THURSDAY, MARCH 27, 1879

ORGANISATION AND INTELLIGENCE

Habit and Intelligence: a Series of Essays on the Laws of Life and Mind. By Joseph John Murphy. Second Edition, illustrated, thoroughly revised, and mostly rewritten. (London: Macmillan and Co., 1879.)

Life and Habit. By Samuel Butler. (London: Trübner and Co., 1878.)

THE first edition of Mr. Murphy's work was reviewed in NATURE, vol. i. pp. 288 and 315, a little more than nine years ago, and on reading the article we find little or nothing in the remarks and criticisms then made which require modification on account of subsequent discoveries. The present work is, however, very largely new, about one-third of the matter in the first edition, which treated of physical questions, being omitted, and replaced by a series of new chapters on biological subjects. It is to these new chapters that we shall mainly confine our present notice.

Chapter XI. gives a very good summary of the facts of variation from Darwin's "Domestication of Animals and Plants," and other works; and in Chapter XII. we have these facts discussed in regard to the sufficiency of natural selection for the origin of species. The greatest use is here made of the argument (said to be Prof. Tait's) in the *North British Review* (June, 1877), and which another writer has summed up as follows:—"The final establishment of the superior type is dependent at each step upon three accidents. First, the accident of an individual sort or variety better adapted to the surrounding conditions than the then prevailing type; secondly, the accident that this superior animal escapes destruction before it has time to transmit its qualities; and thirdly, the accident that it breeds with another specimen good enough not to neutralise the superior qualities of its mate." Put in this way, the difficulty staggers most persons who are not practical naturalists; yet it has always seemed to me to be really beside the question, and by no means of the importance which Mr. Darwin himself has given to it by acknowledging that the argument had not occurred to him. Even acute writers like Mr. Murphy do not see that individual variations or "sports" are of no importance whatever to the theory of natural selection, or he would never bring forward the argument at p. 380, that with an animal born of two parents "there is an almost overwhelming probability that the favourable variation is found only in one," and will therefore diminish in each succeeding generation till it disappears, unless the same favourable variation recurs again and again to counteract this tendency. In what may be termed normal variation, however (which Mr. Darwin has always considered the main agent in supplying materials for natural selection), none of these difficulties occur, and as it is very important to make this clear, I will give a few illustrations of it. There is no part, organ, or character of an animal or plant but what is sometimes more sometimes less developed in different individuals. The whole population of a species in any given year may therefore be divided into two equal portions, with regard to any such organ or character—the less developed and the more developed. Thus, for example, all the foxes of the species *Canis vulpes* are necessarily divisible into a lighter and a darker coloured group; into a longer and a shorter tailed group; into a fleeter and a less fleet group; into a group with more developed and less developed canine teeth; and so on with regard to every character, external and internal. This can only be denied by asserting that there are characters which in the species in question are *absolutely unvarying*, an assertion which I am not aware that any one has made or attempted to prove, while it is certainly contradicted by the observations of all who have ever studied nature.

But if so, what happens when changed conditions occur, rendering the increased development of some faculty or organ beneficial? Can any one doubt that the one or five, or twenty per cent. of individuals which annually survive will belong, wholly or almost wholly, to the moiety in which that organ or faculty is better developed rather than to that in which it is worse developed? It matters not at all whether the *most* perfect individual or the twenty most perfect individuals survive or not; but the survivors will certainly be found among the better adapted rather than among the worse adapted half, and most likely will include a majority of individuals in the better half of the better half. And this process will be repeated *every year* without fail. There is thus no waiting for favourable variations to occur; no series of coincident improbable accidents is required; but the process goes on continuously with ever increasing power owing to the influence of heredity, till the species is modified up to the requirements of the changed conditions. By this process, leading to a decided advance *every year*, we can quite understand how any dominant species (that is, one which occupies a wide area and has a large population) may be modified quite as rapidly as is required by all ordinary changes of conditions, although extraordinary changes may lead to the extinction even of dominant species. It is hardly possible to conceive any improvement or modification of a species which might not be brought about by so powerful a selection as this, acting on variations which seem to us very trivial; while, on the other hand, the effect of greater individual variations or "sports" is very uncertain, and may perhaps never be used in nature as a means of modifying species.

