather more open than was the case in photographing the spectrum of the comet of last year; this would make these lines less distinct, but the lines G and H are well seen in the star's spectrum taken under the same conditions. We may therefore conclude that the part of the comet's original light which gives a continuous spectrum is much stronger relatively to the reflected solar light in this comet, than was the case in the comet of last year, and for this reason the Fraunhofer lines are not distinguishable.

Observations of the visible spectrum had already shown that the comet differs remarkably from the hydrocarbon type common to all the comets, some twenty, which have appeared since spectrum analysis has been applied to these bodies.

The photographic spectrum shows, as was to be expected, that this essential difference of spectrum exists also in the more refrangible region. The very strong ultra-violet group assigned to cyanogen is not to be seen on the plate, and the bright groups between G and *h*, and between *h* and H do not appear to be present.

The head of the comet was in sharp focus upon the slit, and the continuous spectrum with defined edges corresponds to the nucleus which in this comet was very distinct. In this continuous spectrum at least five separate places of greater brightness are seen, which very probably represent groups of bright lines, though they are not sufficiently distinct in the photograph to admit of resolution. That this interpretation is correct, seems probable, from the circumstance that these groups, as shown in the diagram, project beyond the strong continuous spectrum on one side. This side corresponds to where the light of the coma, on the side of the nucleus next the sun, falls upon the slit. We learn, therefore, that the light of this part of the coma consists for the most part in this part of the spectrum of these groups, as here on the plate only an exceedingly faint continuous spectrum can be seen.

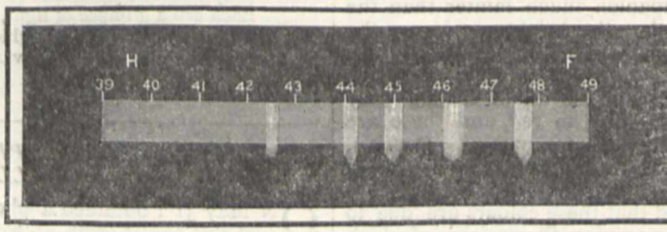
It is not possible to measure with any useful accuracy the beginnings and endings of the groups, as they are too faint at these points. Measures as accurate as the circumstances would permit have been taken of the brightest parts of the groups. The wave-lengths of these brightest parts are: λ 4253, λ 4412, λ 4507, λ 4634, λ 4769.

In the visible spectrum the bright lines of sodium

[1] Substance of note read before the Royal Society, June 15, 1882.

appear to have been strong, and it may be that some of the light of some of the groups may be due to this substance.

Prof. A. Herschel and Dr. von Konkoly showed long ago that the spectra of the periodic meteors are different for different swarms, and it does not seem surprising that we have now a comet, the matter of the nucleus of which under the sun's heat shows an essential chemical differ-



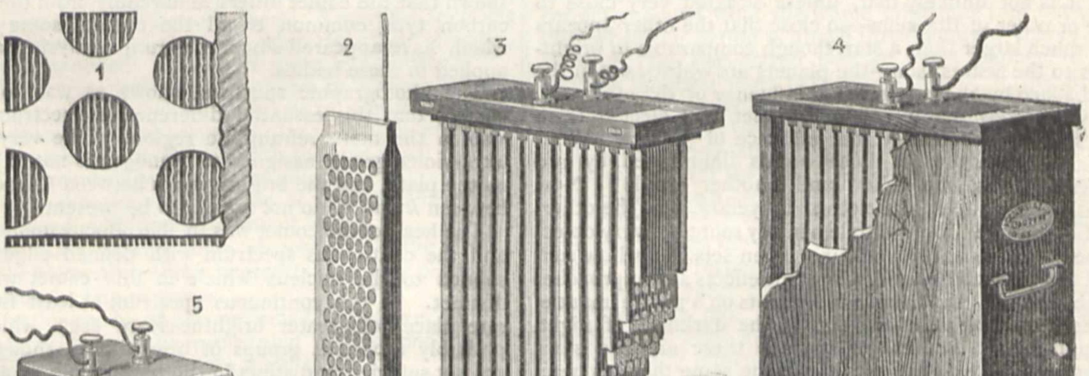
ence from the long series of hydrocarbon comets which have appeared since 1864.

Mr. Hind has kindly furnished me with the distance of this comet from the sun at the time the photograph was taken. The comet was then 42,380,000 miles distant from the sun, while the comet of last year was 69,420,000 miles when I obtained the photograph of its spectrum.

WILLIAM HUGGINS

KABATH'S ELECTRIC ACCUMULATORS

AT intervals since the introduction of accumulators or secondary batteries by M. Gaston Planté, various modifications have been made with a view to constructing cells on a commercial scale. The most renowned of these modifications was that of Faure, who applied red lead to the surface of the lead plates, so as to furnish a greater thickness of the spongy mass that is the effective agent in the storage process. In this development he had been anticipated by d'Arsonval, who sought to increase the efficacy of the oxidised lead electrode by covering it with a layer of lead dross. De Meritens, Tommasi, and others have worked in another direction by employing many sheets of lead foil presenting a large amount of surface, whilst Swan, Sillon, and Volckmar have taken another departure in applying perforated plates of lead with disintegrated material packed into the interstices. From our contemporary *La Nature* we have borrowed the accompanying illustration of another form of accumulator due to M. Nicolas de Kabath, whose suggestion is



Kabath's Accumulators made of corrugated lead plates. Figs. 1, 2, 3 showing details of manufacture. Fig. 4, Commercial Accumulator. Fig. 5, Laboratory Accumulator.

to employ thin plates of gauffered or corrugated lead, so as to secure a large amount of effective surface. The thin corrugated sheets are cut into narrow strips and packed between two stouter sheets of lead pierced with holes, through which the dilute acid liquor can circulate freely. The details of construction will be amply explained by the figures. The object of the perforated exterior is to prevent the cells from becoming short circuited by the possible falling down of the thin corrugated strips which are rapidly disintegrated during the preliminary charging or "formation" of the cell. The perforated leaden cases are themselves placed side by side in an appropriate cell, and are connected so that they serve alternately as positive and negative plates. Smaller cells are used for laboratory work. No details have yet been published, so far as we are aware, of their performances or capability of retaining the charge that has been imparted.

SCIENTIFIC RESULTS OF THE ECLIPSE

THE following communication appeared in the *Daily News* of Tuesday from its special correspondent with the English Eclipse Expedition :—

In my last letter,[1] written as it seems, an age ago, for the incidents since the eclipse have been more or less emotional, I promised in a final one to give the opinions of the astronomers as to the bearing of the work they have been fortunate enough to do in Egypt this year upon the general question of solar inquiry. Hence we have to consider both results and methods, and the latter should include the questions which it seems now most desirable to put to the sun the next time he is eclipsed. First, then, as to results. There seems good ground for supposing that the outermost part of the sun's atmosphere suffers changes as does that lower portion in which sun-spots are observed, and that the changes synchronise. Now, in the case of spots, as is generally known, we have a maximum number every eleven years or thereabouts, and in the interval we have a minimum, during which no spots are seen for weeks together. Hence the so-called maximum sun-spot period and minimum sun-spot period. This supposition was first hazarded in 1878, a minimum sun-spot year, as a result of the comparisons of the observations of that year with those obtained in 1871, a maximum sun-spot year. In 1871 the corona was most extended away from the equator; there was no special structure at the poles, and the hydrogen in it was strongly developed and it was very luminous. In 1878 it was most extended along the equator; there was very special structure at both poles, the hydrogen had almost disappeared, and it was faint. Now in 1882, that is eleven years from 1871 and in another maximum sun-spot year, the corona has again put on its condition of eleven years ago. Hence observation has shown that the supposition is so far quite justified by the facts, and accepting this connection as a working hypothesis astronomers and physicists have now to try to connect the absence of spots in the lower atmosphere with a condition of things which gives us a great equatorial extension of the atmosphere, and very definite structure at the poles, associated in all probability with a lower temperature, or at all events a greater admixture of cooling material.

Before the Eclipse Expedition had left England Dr. Siemens had proposed a theory of the solar atmosphere which postulated exactly such conditions as appeared to be revealed in years of least solar activity. The coincidence between hypothesis and fact was, to say the least, extremely curious, and there is no doubt that the fact that when the sun is most active the correspondence seems to vanish, will have to be carefully considered. But we have learned more touching the outer atmosphere than its changes. There has been a chemical touch added. When in 1869 its chemical nature was first investigated by means of the spectroscope, it seemed to be built up almost entirely of a substance of which we knew nothing here—a substance revealed by a line in the green part of the spectrum, at 1474 of the scale employed by Kirchhoff for his maps, which were then generally in use. In 1870 hydrogen was added to this unknown substance, if we are to interpret spectroscopic phenomena in the usual manner; and now again, with the same proviso, calcium has been added; that is to say, some lines seen in the spectrum of calcium have now been detected in the spectrum of the sun's outer atmosphere. It is now some years since the strange behaviour of calcium when observed in the spectroscope was noticed, and it was the first substance used to point the moral that the spectra of terrestrial substances are sometimes strangely transformed when their lines are examined among those visible in the ordinary spectrum of the sun. Thus the widest lines of all in that spectrum—the lines lettered H and K for purposes of reference—are seen in the spectrum of calcium when high temperatures are employed, though they are absent at low temperatures, when, however, a line in the blue which is but feebly represented among the solar lines is thick and brilliant. The observations of the eclipse in Siam in 1875 strongly suggested that the so-called calcium was really an important constituent of the lower layers, while it is now known that it plays an important part in every spot and prominence; indeed, in the spectra of sun-spots photographed by Mr. Lockyer at South Kensington, the H and K lines behave differently from all the other lines photographed. But the point of this year's work is that this calcium has been carried very high into the solar atmosphere, where it exists in such tremendous quantity that the eclipse colouring in all its weirdness can be traced to it, and the proof that this violet light is lighting up our atmosphere more powerfully than any other is found in the fact that in one of the photographs taken on Abney's plates the air between us and the dark moon is shown to be of this colour. This is photographic proof certain and sure, and will remind those learned in these matters of an observation made by Captain Maclear during the eclipse of 1870. It will be seen then that this year's work has left its marks both on the physics and chemistry of the outer atmosphere. We must now descend a little into the lower regions of the solar incandescent air.

