THURSDAY, JUNE 4, 1874

SCIENTIFIC WORTHIES

III.—CHARLES ROBERT DARWIN

CHARLES ROBERT DARWIN was born at Shrewsbury on Feb. 12, 1809. He is the son of Dr. Robert Waring Darwin, F.R.S., and grandson of Dr. Erasmus Darwin, F.R.S., author of the "Botanic Garden," "Zoonomia," &c.; by the mother's side he is grandson of Josiah Wedgwood, F.R.S., the celebrated manufacturer of pottery. Mr. Darwin was educated at Shrewsbury School under Dr. Butler, afterwards Bishop of Lichfield, and in the winter of 1825 went to Edinburgh University for two years. He there attended to Marine Zoology, and read before the Plinian Society at the close of 1826 two short papers, one on the movement of the ova of Flustra. From Edinburgh Mr. Darwin went to Christ's College, Cambridge, where he took his Bachelor of Arts degree in 1831. In the autumn of 1831, Capt. FitzRoy having offered to give up part of his own cabin to any naturalist who would accompany H.M.S. *Beagle* in her surveying voyage round the world, Mr. Darwin volunteered his services without salary, but on condition that he should have the entire disposal of his collections, all of which he deposited in various public institutions. The *Beagle* sailed from England Dec. 27, 1831, and returned Oct. 22, 1836.

Mr. Darwin married his cousin, Emma Wedgwood, in the beginning of 1839, and has lived since 1842 at Down, Beckenham, Kent, of which county he is a magistrate.

The Royal Society awarded to Mr. Darwin, in 1853, the Royal Medal, and in 1864 the Copley Medal. In 1859 the Geological Society awarded him the Wollaston Medal. He is an honorary member of various foreign scientific Societies, and is a Knight of the Prussian Order of Merit.

Since his return from South America in the *Beagle* Mr. Darwin's life has been comparatively uneventful, even for a scientific man; indeed, so far as the public is concerned, the main events in Mr. Darwin's career have been the publication of his works and papers, which have been far more numerous than many are aware of. We give below a list of them.

General Works

Journal of Researches into the Natural History and Geology of the countries visited by H.M.S. *Beagle*, 1845.

On the Origin of Species by means of Natural Selection, 1859.

This was preceded by a sketch, entitled "On the variation of organic beings in a state of nature;" published in the *Journal of the Linnæan Society*, vol. iii. (Zool.), 1859, p. 46.

The Variation of Plants and Animals under Domestication. 2 vols. 1868.

The Descent of Man, and Selection in relation to Sex. 2 vols. 1871.

The Expression of the Emotions in Man and Animals. 1872.

Zoological Works

The Zoology of the voyage of H.M.S. *Beagle*, edited and superintended by C. Darwin, 1840; consisting of five parts.

A monograph of the Cirripedia, Part 1, Lepadidæ; Ray Soc., 1851, pp. 400.

A monograph of the Cirripedia, Part 2, the Balanidæ; Ray Soc., 1854, pp. 684.

A monograph of the Fossil Lepadidæ; Pal. Soc., 1851, pp. 86

A monograph of the Fossil Balanidæ and Verrucidæ; Pal. Soc., 1854, pp. 44.

Observations on the Structure of the genus Sagitta; Ann. Nat. Hist., vol. xiii., 1844.

Brief descriptions of several terrestrial Phanariæ, and of some marine species; Ann. Nat. Hist., vol xiv., 1844, p. 241.

Botanical Works

On the various contrivances by which British and Foreign Orchids are fertilised, 1862.

On the Movements and Habits of Climbing Plants; Journ. Linn. Soc., vol ix., 1865 (Bot.), p. 1.—This Paper has also been published as a separate work.

On the action of Sea-water on the Germination of Seeds; Journ. Linn. Soc., vol. i., 1857 (Bot.), p. 130.

On the Agency of Bees in the Fertilisation of Papilionaceous Flowers; Ann. Nat. Hist., vol. ii., 1858, p. 459.

On the Two Forms or Dimorphic Condition of the species of Primula; Journ. Linn. Soc., vol. vi., 1862 (Bot.), p. 77.

On the Existence of Two Forms and their reciprocal Sexual Relations in the genus Linum; Journ. Linn. Soc., vol. vii., 1863 (Bot.), p. 69.

On the Sexual Relations of the Three Forms of Lythrum; Journ. Linn. Soc., vol. viii., 1864, p. 169.

On the Character and Hybrid-like nature of the illegitimate Offspring of Dimorphic and Trimorphic Plants; Journ. Linn. Soc., vol. x., 1867 (Bot.), p. 393.

On the Specific Difference between *Primula veris* and *P. vulgaris*, and on the Hybrid Nature of the common Oxslip; Journ. Linn. Soc., vol. x., 1867 (Bot.), p. 437.

Notes on the Fertilisation of Orchids; Ann. Nat. Hist., Sept. 1869.

Geological Works

The Structure and Distribution of Coral-reefs, 1842; pp. 214.

Geological Observations on Volcanic Islands, 1844; pp. 175.

Geological Observations on South America, 1846; pp. 279.

On the Connection of the Volcanic Phenomena in South America, &c.; Trans. Geol. Soc., vol. v.; read March, 1838.

On the Distribution of the Erratic Boulders in South America; Trans. Geol. Soc., vol. vi.; read April, 1841.

On the transportal of Erratic Boulders from a lower to a higher level; Journ. Geol. Soc., 1848, p. 315.

Notes on the Ancient Glaciers of Caernarvonshire; Phil. Mag., vol. xxi., 1842, p. 180.

On the Geology of the Falkland Islands; Journ. Geol. Soc., 1846, pp. 267.

On a Remarkable Bar of Sandstone off Pernambuco; Phil. Mag., Oct. 1841, p. 257.

On the Formation of Mould; Trans. Geol. Soc., vol. v., p. 505; read Nov. 1837.

On the Parallel Roads of Glen Roy; Trans. Phil. Soc., 1839, p. 39.

On the Power of Icebergs to make Grooves on a Submarine Surface; Phil. Mag., Aug. 1855.

An account of the Fine Dust which often falls on vessels in the Atlantic Ocean; Proc. Geol. Soc., 1845, p. 26.

Origin of the Saliferous Deposits of Patagonia; Journ. Geol. Soc., vol. ii., 1838, p. 127.

Part Geology; Admiralty Manual of Scientific Inquiry, 1849. Third ed., 1859.

TWO British naturalists, Robert Brown and Charles Darwin, have, more than any others, impressed their influence upon Science in this nineteenth century. Unlike as these men and their works were and are, we may most readily subserve the present purpose in what we are called upon to say of the latter by briefly comparing and contrasting the two.

Robert Brown died sixteen years ago, full of years and scientific honours, and he seems to have finished, several years earlier, all the scientific work that he had undertaken. To the other, Charles Darwin, a fair number of productive years may yet remain, and are earnestly hoped for. Both enjoyed the great advantage of being all their lives long free from any exacting professional duties or cares, and so were able in the main to apply themselves to research without distraction and according to their bent. Both, at the beginning of their career, were attached to expeditions of exploration in the southern hemisphere, where they amassed rich stores of observation and materials, and probably struck out, while in the field, some of the best ideas which they subsequently developed. They worked in different fields and upon different methods; only in a single instance, so far as we know, have they handled the same topic; and in this the more penetrating insight of the younger naturalist into an interesting general problem may be appealed to in justification of a comparison which some will deem presumptuous. Be this as it may, there will probably be little dissent from the opinion that the characteristic trait common to the two is an unrivalled scientific sagacity. In this these two naturalists seem to us, each in his way, pre-eminent. There is a characteristic likeness, too—underlying much difference—in their admirable manner of dealing with facts closely, and at first hand, without the interposition of the formal laws, vague ideal conceptions, or "glittering generalities" which some philosophical naturalists make large use of.

A likeness may also be discerned in the way in which the works or contributions of predecessors and contemporaries are referred to. The brief historical summaries prefixed to many of Mr. Brown's papers are models of judicial conscientiousness. And Mr. Darwin's evident delight at discovering that someone else has "said his good things before him," or has been on the verge of uttering them, seemingly equals that of making the discovery himself. It reminds one of Goethe's insisting that his views in Morphology must have been held before him and must be somewhere on record, so obviously just and natural did they appear to him.

Considering the quiet and retired lives led by both these men, and the prominent place they are likely to occupy in the history of Science, the contrast between them as to contemporary and popular fame is very remarkable. While Mr. Brown was looked up to with the greatest reverence by all the learned botanists, he was scarcely heard of by anyone else; and out of botany he was unknown to Science except as the discoverer of the Brownian motion of minute particles, which discovery was promulgated in a privately printed pamphlet that few have ever seen. Although Mr. Darwin had been for twenty years well and widely known for his "Naturalist's Journal," his works on "Coral Islands," on "Volcanic Islands," and especially for his researches on the Barnacles, it was not till about fifteen years ago that his name became popularly famous. Ever since no scientific name has been so widely spoken. Many others have had hypotheses or systems named after them, but no one else that we know of a department of bibliography. The nature of his latest researches accounts for most of the difference, but not for all. The Origin of Species is a fascinating topic, having interests and connections with every branch of Science, natural and moral. The investigation of recondite affinities is very dry and special; its questions, processes, and results alike—although in part generally presentable in the shape of Morphology—are mainly, like the higher mathematics, unintelligible except to those who make them a subject of serious study. They are especially so when presented in Mr. Brown's manner. Perhaps no naturalist ever recorded the results of his investigations in fewer words and with greater precision than Robert Brown: certainly no one ever took more pains to state nothing beyond the precise point in question. Indeed we have sometimes fancied that he preferred to enwrap rather than to explain his meaning; to put it into such a form that, unless you follow Solomon's injunction and dig for the wisdom as for hid treasure, you may hardly apprehend it until you have found it all out for yourself, when you will have the satisfaction of perceiving that Mr. Brown not only knew all about it, but put it upon record long before. Very different from this is the way in which Mr. Darwin takes his readers into his confidence, freely displays to them the sources of his information, and the working of his mind, and even shares with them all his doubts and misgivings, while in a clear and full exposition he sets forth the reasons which have guided him to his conclusions. These you may hesitate or decline to adopt, but you feel sure that they have been presented with perfect fairness; and if you think of arguments against them you may be confident that they have all been duly considered before.

The sagacity which characterises these two naturalists is seen in their success in finding decisive instances, and their sure insight into the meaning of things. As an instance of the latter on Mr. Darwin's part, and a justification of our venture to compare him with the *facile princeps botanicorum*, we will, in conclusion, allude to the single instance in which they took the same subject in hand. In his papers on the organs and modes of fecundation in Orchideæ and Asclepiadeæ, Mr. Brown refers more than once to C. K. Sprengel's almost forgotten work, shows how the structure of the flowers in these orders largely requires the agency of insects for their fecundation, and is aware that "in Asclepiadeæ . . . the insect so readily passes from one corolla to another that it not unfrequently visits every flower of the umbel." He must also have contemplated the transport of pollen from plant to plant by wind and insects; yet we know from another source that he looked upon Sprengel's ideas as fantastic. Instead of taking the single forward step which now seems so obvious, he even hazarded the conjecture that the insect-forms of some Orchideous flowers are intended to deter rather than to attract insects. And so the explanation of all these and other extraordinary structures, as well as of the arrangement of blossoms in general, and even the very meaning and need of sexual propagation, were left to be supplied by Mr. Darwin. The aphorism "Nature abhors a vacuum" is a characteristic specimen of the Science of the Middle Ages. The aphorism "Nature abhors close fertilisation," and the demonstration of the principle, belong to our age, and to Mr. Darwin. To have originated this, and also the principle of Natural Selection—the truthfulness and importance of which are evident the moment it is apprehended—and to have applied these principles to the system of nature in such a manner as to make, within a dozen years, a deeper impression upon natural history than has been made since Linnæus, is ample title for one man's fame.

There is no need of our giving any account or of estimating the importance of such works as the "Origin of Species by means of Natural Selection," the "Variation of Animals and Plants under Domestication," the "Descent of Man, and Selection in relation to Sex," and the "Expression of the Emotions in Man and Animals,"—a series to which we may hope other volumes may in due time be added. We would rather, if space permitted, attempt an analysis of the less known but not less masterly, subsidiary essays, upon the various arrangements for ensuring cross-fertilisation in flowers, for the climbing of plants and the like. These, as we have heard, may before long be reprinted in a volume, and supplemented by some long-pending but still unfinished investigations upon the action of Dionæa and Drosera—a capital subject for Mr. Darwin's handling.

Apropos to these papers, which furnish excellent illustrations of it, let us recognise Darwin's great service to Natural Science in bringing back to it Teleology: so that, instead of Morphology *versus* Teleology, we shall have Morphology wedded to Teleology. In many, no doubt, Evolutionary Teleology comes in such a questionable shape, as to seem shorn of all its goodness; but they will think better of it in time, when their ideas become adjusted, and they see what an impetus the new doctrines have given to investigation. They are much mistaken who suppose that Darwinism is only of speculative importance and perhaps transient interest. In its working applications it has proved to be a new power, eminently practical and fruitful.

And here, again, we are bound to note a striking contrast to Mr. Brown, greatly as we revere his memory. He did far less work than was justly to be expected from him. Mr. Darwin not only points out the road, but labours upon it indefatigably and unceasingly. A most commendable *noblesse oblige* assures us that he will go on while strength (would we could add health) remains. The vast amount of such work he has already accomplished might overtax the powers of the strongest. That it could have been done at all under constant infirm health is most wonderful.

ASA GRAY

THE AUSTRALIAN MUSEUM

THE authorities of the British Museum may congratulate themselves on their not being the only governing body which is considered to be on an antiquated and improvable foundation, which calls for a radical and speedy change. In Australia the same cry has been raised before the Parliament of the Colony, with respect to the Museum at Sydney. There the biological collection seems to be much in need of improvement, of a greater spirit of enterprise in its management, and of a more liberal view being taken by its authorities of the rapid advances which are adding day by day to the importance of the subject which it so materially assists in teaching.

We may reasonably ask, what is given as the cause of this want of energy and progressive spirit in the colonial institution? Curiously enough it is the same as that which is being urged by all scientific men in this country against our national collection, which has found its most powerful expression in the Report of the Royal Commission on Scientific Instruction and Advancement of Science, noticed by us a short time ago (NATURE, vol. ix. p. 397), namely, that it is in the hands of a body of irresponsible trustees with a distributed authority, instead of under the management of a paid superintendent, who alone is accountable for all that is done.

It is the so-called "conservative spirit" of the authorities against which so much evidence of inefficiency is becoming so prominent. Science—and Natural Science especially—has been making such rapid progress of late years, that the mechanism by which it has to be taught, the elaborate nature of which is only fully understood by those who are actual workers within its confines, has not a sufficient inherent "go" to do the work expected of it. Just as by means of manual labour it was possible to thrash the cereal products of this country with profit in former times, whilst in the present day foreign competition makes the much more speedy steam apparatus absolutely essential; so when libraries of ancient manuscripts and the beautiful artistic remains of bygone days were the subjects which formed the most important topics for the consideration of the museum government, the bodies of trustees worked very well. The task they had on hand, being stamped with the name of fine art, was rather a pleasure than a labour; and the members of the board derived a *prestige*, and other advantages, from being able to follow their wonted tastes without any feeling of incompetency, or any scruples as to the general acceptance of their decision.

The biological element in our national collection has, however, introduced a different state of things. Those who can afford, from their pecuniary advantages, to spend their time and energies in unremunerative committees, are not the class who dirty their hands with the preliminary training necessary for a zoological or a botanical education. Neither of these subjects were whipped into them at Eton or at Harrow; they were too old to begin them, except perhaps in a very amateur manner, at Oxford or at Cambridge; and consequently when they find themselves appointed to any authoritative post in after life they set to the work with the antipathy they have always felt against "stinks."