These considerations also show the true bearing of "Delbœuf's Law," to which Mr. Murphy attaches much importance. It is proved mathematically that if, in any species, several individuals are in every generation born with any particular variation which is neither beneficial nor injurious to its possessors, and if the effect of the variation is not counteracted by reversion, the proportion of the new variety to the original form will constantly increase until it approaches indefinitely near to equality. But as, in every species, there are not one only, but hundreds of distinct variations in every generation, all subject to change in amount and direction in each succeeding generation, and as each of these will by the above law tend to equality with all others, the result must be that every slight recurrent variation will maintain itself in the species on terms of approximate equality with all other variations; and this will evidently be useful, by keeping up a vast stock of slightly varied forms within the species, which will be ready at any moment to furnish the material on which selection may work when variations of a particular kind are needed.

The next three new chapters, on "Fixation of Characters," on "Effect of Change of Conditions," and on "Mimicry, Colour, and Sexual Selection," contain much interesting matter, with a number of suggestions of difficulties mostly dependent upon our total ignorance of the peculiar conditions or laws under which certain characters first arose. Such difficulties are of little importance, because they are always liable to disappear with an increase of knowledge. What, for instance, is the value of such a criticism as this: "The abnormal position of the left carotid artery in some groups of parrots is as good an instance as can be mentioned of a character which is constant throughout an entire group, which *must* have arisen suddenly, and *cannot* have been fixed by natural selection, because it *cannot* be useful." The three words I have italicised mark three positive statements which cannot possibly be verified, and which may very probably all be wrong. The parrots form not a *family* only, but a very distinct *order* of birds, and, from the occurrence in the miocene of France of a parrot of a living genus, are probably of immense antiquity. Not only do we know nothing of their early history, but, owing to their so rarely breeding in this country we know nothing of their embryology, and can therefore have no grounds for assertions as to what could or could not have been in the remote past, when they were developing into the varied forms that now exist, under conditions of which we are perfectly ignorant.

Another new chapter, on "Metamorphoses and Metagenesis" gives an interesting outline of the metamorphosis of insects, crustacea, and hydroids, illustrated by a number of excellent figures, and remarks on the difficulty of explaining many of the facts by variation and natural selection, the conclusion being that "many of the transformations, especially among the crustacea and the hydrozoa, do not consist in adaptations to any new or special mode of life, and consequently cannot be accounted for by the Darwinian or any similar theory, but must be due to a formative impulse impressed on living matter at the beginning."

The next chapter, on "Structure in Anticipation of Function," is not so good as some of the others, and here again objections are brought forward whose whole weight depends on our ignorance of the conditions under which certain structures were modified. Thus, it is said to appear impossible to account for the transition from the fin of *Ceratodus* to the simple fin-ray of *Lepidosiren* by any means which Darwinism admits, because it seems impossible that the loss of the membranes of its fins can be beneficial to a fish. But in this case there seems to be a difference of habits which may show how the "impossible" occurred. The *Lepidosiren* of the Gambia burrows in the mud, where it remains during the dry season, and for this "burrowing" the cylindrical rays may be better adapted than the broad fins of *Ceratodus*.

In the chapter on the "Origin of Man" Mr. Murphy replies to my argument that the brain of savage man is an instrument beyond his needs, by pointing out that "the real superiority of man consists in the faculty of language, and that the mental power implied in this unique faculty is represented by the very great excess in the size of the human brain over that of the highest apes;" and he goes on to say: "If, then, the Darwinian theory is true of man, the difference between the highest ape and that of the lowest man is due to the exercise of the brain during the period while the power of language was in process of evolution, aided by the natural selection of the largest brains, in which, of course, this new power would be most highly developed." This appears to me a very forcible objection, and I must acknowledge that it is "a sufficient answer" to my argument, so far as regards the difference between the brain of savage man and apes. The question remains, however, of the latent powers in the brain of savages; and Mr. Murphy maintains that the languages of many savages—of the Kafirs, for example, are so much in advance of their needs that they could not have been evolved by natural selection. In most other respects he agrees with the arguments in my essay on "The Limits of Natural Selection as applied to Man."