Here we approach a very interesting part of the subject, but one on which it is difficult to say anything without going somewhat into detail. Up till a few years ago the idea that our terrestrial elements, such as iron, hydrogen, and the like, were anything but elements never entered the heads of astronomers as they were daily recording solar phenomena. It was obvious that the sun was very hot—so hot that it may be said the vapour of iron plays the same part there as the vapour of water plays here; but the possible result of the high temperature remained practically unconsidered, and our notions of the structure of the solar atmosphere were influenced by terrestial chemistry. Hence, when it was found that the upper atmosphere consisted mainly of hydrogen, all the lines of the solar spectrum except those due to hydrogen were supposed to owe their origin to absorption of the solar light at very low levels, and close to the sun there was supposed to be a thin stratum, the work of which was so efficient in this direction that it was called the "reversing layer." But after a time, as facts were accumulated, the question whether our elements really could and did stand the temperature of the sun without breaking up into something more elementary still was fairly asked; and as in other cases the question had to be discussed in a scientific manner—that is, cases had to be taken in which the question could be put to the facts in such a way that if the observations were of one kind one view would be strengthened; if of another kind then the other explanation would be more likely the correct one. It was pointed out some time ago that there are two very definite kinds of observations which can be made during eclipses, by which much information might be gained bearing directly upon this question of dissociation—that is to say, the question whether our "elements," as we know them, are or are not capable of existing at solar temperatures. These observations had to do, one with the "reversing layer," the other with the outer atmosphere. The challenge was of the most direct kind touching the reversing layer It went so far as to say that the former observations had been erroneously interpreted. This, however, must not be held to cast doubt upon former observers. The contention was that the former work, dating from 1870, had been of too general a nature, and that when a small part of the field of observation was studied with minute accuracy it would be found that the general statement would be untenable, that general statement favouring the view that the elements are still truly

[1] Reprinted in NATURE, vol. xxvi. p. 129.

elementary at solar temperatures. It will be seen that the issue raised then could not be complained of as lacking crispness and definiteness. What then are the facts? The facts have been exactly as they were predicted on the rival hypothesis—the hypothesis, namely, that the elements are not elementary; and in future we are not likely to hear much more of the "reversing layer." The solar spectrum, indeed, appears now to be the result of the absorptive work of an innumerable number of strata one over the other, from top to bottom of the solar atmosphere. If we could see the work of any one of these layers by itself, it would be impossible for us, with our mere terrestrial laboratory experience, to recognise it, whereas we do recognise the sum total, because we get, and can only get as a rule, a sum total in our laboratory experiments. Should this result be generally accepted as one of the results of this year's work a great step will have been gained. Whether accepted or not, it is quite clear that such observations as those to which attention has been directed will demand much attention when next the sun is eclipsed. Nor is this all. It is not too much to hope now that M. Thollon has so admirably succeeded in furnishing astronomers with a spectroscope which combines the maximum of dispersion and light that observations suggested by the new view may be made on the uneclipsed sun and bring their tribute of precious facts every day the sun shines. Such work, indeed, was actually started at Sohag, and the test then afforded gave out no uncertain sound; but on this point it is not necessary to enlarge upon the present occasion, as both MM. Thollon and Trépied are pledged to utilise the beautiful climates of Nice and Algiers in carrying on this new survey at the earliest possible moment, and the world of science will doubtless soon hear something of the result of this new attack.

There is little doubt that on the occasion of future eclipses attention will be much more concentrated on the spectrum of the corona, and more specially-constructed instruments will be brought to bear on it than has been the case hitherto. We may already take for granted that the blue lines photographically recorded (in addition to H and K in the violet,) will have their position determined with the greatest accuracy, and their coincidence or not with marked Fraunhofer lines will have an important bearing upon the questions to which attention has been directed in the present letter. The work, too, has shown that the new plates are so sensitive that it will be quite easy at the next eclipse by means of a circular rotating plate, or some such contrivance, to record all the spectroscopic phenomena, however evanescent they may be, visible at the moment of disappearance or reappearance of the sun. Such a method will not only give us a complete history of what goes on, but will furnish us with a scale of exact reference. So science advances. Each effort, and especially the one most wisely planned, instead of exhausting the supply of new phenomena brings still newer efforts and richer harvests in its train.

I have been very unfaithful to the task imposed upon me if I have not convinced your readers that the expeditions whose work it was my duty to chronicle have been richly rewarded for their long preparations and tedious journeyings. They will all leave Egypt with the liveliest sense of gratitude for the manner in which all their efforts for the advancement of knowledge among men have been seconded by the Khedive and the Egyptian Government.

PROF. W. B. ROGERS

THE death is announced of Prof. William Barton Rogers, whose name is so well known in connection with the Massachusetts Institute of Technology at Boston, U.S. Prof. Rogers died suddenly of apoplexy, while giving an address, on May 30 last, in connection with the Annual Graduating Exercises of the Institute. From the *Boston Daily Advertiser* we obtain some facts concerning Prof. Rogers' life and work:—

William Barton Rogers, the second son of four in a family noted for its scientific acquirements, was born in Philadelphia, in December, 1805. His father, Patrick Kerr Rogers, was a learned and enthusiastic lover of natural science, and is credited with being among the first in the United States to establish systematic courses of instruction in chemistry and experimental physics for the general public. Young Rogers was educated at William and Mary College, in which institution his father had been appointed Professor of Natural Philosophy and Chemistry. At the age of twenty-one he delivered, at the Maryland Institute, Baltimore, his first lectures on science, and one year later he succeeded to his father's position as professor at William and Mary College. In 1835 he accepted the appointment to the chair of natural philosophy in the University of Virginia, and there began instructing in mineralogy and geology. He remained there until 1835, and was next appointed to the chair of natural philosophy in the University of Virginia. There he added the subjects of mineralogy and geology to his course of instruction, and organised the geological survey of the State. He remained at the head of the Geological Survey until its discontinuance in 1842, and published annual reports, together with much valuable material which had been carefully collected. While at the University he published, for the use of the students, a short treatise on "The Strength of Materials," and a volume on "The Elements of Mechanical Philosophy." This period of his life was a very busy and attractive one, much of his time being given to original work in geology, and largely also in chemistry and physics. In the Association of American Geologists and Naturalists, organised in 1840, Prof. Rogers took a leading part. He contributed to its volume of *Transactions* many valuable memoirs, among them observations on the subterranean temperature in the coal mines of Eastern Virginia. In the exploration of the physical structure of the Appalachian chain, which formed the subject of one of the memoirs above alluded to, Prof. W. B. and H. D. Rogers were associated. Their generalisations were so novel and important in the estimation of European, as well as American geologists, as to give "the Gebrüder Rogers" a prominent place among their number. While a member of the Association of American Geologists he was elected several times its chairman. He presided at the meeting which expanded this last-mentioned Society into the American Association for the Advancement of Science, in 1847, and presided over the latter association at its meeting in Buffalo in 1876.

In 1853 Prof. Rogers removed to Boston, and at once identified himself with prominent educational interests here. With a committee of gentlemen no less interested than himself in the establishment in Boston of a school which should place the teachings of science upon a more practical plane than had hitherto been attempted, he drew up a scheme entitled "Object and Plan of an Institute of Technology," and embraced therein also a school of industrial science, a museum of arts, and a society of arts. To the accomplishment of this purpose he bent every energy, and at length a charter from the State was granted, providing the land upon whieh the institute buildings now stand. Subsequently the plans prepared by Prof. Rogers were almost completely carried out; and he, more perhaps than any other one man, brought about that admirable system of teaching which so characterises the institute, and which finds its place in the laboratories. His connection with the institute has been a most prominent one. He occupied the chair as president for many years, and at the start was at the head of the department of physics and geology. Since his removal to Boston, as well as before, Prof. Rogers has contributed largely to scientific journals in the United States and

Great Britain, and his articles embrace a very wide range of topics relating to the several departments of scientific research to which he had devoted himself, and many of his researches have attracted unusual attention from their value as adding to scientific knowledge.

At the age of seventy-six his failing health compelled him to give up active duties as president, and he resigned to give place to Mr. Francis A. Walker. He still continued to hold the position of professor *emeritus*, and retained it at the time of his death. His health has permitted him to continue at his duties, but he has not been strong, and the cause of his death, as above stated, is supposed to have been apoplexy. He was appointed by President Hayes as President of the National Academy of Sciences, and had returned from Washington only a few days before his death, where he had been presiding over a meeting of the academy.

The *New York Nation*, in reference to the death of Prof. Rogers, says:—

The death of Prof. Wm. B. Rogers, in Boston, on May 30, removes not only one of the foremost of our scientific men, but perhaps the one who had in the highest degree the faculty of presenting the claims of science on popular interest and respect with force and lucidity. He had a remarkable gift of expression, and an unusually winning and persuasive manner, both of which were supported by a character of the utmost purity and simplicity.

NOTES

THE Council of the Society of Arts have awarded the Albert Medal of the Society of the present year to Louis Pasteur, Member of the Institute of France, For. Memb. R.S., for "his researches in connection with fermentation, the preservation of wines, and the propagation of zymotic diseases in silk worms and domestic animals, whereby the arts of wine making, silk production, and agriculture, have been greatly benefited. The Council have awarded the Society's Silver Medals to the following readers of papers during the Session 1881-2:—To Prof. Silvanus Thompson, D.Sc., for his paper on "Storage of Electricity"; to J. Emerson Dowson, for his paper on "The Production and Use of Gas for Purposes of Heating and Motive Power"; to Col. G. F. Pearson, for his paper on "The Teaching of Forestry"; to Prof. Barff, M.A., for his paper on "A New Antiseptic Compound, and its Application to the Preservation of Food"; to Spencer Walpole, for his paper on "The Fish Supply of London"; to George F. Deacon, for his paper on "The Constant Supply and Waste of Water"; to Capt. Richard F. Burton, for his paper on "Gold on the Gold Coast"; to R. Warington, for his paper on "Some Practical Aspects of Recent Investigation in Nitrification"; to S. G. Thomas and Percy C. Gilchrist, for their paper on the "Manufacture of Steel from Phosphoric Pig-iron"; to Alexander M. Chance, for his paper on "The Recovery of Sulphur from Alkali Waste by Schaffner's Process, a record of recent results"; to James Mylne, for his paper on "Experiences of an European Zemindar (landholder) in Behar." Thanks were voted to the following Members of Council for the papers they had read:—To Capt. Douglas Galton, C.B., F.R.S., for his paper on "The American System of Heating Towns by Steam"; to W. H. Preece, F.R.S., for his paper on "Electric Lighting at the Paris Electrical Exhibition"; to Lieut.-Colonel C. E. Webber, R.E., for his paper on "Telephonic Communication"; to Sir Rutherford Alcock, K.C.B., for his paper on "The Opium Trade."

THE following has been forwarded to us from the Royal Society for publication:—

Florence, May 23, 1882

MY LORD,—An interesting commemoration in honour of Charles Darwin was held on Sunday last, the 21st instant, in the great hall of the Florence "Istituto di Studi Superiori." The commemoration was promoted by the Students in Medicine and Natural Science. The proceedings were simple, consisting of a few opening words by the Chairman of the Committee, Signor Fairman, a medical student, and a well turned and appreciative discourse by Prof. Mantegazza, whose scientific reputation is not confined to Italy. A bust of Darwin, in terra cotta, stood on the platform and marked the occasion. There were present the Prefect of Florence, the Council and Professors of the Institute, &c., while the large hall was crowded to overflowing with a mixed and attentive audience of ladies and gentlemen, showing the interest that the occasion had evoked.

I have, &c.,
(Signed) D. E. COLNAGHI,
H.M. Consul-General.

The Right Hon. Earl Granville, K.G., &c.

AN appreciative paper on Darwin, by the eminent naturalist, M. Alph. de Candolle, appears in the May number of *Archives des Sciences*. Darwin was prompt to acknowledge the work of his predecessors—Lamarck, Erasmus Darwin, and others—but seems with others to have overlooked the observations and ideas of Duchesne (1766), an evolutionist before Lamarck, to which M. de Candolle was able to call his attention while visiting him in 1880. Duchesne says: "The genealogical order is the only one that nature indicates, the only one that fully satisfies the mind; every other is arbitrary and vain (*vide d'idées*)." In the manner of exposition of facts and in reasoning, Charles Darwin (in M. de Candolle's opinion) rather resembles Duchesne than Lamarck or Erasmus Darwin. Lamarck is more systematic. Erasmus perceives much that he does not profoundly investigate; he is diffuse and lacks scientific method. Among other things. M. de Candolle remarks that nearly all *littérateurs* and men of science of the first rank have lived, during part of the year at least, in a town. One can hardly cite more than two exceptions (and they are very different), viz. Voltaire and Charles Darwin. The author gives an interesting picture of his visit to Darwin, who, as a septuagenarian, he says, "etait plus animé et paraissait plus heureux que je ne l'avais vu quarante-et-un ans auparavant, Il avait l'œil vif et une expression enjouée, tandis que ses photographies montrent plutôt sa conformation de tête d'un philosophe de l'antiquité. Sa conversation variée, franche, gracieuse, tout à fait d'un *gentleman*, me rappelait celle des savantes d'Oxford et de Cambridge." The author was struck with the sight of the domestic animals at Down, showing a "tranquillité qui suppose de bons maitres. . . . Vraiment, me disais-je, l'histoire des variations chez les animaux a été faite ici, et les observations doivent continuer, car Darwin n'est jamais inactif."