How can a body so constituted be expected to forward the progress of Natural Science? The subject is a modern one. It is in need of hard organising work being done by experienced men who take a true interest in the object to be attained. Such men must be paid, not by paltry salaries no better than that of a banker's clerk; for how can men of ability and education be expected to present themselves as candidates for the posts, when there are so many much more remunerative ways in which they may get a larger competency?

If we look round at our public institutions we find that the machinery of those which prove themselves to be the most successful is that in which a single officer has the control, he being frequently re-elected, and responsible only to a body which criticise all his actions, and to which he refers all serious questions of finance and management. Inefficiency on the part of the officer under this arrangement allows of his replacement without difficulty, at the same time that he is continually kept up to his work by the superior governing body, who find it a much easier task to detect faults than they would to remedy them themselves.

The case of the Australian Museum is somewhat peculiar. That institution seems to be in the hands of a few collectors of the old school, who treat it as a plaything of their own, rather than a public institution, supported by public funds. They have a curator, Mr. Gerrard Krefft, of whose very high scientific position in the mother country they cannot be fully aware, or they would be more liberal to him, and give him more opportunities for the employment of his abilities. The naturalist who on seeing the curious new mud-fish from Queensland was enabled to say from a superficial examination, that it "is allied to Lepidosiren, and is Ceratodus"—a statement which Dr. Günther's superb monograph on that fish so strongly substantiates—and who has done such excellent work with regard to the Marsupialia, both recent and extinct, deserves greater opportunities than he evidently possesses under the tender mercies of amateur trustees, especially when they include among their numbers men such as a Mr. Macleay, who has thought it worth his while to refer to this journal in terms which clearly indicate either that he has never heard of it or of the Royal Commission whose recommendations we reproduced, or that he has not the least sympathy with the subjects of which it treats; the latter of which tendencies must make him quite unsuitable for the position which we regret to see he holds as one of the governing body.

The complaint of Mr. Cooper, who applied for a select committee to inquire into and report upon the condition and system of management of the museum, was that—

"As a rule a body of trustees was not the proper body to manage such institutions. Persons who were unpaid and irresponsible did not take that interest in the institution they ought to do, and would not devote their time to it. The Government found the whole of the money to pay the cost of the institution, and surely they ought to have a voice in its management. In asking for the committee, he had not the slightest desire to censure the trustees. He believed they did the best they could, but many of them could not devote the time that was necessary."

In the discussion which followed it was shown that on all occasions it is difficult to get a quorum, except on an occasion like that in which it was proposed to employ the museum-building as a ball-room during the visit of the Duke of Edinburgh to Sydney, when of the twenty members of the committee, the ten official were in favour of its employment as such, in opposition to those who sat by election.

A committee was finally appointed to consider the question of appointing a permanent officer, and if they then conclude their deliberations by placing Mr. Krefft in a position worthy of his scientific attainments, they will confer as great a benefit on zoology generally, as they will show a power of appreciating worth, independent of petty party-spirit.

RIBOT'S "ENGLISH PSYCHOLOGY"

English Psychology. Translated from the French of Th. Ribot. (Henry S. King and Co.)

SEEING that the doctrines of the English school of Experimental Psychology are "unknown, or very nearly unknown, in France," M. Ribot has certainly done a very useful work in giving to the French people an analysis of the conclusions in mental science arrived at by Hartley, James Mill, Herbert Spencer, A. Bain, G. H. Lewes, Samuel Bailey, John Stuart Mill. The most substantial objection that could be urged against such an undertaking is the difficulty of doing satisfactorily the thing attempted. In no department of knowledge claiming the name of Science is there so little settled doctrine; indeed, Mr. Lewes has just told us in his "Problems of Life and Mind" that there is still wanting the materials for its construction as a science; nor is there in any science so little agreement among the authorities, or so great probability that honest application may be rewarded with an entire misapprehension of their meaning. The book before us is of course M. Ribot's answer to this objection; and we are bound to say that, considering the special difficulty of the task, and remembering the object he had in view, it is a very worthy and valuable performance. While there is probably not one of the writers whom he has undertaken to expound who would not object to his rendering of one or other of their opinions, all must, we think, agree in regarding M. Ribot as a highly appreciative student, and must feel grateful to him for this attempt to spread their opinions. Indeed to us M. Ribot seems rather to err in the direction of wishing to present in the most favourable light, and to make the most of, the views of each writer in turn.

Partly, perhaps, to this same amiable disposition may be referred the impression of greater agreement among the authorities given by a perusal of M. Ribot's pages than by a study of the authors themselves. Mr. Herbert Spencer is, and with all justice, placed at the head of our psychologists; and Prof. Bain is made to differ from him in no essential particular—an interpretation which we are inclined to believe would be accepted much more willingly by Prof. Bain himself, who now recognises the doctrine of inheritance, and would fain have it understood that his disagreements with Mr. Spencer on some other points "are more apparent than real," than by his less clear-sighted disciples. The account of Prof. Bain's theory of the *supposed* acquisition of voluntary power opens with a statement that here we have "the idea of progress, evolution, and development." But the instructed student in these matters must know that the growth of voluntary power that Prof. Bain would explain is not the evolution of Mr. Spencer; it is, on the contrary, a description of the manner in which, according to his imagination, each individual acquires those powers which, according to the doctrine of evolution, they do not acquire, but inherit. For the benefit of those who would now save this theory by maintaining that it meant or means something that was never intended, we would quote the example given in illustration by M. Ribot:—"Few of our necessities are so pressing as thirst; nevertheless an animal does not distinguish at first that the water in the pond can appease it; it is only later in his wanderings that he comes to apply his tongue to the surface of the water (happy accident) and to feel the relief which it affords, and thus to learn what he ought to will." Few of the poor animals, we fear, would ever reach maturity if they had not more of instinct than Prof. Bain would here allow them. Yet what Prof. Bain has written about instinct he claims, and M. Ribot thinks "justly, as one of the most original portions of his work." Unfortunately for the fame of this celebrated psychologist, it appears from the progress of research that exactly in those departments where he has been most original have his conceptions been least in accordance with the order of Nature.

M. Ribot's most serious labour seems to have been in bringing together, in a more or less connected form, the psychology which has hitherto been scattered through the writings of Mr. George Henry Lewes. This original thinker and highly suggestive writer is the only one of our psychologists whose work may not be regarded as finished. The volume recently published ("Problems of Life and Mind") does not supply material for an estimate of the work on which he has long been engaged. But while continuing to agree with Mr. Spencer much more than any other of the authorities, Mr. Lewes encourages his readers to hope for important and permanent additions to mental philosophy; and to put the prospects of the work at the lowest, he will certainly compel the school to which he belongs to gravely reconsider some of their fundamental positions.

When in his conclusion M. Ribot attempts to bring forward the points on which the writers are agreed, the "fundamental propositions" to which he reduces them are unsatisfactory in two ways. Many of them are so vague in expression as not to exclude rival theories; while others have a sufficient amount of precision to make them flat contradictions of the clearly expressed and reiterated opinions of some of the authorities. We are, for example, not surprised to hear a disciple of Mr. Mill and Prof. Bain express his astonishment that his masters should have fathered on them the realism they have so vigorously opposed. M. Ribot's words are explicit:—"Outside of us, and independently of our perceptions, there exists a material world which condemns idealism. It is conformable to the data of the sciences to believe that this material world, taken in itself, does not resemble the perceptions of it which we have; this condemns vulgar realism." It surely says little for idealism that M. Ribot, after studying and expounding the arguments in its favour, should thus end with making our idealists agree with that very realism which Prof. Bain has described as unworthy the name of Philosophy.

After recognising the shortcomings referred to, it remains to be repeated that the author deserves the thanks of everyone interested in the spread of mental science in France. But we are unable to find any reason for the book having been translated into English. No English student ought to go to M. Ribot for the opinions of Mr. Mill or Mr. Spencer. Should any not already familiar with the topics discussed attempt to read the work, they will frequently be much perplexed by the exceeding carelessness of the translation. If they are amused to read that "*melodies* are described in pathological treatises," they may be a little puzzled to make out how "all Science is *contradicted* by the double action of analysis and synthesis," or in what sense "so long as the living being has no consciousness he leads a purely psychological life." And we would hint to any innocent young persons disposed to pin their faith to Locke, that they may be in some danger of being misunderstood should they follow the uniform usage of the translator and describe themselves as "*sensualists*." DOUGLAS A. SPALDING

OUR BOOK SHELF

Africa: Geographical Exploration and Christian Enterprise. By A. Gruar Forbes. (London: Sampson Low and Co. 1874.)

WE can recommend this moderate-sized volume as an interesting popular *résumé* of the progress of discovery in Africa from the earliest time to the present day. The author writes mainly from the point of view of missionary enterprise, but seems to have read with diligence and intelligence the greater part of the literature of modern African travel, with which his book is mostly concerned, and has made therefrom a creditable compilation showing the progress of discovery from Bruce downwards. The first chapter gives a brief account of the topography, climate, and productions of Africa; and the accompanying pretty clear map shows the route of the leading explorers. We notice one or two signs of carelessness or haste; for example, on p. 4, Mr. Forbes states that "the most westerly point is Cabo Verde, in long. 51° 25′ E., lat. 10° 25′ N., the distance between the two points being about the same as its length." Again, at p. 115, "Sahara Desert" ought surely to be "Kalahari Desert."

LETTERS TO THE EDITOR

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

Ocean Circulation—Dr. Carpenter and Mr. Croll

IN the interests of Science, of scientific discussions, and of scientific men let me be allowed to protest very earnestly against the manner in which Dr. Carpenter has thought fit to reply in your columns to the defence which Mr. Croll made against the representation of his views, given in NATURE, vol. ix. p. 423. I take much interest in the subject under discussion—the great fundamental cause of the distribution of heat over the globe, and am most anxious to arrive at the true solution of the problem—a result, however, which will be indefinitely postponed if such letters as that of Dr. Carpenter in NATURE, vol. x. p. 62, are to become common.

Mr. Croll, discarding unimportant details, asked attention to one or two cardinal "misapprehensions" on which Dr. Carpenter had been proceeding. But the Doctor, instead of plainly grappling with these alleged "misapprehensions," runs off to call attention to a footnote of another paper of Mr. Croll's, brings forward some statement of Mr. Croll's views about the relative saltness of different portions of the ocean (about which, however, not a single word is said in the letter that has called forth Dr. Carpenter's reply), and concludes by another *argumentum ad hominem*, of which I am sure every reader of his papers must now be weary.

Now I strongly object to have dust thrown in my eyes in this way. Dr. Carpenter does not attempt to deal with any one of the cardinal and crucial arguments in Mr. Croll's letter. He raises a cloud about "averages," repeats his joke about ten children to every marriage, and with other irrelevant matter, including an introduction of the Astronomer Royal and Prof. Mohn, suddenly disappears. Not, however, without adding a sentence which I am sure he will in the end regret. He says he has "been forced by the personal attacks in which Mr. Croll has latterly thought fit thus to indulge to retort upon him." Why, the discovery of anything "personal" in Mr. Croll's writings would be as great a find as the true theory of oceanic circulation. I do not know any papers in our contemporary scientific literature more thoroughly undeserving of such a charge. Surely a man may call in question, nay, may even take a little quiet fun out of another man's opinions or crotchets without laying himself open to the stigma of being guilty of "personal attacks." Besides, it seems to me that Dr. Carpenter's charge is inappropriate. Mr. Croll, remarking "with some reluctance" that he was "compelled to refer" to Dr. Carpenter's continual quotation of eminent physicists who had adopted his views, while none had shared in the objections to them, merely assured Dr. Carpenter that such was not the case, and made reference to one person as an illustration, but without giving the person's name. The Doctor, as everybody knows, has been profuse in his use of this kind of argument. And now the moment it is used against himself, he denounces the introduction of "personal attacks!"

I purposely avoid entering into the merits of the question. What, in common with every sincere well-wisher of Science, I desire to see, is its thorough, honest and courteous discussion. Dr. Carpenter's high position gives a weight to what he says and does, which adds much to the regret with which his letter will be perused. That this protest may be received on its own merits and without reference to the pen which holds it, I withhold my name.

F. R. S.

Proportionality of Cause and Effect

MR. HAYWARD now affects the air of an injured man, and complains of being charged with "confusing issues" which he neither "raised nor accepted." He may be convicted out of his own mouth. The following passage occurs in his last letter but one (NATURE, vol. x. p. 25):—"It should be noted that my principal 'exemplification of *unconsciously-formed preconceptions*' was of Mr. Spencer's own choosing, namely, Newton's 'Second Law of Motion.'" In his last he says:—"The object of my remarks was simply to test the truth of a definite assertion of Mr. Spencer that 'the Second Law of Motion is an immediate *corollary of the preconception* of the exact quantitative elation between cause and effect.'"

Now let the words italicised be compared. In the first passage Mr. Spencer is said to hold that the Second Law of Motion is a preconception. In the second he is represented as maintaining that it is a *corollary* from a preconception. Is not this "confusing issues"?

Mr. Hayward has the choice of two alternatives. He may admit that one of these statements is a misrepresentation of Mr. Spencer's doctrine, as was alleged. Does he refuse to do this? Then he may transfix himself on the other horn of the dilemma, and boldly assert that in his view a preconception and a corollary from a preconception are one and the same thing. But until Mr. Hayward can arrive at some agreement with himself as to the terms in which he shall state Mr. Spencer's theory, the conclusion of impartial outsiders will probably be that he is not yet in a position to pronounce authoritatively on the merits of it.

"A Senior Wrangler" is good enough to say that my letter makes him feel "something like Alice behind the looking-glass." After this amenity, one may be pardoned for stating the position which *his* mental altitude leaves. A famous metaphysician once wrote an essay to prove that the narrow discipline of mathematics produces an incapacity for general reasoning. Sir W. Hamilton would have found his *à priori* arguments confirmed if he could have read the letter of "A Senior Wrangler."

The "Senior Wrangler" quotes a sentence of mine to the effect that "the experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced; and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved." He does not quote a previous passage in which it is said:—

"Here, as in the examples about to be given, the relation between cause and effect, though numerically indefinite, is definite in the respect that every additional increment of cause produces an additional increment of effect; and it is out of this and similar experiences that the idea of the relation of proportionality grows and becomes organic."

It might have been supposed that the doctrine so expressed was effectually guarded against misapprehension. Are not the preconceptions derived from the child's muscular experiences described as *numerically indefinite* (*i.e.* not expressible in *proportional numbers*)? Is it not said that out of them the idea of the relation of proportionality *grows?*" In the very sentence quoted by the "Senior Wrangler," is it not said that the notion of proportionality is *implicated* in the child's consciousness, and that the physical axiom comes from this *latent* consciousness? And yet the "Senior Wrangler," looking down from his mathematical heights, and catechising me as he would a schoolboy, asks me whether I know "what proportionality means"?

But for the letter of a "Senior Wrangler," one would have believed that it was made clear to everyone that the notion of proportionality generated by these early experiences was vague and general, not exact. How else should I have said that from it "the axiom of the perfect quantitative equivalence of the relations between cause and effect is *evolved?*" After thrice reading "First Principles" does not the "Senior Wrangler" know that being evolved includes passing from indefiniteness to definiteness? How then can he pretend that it is meant that the child gets from his experiences the knowledge that a double effort produces in all cases just double the result? The argument obviously implied is that this is the *finished conception* finally arrived at by the adult, as holding in those cases where causes and effects are uncomplicated.