This concludes the new matter in the physiological part of the book; but before passing on to the psychological portion, I must notice one passage embodying a very common source of confusion as regards the geographical distribution and mode of origin of species. Referring to the marine lizard of the Galapagos, *Amblyrhynchus*, Mr. Murphy remarks: "This singular species is found nowhere except in the Galapagos, and consequently has, most probably, been evolved there; but on Darwinian principles, how can so peculiar and aberrant a form have been evolved during the geologically short time that has passed since these islands first rose above the ocean?" The difficulty thus raised, with many analogous cases, I have endeavoured to explain in the February issue of the *Nineteenth Century*. The idea that this peculiar lizard has been "evolved," in the Galapagos really implies spontaneous generation; for what was it evolved out of? A remote ancestral form *must* have reached the islands from the main land, if there is to be any "evolution" in the case, and if a remote why not a near ancestor? It appears to me, not a mere probability but almost a certainty, that the generic type, if not the actual species, was "evolved" in America; that it was once an abundant, and, in fact, a dominant group; that it then spread to the Galapagos; that the entire group then died out on the main land, but was preserved in the islands, *owing to the absence of enemies and competitive forms*. On this principle almost all the supposed difficulties of geographical distribution may be rationally explained; and this mode of explanation is in accordance with palæontological evidence whenever it is procurable.

The remaining chapters form the psychological part of the work, in which the author develops his theory of the organising intelligence in animal forms. The only new chapter here is that on "Automatism," in which the various questions connected with the automatic motions of plants and animals, and with the instincts and the habits of animals, are set forth and commented on; and here it is very interesting to compare the conclusions arrived at with those of Mr. Butler in his very original and suggestive book on "Life and Habit."

Mr. Murphy says that the actions of a sea-anemone in seizing on its prey with its tentacles, or in closing itself when left uncovered by the receding tide, are probably purely automatic, and completely independent of sensation, consciousness, or will; and further, that there is probably no difference whatever between these motions and those of the leaves of *Dionæa* and *Drosera* which crush insects to death and suck their juices. But though independent of consciousness they are not independent of intelligence: they are instinctive, and instinct is intelligence unconscious of itself. All such actions as these are classed as *primarily* automatic, having no relation to consciousness; but there are also actions which are *secondarily* automatic, which were once conscious actions but have become unconscious through habit. These habits may become hereditary, forming instincts, and can then in some cases not be distinguished from primary automatism. Elsewhere he speaks of "a principle of intelligence which guides all organic formation and all motor instincts, and finally attains to consciousness in the brains of the higher animals, and to self-consciousness in the brain of man."

We will now turn to Mr. Butler's work, and see how he deals with these and analogous facts. He first discusses acquired habits, showing that, as we do things more and more frequently we do them with less thought and effort, till at last, when we do them perfectly we also do them unconsciously. He then shows that the same law applies to knowledge and beliefs, which are only complete and unwavering, when we have ceased to doubt or to think of reasons or facts in support of them, when, in fact, they have become unconscious. We then come to habits acquired at or soon after birth, as walking, or eating, which, though they continue to be voluntary, are often performed quite unconsciously. Swallowing and breathing, though very complex acts, are acquired by the infant a few minutes after birth, and thence performed unconsciously, and we endeavour to explain this by the terms "hereditary instinct," and the "experience of the race." Mr. Butler concludes that these terms are unmeaning, and that, because we see that all actions when performed sufficiently often become automatic, we ought to conclude, whenever we see actions performed automatically, that there *has* been this repeated performance of them somehow or other. He thus sums up his facts on this phase of the question: "We are most conscious of, and have most control over, such habits as speech, the upright position, reading and writing, which are acquisitions peculiar to the human race, and always acquired after birth. We are less conscious of, and have less control over, eating and drinking, swallowing, breathing, seeing, and hearing, which were acquisitions of our prehuman ancestry, but which are still, geologically speaking, comparatively recent. We are most unconscious of, and have least control over, our digestion and circulation, which belonged even to our invertebrate ancestry, and which are habits, geologically speaking, of extreme antiquity." These principles are then applied to a great variety of facts in biology with extreme and, as some may think, perverted ingenuity, of which we can only give a single illustration: "We say of the chicken that it knows how to run about as soon as it is hatched. So it does; but had it no knowledge before it was hatched? What made it lay the foundations of those limbs which should enable it to run about? What made it grow a horny tip to its bill before it was hatched, so that it might pick all round the larger end of the egg-shell, and make a hole for itself to get out at? And is it in the least agreeable to our experience that such elaborate machinery should be made without endeavour, failure, perseverance, intelligent contrivance, and practice? In the presence of such considerations it seems impossible to refrain from thinking that there must be a closer continuity of identity, life, and memory between successive generations than we generally imagine." This is the "unconscious organising intelligence," says Mr. Murphy, ultimately becoming conscious in the complete animal. "It is the result of often repeated conscious acts," says Mr. Butler, "which are now performed unconsciously after countless repetitions."