THE death is announced (though on doubtful authority) of Dr. Jules Crevaux, who has recently done so much for the exploration of French Guiana and the Amazon Valley. Dr. Crevaux, it is reported, has been assassinated, with his whole party, by Tobas Indians, while ascending the Pilcomayo River, on Argentine or Bolivian territory. He had started from Buenos Ayres, and had discovered near Salto the ruins of an ancient native city. The unfortunate explorer was only thirty-five years of age. He was a surgeon in the French Navy, and in July, 1877, undertook his first expedition into the interior of Guiana. Starting from Cayenne, he traversed an almost entirely unknown region, crossed the Tumuc-Humac Mountains, the water-shed between the Maroni and the Yari, a tributary of the Amazon. On a second journey in 1878-79, Crevaux went from Cayenne to the Oyapock, followed it up, and discovered the Kow, an unknown affluent of the Yari, followed the latter to its sources, and visited and explored to their sources the little-known affluents of the Amazon, the Paru, Iça, and Yapura. In 1880 he again set out, this time to the Magdalena and the

Orinoco, whose tributary, the Guyabero, he surveyed. A few months ago the indefatigable explorer started for Rio Janeiro for the purpose of exploring the country between that and the Middle and Upper Amazon, and in attempting to carry out this enterprise he, it is rumoured, has met with his untimely fate. We hope it will turn out to be without foundation.

THE death is announced of Mr. Alexander Leslie, whose name is associated with that of the distinguished Arctic explorer, Nordenskjöld. Mr. Leslie, who was a native of Aberdeenshire, was in his fifty-fourth year. He devoted much time to the study of practical farming, and acquired a considerable knowledge of agricultural chemistry. Proceeding to Sweden, Mr. Leslie resided there for several years, and upon his return to this country he published, in 1879, through Messrs. Macmillan and Co., a narrative of the "Arctic Voyages of Adolf Eric Nordenskjöld, from 1858 to 1879." Our readers will remember that Mr. Leslie was the translator of the famous explorer's own account of "The Voyage of the *Vega* round Asia and Europe." Mr. Leslie was an occasional contributor to the columns of NATURE.

THE Danish steamer *Arcturus*, from Iceland, arrived at Leith on Saturday, and reported heavy casualties and serious loss of life during the recent storms. On May 13, Capt. Schonstrup met with immense ice-floes about fifty miles from the east coast of the island. He afterwards steamed to the south-west, west, and north coasts, but was unable, after leaving Reykjavik, to get near any of the ports for the ice. These coasts were again attempted on June 6, but with the same result, the fields of ice from Spitzbergen and the Polar seas being as extensive and impenetrable as before. Large districts are said to be suffering severely from famine, as the vessels are unable to land the provisions, on the customary arrival of which they depended. The severity of the weather is preventing the growth of the crops, and large numbers of sheep and ponies are dying. Measles, which have not been known in Iceland for the last thirty-five years, are very prevalent, and in Reykjavik alone no fewer than 200 persons were suffering from the epidemic when the steamer left.

DR. HJALTELIN, the distinguished physician of Iceland, so well known for the ready and hearty assistance he gave to all scientific explorers of the island, died suddenly at Reykjavik on June 5.

TORNADOES of unexceptional severity and destructiveness are reported from the Western States of America, particularly Iowa, Illinois, Missouri, and Kansas. One half of the town of Grinnel, Iowa, is stated to have been destroyed, and more than 100 persons killed, this tornado having swept over a tract twenty-five miles long and half a mile wide, with devastating energy.

THE seventh annual report of the Japanese Minister of Education states that there are 28,025 common schools in Japan of which 16,710 are public, and the remainder private; there being an increase of 1316 and 125 respectively, as compared with the previous year. The number of high schools is 107 public and 677 private, there being an increase of 42 and 63 respectively. Besides the above, many *Kindergarten* and primary schools were established. These private schools, even now, play a most important part in Japanese national life and education. Many of them have hundreds of students attracted by the fame of a single teacher. Youths flock from all parts of the country to sit at the feet of a renowned scholar, as men did in Europe to hear Abelard. The most celebrated of these leaders of youth—for this they are, rather than simple schoolmasters in our sense of the word—is Mr. Fukusawa of Tokio, whose translations from European books and original works on the political and social questions of the day, are read far and wide in Japan. The students of this gentleman fill many of the most important offices in the state; some of them recently formed themselves into a patriotic society, and established a newspaper, in which the acts of the government are subject to much caustic criticism. Long after the ordinary educational work of their teacher is done, and the young men have gone out into the world to do for themselves, they continue to reside near him, to study under his direction, and to form classes in which important public questions can be freely discussed under his guidance. One of his classes translated the whole of Adam Smith's "Wealth of Nations" into Japanese, with annotations, and many other important European works, especially those on philosophy and politics, owe their appearance in European dress to Mr. Fukusawa and his pupils. The school has been a real, and, we believe, a highly beneficial power in the state. These "private schools," which have been political associations, and debating clubs, as well as scholastic establishments, have occasionally played important parts at crises of Japanese history. The members of the private schools established in Kagoshima, the capital of Sakuma, originated and led the great rebellion of 1877. Fortunately Mr. Fukusawa's pupils are more peaceful in their objects and methods.

THE French Government has established a prize of 2000*l*., to be given to the person who in the course of five years—from July 1, 1882, to July 1, 1887—will have invented the most useful application of the Volta pile. Foreigners are allowed to take part in this competition, which was instituted for the first time by Napoleon I., almost as soon as Volta invented his admirable instrument, and has been reopened at several periods.

THE proprietors of houses having a view of the Parc Monceaux have subscribed among themselves a sum for illuminating this garden with a number of Jablochkoff lights. Similar steps will be taken for other public gardens in Paris. The tradesmen located in the Palais Royal are establishing a private company for the same purpose. An experimental trial will be made within a few days with incandescent lights.

ON June 15 M. Marcel Deprez delivered, in the large hall of the Conservatoire des Arts et Métiers, Paris, a lecture on the transmission of electricity to great distances. The lecturer proved that magneto-electric machines could be moved by a current which had circulated through four kilometres of german-silver wire, whose resistance was twelve times longer than a similar wire of copper, and having a few millimetres diameter. M. Marcel Deprez declares that he will go almost to any length in diminishing indefinitely the diameter of the wire of his dynamo-magnetic machine, and that it is by resorting to large dynamos that he will be able to produce a current sufficiently powerful.

COL. LAUSSEDAT, director of the Conservatoire des Arts et Métiers, has placed at the disposition of aëronauts, a dynamometer of special construction for testing scientifically the resistance of their canvass before and after varnishing.

Two German expeditions will go to American stations in order to observe the transit of Venus in December next. Observations will be taken at Stratford, Connecticut; at Aiken, South Carolina; at Bahia Blanca; and at Punta Arenas.

MR. GILDER, one of the correspondents of the *New York Herald* in Siberia, telegraphs from the Lena Delta, April 24, that he has found the bodies of Capt. De Long and his companions, who, it may be remembered, were in the missing boat belonging to the *Jeannette*. The poor men had evidently perished of cold and hunger.

DR. HASSELBERG of Pulkova has been able to trace the bright line of sodium seen by many observers in the spectrum of Comet Wells, to some distance in the tail of the comet.

THE Merchant Venturers' Company of Bristol have resolved to erect, at an expense of 30,000*l*., a new Technical School on the site of the old Bristol Grammar School, for the use of the

Bristol Trade and Mining School, founded by the exertions of the late Canon Moseley, in 1855.

A RECENT report by Dr. Bürkner to the Göttingen Royal Society of Sciences, on his "Polyklinik" for ear disorders, gives some instructive facts. In 1881 the number of patients was 516 persons (338 male and 178 female), with 583 different forms of ear disorder. The doctor reckons that a cure was effected in 61·85 per cent. of the patients, and improvement in 15·12 per cent. 211 (or 40·9 per cent) of the patients were of juvenile age, 15 and under. There were 139 cases of injury of the external ear, 15 of the tympanum; 322 of the middle ear, 27 of the inner ear, and 13 sundry. For otorrhœa, pulverised boric acid was largely used. The greatly praised iodoform was fully tried in ear-treatment, but Dr. Bürkner considers it has "no future" in this sense. Leiter's heat-regulator, consisting of very flexible lead tubes, through which water of any desired temperature is conveyed to injured parts of the body, did good service, especially in inflammation.

FOR the Sanitary Institute Congress at Newcastle-upon-Tyne, September 26, the following gentlemen have accepted the presidentship of the various sections:—Dennis Embleton, M.D., F.R.C.P., Section I. Sanitary Science and Preventive Medicine; Henry Law, M.I.C.E., Section II. Engineering and Sanitary Construction; Arthur Mitchell, M.A., M.D., LL.D., F.R.S., Section III. Meteorology and Geology.

MR. W. G. INNES, of Great St. Helens, has sent us a few specimens of photographs of New Zealand scenery, taken by Burton Brothers, of Dunedin. They are beautiful specimens of the photographic art, and many of them are of interest from a geological and ethnological point of view. One photograph gives an excellent idea of the White Terrace at the Rotomahana Hot Springs, others show some of the grand mountains and beautiful bays, native life, &c.

MR. BRYCE WRIGHT has, we understand, received a very fine specimen of the interesting gem known as Alexandrite, from India.

THE enormous glacier, Fon or Svartisen (69° 25′ N., 35° 15′ E.) on the Senjen Island in Norway, and which is the northernmost of its kind in Europe, will shortly be made the object of a remarkable enterprise. It appears that a number of speculative merchants in Bergen have obtained the right of cutting block-ice for export from its surface. Some blocks have already arrived at the latter place, and as the quality of the ice has been found to be good, large shipments may be expected. The glacier is about 120 square miles, and as the distance from its border to the sea is only a couple of miles, the ice may be obtained very cheaply. A similar attempt to utilise the glacier Folgefonden was made some years ago, but failed, owing to the blocks in their downward course repeatedly breaking through the wooden bore or conductor in which they were slid down to the sea.

THE Zoological Museum of the Lund University has just received as a gift from Prof. Nordenskjöld a splendid specimen of the sea-cow, *Rhytina Stelleri*, now extinct, brought by the *Vega* from Behring Island.

PASTEUR's discoveries having been doubted in Germany, they have been submitted to the appreciation of a special commission in Berlin, and M. Pasteur sent thereto one of his assistants to perform vaccination on sheep. The report has been sent to Paris, and is said to approve the process and to show that it has been quite as efficient in Germany as in France.

THE *Daily News* correspondent at Maritzburg reports that a brilliant comet has been observed there for the last two or three days, in close proximity to the sun.