Having but limited space, and assuming that the requisite qualifications would be made by unbiased readers, I passed over all those details of the child's experiences which would have been required in a full exposition. Of course I was aware that in the bending of a stick the visible effect does not increase in the same ratio as the force applied; and hardly needed the "Senior Wrangler" to tell me that the resistance to a body moving through a fluid increases in a higher ratio than the velocity. It was taken for granted that he, and those who think with him, would see that out of all these experiences, in some of which the causes and effects are simple, and in others of which they are complex, there grows the consciousness that the proportionality is the more distinct the simpler the antecedents and consequents. This is part of the preconception which the physicist brings with him and acts upon. Perhaps it is within the "Senior Wrangler's" knowledge of physical exploration, that when the physicist finds a result not bearing that ratio to its assigned cause which the two were ascertained in other cases to have, he immediately assumes the presence of some perturbing cause or causes, which modify the ratio. There is, in fact, no physical determination made by any experimenter which does not assume, as an *à priori* necessity, that there cannot be a deviation from proportion without the presence of such additional cause.

Returning to the general issue, perhaps the "Senior Wrangler" will pay some respect to the judgment of one who was a Senior Wrangler too, and a great deal more—who was distinguished not only as a mathematician but as an astronomer, a physicist, and also as an inquirer into the methods of Science: I mean Sir John Herschel. In his "Discourse on the Study of Natural Philosophy," he says:—

"When we would lay down general rules for finding and facilitating our search, among a great mass of assembled facts, for their common cause, we must have regard to the characters of that relation which we intend by cause and effect."

Of these "characters" he sets down the third and fourth in the following terms:—

"Increase or diminution of the effect, with the increased or diminished intensity of the cause, in cases which admit of increase and diminution."

"Proportionality of the effect to its cause in all cases of *direct unimpeded* action."

Observe that, in Sir J. Herschel's view, these are "characters" of the relation of cause and effect to be accepted as "general rules for *guiding* and facilitating our search" among physical phenomena—truths that must be taken for granted *before* the search, not truths derived *from* the search. Clearly, the "proportionality of the effect to its cause in all cases of direct and unimpeded action" is here taken as *à priori*. Sir J. Herschel would, therefore, have asserted, with Mr. Spencer, that the Second Law of Motion is *à priori;* since this is one of the cases of the "proportionality of the effect to its cause."

And now let the "Senior Wrangler" do what Sir J. Herschel has not done or thought of doing—*prove* the proportionality of cause and effect. Neither he, nor any other of Mr. Spencer's opponents, has made the smallest attempt to deal with this main issue. Mr. Spencer alleges that this cognition of proportionality is *à priori:* not in the old sense, but in the sense that it grows out of experiences that precede reasoning. His opponents, following Prof. Tait in the assertion that Physics is a purely experimental science, containing, therefore, no *à priori* truths, affirm that this cognition is *à posteriori*—a product of conscious induction. Let us hear what are the experiments. It is required to establish the truth that there is proportionality between causes and effects, *by a process which nowhere assumes* that if one unit of force produces a certain unit of effect, two units of such force will produce two units of such effect. Until the "Senior Wrangler" has done this he has left Mr. Spencer's position untouched.

Bayswater, May 20 JAMES COLLIER

The Great Ice-Age

IN reply to Mr. Belt's letter (p. 62), I did little more than express an adverse opinion to his theory, because to discuss it would have required an essay. I expressed this because I notice that unless something like a demurrer is entered against a new theory it is apt to be taken for granted in subsequent text-books and papers written by those who have had no opportunities of obtaining a practical knowledge of the subject. For the above reason I must answer his strictures very briefly.

(1) I fail to see why the Scandinavian sea-beaches are irrelevant. (2) I have more than once read Mr. Tiddeman's paper, and without committing myself to all its conclusions, think I may quote it as assuming that the Lake district (as distinguished from North Lancashire) was the centre of a great ice-sheet; not that it was over-ridden by ice coming from somewhere further north. The same might be expected to be the case with the Welsh mountains; and Mr. W. Kingsley has brought forward good evidence of the existence of an ice-sheet there also. (3) Mr. Belt appears to forget that shells have been found not only at Moel Tryfaen, but also near Llyn Ffynnon-y-gwas, about two miles west of the peaks of Snowdon. Does Mr. Belt mean to say that Snowdon could not protect itself in the heart of its own domain better than this? If the Lake mountains had an ice-sheet, surely Snowdonia? Mr. Belt asks for evidence of the shore of the glacial sea. I reply that to me these and the Moel Tryfaen beds, not to mention others, appear to be far more probably littoral deposits than transported. For example, I think it in the highest degree improbable that the Vale Royal shells (Lyell, "Antiquity of Man," p. 317) could be brought to their present position (more than 1,100 feet above the sea) by any ice-sheet without the cold being enough to cover *all* the higher ground in Britain with ice, and so protect it. I did not deny a glacier might push a stone before it up-hill; my contention was that the enormous force which would be exerted on beds scooped out as described, and shoved some 1,500 feet up-hill for miles over broken ground, would crush the shells to a far more comminuted state than they are now in. With regard to Holderness, Mr. Croll's view of the shells there appears to me to be at present only a *theory* of which Mr. Searles V. Wood, jun., has effectually disposed (Geol. Mag. 1872). I grant there are some difficulties in the submergence theory; my position is that those in Mr. Belt's are very much greater.

A recent perusal of Mr. J. Geikie's suggestive book, the "Great Ice Age," has brought before my mind more strongly than ever a dilemma, which, as it appears to me, the modern school of Glacialists cannot escape.

He speaks of the till as a *grund moräne* or *moraine profonde* formed between the glacier and the rock, while he attributes the majority of rock-basins to the action of the glaciers. Now it appears to me that if the glaciers could pass over considerable deposits of this *moraine profonde* without sweeping it clean away, then their action as erosive agents must have been comparatively feeble; or, if they could scoop out great rock basins like the Alpine and (buried) Highland lakes, then they would have peeled off almost all the till from the land. As it appears to me, the analogy with a river, by means of which Mr. J. Geikie (p. 88) seeks to escape from a portion of this difficulty, does not hold. When a river begins to deposit sand and gravel largely, its work as an erosive agent at that place is almost over. Besides we cannot conceive a nearly solid mass, like a huge glacier, changing its motion so rapidly as a stream of water. Difficult as it undoubtedly is to explain some of the lake-basins, it appears to me that the great bulk of his evidence, with regard to till and other deposits over which ice-streams have passed, shows how slight under ordinary circumstances is their erosive power; and this has been confirmed by every journey that I have made among the Alps. I may add also that from study of the same regions my faith in a *moraine profonde* is much shaken. I believe that, except possibly as a very local and exceptional phenomenon, it exists solely in the imagination of the eminent geologists of whom Mr. Geikie is a disciple. T. G. BONNEY

St. John's College, Cambridge, May 26

Photographic Irradiation

IN the paper referred to by Prof. Forbes (NATURE, vol. x. p. 29) what is ordinarily called Photographic Irradiation was attempted to be explained by us, not as being caused by reflections from the back of the plate, but as being due to the sum of all the optical imperfections of the instrument with which the photograph is taken.

If Mr. Stillman (p. 63) will refer to our original paper, published in the *Monthly Notices* for June 1872, he will find that only the cloudy indefinite haze which surrounds the image of a luminous object, and which has frequently been called halation, was referred by us to reflection from the back of the plate.

When an over-exposed photograph is taken upon an opaque plate a marked fringe of irradiation still remains, and experiments were instituted by us which appeared to show that this is not to be accounted for by any circulation taking place within the thickness of the collodion or by the chromatic dispersion of the lenses; but when the oblique pencils from the edges of the lenses were stopped out the irradiation fringe was found to be greatly decreased. We were led to conclude that irradiation is to be accounted for by the fact that each luminous point in the object is not accurately represented by a luminous point in the image, but rather by a luminous patch of sensible area, the central and more intense portion of which prints itself first in the photograph, giving comparatively sharp picture prints when the exposure is short; but as the picture is still further exposed, the outer portions of the luminous patches imprint themselves, and by their overlapping cause the blurred appearance to which has been given the name of irradiation. LINDSAY

A. COWPER RANYARD

Uncompensated Chronometers and Photographic Irradiation

WITH regard to the employment of uncompensated chronometers (NATURE, vol. x. p. 63), I have every reason to believe that the Russians alone have tested them. For some reason which is not easily discovered, the employment of a negatively compensated chronometer has not given any very remarkable results. The Russians have employed simply an uncompensated chronometer; and have obtained very remarkable results as mentioned in my article on the Transit of Venus to which Prof. Everett has alluded.

With regard to the prevention of photographic irradiation, of course various means have been employed for dry plates; but I believe that Lord Lindsay and Mr. Ranyard were the first to experiment on the matter exhaustively. I believe Mr. Stillman would be interested in reading their paper in the *Monthly Notices*. At the same time all honour is due to the photographers named by him for their experiments.

Birkenhead, June 1 GEORGE FORBES

The Seal Fishery

CAPT. DAVID GRAY, of the steamship *Eclipse*, has done good service to the cause of humanity in writing, and Mr. Buckland in publishing, the letter on the seal fishery which appears in *Land and Water* for May 9. The fearful cruelties perpetrated year after year, and the enormous waste of life entailed by the reckless manner in which the seal fishery is prosecuted, are well known, but no steps have hitherto been taken to regulate a trade which, if carried on within proper bounds, would continue to yield great profits, but if still pursued with such utter disregard to consequences must soon end in the extermination of the whole race. As an instance of the wastefulness of the mode of proceeding, Capt. Gray says that five ships attacking a pack of seals, in four days killed about 10,000 old seals; "add 20 per cent. for seals mortally wounded and lost, gives an aggregate of 12,000 old ones; add 12,000 young which died of starvation, gives 24,000; but this is not all. The men spread on the ice, so that the old ones that were left alive could not get on to suckle their young. The consequence was that the whole of the young brood was destroyed, and had these seals been left alone for *eight or ten days*, I am quite within the mark when I say that, instead of only taking 300 tons of oil out of them, 1,500 could as easily have been got, and that without touching an old one." In one day by the men of the five ships upwards of 4,000 old seals were taken, "the young ones in thousands yelling for their mothers, following the skins as the men dragged them to the ships, and sucking the crangs, *i.e.* skins, in desperation." The maternal love for its offspring was made use of to save the men trouble, as a seal killed when giving suck was more easily secured, and often seals desperately wounded were seen administering nourishment to their young ones. The plight of the young ones which had lost their mothers was pitiful in the extreme; they were seen huddling together for heat, "and trying to suck one another," till they at length succumbed. Capt. Gray exclaims, "surely there is influence enough left in Great Britain to prevent a continuation of such barbarity. I overheard some of my men saying to one another, 'It is a shame this sort of work;' and so it is. It is a shame that any civilised Government should allow its subjects to perpetrate such cruelty when it could so easily be prevented. The remedy is simply, *let the ships be kept from sailing before March* 25; ships now sail from Feb. 25 to March 1. This would give a fortnight to make the passage, and find the seals in; by that time the young would be beginning to be worth taking, and a fearful waste of life put a stop to that now annually occurs." The accounts of the cruelties practiced in sealing are sickening in the extreme, the only thing considered being how to deprive as great a number of their skin and blubber in as short a time as possible. Mr. Brown (Proc. Zool. Soc., 1868) remarks: "Seals are very tenacious of life, and difficult to kill, unless by a bullet through the brain or heart. They are so quickly *flensed* (the operation of removing the blubber and skin) that after having been deprived of their skin they have been seen to strike out in the water; so that the sympathies of the rough hunters have been so excited that they will pierce the heart several times with their knives before throwing away the carcass." These movements Mr. Brown attributes to reflex action, but considering the haste of the operation, and the seal's known tenacity of life, it is quite as likely that it was merely a stunned and not a dead animal thus deprived of its skin and blubber. It is terrible to dwell thus upon the horrors of this cruel trade, which make even the hardened participators sicken and relent, but it is necessary that it should be done, in order, if possible, to reach the hearts of Englishmen, and enlist their sympathies. If these beautiful and harmless creatures must be sacrificed for our requirements, it is a duty incumbent upon us to see that their destruction is carried out mercifully, and with the infliction of as little suffering and waste of life as possible.

In a commercial point of view the reasons for exercising some supervision over the seal fishery are as strong as those dictated by mere humanity. The revenue produced by this branch of industry is considerable. Mr. Brown estimates the annual value of the Greenland fishery alone at 116,000*l.* (Proc. Zool. Soc., 1868, p. 439), and ominously adds: "Supposing the sealing prosecuted with the same vigour as at present, I have little hesitation in stating my opinion that, before thirty years shall have passed away, the 'seal fishery' as a source of commercial revenue will have come to a close, and the progeny of the immense number of seals now swimming about in the Greenland waters, will number comparatively few." We cannot plead want of warning, for we have numerous instances of marine animals which have been exterminated by untimely slaughter (See Prof. Newton's "Extermination of Marine Mammalia," NATURE, vol. ix. p. 112). Steller's Mantee survived its discovery only about twenty-seven years; the Atlantic Right Whale, which formerly gave employment to a great number of hardy fishermen in the Bay of Biscay and English Channel, is probably exterminated; the Northern Right Whales are gradually driven farther and farther north, and the risk of following them is becoming proportionately greater; the same may be said of the walrus. The northern fur-seal was rapidly passing away, and but for the timely intervention of the Russian and American Governments would probably have been lost; and from our antipodes comes an appeal repeating all the cruelties and waste of life to which our northern seals are subjected, and pleading for protection on behalf of the southern fur-seals (W. A. Scott, "Mammalia, Recent and Extinct," Sydney, 1873).

The question arises, how is this wanton destruction to be stopped and the fishery to be placed on a sounder footing? In order that it may be done effectually, the regulations must, without doubt, be "international;" and no time should be lost in carrying them into effect. The British Association has rendered good service in obtaining an Act to protect sea-birds during their breeding-time, and if, assisted by men of practical experience such as Capt. Gray, they were to urge upon the Government some course of action, they would be supported by all the scientific bodies and leading naturalists in the kingdom.

Norwich, May 12. THOS. SOUTHWELL

THE COMING TRANSIT OF VENUS *

VII.

IN our last article the preparations of Britain, Germany and Russia were enumerated; those of the French, Americans, Dutch, and Italians must now be spoken of.

V. The French will occupy the following stations:—Yokohama, Pekin, New Amsterdam or St. Paul's, and Campbell Island; all equipped as first-class stations, besides Tientsin, Sagou, Numea, and probably Nukahiva in the Marquesas, as secondary stations. Yokohama and St. Paul's will make an excellent combination for the method of durations; at Campbell Island also the durations will be considerably lessened. But the longitude of these places will be determined, so that if only one contact be observed, De l'Isle's method will be applied. MM. Wolf and André have made a series of experiments on the formation of the "black drop;" numerous trials have also been made with a view of employing the photographic method as successfully as possible, and it is possible that spectroscopic observations of external contact will be made. The preparations are by no means so far advanced as might have been wished. This is partly due to the disturbed state in which the country has been since the late war.

We are glad to be able to state that the French will employ the daguerreotype process of photography. This method has many advantages, and it is much to be regretted that no experiments have been made by other nations to test its applicability. Photographs taken by this process are well known to be much more delicate and give clearer details than any others, while photographic irradiation is reduced to a minimum. It is even possible to correct for curvature of field by employing prepared plates whose surfaces are portions of spheres, a thing which would be impossible by any other process. There can be no shrinking of the film. The only objection is, that we cannot print copies from the plates conveniently. But it is not likely that we should trust to measurements of a printed copy even from a glass negative. The French are relying mainly upon the photographic method, and have chosen their stations for determining thus directly the least distance between the centre of the sun and Venus. With the apparatus proposed by MM. Wolf and Martin, the size of the sun's image will be 60 millimetres; they hope to determine the instants of internal contact with a probable error of one second of time. The commission into whose hands the business has been entrusted has drawn up a detailed report containing contributions not only from the astronomers of France, but also from the most celebrated physicists and experimentalists: 300,000 fr. has been voted for the enterprise. M. Tisserand of the Toulouse Observatory will aid in the actual observations; and M. Jannsen will proceed to Yokohama.