At first sight we seem to have here only the "ancestral experience" which has already been objected to as unmeaning. But this difficulty is overcome by the strange assumption that "it is the same chicken which makes itself over and over again; for such unconscious action is not won, so far as our experience goes, by any other means than by frequent repetition of the same act on the part of one and the same individual." Let no reader throw the book aside on coming to this astounding sentence, till he has read the two succeeding chapters on "Personal Identity," which are full of curious facts and subtle reasoning, and which lead to the conclusion that life is the one great personality, of which all living things are but differentiated offshoots still retaining a latent memory of a long succession of ancestral habits and experiences. This idea is carried further in the next chapter, on "Our Subordinate Personalities," in which it is shown that the highest authorities maintain the distinct individuality of the countless cells or physiological units of which our bodies are composed, and Mr. Butler remarks: "With the units of our bodies it is as with the stars of heaven, there is neither speech nor language, but their voices are heard among them. Our will is the *fiat* of their collective wisdom as sanctioned in their parliament, the brain; it is they who make us do whatever we do. When the balance of power is well preserved among them, when they respect each other's rights, and work harmoniously together, then we thrive and are well; if we are ill, it is because they are quarrelling among themselves, or are gone on strike for this or that addition to their environment, and our doctor must pacify or chastise them as best he may."

Passing on to Chapter IX.—on the "Abeyance of Memory"—it is shown that we remember best two classes of phenomena, either very unfamiliar objects or combinations—as if we were once in our lives shipwrecked on an iceberg, or very familiar object or acts, which produce their effect by repetition. These last, however, are apt to become unconscious, or to be wholly lost sight of, except when the usual conditions call them up, an amusing illustration of which is given as follows:—"Men invariably put the same leg first into their trousers—this is the survival of memory in a residuum; but they cannot, till they actually put on a pair of trousers, remember which leg they *do* put in first; this is the rapid fading away of any small individual impression." It is on the same principle that every act of growth of cells and organs is said to be unconsciously remembered, when the same or analogous conditions recalls it to the dormant memory.

In another very ingenious and suggestive chapter entitled "What we might expect," it is maintained that the preceding facts and principles lead up to and explain all the curious phenomena of growth, reproduction, variation, and heredity, as set forth in the works of Darwin, Spencer, and other writers; and the same principles are applied in succeeding chapters to the phenomena of instinct, and the theories of Lamarck, Darwin, Mivart, and others. The argument is then summed up, and the conclusion arrived at that "Life is that property of matter whereby it can remember. Matter which can remember is living; matter which cannot remember is dead. The life of a creature is the memory of a creature. We are all of the same stuff to start with, but we remember different things. As for the stuff itself of which we are made, we know nothing save only that it is 'such as dreams are made of.'"

Such a brief notice as this can give no adequate idea of the originality and the logical completeness of Mr. Butler's remarkable work, which is far less known than it deserves to be. It may be truly said of it that it is more amusing than most novels, while it contains more material for thought than is to be found in most books of double the size. It will be seen that there is a certain agreement with Mr. Murphy, but Mr. Butler goes much further, in tracing the former writer's vague and unlocalised "unconscious intelligence" to the physiological elements of all organisms; and, however wild and improbable the theory may seem, it receives, strange to say, considerable support from the views of Haeckel and other German physiologists of the most advanced school. If the reader will turn to NATURE, vol. xix. p. 115, he will find Haeckel maintaining that "in the *Infusoria* a single cell performs all the different functions of life, including the mental functions." . . . . "By the same right by which we ascribe an independent 'soul' to these unicellular *Infusoria*, we must ascribe one to all other cells, because their most important active substance, the protoplasm, shows everywhere the same psychic properties of sensitiveness (sensation) and movability (volition). The difference in the higher organisms is only that there the numerous single cells give up their individual independence, and like good state citizens, subordinate themselves to the 'state-soul,' which represents the unity of will and sensation in the cell-association."