WE have on our table the following books:—China, by Prof. R. K. Douglas (S.P.C.K.); White's Manual of Naval Architecture, 2nd edition (John Murray); Electric Lighting, by Th. du Moncel, translated by R. Routledge (George Routledge and Sons); La Bourboule, by Dr. G. H. Brandt (H. K. Lewis); the Funeral Tent of an Egyptian Queen, by Villiers Stuart (John Murray); Hot Water Heating, by F. A. Fawkes (Batsford); Notes on Cage Birds, edited by W. T. Green (Upcott Gill); Botanical Atlas, Part II., by Dr. M'Alpine (W. and A. K. Johnstone); Im Fernen Osten, 2 vols, by Gustave Kreitner (A. Hölder); Results of Rain and River Observations made in New South Wales during 1881, H. C. Russell, Sydney; Handbook of Invertebrate Zoology, by W. K. Brooks (Cassino, Boston, U.S.); How to Overcome the Potato Disease, by J. S. Jensen (Menzies); A Synopsis of Elementary Results in Pure and Applied Mathematics, vol. i., section 9, by G. S. Carr (Hodgson and Son).

THE additions to the Zoological Society's Gardens during the past week include an Arabian Baboon (*Cynocephalus hamadryas* ♂) from Abyssinia, presented by the Messrs. James; a Bonnet Monkey (*Macacus radiatus* ♂) from India, presented by Master G. H. Clark; a Chima-chima Milvago (*Milvago chimachima*) from Demerara, presented by Mr. G. H. Hawtayne; two Upland Geese (*Bernicla magellanica* ♂ ♀), five Ruddy-headed Geese (*Bernicla rubidiceps*), a Loggerheaded Duck (*Tachyeres cinereus*) from the Falkland Islands, presented by Mr. F. E. Cobb, C.M.Z.S.; a Rufous-necked Weaver Bird (*Hyphantornis textor*) from West Africa, a Common Lapwing (*Vanellus cristatus*), European, presented by Mr. J. S. Baldwin, F.Z.S.; a Loggerhead Turtle (*Thelassochelys casuana*) from the Straits of Bonifacio, presented by Lord Lilford, F.Z.S.; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, deposited; a Black-fronted Antelope (*Cephalophus nigrifrons*) from Africa, a Water Chevrotain (*Hyomoschus aquaticus*) from West Africa, three Darwin's Rheas (*Rhea darwinii*) from Patagonia, two Spanish Blue Magpies (*Cyanopolius cookii*) from Spain, purchased; an Egyptian Goose (*Chenalopex ægyptiaca*), a Chiloe Wigeon (*Mareca chiloensis*), five Mandarin Ducks (*Aix galericulata*), bred in the Gardens. The following insects have emerged during the past week:—Silk Moths: *Actias selene*, *Samia cecropia*; Moths: *Sphinx pinastri*, *Deiliphila euphorbiæ*, *Trochilium apiformis*, *Sciapteron tabaniformis*, *Sesia conopiformis*, *Sesia muscæiformis*, *Hypochera io*, *Callimorpha dominula*; Butterflies: *Vanessa xanthomelas*, *Vanessa urticæ*, *Aporia crabægi*.

OUR ASTRONOMICAL COLUMN

THE APPROACHING TRANSIT OF VENUS.—In deducing the following expressions for determining the times of contacts in the transit of Venus on December 6, for any point upon the earth's surface, the positions of the planet have been taken from Hill's Tables, which had an advantage over Leverrier's at the last transit, and Auwer's semi-diameter is adopted. For the sun the semidiameter deduced by Leverrier from the transits of Mercury has been employed.

For *first external* contact, there results—
Dec. 6, 1h. 56m. 12s. $+[2\cdot5442]\ r \sin l$
$-[2\cdot4793]\ r \cos l \cos (L - 87^\circ\ 35'\cdot0)$.
For *first internal* contact—
Dec. 6, 2h. 16m. 52s. $+[2\cdot5822]\ r \sin l$
$-[2\cdot4768]\ r \cos l \cos (L - 85^\circ\ 31'\cdot9)$.
For *last internal* contact—
Dec 6, 7h. 54m. 0s. $-[2\cdot2894]\ r \sin l$
$+[2\cdot6261]\ r \cos l \cos (L - 138^\circ\ 18'\cdot8)$.
For *last external* contact—
Dec. 6, 8h. 14m. 41s. $-[2\cdot2152]\ r \sin l$
$+[2\cdot6142]\ r \cos l \cos (L - 134^\circ\ 38'\cdot1)$

The angles from N. point for *direct* image are respectively 145°·1, 148°·4, 116°·9, and 113°·5.

In the above formula r is the radius of the earth at the place, l the geocentric latitude, and L the longitude from Greenwich, reckoned *positive* towards the *east*. The resulting times are

Greenwich mean times. The quantities within square brackets are logarithms of seconds of time.

THE CORDOBA OBSERVATION OF COMET 1881 II., ON JUNE 11.—In NATURE, vol. xxv. p. 519, we gave an account of Dr. Gould's observations of the great comet of last year, on the evening of June 11, when he compared it with an object which he could not identify as a fixed star, and it was mentioned that Mr. Tebbutt had suggested that the objects really observed were not the comet and a star, but the two stars λ Eridani and Bradley 718, which have almost precisely the differences of right ascension and declination that were recorded on the night in question. This explanation we considered a probable one, and the same view was taken by the editor of the *Astronomische Nachrichten*, which has occasioned a further communication on the subject from Dr. Gould, who rejects Mr. Tebbutt's suggested solution of the difficulty.

Dr. Gould says the appearance of the comet on June 11 precluded the slightest doubt as to its identity. "The tail itself could not be seen with the telescope, it is true, but the large, diffuse, and very elongated head, much brighter and more definite on the advancing side, was sufficient to enable the veriest tyro to recognise it as a comet." He was placed necessarily on the top of a high observing chair, which he did not leave during the observations, the records being made by his assistant. He had made several sweeps to find a suitable comparison-star, and was about to commence a new one, when he saw the object referred to above, "at the upper part of the field on the left, while the comet was on the right, below." The four published comparisons were then made, and whilst he was in the act of pointing the micrometer-thread upon the comet for a fifth, it disappeared below the horizon. He adds, that no jar of the instrument had taken place; "the field of the telescope was fully under control from the beginning, the declination-clamp remaining tight throughout," and he insists that no one who saw the comet could have entertained the idea that any amount of blurring could have given such an aspect to a fixed star, though it were far brighter than λ Eridani. And he doubts whether a star of the sixth magnitude would have been visible under the circumstances. He made experiments on subsequent evenings, by looking at known stars of different magnitudes when close to the horizon and through different degrees of haze, but in no case did he find one offer the appearance noted on June 11. Hence, he proceeds: "I can only suppose another comet to have been in the field. That it was not a companion-comet is manifest, not only from the relative motion, and from an examination made the next day, but still more from the abundant scrutiny in the northern hemisphere, which could not have failed to detect any companion. That it was not a fixed star, was evident from the beginning."

Thus the matter is left by Dr. Gould, who, it must be admitted, is by far the most competent judge of the probable explanation of the difficulty.

MASKELYNE'S SOLAR PARALLAX.—By communications from Mr. J. Morris, Hatfield Hall, Durham, and Mr. B. J. Hopkins, Marlborough Road, Dalston, we learn that the value of the solar parallax given by Maskelyne, to which allusion was made in this column last week, appears in the third edition of Vince's "Elements of Astronomy," Cambridge, 1810: it was therefore published during his life-time.

THE ROYAL SOCIETY OF CANADA

THIS Society, which has been founded under the auspices of the Marquis of Lorne, and is intended to be to Canada what the Royal Society and the Institute are to England and France respectively, held its first meeting on May 25, 26, and 27. Inaugural addresses were delivered on the 25th by the Marquis, Principal Dawson, and the Hon. P. J. O. Chauveau. For the purpose of reading and discussing papers, the Society is divided into four sections:—(1) French literature, history, and allied subjects; (2) English literature, history, and allied subjects; (3) mathematical, physical, and chemical sciences; (4) geological and biological sciences. The following papers were read in Section 3:—Note on zinc sulphide, by T. McFarlane. On the "transition" resistance to the electric current at the bounding surface between amalgamated zinc and solutions of zinc sulphate, by Prof. J. G. MacGregor, D.Sc. The "transition" resistance in this case was shown to be at any rate not greater than a small fraction of an ohm, the current being weak and the electrodes large. The method of measurement employed was a modification of that formerly used by Beetz.—On the measurement of the resistance of electrolytes by means of Wheatstone's bridge, by the same. In this paper, a new mode of using the bridge was described. Alternate currents were sent through the bridge, and brought into the same direction by a commutator in the galvanometer branch, in which one of Thomson's galvanoscopes was inserted. Two of the arms contained equal metallic resistances; the other two contained, besides metallic resistances, electrolytic cells the same in all respects, except as to length. Thus the errors due to polarisation and possible "transition" resistance were eliminated.—On molecular contraction in natural sulphides, by Prof. E. J. Chapman.—On the law of facility of error in the sum of n independent quantities, each accurate to the nearest unit, by Chas. Carpmael, M.A. The chance of the error in the sum lying between y_1 and y_2, where $y_2 - y_1$ is small, was shown to be

$$\frac{1}{\lfloor n-1} \left\{ \left(\frac{n}{2}+y_1\right)^{n-1} - n\left(\frac{n}{2}+y_1-1\right)^{n-1} + \frac{n \cdot \overline{n-1}}{1 \cdot 2}\left(\frac{n}{2}+y_1-2\right)^{n-1} - \&c. \right\} (y_2 - y_1),$$

the series to be continued as long as the part raised to power $n-1$ is positive. This series is approximately equal to

$$\sqrt{\frac{6}{\pi n}} \cdot e^{-\frac{6y^2}{n}} (y_2 - y_1).$$

—A symmetrical investigation of the curvature of surfaces; including a discussion of the plane sections of quadrics, the axes of conic sections and of quadrics, by Prof. A. Johnson, LL.D. In this paper it was shown that the leading theorems concerning principal radii of curvature, directions of principal sections, umbilics, lines of curvature, &c., can be obtained directly by a purely analytical investigation, elementary and symmetrical in its character, of the plane sections of a quadric.—Note on the deduction of the equation of continuity, by Prof. Loudon.—Note on the motion of a chain on a fixed curve, by Prof. Cherriman.—Note on the application of a remarkable determinant, by the same.—Note on a question of probabilities, by the same.—On the general regulation of civil time, by Sandford Fleming, C.E.—On the utility of geometry as applied to the arts and sciences, by Chas. Baillarge.