M. Dumas takes the lead in the preparations. In a letter dated May 12, he says tha the expeditions are on the point of starting, and that the Marquesas probably, and Numea certainly, will be occupied for De l'Isle's method.

VI.—The Americans have a grant of 150,000 dols. They have paid great attention to the application of photography with the assistance of Mr. Rutherford, whose success in photographing the moon is so well known,

* Continued from p. 69.

They employ a lens of 40 ft. focus, as already described. They will measure both angles of position and distances from the centre, and the probable error of any measurement will be less than 1-100 per cent. They have encountered some trouble in the manufacture of their siderostats. Besides photography eye-observations of contact will also be made. A very able report has been drawn up from the computations of Mr. Hill, who deserves great credit for the manner in which he has completed it. This report has reference to the choice of stations; and is accompanied by very valuable charts. Other reports have been made upon the application of photography.

The expeditions are to be composed of five persons each. The stations of observation and the heads of parties are as follows:—Wladivostock, Siberia, Prof. A. Hall, U.S.N.; Nagasaki, Japan, Mr. G. Davidson, U.S. Coast Survey; Peking, China, Prof. James C. Watson; Crozet's Island, South Indian Ocean, Capt. Raymond, U.S.A.; Kerguelen's Island, South Indian Ocean, Lieut.-Commander George P. Ryan, U.S.N.; Hobart Town, Tasmania, Prof. W. N. Harkness, U.S.N.; New Zealand, Prof. C. H. Peters; and Chatham Island, South Pacific, Mr. Edwin Smith, U.S. Coast Survey.

The whole organisation has been entrusted to a commission, the secretary of which is Prof. Newcomb, who has done so much valuable work for astronomy; he has taken great pains to insure success for the expedition, and has visited Europe to discuss the preparations necessary and to examine the instruments to be employed.

VII.—The Italians have arranged to send out three expeditions furnished with spectroscopes for the observation of external contact. Little is known about these expeditions.

VIII.—The Dutch are sending one expedition to the island of Bourbon or Réunion. It will be furnished with a photo-heliograph, which Dr. Kaiser will manipulate; Dr. Oudemans will also make observations with a heliometer.

Having now completed our description of the details, and having also given an account, so far as possible, of the preparations of the various nations for the observations, we shall cast a general view over the whole subject, and recapitulate some of the principal details.

The coming transit of Venus will be observed from about 75 stations, at many of which there will be a large number of instruments. The expense of the whole of the expeditions will amount to between 150,000*l.* and 200,000*l.* It may seem to some that the results to be arrived at are not worth so great an outlay, but the general voice of the non-scientific as well as of the scientific world has contradicted this. Wherever knowledge can be gained it is worth being gained; and when individuals are unable to bear the cost, it is fitting that the expenses should be incurred by those governments that are really the gainers from many scientific researches for which the investigator himself frequently receives no reward. But apart from this, these expeditions will lead to most valuable results. The sun's distance being known, the Lunar Theory may be vastly improved, and it will be possible to determine longitudes with much greater accuracy than at present. Still more will the tables of Venus be capable of re-adjustment. Even now we can calculate her place with great accuracy, and this is fortunate, since it enables us to predict the exact time at which Venus will first come in contact with the sun, viz. 1874, Dec. 8d. 14h. 4m. The error to which this is liable, owing to the tables, is not likely to exceed five minutes. Mr. W. H. M. Christie, chief assistant of the Royal Observatory, has determined the probable error in the calculated time of contact arising from this cause.* He has employed observations of Venus taken at this node at the following dates:—1872, June 28; 1873, Jan. 18; 1873, Sept. 14; he has thence deduced the error in the tabular position of Venus, and from this the error in the time of contact in the coming transit. It appears from each of these three comparisons that the tables of Venus give us the time of contact too early; according as we adopt the first, second, or third of the above observations, the error will be 7·4m., 5·3m., or 4·2m.

* Monthly Notices of the R. A. S. xxxiv. 300.

Besides the astronomical advantages to be gained from the coming transit, there are several collateral issues of no small importance. In the first place, the longitudes of a host of stations all over the globe will be accurately determined, and it is a remark by no means unworthy of notice that the simple observation of the local time of contact will give the inhabitants of east Africa and of all Asia an accurate means of determining their absolute longitudes. If, moreover, as has been proposed, San Francisco and Japan are to be compared directly as to longitude, the whole circuit of the globe will be completed by telegraphic and accurate chronometric determinations.

Again, with the host of vessels by which scientific men will proceed to their stations, meteorological, and sometimes even magnetical, instruments will be provided. These vessels will be traversing the different oceans of the globe about the same time, and thus the meteorology of the world will be much better understood. Many observers will be enabled to take note of interesting phenomena, such as hurricanes, volcanoes, and earthquakes. In addition, naturalists will be appointed to accompany some of the expeditions; birds and marine animals will be probably very generally collected; the Royal Society has given funds to aid in this matter. The Rev. A. E. Eaton, who has made valuable collections at Spitzbergen, will examine the marine life of Kerguelen's Island. Rodriguez is particularly interesting from a naturalist's point of view; it is one of the few islands in mid-ocean which have not been raised by volcanic agency. The remains of some extinct birds have been found there. The Royal Society has appointed a geologist, a botanist, and a naturalist to go to this island. There is little doubt that Science in general will gain greatly by these expeditions.

As to the main observation we can have no doubt from the large number of expeditions, and from the multiplicity of methods to be employed, that we shall obtain excellent results, although the actual reduction of the observations will be exceedingly laborious. Each nation, while it generally adopts some special method for its choice of stations, will also utilise other methods. We have seen that the English, while they rely chiefly on De l'Isle's method, will employ all the others except the heliometric, while the Germans depend mainly upon the heliometric method. The French and Americans have chosen their stations with reference to photography. The Russians are to compare observations of all kinds with different nations. These countries have all co-operated in the most harmonious manner, partly by correspondence, and partly by the personal visits of astronomers to different nations.

Although the observations are to be made at the end of the present year, the actual reduction of the observations will take so long that we cannot hope for the complete and final results as to our distance from the sun before the year 1876. At each of the British stations the observers will remain at least three months to determine their longitudes.

Here we may leave the subject. The preparations are for the most part completed; many of the observers of different nations are on their way to their various posts. It says a great deal for the civilisation of the world that on December 8 of the present year those quarters of the globe will be thickly studded with emissaries from so many nations to observe an important astronomical phenomenon.

It will be well to conclude this series of articles with a statement of the arrangements which have been made as to observers on the British expeditions. It is extracted from instructions published under authority:—

Appointments of Observers to the several Districts of Observation, and Subordination of Observers

1. Capt. G. L. Tupman, R.M.A., is head of the entire enterprise, and is responsible through the Astronomer Royal to the Government for every part. Every observer is responsible to Capt. Tupman.

2. When the different expeditions are separated, the observers in each district of observation are responsible to the local chief of the district, and the chief to the Astronomer Royal. The districts of observation and the observers will be the following, the name first following that of the local chief being that of the deputy, who will, if necessary, take his place :—

3. District A. Egypt : Chief, Capt. C. O. Browne, R.A., astronomer ; Observers, Capt. W. de W. Abney, R.E., astronomer and photographer ; S. Hunter, astronomer.

4. District B. Sandwich Islands: General Chief, Capt. G. L. Tupman, R.M.A. : Deputy, if necessary, Prof. G. Forbes.

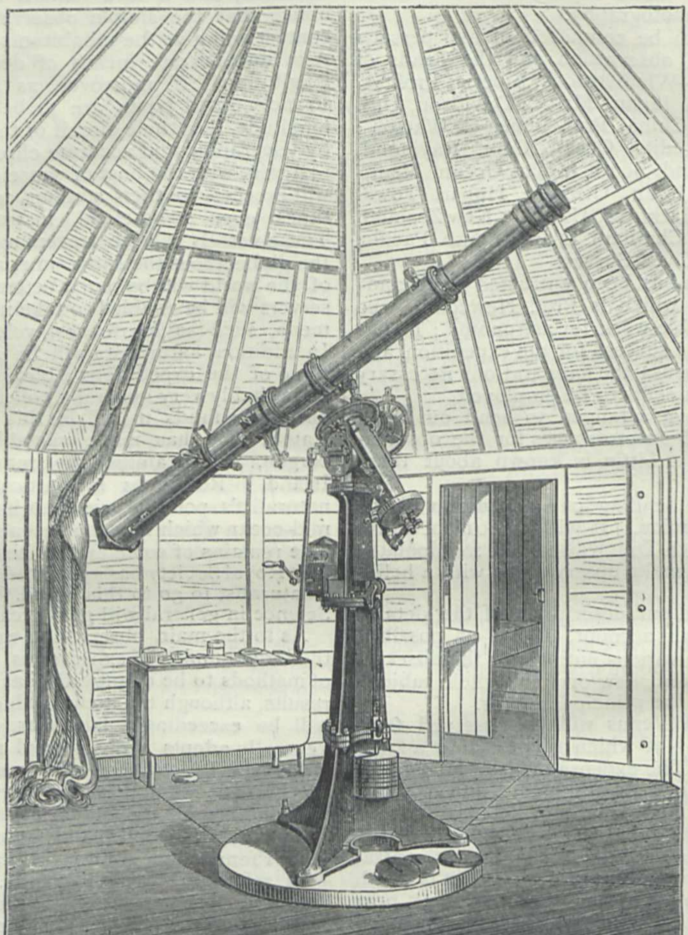

FIG. 19.—Photo-heliograph of the British Expeditions.

Sub-divisions of the Sandwich Islands :—Honolulu : Chief, Capt. G. L. Tupman, astronomer ; Observers, J. W. Nichol, astronomer and photographer ; Lieut. F. E. Ramsden, R.N., astronomer and photographer. Hawaii : Chief, Prof. G. Forbes, astronomer ; Observer, H. G. Barnacle, astronomer. Kauai : Chief, R. Johnson, astronomer ; Observer, Lieut. E. J. W. Noble, R.M.A., astronomer.

5. District C. Rodriguez : Chief, Lieut. C. B. Neate, R.N., astronomer ; Observers, C. E. Burton, astronomer and photographer ; Lieut. R. Hoggan, R.N., astronomer and photographer.

6. District D. Christchurch (New Zealand) : Chief, Major H. Palmer, R.E. ; Observers, Lieut. L. Darwin, R.E., astronomer and photographer ; Lieut. H. Crawford, R.N., astronomer.

7. District E. Kerguelen Island : General Chief, Rev. S. J. Perry ; Deputy, if necessary, Lieut. C. Corbet, R.N.

Sub-divisions of the Kerguelen Island :—Christmas Harbour : Chief, Rev. S. J. Perry, astronomer and photographer ; Observers, Revs. W. Sidgreaves, astronomer ; Lieut. S. Goodridge, R.N., astronomer ; J. B. Smith, astronomer and photographer. Port Palliser : Chief, Lieut. C. Corbet, R.N. ; Observer, Lieut. G. E. Coke, R.N.

8. In addition to these gentlemen, three non-commissioned officers or privates of the corps of Royal Engineers will be attached to each of the five districts, and will be under the direction of the chief of each district.

GEORGE FORBES

ATOMS AND MOLECULES SPECTROSCOPICALLY CONSIDERED *

II.

I now pass on to another part of my subject.

7. *When low temperatures are employed it is generally acknowledged that there is an important difference in kind between the spectra of metals and those of metalloids, taken as a whole.*†

Spectroscopically it is more easy to define the difference between these two great classes of metals than the chemists among you would imagine. I will ask you to take the spectrum of the third class of stars as being as good a representation of the spectrum of a metalloid as anything I can place before you. It is rhythmic, the other two are not. It is a "channelled space" spectrum.* That defines a metalloid spectrum; and a similar spectrum in the case of hydrogen is referred by Angström, Stewart, Schuster, and others to an impurity. I have before referred to temperature and told you that the temperature of a Bunsen burner is enough to set an atom of sodium free from its combination with chlorine and make its vapour give us a bright line. I have told you we cannot do this in the case of iron and other substances. We may say then that we have there a first stage of temperature. Many monad metals give us their line spectra at a low degree of heat. Take some dyad metals such as zinc and cadmium; this first stage of temperature will only make them red or white hot, a much higher temperature is required to drive them into vapour. We get the line spectrum from sodium; do

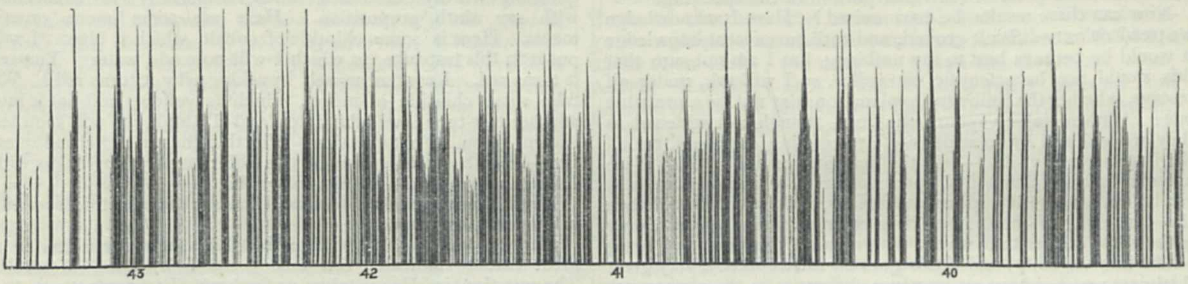

FIG. 3.—Copy of a photograph of the long and short lines of iron between wave-lengths 4,000 and 4,300.

we get that from cadmium when we have melted cadmium? We do not. That is an excessively important point. The first stage of temperature, which gives you a line spectrum in the case of sodium, is powerless to give you such a spectrum in the case of cadmium.

A second stage of heat at least is therefore required to get a line spectrum. If I take sulphur, dealing with it by means of absorption, and heat it, I get a continuous spectrum at the first stage. I increase the heat to the second stage, what do I get then? A line spectrum, as I do in the case of sodium? No! A spectrum like that of the star in the constellation Hercules, not a line spectrum at all. I apply still a higher, a third, stage of temperature and then I get a line spectrum. In the case of the metalloids we have thus three stages of heat with three spectra. If there is such a thing as a particle at all, are we not justified in asking whether there is not some difference between the "particular" arrangements of the metalloids, from those of the metals? and some connection between temperature and the "atomic weights" of the chemist?

Before I go further I will throw these results into a tabular form, which will show you that through these various heat stages in the case of metals like sodium there is a great preponderance of line spectrum, and in the case of metalloids like sulphur there is a great preponderance of channelled space spectrum.‡

	Na.	Cd.	S.
Fifth stage—spark	line spectrum	line	line
Fourth stage—arc	line	line	channelled space
Third stage—white heat	line	(?)	channelled space
Second stage—bright red heat	line	¶continuous absorption in the blue	channelled space
First stage of heat—dull red heat	line	continuous spectrum	continuous absorption in the blue

8. I next state that *A compound particle—that is a particle consisting of two distinct elements—has a vibration which is as peculiar to itself as the vibration of a particle of an element is peculiar to itself.* Thus the salts of strontium have each a distinct spectrum. Take the particle of N_2O_4. The absorption spectrum of this gas you now see on the screen. This particle has a vibration quite of its own. Now it is a gas which it is perfectly easy to dissociate. It is easy to turn it from N_2O_4 to NO_2. We introduce a new spectrum. These facts—and they might easily be multiplied—show then that a compound particle is a perfectly distinct physical thing, with vibrations, rotations, and free paths of its own. There is no apparent connection between the vibrations of a compound particle and those of any of the substances which make up that compound particle.