We have here an extraordinary agreement with Mr. Butler, although, as we are informed, he was quite unacquainted with Haeckel's works when he wrote his book; and this fact should induce us to give a more careful consideration to the views of a writer who, although professedly ignorant of all science, yet possesses "scientific imagination" and logical consistency to a degree very rarely found among scientific men. The want of a practical acquaintance with natural history leads the author to take an erroneous view of the bearing of his own theories on those of Mr. Darwin. There is really nothing to prevent their harmonious combination, and they may even be said to be in great part complementary to each other. Mr. Butler's book is so full of strange fancies and witty conceits, as to have led some readers to look upon the whole as an elaborate jest. Beneath this sparkling surface there is, however, much solid matter, and though we can at present only consider the work as a most ingenious and paradoxical speculation, it may yet afford a clue to some of the deepest mysteries of the organic world.

ALFRED R. WALLACE

RODWELL'S ETNA

Etna: a History of the Mountain and of its Eruptions. By G. F. Rodwell, Science Master in Marlborough College. With Maps and Illustrations. Pp. 142. (London: C. Kegan Paul and Co., 1878.)

IN this little volume Mr. Rodwell has essayed to do for Etna that which the late Prof. Phillips accomplished so successfully in the case of Vesuvius, namely, to write a popular and at the same time accurate account of the past and present conditions of a mountain, which from the very earliest periods to which human history and tradition go back, has powerfully arrested the attention and excited the imagination of mankind. The scope and aim of these two works being so nearly the same it is hard to avoid drawing a comparison between them.

The first and fifth chapters of the work of Mr. Rodwell, which deal with the past history of the mountain and the record of its eruptions, indicate much learning and careful research on the part of the author, and indeed these portions of his volume may compare not unfavourably with the equivalent parts of Prof. Phillips' work; higher praise than this can scarcely be given to it. Almost equally praiseworthy are the second and fourth chapters, which give a general sketch of the physical features of Etna and an account of the origin, the past history, and the present condition of the numerous towns which are crowded about the flanks of the great volcano. The third chapter, giving details concerning the author's own ascent of the mountain, though sufficiently interesting in itself, is perhaps better fitted for the pages of a popular journal than of a work like the present, since ascents of Etna are now sufficiently common and every-day occurrences.

It is when we come to the more purely scientific portions of the volume that a comparison of the work of Mr. Rodwell with that of Prof. Phillips places the former in such a disadvantageous light. It is rather startling to find the more general and popular descriptions occupying five chapters, including 113 pages, while the account of the geology and mineralogy of the mountain is condensed into a single chapter of 29 pages, and when these pages are read we cannot help feeling that the questions treated of in them are handled in a somewhat imperfect and perfunctory manner. Any one turning to a treatise professing to deal with the geology and mineralogy of Etna might fairly expect to find a fuller and clearer account than Mr. Rodwell gives us of the exact relations of the volcanic masses to the stratified and highly fossiliferous deposits with which they are so intimately associated. Equally disappointing is it to find that the important question of the elevation hypothesis of von Buch is so summarily dealt with by Mr. Rodwell, especially when we remember that in the discussion on this subject which took place between Élie de Beaumont and Dufrenoy on the one hand, and Scrope and Lyell on the other, Etna supplied so crucial a test. Nor can we regard the few notes of Mr. Rutley on the microscopic characters of several specimens brought to him by the author, excellent as they are in themselves, as affording anything like an adequate discussion of the nature of the Etnean lavas.

There are not, indeed, wanting indications in the work before us that the author has scarcely succeeded in so far mastering the scientific questions connected with his subject as to qualify himself for giving anything like authoritative opinions concerning them. Thus on page 111 we find him speaking of a crater as "composed of a prehistoric grey labradorite, and of doleritic lava." Again, so far as can be gathered from the work before us, the hypotheses of elevation craters and eruption craters are of about equal value. We are informed simply that "the opinion of geologists is divided as to the manner in which a volcano is formed;" and then follows a statement of the two rival hypotheses. Surely after the convincing reasoning of Scrope, and the patient observations of Lyell on Etna itself, as detailed in the celebrated memoir read before the Royal Society in 1858, it is strange to find such language used upon the subject, more especially when we recollect that no attempt was ever made by Lyell's opponents to discredit his observations or to reply to his deductions. We should almost as soon expect to read in a modern work on astronomy that the opinion of astronomers is divided as to whether the earth moves round the sun or the sun round the earth.