The following papers were read in Section 4:—The distribution of some saline and other plants in the West, by Prof. Maconn. This was an oral exposition, aided by a large map of certain peculiarities in the distribution of maritime Eastern and Western plants in the interior of the continent, and of some peculiar extensions of Southern plants to localities far north of their usual range.—Note on a general section from the Laurentian axis to the Rocky Mountains, north of the 49th parallel, by Dr. G. M. Dawson. This paper gave a summary of the latest facts respecting the succession and distribution of Cretaceous and early Tertiary beds in the North-West Territories, and of the facts obtained respecting their subdivision into groups, and the useful deposits of coal and lignite contained in them.—On the cretaceous and tertiary floras of British Columbia and the North-west territory, by Dr. J. W. Dawson, F.R.S., &c. The researches of the Geological Survey have resulted in the collection of series of fossil plants from a number of localities in the cretaceous of the Pacific coast, and of the eastern base of the Rocky Mountains, in the laramie or lignitic group of the plains, and in the Tertiary Lake Basins of British Columbia. From these it appears that while up to the Middle Cretaceous a flora of strictly Mesozoic character, consisting of pines, cycads, and ferns prevails, the Middle and Upper Cretaceous show the introduction of a larger number of broad-leaved evergreens of modern types. Though there seems to be a continuous prevalence of warm and temperate conditions, from the Upper Cretaceous, up to the Pliocene, the groups of plants observed may be classed as—(1) Lower and Middle Cretaceous; (2) Middle and Upper Cretaceous, with modern evergreens, as *Salix*, *Populus*, *Magnolia*, *Betula*, *Quercus*, &c., and fan palms and cycads; (3) Laramie or Fort Tunis group, probably a transition from the Cretaceous to the Eocene, with many new forms; (4) Tertiary Flora of the probably Miocene Tertiary of British Columbia. Descriptions and figures of these plants are being prepared, and it is hoped may soon be published.—On the anatomy and development of cestoid worms, by Prof. Ramsay

Wright.—On lacustrine concretions from Grand Lake, N.S., by Prof. Honeyman, D.C L.—Illustrations of the fauna of the St. John, N.B. group, by G. F. Matthew.—On birds from Hudson's Bay, by Prof. Bell.—On a new classification of Crinoids, by Prof. E. J. Chapman. This classification is based essentially on the presence or absence of a canaliculated structure in the calyx and arm plates. Three leading divisions are thus recognised. In one, the plates are without internal canals; in the second, the arm plates are perforated internally; and in the third, a system of canals radiates from the base of the calyx to the extremities of the arms. The subdivisions have been worked out to bring readily under grasp the more salient or broadly distinctive features of all the better-known families and types; and as the common names of families embody very little indication of these features, an additional grouping into sections is adopted.—On the Lower Cretaceous rocks of British Columbia, by J. F. Whiteaves.—On the introduction and dissemination of some noxious insects, by Wm. Saunders.—On the geological history of the St. John (N.B.) river valley, by Prof. L. W. Bailly.—On recent discoveries in the life-history of *Botrydium granulatum*, a terrestrial Canadian alga, as illustrating phases of development in the lower forms of vegetation, by Prof. G. Lawson, Ph.D., LL.D.—On the Quebec group of rocks, by Dr. A. R. C. Selwyn.

The following officers were elected: President, J. W. Dawson, C.M.G., LL.D., F.R.S., Principal of McGill College, Montreal; Vice-President, Hon. P. J. O. Chauveau, LL.D.; Hon. Secretary, J. G. Bourinat, F.S.S., Ottawa; Hon. Treasurer, J. A. Grant, M.D., Ottawa.

ON SMELL

THE sense of smell is caused by the contact of certain substances with the terminal organs of the olfactory nerves, which are spread as a network over a mucous membrane lining the upper part of the nasal cavity. Each nerve consists of a number of small bundles, themselves capable of being split into extremely fine nerve fibres. There are spindle-shaped cells connected with these nerves, from which proceed two processes—one to the surface, provided with bundles of long hairlets; the other passes to the interior. It is these hairlets which are proprobably the proximate cause of smell.

Let us consider, first, by what are smells excited? The operation of smelling is performed by sniffing, that is, by a series of short inhalations of air, bearing with it the odorous body. The first question which suggests itself is: Is the substance which excites sensation a liquid, solid, or gas? It has been tried by Weber, to fill the nose with eau-de-Cologne and water, lying on the back for that purpose, and pouring the liquid into the nostrils by a funnel. No sensation is produced. I have myself tried the experiment, and can confirm his observation. There is an irritating feeling, but no smell. Of course, on washing out the nose, or blowing it, the characteristic smell is at once noticeable.

It is easy to prove that solid particles are not the cause of smell. If the air conveying the odour be filtered through a tube filled with cotton wool, and inserted into the nose, a smell is still discernible, although all solid particles must thereby be kept back. But it is a very remarkable circumstance that it is so, for one would not suspect such extremely non-volatile substances as copper, iron, silver, &c., to give off gas, if indeed the smell which they most certainly evolve when rubbed is due to the gas of the substance.

We must, therefore, conclude that the sense of smell is excited by gases only. It is of course necessary to include under the name gases the vapours of liquids or solids which have low vapour-tension, and which, in consequence, give off vapour at the ordinary temperature. It has been proved that this is the case even with mercury, the boiling point of which is over 300° Centigrade. We may consequently conclude that many other substances of which it is impossible to measure the vapour-tension at ordinary temperatures, owing to its extreme minuteness, also evolve gas, if only in very small quantities. But it is well known that all gases have not the power of exciting a sense of smell. Let us compare some gases which have smell, with some which have none, and endeavour to discover if those which have smell have any other property in common.

The following is a list of gases which have no smell:—Hydrogen, oxygen, nitrogen, water-gas, marsh-gas, olefiant-gas, carbon monoxide, hydrochloric acid, formic acid vapour, nitrous oxide, and ammonia. Those which possess smell are chlorine, bromine, iodine; the compounds of the first two with oxygen and water, the second three oxides of nitrogen (or perhaps it is right to say nitric peroxide, for the other lower oxides are changed into it when they come into contact with air); the vapours of phosphorus and sulphur; arsenic and antimony; sulphurous acid, carbonic acid, and almost all the volatile compounds of carbon, save those already mentioned; some compounds of selenium and tellurium; the compounds of chlorine, bromine, and iodine, with the above-named elements; and some metals.

In considering this list, I submit first, that the property of smell is peculiar to some elements and their compounds. Thus, chlorine, bromine, iodine, sulphur, selenium, and tellurium, which are volatile or give off vapour at ordinary temperatures, have a characteristic smell. We should expect their compounds to have a smell, and we find this to be the case. Second, those substances which have no smell, or produce simple irritation of the nostrils have all low molecular weight. Such is the case with hydrogen, the element of lowest specific gravity. Such also is the case with oxygen and nitrogen; but this as well as the absence of smell in water-vapour, may be ascribed to the constant presence of these gases in our atmosphere, and their necessary constant presence in our nostrils, so that we may be insensible to their smells because we are always inhaling them; but I think it probable that this is not so. Hydrochloric, hydrobromic, and hydriodic acids, and ammonia, have purely an irritating effect, and cannot be described as smells. When ammonia is pure and free from compounds containing carbon, it has no trace of smell. Nitrous oxide is also the lowest of the oxides of nitrogen, and as such has the lowest specific gravity. But it is when we turn to compounds of carbon that we are best able to draw general conclusions; for that element, *par excellence*, has the faculty of forming almost innumerable compounds, and series which resemble each other in properties, but differ in specific gravity. And here we are most struck with the fact that increase of molecular weight, *i.e.* increase of specific gravity in the form of gas, produces, to a certain point, smell. Let us examine the simplest series, viz. the marsh-gas or methane series, commonly called the paraffins. The first two of these have no smell. Ethane, indeed, which is fifteen times as heavy as hydrogen, begins to have a faint trace, but it is not till we arrive at butane, which is thirty times heavier than hydrogen, that a distinct sensation of smell is noticed. In the same manner, the olefine series, of which the first member is ethene, or olefiant gas, gains in smell with rise of molecular weight. Of course, the highest members of this series have no smell, for they are non-volatile, but this is the case with most carbon compounds of which the molecular weight is high.

A similar relation is noticeable among the alcohols. Methyl alcohol, in a state of purity, is smell-less; ethyl, or ordinary alcohol, when freed from ethers and as much as possible from water, has a faint smell, and the odour rapidly becomes marked as we rise in series, till the limit of volatility is reached, and we arrive at solids with such a low vapour tension that they give off no appreciable amount of vapour at the ordinary temperature. Again, with the acids, formic acid is smell-less, and produces a pure sensation of irritation. Acetic acid has a slight but characteristic smell; and the higher acids of the series, propionic butyric, valerianic acid, &c., gain in odour with increase in density in the form of gas. If we consider the nitrogenous compounds of carbon, we are led to the same conclusion. Prussic acid is not smelt by more than four persons out of every five; but the nitriles, which bear the same relation to prussic acid as the higher members of a series bear to the lower, have all very characteristic odours. Acetylene would appear to form an exception to this rule; but carefully purified acetylene has little odour, and it is surpassed by its higher homologues. We may therefore, I think, accept this as a principle—that the intensity of the smell rises with rise in molecular weight.

It is also noticeable that the *character* of a smell is a property of the element or group which enters into the body, producing the smell, and tends to make it generic. Thus we can characterise the compounds of chlorine and its oxides as chlorous; indeed we may group the three elements—chlorine, bromine, and iodine, together, and name the characteristic odour of them and their oxides haloid smells. Similarly, sulphur, selenium, and tellurium, in their compounds with hydrogen, have a generic smell; and likewise arsenic and antimony. The only oxide of nitrogen which is smelt is nitric peroxide, so that it is impossible to pronounce on a generic smell for this substance. It is, again, easier to classify carbon compounds. The smell of the paraffins is generic; so is that of the alcohols, the acids, the nitriles, the amines with their irritation like that of ammonia, the bases of the pyridine series, the hydrocarbons of the benzene group, the higher hydrocarbons, such as naphthalene, anthracene, and phenanthrene. Give any one of these to a chemist familiar with the smell of any one of each series, and accustomed to use his sense of smell, and he will at once refer the body to its class.

The tendency of a rise in the series is to make the smell "heavier," less ethereal, and more characteristic. It also becomes more able to affect the olfactory nerves.

The rate at which smell travels is doubtless the rate at which the vapour which gives rise to it diffuses. Still it is impossible to test this experimentally. For the ease with which a smell is perceived varies with the molecular weight of the substance. Thus, if a piece of cotton wool be impregnated with ethyl alcohol, and placed in one end of a long tube, which is immediately corked, and a similar arrangement be adopted with amyl-alcohol, the fifth of the series of which the former is the second; although their specific gravities have the ratio of 23 to 44, and the ethyl-alcohol should diffuse $1\frac{3}{8}$ times as rapidly as amyl-alcohol, yet the smell of the latter will be perceived first, because a much smaller quantity produces the sensation.

It is possible, with practice, to make a fairly accurate analysis by means of the sense of smell. The method is, knowing the constituents of a mixture, to prepare one which has the same smell, measuring the proportions of the ingredients. The only precaution to be observed is that the smell of no member of the mixture be so overpowering as to mask those of the others. Thus I have analysed, or rather synthesised, a mixture of chloroform with ether, alcohol with ether, and these liquids with carbon disulphide, provided the latter be pure, to within 2 per cent.; but I failed with members of the pyridine series. Yet it was possible to detect the proportions of members of that series to each other; and it is not difficult, however extraordinary it may appear, to guess approximately the boiling-point of a mixture of members of a series, after some practice, purely by its smell.

So far as I know, no theory has been brought forward to account for the sense of smell; and I therefore venture to supply this want, premising that what follows is merely a tentative explanation, and as such will, I hope, not be too severely criticised.

There is a probability that our sense of smell is excited by vibrations of a lower period than those which give rise to the sense of light or heat. These vibrations are conveyed by gaseous molecules to the surface network of nerves in the nasal cavity. The difference of smells is caused by the rate and by the nature of such vibrations, just as difference in tone of musical sounds depends on the rate and on the nature of the vibration, the nature being influenced by the number and pitch of the harmonics.