9. I now come to another important point: *On the whole certain kinds of particles affect certain parts of the spectrum.* Take the bright lines of the metals; if you were to mix together all the known metals in the sun, make a compound which should consist of all of them, put it into the lower pole of an electric lamp and photograph the spectrum, then you would find the majority of the lines would be in the violet end of the spectrum, scarcely any in the red end. That is the reason why the spectrum of the sun, which contains so many of the metals, is so complicated in the violet. If you combine a metal and a metalloid, you will find, in many cases at all events, that the vibrations will lie in the red end of the spectrum; you will also find that there is a connection between the atomic weight of the metalloids and the region of the spectrum in which their lines appear under similar conditions.

You have, in fact, simple particles and short waves, compound particles and long waves. Nor is this all. In many cases we find both ends of the spectrum, and in many cases the more refrangible end only, blocked by continuous absorption. This occurs so often in absorption spectra that one is led to suspect that it is due to some arrangement of particles.

10. Here is another proposition: *In the case of metalloids, and compound gases containing them, the spectrum to a large extent depends upon the thickness of the vapour through which the light passes, and often, if not invariably, the absorption increases towards the red end as the thickness is increased.*

Here is one of the points of the most extreme theoretical importance, and one about which least is known. There is a statement in Prof. Maxwell's book, that if you take a metallic vapour and employ a great thickness of it, you will get from it the same spectrum as you would from a small thickness of great density. This is Prof. Maxwell statement; I venture to think that this is somewhat doubtful, for in questions of thickness the spectroscope can offer the physicist a million of miles or a millimetre to work with, and one would think that such a difference should be enough. If I had a tube with a bore of the size of the lead in this pencil, and had some hydro-

* Continued from p. 7.

† Since this lecture was delivered Prof. Roscoe has established the existence of new spectra of sodium and potassium closely resembling the well-known ones of the metalloids.

‡ Since this lecture was delivered I have carried this branch of the research much further, and it seems one well deserving of the attention of physicists and chemists, as when it comes to be acknowledged that different classes of spectra do truly represent different "particular" aggregations, the contrast between the extremes of metals and metalloids will be beyond question. The result marked thus ¶ I have added from later work.—J. N. L.

* By an oversight last week the illustration here referred to was inserted instead of the present one.—J. N. L.

gen gas rendered incandescent, you would see a line of a certain thickness, with a certain pressure. Looking through the sun's coronal atmosphere in an eclipse, you pierce seven or eight hundred thousand miles of hydrogen gas. The thickness of the lines is the same. Various thicknesses of sodium vapour do not alter the thickness of the lines. But if we pass from metals to the metalloids, then certainly one is prepared to go on with the professor to any extent. I can show you how true his statement is photographically. There is considerable interest attached to the question whether there is or is not any chlorine in the sun's outer atmosphere. I have endeavoured to settle this question, contrasting the absorption chlorine spectrum with the solar spectrum; different thicknesses of chlorine have been employed. It seems that, if we take the metalloids, the absorption of a small thickness often takes place in the violet portion of the spectrum.

Now can these results be harmonised? Here I acknowledge we tread on very difficult ground, and with our present knowledge it would be perhaps best to say nothing; but I am not sure that this would not be scientific cowardice, so I will ask, under all reserve, whether the following explanation may not be a probable one? With metallic vapours the lines, though not widened as they are widened by great density, are certainly darkened, but all the lines are not visible—only the longest, generally. Now if we assume that the channelled space spectrum of the metalloids is really, even where it appears continuous, built up of lines,* then the darkening of these lines by greater thickness will not only make those darker that we see with a small thickness but bring others into visibility; and if this goes on till we have a very great thickness we may have an immense difference in the appearance of the spectrum.

11. *Some of the vibrations are very closely connected with others, as evidenced by repetitions of similar groups of lines in different parts of the spectrum.*

Here we are brought face to face with a revelation of the vibrations of particles, which, if I am not mistaken, will be made much of by the mathematical physicist in the future.

I will content myself by giving two or three striking instances, first noticed by Mascart. You will see that the longest line is at work in all of these.

In sodium we may say that the longest line is double; I refer to D' and D''. All the lines are double.

In magnesium the longest line is a triple combination. This is repeated exactly in the violet.

In manganese we may almost say that the same thing happens, but the phenomenon is much more absolute in the case of those particles such as sodium and magnesium, which, on other grounds, I suspect to be of the simplest structure.

12. *Our knowledge of the vibrations of particles will be incomplete until the vibration is known from the extreme violet (invisible) to the extreme red (invisible).* In the meantime great help may be got from inferences, and, in the case of metalloids at low temperatures, from the position of their continuous absorption; and it is a question whether light may not be thus thrown upon the opacity of some solid substances and the transparency of others.

I think it not too much to say already, that in the case of some gases and vapours which are apparently transparent it is as certain in some cases that their absorption is in the ultra red, as it is certain that in the case of others the absorption is in the ultra violet. And further it is probable that this absorption is of the continuous or channelled space kind—in other words that no gas is "atomic" in the chemist's sense.

13. *From the fact that we have lines in the spectra of compound gases, it would be hazardous to affirm that the aggregate, which, with the highest dissociating power we can employ, gives us line spectra, could not be broken up if a still higher dissociating power could be employed.*

This proposition has a bearing on the celestial rather than on the terrestrial side of the inquiry, and as my time is drawing to a close I will refrain from enlarging upon it.

There is another branch of the research I am anxious to bring to your notice. I can do this better by experiment than by a simple statement.

The substance which you see here is a piece of gold leaf; it is yellow, as you know, but gold is sometimes blue and sometimes red. It must be perfectly clear to you, that if particles vibrate the colours of substances must have something to do with the vibrations. If the colours have anything to do with the particles it must be with their vibrations. Now as the spectrum in the main consists of red, yellow, and blue, the red and the blue rays are doing something in a substance which only transmits or reflects the yellow light; put the gold leaf in front of the lime light, you will see whether the yellow light does or does not suffer any change. The yellow has disappeared; you have a green colour; the red and blue are absent. The gold leaf is of excessive thickness. What would happen could I make it thinner? Its colour would become more violet. This I have proved by using aqua regia. But here is a solution of fine gold, which lets the red light through. Its particles are doing something with the blue vibrations, or *vice versâ*. Now what is the difference—the "particular" difference between the gold in this solution which is red, and that which is yellow by reflected, and green or violet by transmitted light? It is a question worthy of much study, especially in connection with my ninth proposition. Here are some more experiments. Here is some chloride of cobalt, which is blue. I will put it in this test-tube, to which I will now add water. You see it turns red. I content myself by asking why it turns red? We take some chloride of nickel, which is yellow, and put it into another test-tube: we add water, and I think you will soon see it turns green. First question—Why this change? Second question—Has the green colour of this solution anything to do with the red colour of the solution of gold?

I ask these questions because I believe the spectroscope will in time answer them.

I hope you will acknowledge that the spectroscope has to a great extent vindicated the theory stated by Prof. Maxwell. The question is, Has it taken us further? Perhaps not yet, but I think it will be found that what chemists picture to themselves as the atom, as contradistinguished from what they weigh, and physicists the molecule, is that particular atom, molecule, particle, or whatever name you may choose to call it, which with high-tension electricity gives us a spectrum of lines. You recollect that I said that in many of the monad metals it was obtained in the first stage of temperature; in the case of the dyads and metalloids with higher stages. If the true atom be that which gives a line spectrum, many anomalies will fall to the ground. These are questions the spectroscope raises. If you allow that in the line spectrum an atom is at work, in channelled spectra and continuous spectra molecular aggregations, you will see at once that Prof. Maxwell and others will be able to get a much sharper definition of atom and molecule than they have now; and though atoms are little things, you know they lie at the root of everything, and time spent in investigating them will not be lost.

J. NORMAN LOCKYER

* Thalen's beautiful researches on the spectrum of iodine quite bear out this view.

A BOTANICO-GEOLOGICAL EXCURSION INTO THE GRAMPIANS

THE Scottish Alpine Botanical Club is wont to hold a spring meeting for mingled plant-hunting and conviviality in some Highland district where the Alpine flora can be reached at not too great a distance from oatcakes and whiskey. The Geological class in the University of Edinburgh is in the practice of terminating its labours for the winter by taking an excursion of a week's duration to some part of the country where professor and students can find interesting rocks, with enough of food (such as it may be) to eat, and of beds, or shake-downs, to sleep on. This year the two bodies, drawn together perhaps as much by animal spirits as by scientific enthusiasm, coalesced and held a conjoint gathering at Clova—a lonely hamlet on the Forfarshire Grampians, well known to botanists for the richness of its Alpine flora, and to geologists for its glacier relics and its ancient metamorphic rocks. The following notes by the respective leaders of the plant-seekers and the rock-hunters were communicated to the Edinburgh Botanical Society on the 14th ult.:—

1. *Botanical Notes by Prof. Balfour.*—On Friday, April 24, the botanists visited the lower part of Glen Fee and the western side of Glen Dole. They specially examined the rocks in Glen Fee, where *Oxytropis campestris* grows and along with the plant took specimens of the rock for the determination of the geologists. They also visited

the rocks at the upper part of Glen Dole, where *Astragalus alpinus* grows. These rocks are very rich in plants; they consist of remarkably twisted and contorted gneiss, specimens of which were collected. The vegetation of the glen was in an advanced state, and some plants were gathered in flower which rarely blossom so early. Among them may be mentioned—*Arctostaphylos uva-ursi*, *Vaccinium vitis-idæa*, *Anemone nemorosa*, *Saxifraga oppositifolia*, forming large pink patches on the rocks; *Luzula campestris*, *Empetrum nigrum*, *Eriophorum vaginatum*, and *Cardamine hirsuta*. Among the other plants noticed in flower in the glen at Clova were: *Ulex europæa*, *Sarothamnus scoparius*, *Genista anglica*, *Prunus avium*, and *Ranunculus ficaria*. Among the plants not in flower which attracted notice were: *Silene acaulis*, *Saxifraga hypnoides* and *aizoides*, *Draba incana*, *Pyrola media*, *rotundifolia*, *secunda*, *Oxyria reniformis*, *Gnaphalium supinum*, *Dryas octopetala*. The following ferns were also gathered: *Lastræa oreopteris*, *Athyrium filix-fœmina*, *Polystichum lonchitis*, *P. aculeatum*, *Polypodium alpestre*, *P. vulgare*, *Asplenium viride*, *A. trichomanes*, *Botrychium lunaria*, and *Allosorus crispus*. All the species of British Lycopods except *inundatum* were gathered. Mr. W. B. Boyd collected some good mosses, including *Trichostomum glaucescens*, confined to the rock in Glen Fee on which *Oxytropis campestris* grows. It occurred in considerable abundance and in fruit.

On Saturday 25 the party again went to Acharne, and thence up Glen Esk to Bachnagairn, and by Loch Esk to the White Water and Little Gilrannoch. Again the day was all that could be desired. The snow near the summits of the hills was very refreshing, and on one we had a sufficient extent of snow to give us the benefit of a glissade with our poles. This day the botanists and geologists kept together. We specially examined Little Gilrannoch, one of the rocky summits which is interesting as yielding the *Lychnis alpina*, one of our rarest Alpine plants, and associated with it dwarf specimens of *Armeria maritima* and *Cochlearia officinalis*, the Alpine variety, and *Luzula spicata*. The rocks were specially examined by the geologists.

On Monday 27 the botanists examined Loch Brandy and Loch Wharral, and the rocks around them. We noticed particularly the vast crevasse formed at the top of the Snubb by the separation of a great mass of rock, which is gradually giving way, and will ultimately be precipitated into Loch Brandy. *Saxifraga oppositifolia* was seen as formerly in fine flower. *Azalea procumbens* was also gathered. In Loch Brandy *Isoetes lacustris* and *Lobelia dortmanna* were met with. In ascending the mountains this day we saw a fine effect produced by the thick white mist resting in the valley, while we were on the mountain above it enjoying clear sunshine. Among the mosses collected by Mr. Boyd during the trip may be noticed—*Grimmia unicolor*, *G. donniana*, *Leucodon montensis*, *Andræa campestris*, and *Hypnum catenulatum*.

2. *Geological Notes by Prof. Geikie*.—The main object of the geologists of the excursion was to observe some of the phenomena of the metamorphism of the district, to note the more prominent minerals, to trace the remains of old glaciers, and to connect the general structure of the rocks with the forms of hill, valley, crag, and ravine into which they have been carved. Incidentally, however, they took part in some of the botanical work, their attention being specially directed to the Alpine flora and to the circumstances under which some of the rarer Alpine plants occur. There can be no doubt that, as pointed out by Edward Forbes, our Alpine flora is descended from that which was general over these islands during what is known as the last Ice age. It has been supplanted in the lower districts by the vegetation which has come in with a milder climate; and it survives on the bleak and cold mountain ridges only so long as it can find its congenial temperature there, or so long as the chills and mists of these high regions forbid the further ascent of the plants which, swarming over the country, have driven these northern forms step by step up into these high grounds. It is well-known that the Alpine flora is richer in individuals and in species in the eastern Grampians than anywhere else in Britain. A number of plants are found in no other part of the country, and even in that district several are restricted to mere isolated rocks in some glen or some bare mountain brow. The question proposed to the consideration of the geologists was whether any geological reason could be given for this remarkable distribution, and particularly whether or not the nature of the rocks had had anything to do with it.

Some attention was accordingly paid to the habitat of three of the rarer and more local species. The *Astragalus alpinus* was observed on hard quartzose schist, high up in Glen Dole; the crag on which the *Oxytropis campestris* flourishes is a mass of singularly twisted and gnarled quartzose gneiss, with hard siliceous ribs projecting from its surface and showing the crumpled nature of the rock. But in neither of these cases does the rock apparently differ from many other crags in the neighbourhood, where the peculiar plants nevertheless are not found. In the case of the *Lychnis alpina* a special case seemed at first to be made out in favour of a relation, or at least a coincidence of a local plant with a local rock; for the locality noted as the habitat of this rare plant was found to present shattered knobs of serpentine projecting through the turf, and on these knobs the *Lychnis* grows, together with the *Cochlearia officinalis* and the *Armeria maritima*. This rock was not observed by the party *in situ* in any other part of the district examined. Before it was quitted, however, one of the botanists, who strayed farther over the mountain, returned with a piece of mica-schist, as the rock on which the same plants were found growing only a short distance away. It appeared, therefore, probable that, at the most, difference of rock can have had but a very slight influence in the survival and present distribution of the Alpine flora.

A much more effective influence may be traced to the general physical geography of the country, and especially of the eastern as contrasted with the western districts. The richness of the Aberdeenshire and Forfarshire mountains in Alpine plants, as contrasted with those of equal elevation in Invernesshire and Argyllshire, has long been a familiar fact to botanists. The cause of this contrast seems referable not to any difference in rock and soil, nor to mere differences in height; it appears to be explicable by the much greater breadth of high ground in the east than in the west. Everyone who has ascended some of the Grampian ridges remembers the wide undulating moors which spread out before him at heights of 2,000 or 3,000 ft. The summits are not peaks, so much as huge swells or mounds rising higher than the rest of the vast tableland. In the western counties, however, the craggy mountains tower often into sharp ridges. They are deeply trenched by glens and arms of the sea, so that relatively a smaller area of land rises out of the ordinary lowland vegetation into the chiller regions above. Add to this that the Invernesshire and Argyllshire hills lie nearer to the warm winds and currents of the Atlantic, and that the Grampian uplands receive the prevalent south-westerly winds after they have been chilled by passing over many leagues of high cold mountain ground. It is in these eastern parts of the Highlands that snow lingers longest, widest, and deepest—a good index, indeed, of the greater severity of the climate. These facts are suggested as affording some explanation of the comparative abundance of the Alpine flora in that part of Scotland.