We find so much to praise in this little book, especially in the clear *résumé* of the history of the mountain and its eruptions, and the illustrations so carefully selected and reduced from those of larger works which are not easily accessible to general readers, that we regret we cannot express more unqualified approbation of that portion of the book which calls for especial notice in the pages of this journal. We can only hope that in a second edition the author may find an opportunity, which he will not neglect, of considerably lengthening and very greatly strengthening this scientific portion of his work; and in order to do so, without at the same time impairing its popular character, we can scarcely suggest a better example for him to follow than the work of Prof. Phillips, to which we have alluded at the commencement of this article.

LETTERS TO THE EDITOR

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

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

The Trans-Neptunian Planet

THE explanation given by Prof. Peters (*Astr. Nach.*, 2,240) of the observations made at Washington in 1850 of this supposed planet is put beyond doubt by the examination of Mr. Ferguson's observing-book. It is due, however, to Mr. Ferguson to say that his record is full and complete, and that his changes in the reductions were honestly made. The record is in pencil, and no figures were erased or rubbed out. They are crossed out, and the assumed figure is put by the side of the original one, while at the bottom of the page is a note with pen and ink, and in Mr. Ferguson's handwriting, stating the changes that were made. Prof. Peters's ingenious discovery of the truth was made without knowledge of the observing-book.

Such criticisms are instructive, showing how unsafe it is to build theories before we are sure of the facts. They may also be a means of avoiding a waste of labour. It is known to me that at least two American astronomers, armed with powerful telescopes, have been searching quite recently for a trans-Neptunian planet. These searches have been caused by the fact that Prof. Newcomb's tables of Uranus and Neptune already begin to differ from observation. In this connection the note of Mr. Dunkin on the errors of Leverrier's tables of Saturn is interesting. But are we to infer from these errors of the planetary tables the existence of a trans-Neptunian planet? It is possible that such a planet may exist, but the probability is, I think, that the differences are caused by errors in the theories of these planets. My observations of the satellites of Saturn are not yet discussed, but they indicate that Bessel's mass of Saturn is nearly correct. Now Leverrier has diminished this mass by about $\frac{1}{125}$th, and it seems probable that this diminution was caused by some error in his theories of Jupiter and Saturn.

A few years ago the remark was frequently made that the labours of astronomers on the solar system were finished, and that henceforth they could turn their whole attention to sidereal astronomy. To-day we have the lunar theory in a very discouraging condition, and the theories of Mercury, Jupiter, Saturn, Uranus, and Neptune, all in need of revision; unless, indeed, Leverrier's theories of the last two planets shall stand the test of observation. But after all, such a condition of things is only the natural result of long and accurate series of observations which make evident the small inequalities in the motions, and bring to light the errors of theory.

Washington, March 7 ASAPH HALL

Rats and Water-Casks

MR. NICOLS says, in NATURE, vol. xix. p. 433:—

"A ship's carpenter told me that, in the old days, before the use of iron tanks on board ship became general, the rats used to attack the water-casks, cutting the stave so thin that they could suck the water through the wood without actually making a hole in it. If any one could substantiate this it would have an important bearing on the question under consideration."

Capt. Wickham, when First Lieutenant on board H.M.S. *Beagle*, told me that when he was a midshipman it was his duty, on one of the king's ships to see that certain vessels on deck were always kept full of water, in order to prevent the rats gnawing holes through the water casks, and that through such holes nearly all the water in a cask would leak away.