Let us see what evidence can be adduced for the theory. Among the lightest substances which have smell are sulphuretted hydrogen and phosphoretted hydrogen, both of which are seventeen times as heavy as hydrogen itself. Prussic acid is fifteen times as heavy as hydrogen, and has a smell. But all persons are not able to perceive it. I have remarked an average of one in every five persons who are totally unable to detect its odour. Here we reach the lowest limit of molecular weight. *To produce the sensation of smell, then, a substance must have a molecular weight at least fifteen times that of hydrogen.* If we compare the hydrocarbons of the paraffin series, with each other, and similarly the olefine series, we notice that the lower members have no smell. The specific gravity of marsh-gas, CH_4, is 8; that of ethane, C_2H_6, 15; propane, C_3H_8, is twenty-two times as heavy as hydrogen, and here we first notice smell. Olefiant gas, C_2H_4, has the specific gravity 14, and has no smell; propene, C_3H_6, has a faint smell with a specific gravity of 21; and the higher members of the series increase in intensity of smell with increase in specific gravity. Hydrocyanic acid is smelt by most persons, but not by all. Its specific gravity is 15. The higher members of the series, called the nitriles, have all very characteristic smell. Formic acid vapour has the specific gravity 23, and has a purely pungent odour. Acetic acid, 30 times as heavy as hydrogen, has a faint smell when pure; propionic, butyric, and valerianic acids have strong smells. Methyl alcohol has no smell; its specific gravity is 16; ethyl alcohol, 23 times heavier than hydrogen, has a faint smell; and, as usual, the intensity, and if I may so term it, the flavour of the smell, increases as we rise in series.

These are the most typical instances of the carbon compounds, and they suffice, I think, to show the justice of the assertion that the intensity of smell increases with rise of molecular weight. The hypothesis of vibration satisfactorily explains this. The period of vibration of the lighter molecules is too rapid to affect our sense; there is a limit to this power; and just as some people have the power of hearing more acute sounds than others, so some senses are limited by a specific gravity of 15, and cannot smell prussic acid. Such people also have difficulty in perceiving the odours of bodies of slightly higher molecular weight than prussic acid.

Let us now inquire what is the probable rate of such vibrations. Mr. Johnston Stoney and Prof. Emerson Reynolds have made investigations of the ratio of the bright lines of some spectra, and have calculated their relations to each other. An analogy will make the nature of this relation more evident. When a note, say C, below the treble clef is sounded on a piano, not only the tone C is heard, but its octave C on the third space; also G above the line, C on the third leger line, E on the fourth, G on the sixth, B flat above the G, and other notes. These are called harmonics, or over-tones. Now if we knew these over-tones, it would be possible to refer them to their fundamental. So with light. The light evolved by incandescent gases consists of certain colours, which have each their own rate of vibration. Knowing these rates it is possible to calculate the rate of vibration of the fundamental. This has been done by Mr. Stoney (Royal Irish Academy, January 9, 1871) with hydrogen, with the following results:—

Wave-lengths,	*h*,	4102·37	tenth-seconds
,,	F.	4862·11	,,
,,	C,	6563·93	,,

These are the 32nd, 27th, and 20th harmonics of a fundamental whose wave-length is 0·1313 millimeters. The time of vibration is 4·4 fourteenth-seconds. It may be objected that these coincidences are not a proof. But Mr. Stoney and Prof Reynolds have measured the lines of the spectrum of chromyl chloride, and its 31 lines coincide with those calculated. The probability of the correctness of such a calculation approaches to almost absolute certainty. Now we have no means of recognising such fundamental vibrations, unless, indeed, the sense of smell is one means of receiving them. And it is this which appears to me probable; so probable, indeed, as to form a working theory.

But it is to radiant heat, I think, that we must look for indications of harmonics of the fundamental vibrations which are, according to this theory, the cause of smell. And a fresh proof may be drawn from the indications already seen. Prof. Tyndall has shown the power which odours have of absorbing heat-rays. There is no doubt that by refracting such heat-rays by means of a rock-salt prism, after they have passed through an atmosphere of odour, certain portions of the heat-spectrum show colder spaces, each corresponding to the particular rate of vibration which is absorbed by the vapour, through which the heat-rays have passed. By measuring the position of such gaps in the heat-spectrum, calculating the particular rate of vibration of the rays at such gaps, and referring them to their fundamental, we should arrive at the rate of vibration of the molecule which causes smell.

We may now inquire what it is which produces quality of smell. This, I think, can also be explained by the vibration theory, and depends on the harmonics of the vibration. Thus, the quality of tone of a violin differs from that of a flute by the different harmonics or overtones, peculiar to each instrument. I would ascribe to harmonics the quality of smell possessed by different substances. And it is to this that compounds of chlorine, phosphorus, &c., owe their peculiarity of odour. The odour of compounds resembles that of these elements to some extent; this may be accounted for by the similarity of overtones of compounds and their elements. Then we notice a similarity in quality of the odour of a compound of a series like the alcohols, and yet the quality grows flatter and heavier with increase in molecular weight.

Smell, then, may resemble sound in having its quality influenced by harmonics. And just as a piccolo has the same quality as a flute, although some of its harmonics are so high as to be beyond the range of the ear, so smells owe their quality to harmonics, which, if occurring alone, would be beyond the sense. It must be remembered that the harmonics are not heard separately from the fundamental, unless special means be adopted to render them audible, but they add their vibrations to those of the fundamental.

When two sounds are heard simultaneously, they give a concord, or a discord, but each may be separately distinguished by the ear. Two colours, on the other hand, produce a single impression on the eye, and it is doubtful whether we can analyse them. But smell resembles sound and not light in this particular. For in a mixture of smells, it is possible, by practice, to distinguish each ingredient, and as I have shown, to match the sensation by a mixture.

With regard to the mechanism by which smell is conveyed to the nerve, all that can be said is pure speculation. But as it is supposed that the vibrations of sound are conveyed to the auditory nerve through the small cirrhi, or hairs which spring out of round cylindrical nerve-cells in the superficial layer of connective tissue of the epithelium of the internal ear, and that each is attuned to some particular note of vibrations, so it may be imagined that the hair-like processes connected with the spindle-shaped cells, themselves communicating with the nerve-fibres of the olfactory nerve, are the recipients of the vibrations causing smell. Although the rate of such vibrations is extremely rapid, no less indeed in the case of hydrogen than 4,400,000,000,000,000, or the four quadrillions, four trillionth part of a second, yet the wave-length is by no means so small, for it averages the 2-100th of an inch, a magnitude quite visible with the naked eye. And hydrogen has no smell; those bodies which have smell, and higher molecular weight, must necessarily have a slower period of vibration, and possibly greater wave-length.

It is doubtful whether there exists a lower limit to our sense of smell. The vapours of osmic acid, carbon tetrabromide, selenium, tellurium, and arsenicous and antimonious oxides are among the heaviest known, and they have a most distinct smell. There appears to be a limit in practice, however, owing to the non-volatility of substances of high molecular weight at such temperatures at which smell may be perceived. The intense perfume of flowers is to be ascribed to the terpenes, of which common turpentine is one, or to their products of oxidation, and these bodies all possess a molecular weight of 136, and the specific gravity 68, a specific gravity which appears to excite the olfactory nerve most powerfully.

I bring forward the theory adduced with great diffidence. The problem is to be solved, in my opinion, by a careful measurement of the "lines" in the spectrum of heat-rays, and the calculation of the fundamentals, which this theory supposes to be the cause of smell. Such measurements and calculations, even if they proved the theory untenable, would have great value for their own sake, and labour expended in this direction would not be lost. Whether successful or not, it would at least be a first assault on what old John Bunyan called "Nose-gate of the City of Mansoul." WILLIAM RAMSAY

University College, Bristol

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—A syndicate, on which Dr. Ferrers, Prof. Stokes, Prof. Balfour, Mr. Todhunter and Mr. Trotter may be taken to represent the interests of science, has been appointed to frame regulations for the new degrees of Doctor of Science and of Letters. Candidates for these degrees are required by the new statutes to have made some original contribution to the advancement of science or of learning.

In the last Mathematical Tripos under the old regulations (January, 1882) the full marks were 27,150, and the average marks of the first ten wranglers were 6712, of the last ten 2890; first ten senior optimes average 2093; first ten junior optimes, 818. Out of 1407 marks given to the problems in the first three days, the first ten wranglers gained an average of 255; out of 8161 given to the problems in the last five days, the same ten averaged 849.

An important report by Mr. G. H. Darwin, who was the additional examiner in the same tripos, criticises severely the style in which the work of many men was done. Not a few sent up answers in atrocious handwriting, and omitted to define many symbols employed. The subjects which exhibited the average weakness of grasp most flagrantly was thermodynamics. A great many men had read something of it, but very few really understood what they attempted to explain. "Extraordinary muddle and confusion" was sent up in answer to a question on the absolute scale of temperature. On another question, while the very elements of the subject were unknown to those who answered, the same men reproduced faultlessly the algebraical calculation of the thermodynamic function for a perfect gas. Mr. Darwin strongly recommends such changes in the style of questions as that half intelligence may be more stringently treated, and men induced to read less and master more, and to gain a comprehension of physical principles.

The affiliation of University College, Nottingham, to Cambridge University has been formally recommended, so soon as the constitution of its governing body has been altered so as to admit a representative of the University. Scientific subjects have full recognition in the college course of study, by which exemption from one year's residence at Cambridge may be obtained, provided the student takes a degree in honours.

When Statute B comes into operation, the present Board of Natural Science Studies is to be replaced by two Boards—that of Physical and Chemical Studies and that of Biological and Geological Studies. These Boards will include, besides the Professors and Readers belonging to these studies, the Tripos Examiners of two years in the respective subjects belonging to the Boards, and three members of the Senate elected to serve for three years.

The second part of the Natural Sciences Tripos this year has no name in the first class, a result probably attributable to the transition state of the Tripos. Next June a better result may be anticipated, unless students with one consent let alone the more advanced parts of all the subjects. If this is the consequence of the recent changes, it will be much to be regretted.

OXFORD.—The term that has just ended has been chiefly remarkable for the fact that the new Statutes have come into operation in default of obstruction in Parliament. Already at several of the colleges, tutors and lecturers who have vacated their fellowships by marriage, have been re-elected "official Fellows;" and others who hold fellowships under the old ordinances have transferred themselves to the new official class.

But little legislation has been effected in Convocation: the only proposal of the Hebdomadal Council which provoked opposition was that to raise the University dues from five shillings to seven and sixpence a term, and to double the fee for Responsions (smalls), making it 2*l*. instead of 1*l*. Both proposals were carried on a division. The new Statute on Private Halls—containing provisions for bringing the master and students of such halls under the direct supervision of the University—was passed after being amended in Congregation. A Statute postponing the date of the University Examinations was also passed; so that in future the final honour schools will not commence before the last week of term.

During Michaelmas term, there will be offered two scholarships for proficiency in Natural Science. At Balliol there will be an election to a scholarship on the foundation of Miss Hannah Brakenbury, "for the encouragement of the study of Natural Science," worth 80*l*. a year (55*l*. and tuition free), tenable during residence for four years: open to all such candidates as shall not have exceeded eight terms from matriculation. This examination will begin on Thursday, November 16, at ten o'clock. Papers will be set in the following subjects:—(1) Mechanical Philosophy and Physics; (2) Chemistry; (3) Biology. But candidates will not be expected to offer themselves in more than two of these. There will be a Practical Examination in one or more of the above subjects, if the Examiners think it expedient. There will also be an optional paper in Mathematics; and the literary qualifications of the candidates will be tested by an English essay, or by a paper of general questions.

At Trinity College one Millard and Combe Scholarship, of the annual value of 80*l*., without limit of age, will be awarded in October next for proficiency in Natural Science if any Candidate of sufficient merit offers himself. The Scholarship is tenable in the first instance for two years, and will be prolonged for two years more, if the President and Fellows are satisfied with the industry and good conduct of the scholar. For special reasons it may be prolonged for a fifth year. The subjects of examination will be Chemistry and Physics. Candidates may also offer Mathematics, if they wish to do so and give notice a week before the examination. Special weight will be attached to excellence in one or two subjects, rather than to a less thorough knowledge of all. Candidates will also have an opportunity of doing one Classical paper. The scholar elected will not necessarily be required to commence residence immediately. The President will receive the names of candidates and their testimonials of character on Tuesday, October 10, between 8 and 9 p.m. The examination will commence on Wednesday, October 11, at 9 a.m.