Why in that limited district certain plants should be restricted to mere isolated rocks is a question to which no intelligible and satisfactory answer can at present be given. But even more perplexing is the problem presented by the survival of maritime plants upon some of the highest and bleakest mountain-tops. In such portions the *Cochlearia* or scurvy grass, the *Armeria* or sea pink, with *Silene maritima* and *Plantago maritima*, are found abundantly. They are poor dwarfed forms, it is true, when compared with their contemporaries on the coast, so that the latter habitat is evidently more congenial to them than the bleak uplands. Descendants of the old arctic flora once indigenous in this country down to the sea-level, as it is in northern Scandinavia at the present day, how have they come to be left on our mountain tops? Were they maritime plants originally, and have they been carried up by the gradual elevation of the land? This would involve a former submergence of the country to a depth of at least 4,000 ft.—a limit much beyond that suggested by other geological evidence. Or did they form part of the generally distributed flora whereof some species keeping to the shores have been able amid bare rocks and salt spray to maintain themselves there ever since, while farther inland they have succumbed to the march of the invading Germanic flora, and have been allowed to struggle on in dwarfed and stunted forms only on the bare chill mountain tops, whither the invaders did not care to pursue them?

Some light might possibly be cast on these questions by an examination of the contents of our older peat-mosses. There is reason to suppose that some of these mosses may date back into Glacial times. It would be interesting to discover whether among the plants whose remains went to form the peat any northern species could be detected no longer living in this country, even in our Alpine zone. This line of inquiry is now being prosecuted in Scandinavia, and it is suggested to the botanists of Scotland as a fit subject for their attention.

The more purely geological work by the brethren of the hammer during this excursion, whether when with the botanists among the Grampians or afterwards by themselves along the shore between Dunnottar and Aberdeen, is hardly appropriate in a communication to the Botanical Society.

ON THE FERTILISATION OF CERTAIN LABIATÆ

IN the early part of April of the present year I had an opportunity of watching somewhat closely the mode of fertilisation of some species of Labiatæ, on which some notes may be interesting. The species observed were the three most abundant of the early flowering representatives of the order, *Lamium album*, *L. purpureum*, and *Nepeta glechoma*; the post of observation a bank covered by the three species growing completely intermixed, just outside a cottage-garden where were several hives of bees; the time occupied, several hours on three sunny mornings. The point which interested me most was the constancy with which the same species of insect confined its visits to the same species of flower, notwithstanding the close proximity in which the three were growing, this being perfectly in harmony with Mr. Traherne Moggridge's observations of a similar character respecting the visits of insects to fumitories and other flowers.

My conclusion is not based merely on actually noticing the visits of insects, but on the microscopic examination of the pollen collected on the captured insects. For this purpose the pollen-grains of the three species named offer unusual facilities, those of *Lamium album* being yellow, of *L. purpureum* red, and of *Nepeta glechoma* white.

In *Lamium album* the length of the style is such as to bring the stigmatic surface exactly on a level with the anthers of the shorter pair of stamens, as represented in Fig. 1; one branch of the style is nearly straight and is hidden among the anthers, the other projects at right angles into the opening to the tube of the corolla, so that it must necessarily be struck by any insect entering the flower. The only visitors to the flower seen were two species of humble-bee, *Bombus pratorum** (female) and *Anthophora retusa* (female), the former in large numbers, the latter much more rarely. From the position of the stigmatic surface, both it and the stamens must be struck by about the centre of the head of the bee; and it was on this part that the greater number of pollen-grains were found, and proved to belong exclusively to this species. In no single instance was a hive-bee seen to visit the flowers; Müller states that they obtain the honey from this species entirely by sucking it through holes bitten in the corolla by *Bombus terrestris*.

In *Lamium purpureum* the difference in length between the two pairs of stamens is less considerable and the anthers are consequently closer together, both branches of the style being bent forwards into the mouth of the corolla, as shown in Fig. 2. Although hive-bees were constantly hovering over the flowers, in no single instance did I see either them or the humble-bees visit this species; the only insect observed to settle on it being a butterfly (*Vanessa urticæ*) twice.

The position of the parts in *Nepeta glechoma* is very different. The two pairs of anthers are at a considerable distance from one another (Fig. 3), and the length of

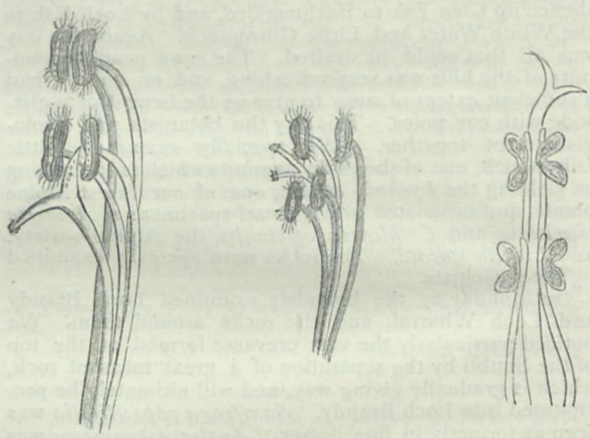

FIG. 1. FIG. 2. FIG. 3.

FIG. 1.—*Lamium album*; stamens, style, and stigma. FIG. 2.—*Lamium purpureum*; stamens, style, and stigma. FIG. 3.—*Nepeta glechoma*; stamens, style, and stigma.

the style is such as to bring the stigmatic surface considerably beyond the longest pair, and projecting beyond the mouth of the much smaller corolla. The flowers were profusely visited by the hive-bees from the other side of the hedge. On no single occasion did I see the *Bombus pratorum*, of which such numbers were flying about, even attempt to enter the flower, and the smaller species, *Anthophora retusa*, only twice; and on each of these occasions she immediately came out again and began industriously to wipe the pollen off her head with her fore-legs, as if she disliked it. Owing to the much smaller size of the flower, and the greater length of the style, the part of the body of the bee touched by the stigma is very different to that in the *Lamium album*, namely, the back of the neck or even of the thorax. Hence even if the insect should visit the two species on the same journey—which, I should infer, is not usual—the pollen of one species would not easily be wiped off on to the stigma of the other. I did not observe any plants of the ground-ivy with the "female" flowers described by

* In this and all other instances I am indebted for the determination of the insects to the kindness of my friend Mr. Edward Newman.

Müller, with which one is familiar in the case of the wild thyme and other Labiatæ; but a large number of flowers in this particular locality had all the anthers bitten off, a depredation which I attributed to the hive-bees, inasmuch as the same was the case in other habitats near hives, but not in those at a greater distance from cottage-gardens. The only other flowers growing on or near the same bank which I observed the bees to visit were the dandelion several times, and *Veronica buxbaumii* once.

At this early period of the year the following species of insects were captured on the dandelion; those marked with an * are not in Müller's list of ninety-three kinds which visit this plant, unless under synonyms which I fail to recognise.—Hymenoptera, *Apis mellifica*, **Halictus lugubris*, **Andrena nana*, *A. varians*, and *A. nitida;* Diptera, **Syrphus clypeata* and *Eristalis arbustorum;* Coleoptera, **Apion apricans*.

The sloe was abundantly visited by **Andrena fulvicrus* (Hymenoptera), both male and female, and by *Eristalis tenax* (Diptera). On opening the abdomen of the latter, it was found to contain abundance of pollen-grains, belonging to the species on which it was then feeding, and to the dandelion, mixed with a few larger triangular pollen-grains, belonging apparently to a Fuchsia; thus confirming the opinion at which I had previously arrived, that the Syrphidæ are large consumers of pollen. The abdomen of the Hymenoptera, on the other hand, contained but a very few pollen-grains, which might easily have been sucked up accidentally along with the nectar; and this was also the case with the hive-bee, the grains in this latter case belonging to the dandelion.

ALFRED W. BENNETT

NOTES

THE Cambridge Museums and Lecture Rooms Syndicate, in their eighth annual report state that the Regius Professor of Physic has again called their attention to the urgent need of better accommodation for the medical examinations. Among the additions which have been made to the collections in the several museums, the bequest of the late Mr. M'Andrew, F.R.S., of the whole of his collection of shells and other specimens, deserves the first mention. It is of the highest scientific value. A most interesting collection of human crania, made by the late Dr. Thurnam, of Devizes, has been presented to the Museum of Human Anatomy, through the liberality of Prof. Humphry. A series of Devonian fossils, of great beauty, presented by Lady Burdett Coutts, deserves special mention, as also does the contribution of several hundred specimens of Palæozoic and other fossils by Prof. Hughes, and the gift of 500 sterna of birds by Prof. Newton and Mr. E. Newton, and of a skeleton of the extinct bird "the Great Auk" by Prof. Newton. The building of the Cavendish Laboratory is now finished, and the Laboratory is open for practical instruction in physics. As the several collections and the number of students in the several departments increase, the current expenditure necessarily increases. The Syndicate are therefore of opinion that the time has arrived when they are obliged to call the attention of the Senate to the necessity of increasing the amount of the annual grant to the museums and lecture-rooms maintenance fund. They suggest, however, that for the current year a special grant of 300*l.* be made to the fund. Appended are the reports of Professors Humphry and Newton, and of the Superintendent, Mr. J. W. Clark, which give details of the past year's work and the additions made to the various collections.

IN a Convocation at Oxford, on May 28, the name of H. S. Smith, F.R.S., Savilian Professor of Geometry, Fellow of Corpus, who had been nominated to the office of Keeper of the Museum, by the delegates, in succession to the late Prof. Phillips, was approved.

THE list of those on whom the honorary degree of LL.D. is to be conferred at the approaching Cambridge commencement is very numerous. We have already mentioned some names; the following is a list of the men connected with Science who are to receive the honour:—Sir Charles Lyell, F.R.S.; Sir James Paget, F.R.S.; M. Leverrier, of the Paris Observatory; Joachim Barrande, of the Royal Society of Sciences of Prague; George Bentham, F.R.S.; and William Lassell, F.R.S.

WE have received the prospectus of a new "College of Science and Literature," which it is proposed to establish at Bristol for the South and West of England and South Wales. Such an institution, if properly organised, would no doubt be of great service, as these extensive and important districts are far distant from any college in which the sciences applied to their various industries can be studied. Judging from the prospectus, the organisers of the scheme have sound notions of what such an institution ought to be, keeping in view as models Owens College and the Newcastle College of Science. Balliol College and New College, Oxford, have come very liberally forward in aid of the scheme, having offered to contribute towards it 300*l.* a year for five years. It is estimated that a capital sum of 25,000*l.* will be required, and an annual subscription of 3,000*l.* for the first five years secured. It is, however, proposed to commence operations when such proportion of these amounts has been guaranteed as may justify the expectation of success. A public meeting is to be held at Bristol on the 11th inst. to inaugurate the undertaking, which we sincerely hope will be taken up heartily by those interested in it.

MR. W. SAVILLE KENT, F.L.S., the late Superintending Naturalist of the Brighton Aquarium, and formerly Assistant in the British Museum, has been appointed to the control of the Manchester Aquarium. This aquarium being constructed on the "circulating" principle, advocated by Mr. Kent, and it being, moreover, intended to make the building subservient more to the instruction and education of the masses rather than for the realisation of extraordinary dividends, we may anticipate from it scientific results of the most gratifying sort. The tank frontage of the Manchester Aquarium presents a length of no less than 750 ft., an amount exceeding that of any aquarium yet constructed. An ample guarantee of the encouraging support this undertaking is likely to receive at the hands of the public is shown by the returns for the first week of its opening, the visitors who passed through the gates during that period numbering over 19,000.

THE Birmingham Natural History and Microscopical Society, whose enterprise we have had frequent occasion to speak of, is contemplating the foundation of an aquarium in Birmingham, and has been seeking information from the managers of various aquaria at home and abroad. The result is not altogether encouraging to those who desire to see an aquarium standing on its own legs as a scientific institution, apart from adventitious attractions. It seems that scarcely any existing aquarium pays that is not attached to or does not form part of some place of amusement; and Mr. Lloyd of the Crystal Palace Aquarium gives it as the result of his large experience that no aquarium can be made to pay its way, unassisted by other attractions, even in the largest centre of population, unless its cost be limited to 3,000*l.* and its annual expenses to 500*l.* Still we hope that, whether as an independent or as a parasitical institution, the Birmingham Society will be brave enough to take steps to establish an aquarium in that busy centre.

FROM the Twelfth Annual Report of the Birmingham Free Libraries Committee, we are glad to see that this system of libraries continues to enjoy increasing prosperity. These annual reports furnish a number of very interesting statistics as to the number and class of books in the libraries, number and occupation of readers, books most in demand, &c. The total number of books in the various libraries amounted at the end of last year to 69,279, a very large proportion of which are of a scientific character. From the statistics as to books most sought after, and the number of readers in the various subjects, we are glad to see that works of Science enjoy a large amount of patronage. The aggregate issue of works in the reference and lending libraries was 525,610.

WE have received several American papers containing descriptions of a marine aquarium in San Francisco, California. It forms part of the many attractions of "Woodward's Gardens," an extensive piece of ground which has been inclosed and laid out by a private gentleman, Mr. Woodward, for the amusement and instruction of the people.

IT is gratifying to learn that the lamented death of Prof. Agassiz will not prevent the continuation of the school of natural history at Penikese Island, the results of which during the season of 1873 proved to be of so much educational importance. A circular from Mr. Alexander Agassiz in regard to this states that two or three times as many persons as can be accommodated have already applied to be received during the coming summer, and that great interest is manifested to prosecute the study of nature under the eminent specialists who have been called to assist in the enterprise. The necessity of a permanent endowment is very justly set forth by Mr. Agassiz, and especially the importance of means for paying for the services of the men of science invited to officiate as instructors. He suggests that provision be made by the Legislatures of the several States for the endowment of scholarships, either by the actual payment of the sum of 5,000 dols., or an annual grant of 350 dols. The payment of this sum on the part of any State would entitle it to nominate two teachers for admission during the summer to the Penikese school, the selection to be made from among those most apt in natural history. No charge is made to the students of this school for tuition. It is announced that this school will open on July 7, and close on Aug. 29. Among the gentlemen mentioned as likely to take part in the instruction are Dr. Packard, Professors Wilder, Morse, Mayer, and Jordan, and Messrs. Putnam, Bickmore, Lyman, and others.

THE Annual Report of the Trustees of the Museum of Comparative Zoology at Harvard College, Cambridge, U.S., shows that that institution is rapidly becoming one of the first of its kind anywhere. Its already large and valuable collections are constantly being added to, and rapid progress is being made in their systematic arrangement. The museum is open not only to regular students of natural history, but to all scientific men who care to make use of it in aid of their researches. It is in connection with the Harvard Museum that the Penikese School of Natural History was instituted; and, between the two, American students have rare advantages for the study at least of Icthyology.

THE German Society for Polar Exploration has, it is said, purchased the harbour of Kristvig, on the Island of Averio, on the west coast of Norway, with the intention of making this in future the starting-point of German explorations of the Arctic regions.

DR. GROSS, the author of "Les habitations Lacustres du lac de Bienne," in which all the stations in that lake of the Stone and Bronze ages are described in detail, has just, says the *Continental Herald*, presented a gem of its kind to the Archæological Museum of the Berne City Library. This is a hatchet of nephrite, 7 in. long, a very scarce kind of stone, and only found in eastern Asia, the occurrence of which in the lake dwellings of Switzerland forms an unsolved puzzle.

THE first ascent of a balloon over the Black Sea was made on April 19 from Odessa in the "Jules Favre," measuring 70,000 cubic feet. The ascent took place at 3.10 A.M. in a north-east direction; but as it mounted higher the wind veered and the balloon went out to sea in a south-east direction. It rose to a height of 7,000 Russian feet at a distance of about 16 miles from land. The balloon came to ground at Peresadovka, about 20 miles north from Nikolaeff, at 6h. 39m. A.M.