CHARLES DARWIN

Tides at Chepstow

I OBSERVE two letters in NATURE lately upon this subject. Many years ago I took some pains to ascertain the greatest known rise of tide at Chepstow, for I doubted the accuracy of the common statement that it was seventy feet and upwards. At the time I made the inquiry the large railway bridge at Chepstow to carry the South Wales Railway across the River Wye was being constructed. I was acquainted with Mr. Oakden, one of the engineers on the work, and he, with great care, took levels of the marks which had been made from time to time recording the very high tides, some of them going back many years. He found the highest of them to be some decimal (of which I have no record) above fifty feet above ordnance datum. I think this may be relied upon. It is corroborated in a paper by the present Astronomer-Royal, on "Tides and Waves," in the "Encyclopædia Metropolitana," vol. v. p. 242, paragraph 7, first edition. He says: "Thus, at the entrance of the Bristol Channel the whole rise at spring-tides is about eighteen feet, at Swansea about thirty feet, and at Chepstow about fifty feet." W. B. CLEGRAM

Saul Lodge, Gloucestershire, March 18

Migration of Birds

PROF. NEWTON in his article on Migration of Birds (NATURE, vol. xix. p. 433) has omitted one, and a very important limit to the height at which birds of passage can perform their journeys. This is *temperature*. The following table of Daniell's will show

how little probability there is of migratory birds flying at great elevations, and that even in low latitudes, the temperature at altitudes exceeding four and a half miles would be prohibitory to the existence of the majority of migrants :—

Altitude.	Temp. F.	Temp. F.
'	°	°
0	+ 80	0
5,000	+ 64·4	− 18·5
10,000	+ 48·4	− 37·8
15,000	+ 31·4	− 58·8
20,000	+ 12·0	− 82·1
25,000	− 7·6	− 109·1
30,000	− 30·7	− 140·3

Calculations by Mr. Glaisher's rule for approximate temperature (decrease of 1° F. for every 300 feet elevation) give less startling results than the above, but even then, with the thermometer marking 80° at sea level, we find that a temperature of 40° of frost must exist at five miles in height.

The advocates of the "sight theory" have rather more in their favour than Prof. Newton has conceded to them. It is not necessary that birds should fly at such heights as to literally view the land they guide their course to or by. The "loom" of land, so well known to sailors, is visible when the land itself is below the horizon ; and I do not think we are entitled to say that birds would not, equally with mariners, notice the indication. Then again the action of one flock of birds when in sight of land, might guide other more distant flocks, and these might influence birds still further off. We know how the circling downward swoop of a vulture on some discovered carrion will draw to the feast vultures from all parts of the sky. We know the power of our own vision, certainly inferior to that of many birds ; and it is therefore well within the bounds of possibility that migrating birds, watchful because weary and hungry, may see and be influenced by the movements of flocks of their companions thirty to forty miles distant. A few flocks might thus bridge a wide expanse of barren ocean.

It is not necessary, however, to insist that sight alone is the guiding faculty in migration. The majority of, if not all, animals possess that marvellous "sense of direction" that has become so blunted in civilised man. Both savages and lower animals will find their way back in a "bee-line" through unknown country, to places whence they have been led by tortuous tracks. Why should not this "sense of direction" then guide birds over oceans without landmarks. The case of first migration of young birds (cuckoos and starlings) quoted by Prof. Newton, is, it must be confessed, a problem difficult to solve ; but when the journey has been once made by an individual bird in a flock I cannot see more mystery in the arrival of that flock at their destination than there is in the perfect accord between the hand and the eye of a good shot or a good billiard player.

We must all concur with Col. Donnelly in desiring further observations, with facility for publication and discussion, and I venture to hope that we shall see many more papers from Prof. Newton's pen on the subject. E. H. PRINGLE

Scientific Club, Savile Row, March 18

The Microtelephone

TOWARDS the end of last year I got constructed a telephonic apparatus which gives results much superior to those of the ordinary Bell telephone. Its construction is based on four principles, two of which have not yet been applied to telephones :—

1. The magneto-electric principles of the Bell telephone.

2. The microphonic principles of Hughes (different quantity of the points of contact).

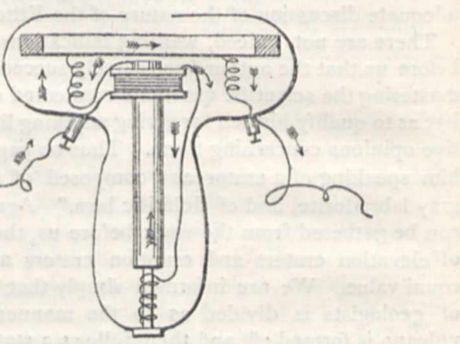
3. The principle discovered by Beatson and De La Rive (1845), and which explains the experiments of Messrs. Blyth and Hughes with the speaking microphone (production of sounds by the passage alone of a discontinuous or undulating electric current).