SCIENTIFIC SERIALS

Proceedings of the American Philosophical Society, vol. xix. No. 109, June to December, 1881.—Continuation of notes on an Egyptian element in the names of the Hebrew kings, &c., by S. P. Lesley.—Notes on the geology of West Virginia, by J. C. White.—Bhotodynamic notes, III. and IV., by P. E. Chase.—On Alaskaite, a new member from the series of bismuth sulphosalts, by G. A. König.—The auriferous gravels of North Carolina, by H. M. Chance.—On some mammalia of the lowest eocene beds of New Mexico, by E. D. Cope.—Notes on the Quinnemont coal group in Mercer Co. of West Virginia and Tazewell co. of Virginia, by J. J. Stevenson.—Notes on the coal-field near Cañon City, Colorado, by the same.—The brain of the cat (*Felis domestica*); I., Preliminary account of the gross anatomy, by B. G. Wilder.—Exploration of the River Bene with the hitherto unexplored regions of Bolivia, by E. R. Heath. —The names of the Gods in the Kiche myths, Central America, by D. G. Brenton.

The Transactions of the Academy of Sciences of St. Louis, vol. iv. No. 2, 1882.—The hieroglyphic tablet of Pompeium grammatically translated and commented on, by E. Seyffurth.—Notes on North American *Microgasters*, with descriptions of new species, by C. V. Riley.—Descriptions of some new *Tortricidæ* (leaf-rollers), by the same.—On certain problems in refraction, by F. E. Nipher.—Magnetic determinations in Missouri during the summer of 1880, by the same.—"Reversion of type" in the digastric muscle of the human being, by C. A. Todd.—Ephemeris of the satellites of Mars for the opposition of 1881, by H. S. Pritchett.—The genus *Isoetes* in North America, by E. Engelman.—Auroral phenomenon, September 12, 1881, by E. A. Engler.

Revue d'Anthropologie, Paris. Deuxieme, Fascicule (1882), contains:—A paper by Dr. Paul Broca—left incomplete at his death—on so-called Ectromelian monstrosities, or those in whom there is an abnormality, but not an absence, of certain parts of the body.—Contributions to the study of muscular variations in human races, by Théophile Chudzinski. This paper is one of a series, the earlier parts of which appeared in the *Revue* for 1873-1874, and which will be continued in subsequent numbers.—On the cephalometric square, and its mode of application, by Dr. Topinard, who also describes the respective merits and demerits of the methods usually employed by artists to determine the facial angle and its relation to other parts of the body.—On the populations of the peninsula of the Balkans, by the late French geographer and traveller, Guillaume Lejean, sometime vice-consul at Khartoum, and at Massaouah. This portion of the author's exhaustive history of the origin and settlements of all the various peoples who have occupied the Hemus peninsula since it was held by the ancient Thracians, ends with the complete subjection, in the thirteenth century of the Slaves by Latin princes holding lands under the Greek Empire.—In a paper entitled "Les Griots," Dr. Berenger-Feraud describes those itinerant musicians who are to be met with in every part of Central Africa, from the shores of the Atlantic to the Indian Ocean, and who, notwithstanding the low castes to which they belong, constitute a distinct confederation under the authority of a chief, who exercises great authority over its scattered members, and levies a heavy tax for his own use from their general receipts. These people, whose name of Griots is a French corruption of the Ouolove word "Gwewonal," are regarded with fear and repugnance by the negro natives of the lands which they traverse, and where they are looked upon as members of an impure caste, whose dead are capable of bringing sterility and perpetual drought to the ground in which they are buried. They are skilled in improvising and reciting; and while some play the violin and guitar, the least gifted among them beat the tam-tam or play on various discordant wind-instruments. The confederation is undoubtedly of long-standing, and while the Griots, who perpetuate many ancient myths and songs, contribute towards the maintenance of some degree of intercommunication among the African races, they are credited with fomenting frequent dissensions, by trafficking with the information which they acquire through the extraordinary license granted them of going where they will among rich and poor, both in times of war and peace.—A critical review of all that is known of the Chukches, or Yu-its, by M. J. Deniker, gives the substance of what has been learnt of the ethnological and social standing of these Arctic peoples from the narratives of Nordqvist, Nordenskjöld, the Russian Argoustinovitch, Krause, Dall, and others.

Mathematische und Naturwissenschaftliche Mittheilungen, &c. (Berlin Academy), Heft 1, 1882.—Report of work in connection with the Humboldt foundation for natural research and travel, by E. Du Bois Reymond.—The thermo-dynamics of chemical processes, by H. Helmholtz.—On abnormal forms of pine-cones, by A. W. Eichler.—On the molecular refraction of liquid organic compounds, by H. Landolt.—The embryonal excretory apparatus of the gill-less *Hylodes martinicensis*, by E. Selenka. —On the differences of phase of electric vibrations, by A. Oberbeck.—On twisted rock-crystals, by E. Reusch.—On geognostic observations by G. Schweinfurth in the desert between Cairo and Suez, by E. Beyrich.—Investigation of volcanic rocks from the region of Abu-Zabel, on the Ismailia Canal, by E. Arzruni. —On the terminal growth of phanerogam roots, by S. Schwendener.—On an abundant exhalation of sulphuretted hydrogen in the Bay of Mesolungi, by G. Von Rath.—On transformations of amide by action of bromine in presence of alkalies, by A. W. Hofmann.—On the phosphates of thallium and lithium, by C. Rammelsberg.—The present state of science, by E. du Bois-Reymond.—On the production of amides of mono-basic acids of the aliphatic series, by A. W. Hofmann.—On the production of mustard-oils, by the same.—Crystallographic researches on sublimated titanite and amphibole, by A. Arzruni.—Congratulatory addresses to Von Bischoff and to Henle on attaining their doctor-jubilee.

THE last number of the *Journal* of the Russian Chemical and Physical Society (vol. xiv. fasc. 5) contains several valuable papers. Prof. Mendeleeff contributes an interesting paper "on the heat of combustion of hydrocarbons," and a note on his experiments on the resistancce opposed by water to the motion of solid bodies.—Prof. Butleroff contributes a notice on the important question as to the variability of atomic weights, and another on the oxidation of isodibutylene by permanganate of potassium; and M. Woeikof discusses the influence of local topographical conditions of meteorological stations on the average temperatures of winter.—Besides, we notice papers on the formation of hypochlorites and chlorates during the decomposition of chlorides by means of a current, by MM. Lyadoff and Tikhomiroff.—On the separation of barium from strontium and calcium by means of chromates, by M. Meschersky.—On the structure of nitrated products of the fat series, by M. Kisel.—On the critical state of bodies, by M. Stoletoff.—On the electrical conductibility of vacuum, by M. Kraewitsch.—On vibratory telephonic signals, by M. Jacoby.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 15.—"On a Deep Sea Electrical Thermometer." By C. William Siemens, D.C.L., F.R.S.

In the Bakerian Lecture for 1871, delivered before the Royal Society (*Proc. Roy. Soc.*, vol. 19, p. 443), I showed that the principle of the variation of the electrical resistance of a conductor with the temperature might be applied to the construction of a thermometer, which would be of use in cases where a mercurial thermometer is not available.

The instrument I described has since been largely used as a pyrometer for determining the temperatures of hot blasts and smelting furnaces, and Prof. A. Weinhold (*Annalen der Physik und Chemie*, 1873, p. 225), using the instrument with a differential voltameter described in my paper referred to, found its indications to agree very closely with those of an air thermometer within the limits of his experiments from 100° to 1000° C. I am not aware, however, that any results have been published of its application to measuring temperatures where a much greater degree of accuracy is required, as in the case of deep sea observations. My friend, Prof. Agassiz, of Cambridge, U.S., ordered last year, for the American Government, an instrument designed by me for this purpose, and during the autumn it was subjected to a series of tests on board the United States Coast and Geodetic Survey steamer *Blake*, by Commander Bartlett.

The apparatus consists essentially of a coil of silk-covered iron wire ·15 millim. diameter, and about 432 ohms resistance, attached to an insulated cable by which it can be lowered to the required depth, and connected so as to form one arm of a Wheatstone's bridge. The corresponding arm of the bridge is formed by a second coil made precisely similar to the former one and of equal resistance. This coil is immersed in a copper vessel filled with water, and the temperature of the water is adjusted by adding iced or hot water until the bridge is balanced. The temperature of the water in the vessel is then read by a mercurial thermometer, and this will also be the temperature of the resistance coil.

To avoid the error, which would be otherwise introduced by the leads of the resistance coil, the cable was constructed of a double core of insulated copper wire, protected by twisted galvanised steel wire. One of the copper cores was connected to each arm of the bridge, and the steel wire served as the return earth connection for both.

Sir W. Thomson's marine galvanometer with a mirror and scale was employed to determine the balance of the bridge.

Mr. J. E. Hilgard, assistant in charge of the United States Coast and Geodetic Survey, has sent me the results of Commander Bartlett's experiments. The apparatus was set up on board the *Blake* in April, 1881, and experiments were made off the east coast during August. In each series of experiments the temperatures at different depths were first taken by Miller-Casella thermometers attached to a sounding wire. A sinker was then fastened to the resistance coil, and it was lowered by the cable to the same depths, and the temperature read by means of the mercurial thermometer attached to the comparison coil. The depths at which readings were taken ranged from the surface down to 800 fathoms, and experiments were made both in rough and still water. The temperatures recorded varied from 38·5° to 81·5° F. In every case the readings of the electrical instrument were precisely the same as those of the Miller-Casella thermometers for the surface and the maximum depth; but for intermediate positions it was observed that the electrical thermometer in almost every case gave a slightly higher reading. This discrepancy may be accounted for, I think, by the circumstance that the electrical thermometer gives the temperature of the water actually surrounding the coil at the moment of observation, whereas the Miller-Casella instrument brings to the surface, or at least its readings are effected by, the maximum or minimum temperatures encountered in its ascent or descent, which may not coincide with that at the point of stoppage. This furnishes a very strong argument in favour of the superior accuracy of the electrical instrument.

It was found that about five minutes must be allowed at each observation for the resistance coil to assume the temperature of the water surrounding it, and a second period of five minutes for adjusting the temperature of the comparison coil on deck. Allowing five minutes more for lowering the cable, fifteen minutes sufficed to complete a deep sea observation.

Chemical Society, June 1.—Dr. Gilbert, president, in the chair.—The following papers were read:—Determination of nitric acid in soils, by R. Warington. The sample should be taken in dry weather from the subsoil, as well as from the surface. It is dried at 55° C., and powdered. About 200 to 500 grms. are extracted in a vacuum filter with about 100 c.c. of water; the extraction requires ten to forty-five minutes. The nitric acid is determined by a modification of Schlœssing's method, the nitric oxide gas obtained being measured.—On a spectroscopic study of chlorophyll, by Dr. Russell and Mr. Lapraick. The authors have not endeavoured to isolate a pure substance, but have endeavoured to follow spectroscopically the changes of a body (or bodies) which gives a particular absorption spectrum. This chlorophyll was extracted by a mixture of alcohol and ether, and gives the well-known absorption spectrum of four bands easily seen, and three other bands in the violet end, which are not noticed with gaslight. This chlorophyll, by treatment either with a small quantity of almost any acid, or with some salts, as ferric chloride, mercuric chloride, &c., or by heat, is changed, and gives another characteristic spectrum, to which the authors gave the laboratory name of "half-acid" chlorophyll; by the action of strong hydrochloric acid, a further change is produced, and an absorption spectrum is obtained, which is named "acid" chlorophyll. Alkalies act on chlorophyll, and give eventually an absorption spectrum of one broad band in the red. Very concentrated caustic potash solution splits this band into two bands, one of which ultimately disappears. The eye observations and measurements were checked with photographs taken by Capt. Abney. All leaves gave similar results, except some acid leaves, from the vine, &c., which gave half-acid chlorophyll instead of the normal product, when extracted with alcohol and ether.