AS the series of annual international exhibitions at South Kensington is to be discontinued after the present year, the Society of Arts have in consideration the organisation of a series of provincial exhibitions of an industrial character, to be held in the centres of the manufacturing districts. The plan is as yet by no means complete, but a principal part of it would be that the special industries of each locality should be, as far as possible, illustrated in its exhibition.

A PUBLIC meeting was held in the Mechanics' Institute, Nottingham, on Tuesday night, to consider the further development of the movement instituted by the Cambridge University for extending its teaching to the masses of the people. The report of the committee for the past session stated that nearly 2,000 tickets for the lectures and classes were applied for in the town; 1,241 persons attended the lectures, and 615 the classes. There were 143 candidates at the examination, of whom 126 obtained certificates of merit. The financial statement was satisfactory, and the report expressed a belief that the movement would shortly be self-supporting.

AT a recent meeting of the Royal Geographical Society of Ireland Mr. W. Harte, County Surveyor, co. Donegal, gave a description of "Supposed evidence of a recent change of level in the surface of co. Donegal." He adduced a number of proofs that there was a general and rapid depression in the surface of the county. Inhabitants had informed him that portions of the coast now covered with 20 ft. of water had been passed over dry-shod by their grandfathers. Many bogs also, of which the trees were still erect and *in situ*, had been recently inundated. That the water had never previously reached a higher level he proved from the fact that none of the bogs now under water had ever been previously inundated, for they were not permeated, as bogs which had been covered by water invariably were, with a fine microscopic sand. The submergence of Donegal was taking place at a rate that was much more rapid than had been suspected; old passes which were used to islands along the coast now no longer existed. The most interesting fact, however, was one brought to light by Mr. Fitzgerald, who found numerous cases of furnaces used by the ancient Irish to smelt the bog iron ore, but which were now under high-water mark.

THE ravages caused by the *Phylloxera vastatrix* among the vineyards of France are becoming very serious. More than 150 various remedies have been tried but without success, and the only hope of many scientific men is in the introduction of varieties of vine which are known to be to a certain extent proof against the attacks of this insect. Many American kinds of vine are said to possess the property of resisting the disease for a much longer time than the French vines, and steps are being taken to introduce roots of these varieties into France. In the Department of Hérault alone the produce of wine has fallen from fourteen millions of hectolitres to eleven millions: not only is the fruit destroyed by the effects of the parasite, but the vine itself is destroyed in a year or two; and one female Phylloxera is said to produce two or three millions of young in a year.

AT a recent meeting of the Boston Society of Natural History Prof. Morse read a paper on Natural Selection among the Molluscs, instancing the usually small size of certain species in the

Bay of Fundy, near Eastport. Here the tide rushes along with great power, and the molluscs are obliged to cling to the bottom with great tenacity to prevent being swept away. Only the smaller individuals can withstand this by getting into the crevices of the rocks. The species is thus perpetuated by the smaller members, and rarely attains any considerable size.

THE Inspectors of Salmon Fisheries of England and Wales have just issued their annual report. Examples are given of the serious injuries inflicted on salmon rivers—and not only on salmon rivers but on the health of the public—by the pollutions poured into rivers, and it is to be hoped that powers will be given to enforce the removal of such matters from our streams. Altogether the prospects of our salmon rivers appear very favourable, and much good is to be expected from the working of the new Act.

A SEAM of coal has been discovered at Sandwell Park, near Birmingham, 418 yards below the surface.

SOME good popular scientific lectures are at present being given by Prof. Gardner at the Polytechnic.

THE May number of *Annals and Magazine of Natural History* contains, among other articles, a list of butterflies taken by Lieut. Bell on the march to Coomasie, with a description of six new species. Dr. Nicholson describes a new genus of Palæozoic corals from the Niagara group of Indiania, which he names Duncanella, in honour of Mr. P. M. Duncan. Dr. Young gives a description of a new genus of carboniferous Polyzoa, and suggests the name Rhabdomeson. A plate is given illustrating *Rhabdomeson gracile*. There is also a brief note of an apparently new species of humming-bird, of the genus Eriocnemis, by Mr. Elliot. The discussion about Eozoon is continued.

AN excellent device has been forwarded to us for use in field-club excursions. It is designed to promote an interest in common flowers, and can of course be varied and worked without a prize. It consists of a large envelope, with a description, but not the name, of a plant, and directions as to what ought to be done with the plant when found. The particular envelope, forwarded to us by Mr. Higgins of the Liverpool Naturalist's Club, contains the following on its back :—

EXTRA PRIZE.

DESCRIPTION OF PLANT.

Leaves opposite, Sessile, Lanceolate, Acuminate.
Sepals 5, half as long as the 5 deeply-cleft Petals.
Stamens 10, Styles 3, height about 12 in.

Members finding a plant answering to this description should take it to the President or Botanical Referee, with their name signed at the foot of this slip. When correct the slips will be initialed and handed to the Secretary. The finder should be prepared to answer questions on the description; but the name of the plant will not be officially announced till after tea.

A Prize or Prizes will be awarded at the end of the Season to those most successful.

Signed, ____________________

THE additions to the Zoological Society's Gardens during the past week include a Beisa Antelope (*Oryx beisa*) new to the collection, from Central Africa, presented by Admiral Arthur Cumming; an Indian Gazelle (*Gazella bennetti*), presented by Mr. J. H. Bainbridge; an Indian Ratel (*Mellivora indica*), presented by Mr. L. Macneill; a Maugé's Dasyure (*Dasyurus maugæi*) from Australia, presented by Mr. F. Kirby; two Little Whimbrels (*Numenius minutus*) from the Navigator Islands, presented by Rev. S. J. Whitmee; a Guilding's Amazon (*Chrysotis guildingi*) from St. Vincent, purchased; a Bennett's Cassowary (*Casuarius bennetti*) from New Britain, deposited.

SCIENTIFIC SERIALS

THE *American Journal of Science*, May 1874.—The May number contains the following papers :—On the polarisation of light, by Prof. A. W. Wright. Prof. Wright instituted a series of observations with different instruments, which he describes, obtaining, however, only faint and uncertain results. At last he has been enabled to make observations he considers reliable. He obtained a quartz plate, cut perpendicularly to the axis, and exhibiting by polarised light an unusual intensity of colour. Examined with one Nicol and unpolarised light the plate is perfectly colourless, and shows no trace of its heterogeneous structure. Placed between two Nicols, it showed bands of colour, the plate being a macle, the body consisting of left-handed quartz, crossed by a band of right-handed quartz, bounded by strips of different structure. The plate was used in a tube 11 in. long, and formed an instrument especially adapted to the detection of small degrees of polarisation. The observations were made facing the south-west in a dimmed room, so that the eye should be sensitive. The results of the numerous observations on different evenings were entirely concordant, and are thus summed up by Prof. Wright :—(1) The zodiacal light is polarised in a plane passing through the sun. (2) The amount of polarisation is, with a high degree of probability, as much as 15 per cent. but can hardly be as much as 20 per cent. (3) The spectrum of the light is not perceptibly different from that of sunlight, except in intensity. (4) The light is derived from the sun, and is reflected from solid matter. (5) This solid matter consists of small bodies (meteoroids) revolving about the sun in orbits crowded together towards the ecliptic.—The second article is the first instalment of a communication by Mr. W. M. Fontaine, On the "great conglomerate" of New River, West Virginia.—The third article is by Mr. S. W. Johnson, On the use of potassium dichromate in ultimate organic analysis. Potassium dichromate, the author thinks, possesses all the properties needful for an oxidant in organic analysis, and ordinary kuolin is the best material for diluting it. He gives the details of some of his experiments.—Then follows an article by Mr. C. H. Hitchcock, On the Helderberg Rocks of New Hampshire, which is illustrated by a map, and is to be continued.—The Rev. H. C. Hovey contributes an interesting article on *Rabies mephitica*. The bite of the common skunk (*Mephitis mephitica* Shaw) is often dangerous, and leads to symptoms somewhat analogous to those which follow the bite of a mad dog. Mr. Hovey has obtained particulars of forty-one cases of *Rabies mephitica*, and of these forty were fatal.—Mr. Carey Lea of Philadelphia finds that when silver bromide is treated with pyrogallic acid, after exposure to light, the black substance which remains contains bromine and is resolved by nitric acid into normal silver bromide (left behind as a pale yellow film) and silver which passes into solution. It is, therefore, either a sub-bromide or an oxy-bromide; not an oxide. The existence of these compounds is evidently an argument for doubling the atomic weight of silver, as has recently been proposed on other grounds.—Mr. Meek continues his notes on the fossils figured in the recently-issued fifth volume of the Illinois state geological report.—The brief contributions from the physical laboratory of the Harvard College are also continued. They include No. v., On a method of freezing a magnetic bar from the influence of the earth's magnetism, by John Trowbridge. No. vi. Note on Melde's experiment, by W. Lowery. No. vii. A spark adjuster for the Holtz machine, by James Minot. No. viii. Effect of condensers on the brush discharge from the Holtz machine.—Mr. E. A. Verrill continues contributions to zoology, giving the results of dredging at three stations on the coast of New England, on Cashe's ledge, Jeffrey's ledge, and Stellwagen Bank.—In the "Scientific Intelligence," the section "Chemistry and Physics" consists of notices of papers published in Europe. In section "Geology and Natural History" there is a notice of a communication in the *Overland Monthly* On mountain sculpture in the Sierra Nevada, and on the method of glacial erosion, by E. S. Carr. He holds that glaciers do not so much mould and shape rocks as that they "disinter forms already conceived and ripe." The grain of a rock determines its surface-forms.—There is also an extract from a letter to Dr. Dana, referring to volcanic action in Hawaii, where Mauna Loa has been in full activity since April 1873.—An abstract is given of Prof. W. S. Clarke's experiments on the amount of pressure in the sap of plants. The mercurial gauge has been used on the sugar maple, and observations were made day and night from April 1 to July 20.

The maximum pressure was found to be equal to sustaining a column of water 31·73 ft. high. One of the most interesting portions of the experiments was to determine, if possible, whether any other force than the vital action of the roots is necessary to produce the sap-pressure. A black birch-tree was selected, and a root was severed at 10 ft. from the trunk, and to it was attached a mercurial gauge. This showed a maximum pressure equal to 85·8 ft. of water, and proved that "the absorbing power of living birch rootlets without the aid of any of the numerous helps imposed upon them by ingenious philosophers, such as exhalation, capillarity, oscillation, &c., was quite sufficient to account for the most essential of the curious phenomena connected with the circulation of sap."

Journal of the Franklin Institute, April. —The following are some of the important papers in the number:—Report of the Committee of the Institute on the Westinghouse car-brake. This brake in its simplest form consists of a small steam-engine placed in the locomotive, which, taking steam from the boiler, works an air-pump, which compresses air into a main reservoir, secured beneath the car. By an ingenious arrangement of pipes and automatically acting valves, the air is admitted into a series of brake cylinders, one under each car, the pistons of which are connected with and act upon the ordinary brake-levers, and thus apply the brakes to the wheels. The inventor has made important improvements on this, by means of which the compressed air may be admitted almost instantaneously into the brake-cylinders, and the train brought to a standstill in an incredibly short space of time; *e.g.* a train, going at the speed of thirty miles an hour up a gradient of 29·6 ft. per mile, was brought to a stop in 16 seconds. Scott's legacy, premium, and medal, were awarded to the inventor by the Institute.—The principles of shop-manipulation for apprentices is continued.—On the mechanical calculation of earthwork (or the results of physical measurements in general) according to the prismoidal or other formulæ, by C. Herschell, C.E. This paper relates mainly to the important uses to which the polar planimeter can be put.—Prof. R. H. Thurston contributes two papers which have been published separately: On the thermal and mechanical properties of air and other gas, subjected to compression or expansion; and On the strength, elasticity, and resilience of materials of machine construction; both papers are illustrated with diagrams.

THE *Journal of Mental Science* for April, opens with the third number of the Morrisonian Lectures on Insanity for 1873, in which Dr. Skae and Dr. Clouston still further exemplify the classification of the various kinds of insanity according to the bodily disease or condition with which they are associated. In speaking of Climacteric Insanity it is contended that men between 50 and 60 have a critical period corresponding to that passed through by women between 40 and 50; but the evidence seems far from conclusive. But nothing can be more striking and terribly instructive than the amount of insanity of one kind or another that is unmistakably connected with the organs and functions of generation.—The morbid psychology of criminals by David Nicolson, M.B., continues; and his observations on this unfortunate class are very valuable and well worth recording—especially perhaps may they prove useful "as a basis of comparison for kindred phenomena occurring in circumstances less definite and uniform." No one is likely to be very seriously injured by the common prison delusion "that their food is poisoned;" but if the same painful fancy take possession, as it sometimes does, of individuals in the outer world, it may not be so readily recognised as a delusion, and the consequences may be very mournful.—A psychological study of the character of Jean Jacques Rousseau, by J. Hawkes, M.D., suggests the idea of a washerwoman sounding the Atlantic with her clothes line, and finding it very shallow all over.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, April 1.—In this number Dr. Mohn furnishes a number of data from three years' observations of the temperature in and near Christiania—at the Institute and the Observatory—and of the decrease of heat with height; a station named Frognersäter having been chosen, situated about five miles NN.W. from the Observatory, and 408 metres above the sea. The air within the city is shown (as in other localities) to be warmer than without. The temperature in general decreases with the height, and most quickly in May; in the winter months the decrease is small, and it passes, in December, into an increase. Dr. Mohn studied the meteorological conditions present in three separate cases:—(*a*) Frognersäter warmer than Christiania; (*b*) colder and excessive; (*c*) change of temperature on fall of rain or snow. As regards (*a*), it occurred in cold weather; the wind N.E. or E., and light; atmospheric pressure about 7 mm. above normal; sky most often clear, but sometimes a mist covered Christiania, while Frognersäter was in sunshine. The author inquires at some length into the causes of change of temperature with height, and points out that the elements of greatest influence here are the strength of wind and the relative moisture. The change increases with the former and decreases with the latter. To this is joined the action of precipitates, in so far as this, accompanied by greater relative moisture, contributes to lessening the decrease of temperature with the height.—Prof. Ebermayer follows with a review (in part) of a new text-book of climatology by Dr. Lorrenz and Dr. Rothe. From personal observation he disputes the authors' assertion that the increase of cells in plants takes place only by night.—Among the "Kleinere Mittheilungen," we note some meteorological observations from the north-west coast of Spain.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 7.—"Preliminary Experiments on a Magnetised Copper Wire," by Prof. Balfour Stewart, LL.D., F.R.S., and Arthur Schuster, Ph.D.

1. The following experiments were made in the physical laboratory of Owens College, Manchester:—

A copper wire was wound fifty-three times in one direction round the poles of a powerful electro-magnet, the length of wire encircling these poles being about twelve metres.

A Wheatstone bridge was employed to measure the resistance of the wire, and a very delicate Thomson's reflecting galvanometer, by Elliott Brothers, was likewise used.

Experiments were made at intervals of two minutes; and on each occasion the current was allowed to pass through the bridge for ten seconds, the measurement being taken by the first swing of the galvanometer, which lasted for about eight seconds. Three cells of Grove's battery were used for producing this current, but on the other hand six similar cells were employed for magnetising the electro-magnet.

2. In the first experiments made, the induction-current due to the wire coiled round the magnet affected the galvanometer, but after Dec. 12 a solid key put into the circuit was taken out, so that no induction-current passed.

The following is a specimen of the observations made:—

Dec. 17, 1873.

Time of putting on current. h. m.	Whole deflection observed (increasing deflection denotes increasing resistance).	Condition of magnet.
11 11	312	off
13	317	off
15	311	off
17	345	on
19	328	off
21	306	on
23	303	off
25	293	on
27	300	off
29	290	on
31	307	off
33	283	on
35	292	off
37	288	on
39	302	off
41	292	on
43	309	off

It will be seen from this experiment that the *first effect* of putting on the magnetism was a marked increase of resistance; but with this exception the resistance, when the magnetism was on, was less than the mean of the two resistances on both sides of it, representing the magnetism off.

3. The arrangement remained untouched, as far as we know, from Dec. 15, when it was finally made, until Dec. 19, when the experiments were interrupted during the Christmas holidays; and in all cases the *first effect* of putting on the magnetism was a marked increase of resistance.

It was soon seen that this *first effect* had some reference to the time elapsing since the last experiments were made. For instance, there was on Dec. 18 a marked increase of resistance when the magnet was first put on; but on the afternoon of that day the experiments were repeated, and there was no apparent increase of resistance in this *first effect*. Next, with regard to the *average effect*; on Dec. 16, 17, and 18, this *average effect* of magnetism was a decrease of resistance.

4. The experiments were resumed on Jan. 7, the arrangement having remained untouched during the holidays. From this date until Jan. 10 inclusive, the key was taken out before beginning experiments in the morning; there was no peculiar *first effect*; while, on the other hand, an *average effect* denoting a decrease of resistance came out very prominently. On Jan. 12 and 13 the key was only taken out before magnetising, and on these occasions the *first effect* denoting increased resistance was sufficiently marked.

Our method of procedure was varied in the above manner up to Jan 27; and it was invariably found that whenever the key was taken out before commencing experiments there was no *first effect*; but when it was kept in until before magnetising, this *first effect* was sufficiently marked. These experiments concur in proving that the *first effect* has some reference to the previous treatment of the wire, but they do not prove that it is at the same time connected with the putting on of the magnetism. To determine this point we made a set of experiments on Jan. 22, 26, and 27. When the current had become constant the key was taken out, but the magnetism was not put on; and on these occasions there was no *first effect* of the current upon itself in the direction of increased resistance, but rather in the opposite direction. It thus appears that the *first effect* which increases the resistance has not only reference to the previous treatment of the wire, but depends also on the magnetism being put on.

This result is confirmed by experiments made previous to Dec. 12, in which the key was not taken out at all. For instance, we have on Dec. 9—

First off.	On: First effect.	Second off.
0	+ 54	+ 45

We have hitherto only spoken of the *first effect* obtained after Jan. 7, we now come to the *average effect*. From Jan. 7 to Jan. 27 inclusive, the magnetism was always put on in the same direction, and the *average effect* invariably denoted a decrease of resistance when the magnetism was on.

5. On Jan. 28 the magnetism was reversed; the effect during this day was very irregular. On Jan. 29, 30, 31, Feb. 2, the key was left in until before magnetisation. The *first effect* was now extremely large, but it was suspected that during these experiments the contact of the key was not very good.

On Jan. 29 the *average effect* denoted a decrease of resistance, but on Jan. 30, 31, Feb. 2, 4, 6, the *average effect* denoted an increase of resistance.

6. From Feb. 6 until Feb. 11 the wires were left broken; on Feb. 11 there was a very slight *first effect* in the direction of increased resistance, and a slight *average effect* in the direction of decreased resistance. On Feb 12 a mercury interruptor was used instead of a metal key, both the wires being broken by it, and its use was continued until Feb. 18. The interruptor was left in over night, and the current was only broken before magnetisation, but no *first effect* was observed.

From Feb. 19 to Feb. 26 one wire only was broken by the fluid interruptor, nevertheless there was no *first effect*.

On Feb. 12, when the fluid interruptor was first employed, there was a very small *average effect* in the direction of increased resistance; but in all the experiments afterwards this *average effect* was in the direction of decreased resistance. The magnetism had been in one direction from Jan. 28, but during the experiment of Feb. 25 it was reversed and retained in this condition through the experiment of Feb. 26 without appearing to affect the results.

7. From these experiments we may perhaps conclude as follows:—

In the *first place* there is a *first effect* in the direction of increased resistance which appears to have reference to three things, namely, the previous state of the wire, the solidity of the circuit, and its magnetisation.

In the *second place* we have an *average effect*, of which the normal state appears to denote a decreased resistance while the magnetism is on, without reference to the direction of the magnetism.

In the *third place*, when in a solid circuit the direction of the magnetism has been recently changed, there appears to be a temporary reversal of the average effect, which appears at first as an increase of resistance. Besides the evidence herein detailed, we have other evidence in favour of the third conclusion; for in some preliminary experiments, in which we frequently reversed the poles, we found an increase of resistance when the magnetism was on.

We are led to conclude, from other experiments besides these, that the effect of the magnetism is not merely confined to the part of the copper wire wound round the poles, but is propagated all along the wire. On Dec. 2, for instance, the current was passed through the wire, the galvanometer being joined as a secondary circuit. The main current was therefore measured. The deflections were as follows:—

297	off	300	off
300	on	302	on
297	off	301	off
300	on		

This shows an average strengthening of the current equal to about 1-200 part of the whole. Were this strengthening due merely to the change of resistance of that part of the wire wound round the poles, the effect as measured by the much more delicate arrangement of Wheatstone's bridge would be much larger than was actually observed.

9. Allusion was made in Article 7 to some preliminary experiments in which increased resistance was observed when the magnetism was put on alternately in different directions. Similar experiments were made, giving the same result with a piece of coke and graphite which were between the poles of the magnet.

10. We have also some evidence that a copper wire, one end of which is wound round the pole of the magnet, changes its position in the electromotive series. Two copper wires were dipped into dilute nitric acid and connected with the galvanometer. A weak current passed through the galvanometer owing to a slight difference in the copper wires, one of which was also connected with the copper wire wound round the magnet. When the magnet was on, the current as a rule changed in intensity; but the effect was small, and the difficulty of having two copper wires which, when joined together and dipped into nitric acid, give a current sufficiently weak and constant, prevented us from getting any decided results.

11. In conclusion we have to state that we regard these results which we have ventured to bring before the Royal Society as preliminary, the correctness of which will, we trust, be confirmed by the further experiments which it is our intention to make.

Mathematical Society, Thursday, May 14.—Dr. Hirst, president, in the chair.—The president having vacated the chair gave an account of his paper On the correlation of two planes. "A correlation is said to be established between two planes, when their points and right lines are so associated that to each point in one of the planes, and to each line passing through that point, respectively correspond, in the other plane, *one* line and *one* point in that line." It was first shown that eight conditions are necessary and sufficient for the establishment of a correlation between two planes: and in the next place it was shown that the problem of determining a correlation between two planes which shall satisfy *any* eight given conditions is susceptible in general of a finite number of solutions. Systems of correlation were then considered: as also the origin and nature of exceptional correlations. Relations were next established between the characteristics and singularities of any system of correlations. An enumeration and classification of the fundamental systems of correlations were then made and illustrated by reference to a table in which the systems were arranged in six groups. Dr. Hirst also touched upon the number and nature of exceptional correlations in the fundamental systems. A table was exhibited showing the number of correlations satisfying eight elementary conditions. If α points in one plane have given polars in the other; β right lines in the first plane have given poles in the second; γ points and δ lines in each plane have given conjugates in the other plane, then $(\alpha\beta\gamma\delta)$ is termed the *signature* of the system of correlations satisfying the above conditions. We see that the systems of correlations corresponding to the signatures $(\alpha\beta\gamma\delta)$ and $(\beta\alpha\gamma\delta)$ are identical. The following two theorems are generalisations of the results arrived at:—(I.) In a system of correlations $(\alpha\beta\gamma\delta)$, the curve of the class $[\alpha\beta(\gamma+1)\delta]$ which represents either of two conjugate points A_1, A_2, breaks up into the other, together with a point on each of the singular lines associated with those which pass through the former. The multiplicity of A_2 on the representative of A_1 is $[(\alpha+1)\beta(\gamma-1)\delta]$, and that of A_1 on the representative of A_2 is $[\alpha(\beta+1)(\gamma-1)\delta]$. The number of singular lines which pass through A_1 is $[\alpha\beta(\gamma+1)\delta]-[(\alpha+1)\beta(\gamma-1)\delta]$, and the number of those which pass through A_2 is $[\alpha\beta(\gamma+1)\delta]-[\alpha(\beta+1)(\gamma-1)\delta]$. (II.) In a system of correlations whose signature is $(\alpha\beta\gamma\delta)$, the curve of the order $[\alpha\beta\gamma(\delta+1)]$, which represents either of two conjugate lines α_1, α_2, breaks up into the other, together with a line through each of the singular points associated with those situated on the former. The multiplicity of a_2 on the representative of a_1 is $[\alpha(\beta+1)\gamma(\delta-1)]$, and that of a_1 on the representative of a_2 is $[(\alpha+1)\beta\gamma(\delta-1)]$. The number of singular points situated on a_1 is $[\alpha\beta\gamma(\delta+1)]-[\alpha(\beta+1)\gamma(\delta-1)]$, and the number of those situated on a_2 is $[\alpha\beta\gamma(\delta+1)]-[(\alpha+1)\beta\gamma(\delta-1)]$.—Mr. Spottiswoode (the chairman *pro tem.*) and Prof. Clifford spoke on the subject of Dr. Hirst's communication.—Mr. Spottiswoode, F.R.S., next briefly stated some of the results given in his paper On the contact of quadrics with other surfaces. The following were amongst those stated :—Through any m (or $m+1$) points of space 3^{m-2} surfaces, having $2m-2$ (or $2m-1$) independent constants in their equation, can be drawn such that a quadric may be described touching any of the surfaces in the m (or in m out of the $m+1$) points. Thus for example :—the equation of a quartic scroll having a triple line is $(ax+by)\,zx^2 + (cx+dy)\,wy^2 - mx^2y^2 = 0$; hence, through any three points of space, three quartic scrolls having the same double line can be drawn such that a quadric may be described touching any one of the scrolls in the three points. Again, the equation of a quartic surface having for its nodal line the twisted cubic $p = xz - y^2 = 0$, $q = xw - yz = 0$, $r = yw - z^2 = 0$, may be put in the form $ap^2 + bq^2 + cr^2 + 2\,(fqr + grp + hpq = 0$, hence, through any four points of space, three quartics, having the same twisted cubic for their common nodal line, may be drawn such that a quadric may be described touching any one of the quartics in three of the points. Remarks were made on the paper by the president and by Prof. Clifford.—A paper by Mr. J. H. Röhrs, communicated by Prof. Cayley, was taken as read. Its subject was "The Rotation of a Hollow Sphere filled with viscous fluid and made to rotate about an axis through its centre under the action of an external impressed given periodic force."

Meteorological Society, May 20.—Dr. R. J. Mann, president, in the chair.—The following papers were read :—Some remarks on the estimation of wind force, and on the relation between pressure and velocity, by C. O. F. Cator, in which he first expressed a strong opinion on the impossibility of *estimating* the force of the wind with any degree of accuracy; but thought that for any useful purpose it must be obtained from *instrumental* observation. He then referred to the different notations for describing the wind, and condemned Beaufort's (0 – 12) as eminently unsatisfactory, both on account of the means by which the numbers were arrived at, and also especially because of the difference of standard for the lower and higher numbers. He suggested that during an observation the wind could not practically be described as an absolute force, on account of its frequent variations, but as a varying force, extending over two or three numbers; and then proceeded to account for the difference of force, as estimated, at any stations from different directions although the velocity as shown by Robinson's cups might be the same—partly by the position of the observer not being identical with that of the cups, and partly from the surrounding objects. He then suggested a new scale, and that whether pressure or velocity were the basis, it should increase in arithmetical progression, and concluded by expressing his preference for the former.—On the weather of thirteen winters, by R. Strachan.—On a new deep-sea and recording thermometer, by H. Negretti and J. W. Zambra.—On a new mercurial minimum and maximum thermometer, by S. G. Denton.

Anthropological Institute, May 26.—Prof. Busk, F.R.S., president, in the chair.—Mr. Hyde Clarke read a paper entitled "Researches in Prehistoric and Protohistoric comparative philology, mythology, and archæology, in connection with the origin of culture in America, and its propagation by the Sumerian or Akkad races." The author began with the illustrations of the common origin of culture in Asia, Africa, and America in a chronological series of the distribution of languages in the old and new worlds in the Prehistoric and Protohistoric epochs. These included the Negritos or Pygmeans, the Cannibal races, the Carib-Whydah-Aino, the Honduras African, the Khond-Wolof, the Agaw-Guarani, the Vasco-Kolaro-Lesghian, the Ugrian, the Sumerian, &c. New facts in comparative grammar were adduced, embracing the names of animals, of weapons, the series of negative terms, and the connection of philology, mythology, and archæology, with a table of convertible equivalents of primary radicals. The second part of the paper was devoted to a special consideration in detail of the community of the Aymara and Quichua of Peru, the Maya of Yucatan, and the Mexican with those of Cambodia, Pegu, and Indo-China, and of these again with the newly-deciphered Sumerian or Akkad (cuneiform) and the connection with Georgian and Etruscan. These were combined with the monuments, arts, and archæology of the respective countries. The author, referring to his identification of the languages of the Brazil with the Agaw of the Nile, and the Akkads of the Caucasus, supported the view that culture had been introduced into South America across the Pacific by Easter Island, and suggested that it was from one original source in high Asia.

PARIS

Academy of Sciences, May 25.—M. Bertrand in the chair.—The Perpetual Secretary announced the death of M. Antoine-Marie-Rémy Chazallon, correspondent for the section of geography and navigation.—The following papers were read :—Note on the movement of the conical pendulum, with consideration of the resistance of the air, by M. H. Resal.—M. P. Desains presented the continuation of his paper on solar radiation. The author has employed in these experiments a modification of Nobili and Melloni's thermo-electric apparatus.—On the transformation of iron into steel, by M. Boussingault. The author's observations and analyses tend to show that melted steels of superior quality are really iron and carbon. As the quality improves sulphur diminishes, and they are generally free from phosphorus, while manganese and silicon rarely exceed 1-1000.—Observations on the spectrum of comets, by P. Secchi. The author has observed the spectrum of Winnecke's and Tempel's comet, and also of Coggia's. The results in the latter case point again to the existence of carbon in these remarkable bodies. In the same paper further evidence was adduced that the line 1,474 does not belong to iron; and the author communicated also an observation on the effect of atmospheric oscillation on the appearance of Jupiter's first satellite just before passing on to the planet's disc.—On the Vidal ebullioscope, by M. E. Malligand and Mlle. E. Brossard-Vidal. This instrument is for the valuation of wines, and other alcoholic liquids.—On a new mineral species from the province of Lerida, by M. X. Ducloux. The analysis agrees with the formula $Sb_2O_5 + 4CuAgCO_3$.—On the conditions of the persistence of sensibility in the peripheral extremity of sectioned nerves, by MM. Arloing and L. Tripier.—On the addition of elliptic functions, by M. E. Catalin.—M. l'Abbé Aoust presented a paper in reply to the observations made by M. Serret on his paper on the integrals of curves which have an even polar surface.—M. Ch. Bontemps communicated his third note on the motion of the air in pipes.—On the action of sulphur urea and of carbon disulphide on silver urea, by M. J. Ponomareff.—Researches on germination, by MM. P. P. Dehérain and E. Landrin. Experiments on grain have shown that no gas is so hurtful to germination as carbon dioxide.—On ammonia and ammonium phenate in the treatment of cholera and diseases produced by ferments *à propos* of serpent bites, by Dr. Déclat.

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ERRATA.—Omit "so" in p. 62, col. 2, line 22 from bottom; p. 63, col. 2, line 20 from top, for "individual" read "undivided."