Zoological Society, June 6.—Prof. W. H. Flower, F.R.S., president, in the chair.—The Secretary called the attention of the meeting to the curious way in which the young Cormorants lately hatched in the Gardens were fed by the parent birds, and exhibited a drawing by Mrs. Hugh Blackburn illustrating this subject.—A communication was read from Prof. St. George Mivart, F.R.S., containing a series of observations on certain points in the anatomy of the Cat-tribe (*Æluroidea*).—Mr. Howard Saunders read a paper on some *Laridæ* collected by Capt. H. H. Markham, R.N., on the coasts of Peru and Chili; comprising, amongst other rarities, the third known example of the large Fork-tailed Gull (*Xema furcatum*), a species which had been vainly sought for on the Pacific coast of America for upwards of thirty years. The author drew attention to the peculiarities distinguishing the various species of gulls found in the Pacific from those of the rest of the globe, and pointed out that, owing to oceanic currents, the connection between the species now only found on opposite sides of the equator had evidently been much more recent in the Pacific than in the Atlantic.—Prof. F. Jeffrey Bell read a paper containing an attempt to apply a method of formulation to the species of the *Comatulidæ*, and added the description of a new species, which he proposed to call *Actinometra annulata*.—Mr. Francis Day, F.Z.S., read some notes on the supposed identity of a specimen of a fish determined by Dr. Günther as *Anguilla kieneri* with a Gadoid *Lycodes*.—Mr. E. J. Miers read the second portion of his paper on the crustaceans received by the British Museum from the Mauritius, and called special attention to what appeared to be a variety of *Palinurus longimanus* of the West Indies which occurred in it.—Mr. W. A. Forbes read the fifth of his series of papers on the anatomy of Passerine birds. The present communication was devoted to the consideration of the structure of the genus *Orthonyx*, which was shown to be a true Oscinine form.—Mr. H. J. Elwes exhibited and made remarks on a Stonechat (*Saxicola*) which he had obtained during a recent expedition to the Aures Mountains of Algeria.—The Secretary exhibited a series of the diurnal and nocturnal lepidopterous insects bred in the Insect House in the Gardens during the present season.

Royal Horticultural Society, May 23.—Sir J. D. Hooker in the chair.—*Foliage injured by salt in the late gale*:—Dr. Church described experiments he had made at Cirencester during the last fifteen years to ascertain the amount of salt in the rain brought by autumnal gales, especially from the south-west. He found from 5 to 7 grs. per gallon, while the ordinary amount was only ·5 grs. The average winter amount was but slightly in excess of the average summer quantity. He noticed that in Oakley Park, one side of the trees was severely injured, and that, if no rain followed for a few days after the gale, the salt sparkled on the trees, even at a distance of thirty-five miles from the sea. The salt abstracted the moisture from the leaf-cells, and formed a condensed solution, so that the leaf became completely dried up, and perished. Mr. McLachlan added that salt had been observed on windows at Lewisham, as at Croydon, and elsewhere. Sir J. D. Hooker remarked that Dalton was the first to record a similar observation at the beginning of this century. With regard to beeches withstanding the gale better than oaks, as mentioned at the last meeting, it was elicited that they were unhurt at Kew, and Valewood, Haslemere, but at Cirencester, in Dorsetshire, and Cornwall, they suffered severely. Mr. Blackmoor exhibited foliage of pears, &c., from Teddington, some of which was quite unhurt; of other trees growing adjacent to them, the leaves were much injured. Vines and peaches showed similar differences. He suggested that it could not be salt in this case. The opinion generally entertained was that such discrimination were due to the trees being of relatively hardy and less hardy kinds.—*Rhododendron triflorum:* Mr. Mangles exhibited sprays of this species from the Himalayas. It belongs to the scaly-leafed section, and he observed that members of this group will not hybridise with any species of rhododendron without scales on the foliage.—*Malformed tulip:* Mr. Smee exhibited a tulip having petals distributed down the peduncle, a not uncommon occurrence. Mr. Henslow remarked on the fact that when such a petal was half-green and half-coloured, the tendency of the latter half is to check the growth and elongation of the peduncle. This causes the latter to bend over towards the side on which the petal is attached, and often so much so that it cracks on the opposite side, and may even decapitate itself.—*Change of sex in Rhododendrons:* The Rev. G. Henslow showed a flower in which the corolla was doubled, the stamens partially petaloid, while the pistil was open below with stamens, a tuft of imperfect petals and stamens arising from the base. He showed a drawing of a somewhat similar condition made in 1875, in which the style had become strap-shaped, was partially coloured red, and bore anther-cells on the margins; the pollen, however, was evidently abortive.

Entomological Society, June 7.—Mr. H. T. Stainton, F.R.S., president, in the chair.—Mr. P. B. Mason exhibited dark varieties of *Zygæna filipendulæ*, and *Callimorpha dominula*, as well as of the insect formerly supposed to be *Agrotis helvetina*, Boisd., but which was now believed to be a remarkable variety of *Noctua augur*.—The President remarked that there had been a great mortality this spring among the young larvæ of the currant saw-fly (*Nematus ribesii*).—Mr. McLachlan read a revised list of British *Trichoptera*.—Mr. W. L. Distant read descriptions of new species and a new genus of *Cicadidæ* from Madagascar.—Mr. A. G. Butler communicated descriptions of heterocerous *Lepidoptera* collected in Chili by Mr. Edmonds: *Geometrites*.

Victoria (Philosophical) Institute, June 15.—Annual meeting; the Right Hon. the Earl of Shaftesbury, K.G., in the chair.—Prior to the delivery of an address on the scientific aspects of the last Palestine survey, by Mr. Trelawney Saunders—who gave a careful analysis of the valuable results of the survey of Palestine, especially noticing the accord of the results with the Bible narrative—the honorary secretary, Captain F. Petrie, read the report, from which it appeared that the total number of members was now upwards of 950, Prof. Pasteur and many other well-known men of science having joined the Society in the past year.

Paris

Academy of Sciences, June 12.—M. Jamin in the chair.—The death of M. Conalia, Correspondent in Rural Economy, was commented upon.—On a point of the mathematical theory of effects in the game of billiards, by M. Resal.—Characters and *rôle* of double salts formed by fusion; by MM. Berthelot and Ilosvay.—Remarks on the use of zinc-carbon couples in electrolysis, by M. Berthelot.—Note on some explosive alloys of zinc and platinum-metals, by MM. Deville and Debray. Osmium is the only one of the platinum metals which does not retain zinc when one treats its alloy having a large excess of zinc, with an acid capable of dissolving this metal. The action of zinc on osmium-iridium is explained, according to laws of thermo-chemistry. (The heat liberated in union of zinc with iridium is enormous, and greatly exceeds that in union of osmium and iridium).—M. de Lesseps reported on the Suez Canal, and gave an account of the s.s. *Austral*.—M. Schlœsing was elected Member in Rural Economy in room of the late M. Decaisne.—Programme of astronomical work to be done by the scientific expedition sent to the south pole, by M. Lœwy. Classing the observations as (1) accidental, and (2) regular, those of the transit alone belong to the first; the second class include determination of the hour, the latitude and the longitude; of the radiant points of the southern heavens; and search for comets.—Observation of the Venus transit at Cape Horn, by M. Mouchez. The Transit Committee reluctantly gave up the island of Cape Horn for the mouth of the River Santa Cruz in Patagonia (for the most southerly station), the chances of good weather being so small; but they urge the importance of providing the Cape Horn scientific mission with instruments for transit observations.—Instructions for the naturalists of the Cape Horn Mission, for investigation of the animals on Terra del Fuego and adjacent islands, by M. Blanchard. *Inter alia*, the small mammals, as unable to cross wide arms of the sea, should throw light on questions in physical geography. Do the land birds migrate to the continent in winter? Are there batrachians in those parts? &c. Special means must be taken in those cold and wet climates for discovery of insects, few species of which attract notice by number or bright hues.—Instructions for the mission to Cape Horn, by M. Duchartre. In botany, special regard should be given to marine algæ.—Geological instructions, by MM. Daubrée and Des Cloizeaux. Search for fossil *débris* and for earthy meteorites and masses of native iron is urged; also investigation of raised beaches.—Programme of meteorological and magnetic observations, by M. Angot. Direct observations to be made at 4 and 8 a.m. and p.m. at midday and at midnight, (the expedition has a complete series of registering apparatus, and for certain instruments, a double series). *Inter alia*, full instructions are given for observation of austral auroras.—Observations of planets 221, 222, 223, 224, and of Comet *a* 1882 (Wells) at Paris Observatory, by M. Bigourdan.—Observations of the same comet with the 7-inch meridian circle at Bordeaux Observatory, by M. Rayet.—Ditto with the 6-inch Brunner equatorial at Lyons, by M. Gonessiat.—On a mode of transformation of figures in space, by M. Venecek.—On the law according to which the electromotive force of a magneto-electric machine varies in function of the resistance of the exterior circuit, by M. Deprez. The diminution of electromotive force in the ring, when the current becomes very intense, is due to insufficiency of the inductors. The wires of the ring cut the magnetic lines of force at an angle increasingly different from a right angle (at which maximum force is had).—Oscillations of the plane of polarisation by the discharge of a battery; simultaneity of electrical and optical phenomena, by MM. Bichat and Blondlot. A Leyden jar was discharged through a coil round a transparent body (*e.g.* flint) between polariser and analyser, and each time there was reappearance of the extinguished light. In one arrangement the image of a slit in the polariser was viewed in a rotating mirror, with a telescope, at each discharge; and one saw a series of bright bands (as in the case of a spark). The plane of polarisation was proved to oscillate about its normal position.—Decomposition of salts by matters in fusion, by M. Ditte.—Action of heat on an acid solution of sulphate of nickel in presence of sulphuretted hydrogen, by M. Baubigny.—On the mechanism of putrid fermentation, and on the alkaloids resulting from it, by MM. Gautier and Etard.—On the decomposing action of certain organic matters on oxygenated water; *à propos* of a memoir by MM. Bert and Regnard, by M. Béchamp.—On the aptitude communicated to cold-blooded animals to contract charbon by raising their temperature, by M. Gibier. Charbon was communicated to frogs (five out of twenty) compelled to live, after inoculation, in water at 35° to 37°. The bacteria developed were longer than usual (due to slow circulation).—Does the mechanism of absorption of virus vary with the nature of the wounds? Does the nature of the wounds affect the efficiency of surgical intervention? by M. Rodet. The nature of the wound affects only the rapidity of the propagation, not at all the mechanism of absorption; the penetration is, in by far the most cases, by the lymphatic vessels, very rarely by the blood-vessels exclusively, and seldom, comparatively, by blood-vessels and lymphatics.—On the sub-basaltic alluvions of the Corions (Ardèche), by M. Torcapel.—Probable lowering of the current water in the valley of the Seine during the summer and autumn of 1882, by MM. Lemoine and de Préandeau.—M. Carré described a new fire-alarm; an iron wire, constantly stretched by a spring, and closing a circuit (with bell) when elongated by heat; communication is made by rupture, as well as by expansion of the wire.

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