4. The principle that the intensity of the sound depends on the density of the air in which it is produced.

All the principles are combined in so simple a manner that the microtelephone differs from the Bell telephone only in the three following points :—

1. The electric current engendered by the approach or withdrawal of the iron membrane, traversed not only the bobbin, but also the magnet and the membrane itself.

2. The communication of the current with the vibrating plate of iron is effected by means of two small springs, which are lightly pressed by the membrane, and as this pressure may be more or less strong during the action of the apparatus, the latter acts as a microphone of a relatively weak sensitiveness, but which permits the telephone to be spoken to at a distance of several centimetres, and of hearing the ticking of a watch, or the sounds of a musical box with the aid of a carbon microphone.



3. Three millimetres above the iron membrane is another membrane of caoutchouc (which should not be very fine), and both membranes inclose a layer of air, moderately compressed, which in this way must vibrate, together with the two membranes.

The microtelephone is regulated once for all, and transmits the feeblest word with a truly perfect precision.

JULIAN OCHOVOWICZ

University, Lemberg, Galicia

Vacuum Tube Phenomena

HAS it been observed that the area of the exposed surface of the negative electrode in a highly exhausted vacuum tube exerts an important influence on the facility with which the discharge takes place?

I have recently been observing Crookes's molecular shadows with a tube constructed by Mr. J. Marr, of Liverpool, in which one electrode is a flat disk about 1 inch in diameter, and the other a piece of platinum wire about ¾ inch long. When these electrodes are connected with the terminals of an induction coil capable of giving a 4½-inch spark in air, and the contact-breaker arranged so that the shadows can just be seen when the disk is in connection with that terminal which becomes negative when the current in the primary wire is broken, a reversal of the commutator causes the discharge to cease.

If, now, the coil-power be increased by the proper manipulation of the contact-breaker, a condition of things is reached in which the dark shadows flash out intermittently, even though the disk is connected with what is called the positive terminal of the induction coil.

This is evidently caused by the passage of the inverse induction current ; I mean that current which is produced when the circuit of the primary is completed. It thus appears that a condition of things can be obtained in which the effect of the greater electromotive force produced on the breaking of the primary circuit is counterbalanced by the influence of the relative size of the electrodes.

The above observation appears to be interesting, and as it may possibly be new, I venture to send you an account of it.

Nottingham, March 8 J. J. H. TEALL

Leibnitz's Mathematics

PROF. TAIT has recently given your readers one mathematician's opinion of Leibnitz as a discoverer. The following extract is serviceable in the same direction, while it has the further merit of attesting to the existence of a still later "vestige of presumption" than has yet been referred to. The extract is from a review by M. Bertrand of Dühring's "Kritische Geschichte der allgemeinen Principien der Mechanik" (Berlin, 1873). M. Bertrand says :—

Les sévérités de M. Dühring sont impartiales, et l'un des plus grands génies de l'Allemagne semble précisément le plus maltraité de tous. Les *Actes* de Leipzig, de 1684, donnèrent, est-il dit dans le texte, la première publicité à la théorie des fluxions de Newton, et en note, on ajoute : "Il n'a pas été possible d'opposer à Leibnitz des preuves complètes qui le forçassent à avouer son emprunt ; mais la connaissance de son caractère donne à l'acte qu'on lui reproche une probabilité voisine de la certitude. Une lettre d'Huyghens à L'Hospital est sur ce point fort instructive.

"'M. Leibnitz,' dit Huyghens, 'est assurément très habile, mais il a avec cela une envie immodérée de paroître, comme cela se voit, lorsqu'il parle de son Analyse des infinis . . . , des lois harmoniques des mouvemens planétaires, où il a suivi l'invention de M. Newton, mais en y meslant ses pensées qui la gastent . . . Encore suis-je fort en doute, pour des raisons que je pourrois alléguer, s'il n'a pas tiré sa construction (de la chaînette) de celle de M. Bernoulli.'

"Dans la préface de son 'Calcul différentiel,' Euler n'attribue à Leibnitz que la réduction des principes de Newton en système. Lagrange, qui cherche chez Fermat l'origine du Calcul différentiel, ne manque pas, dans ses 'Leçons sur le Calcul des Fonctions,' de signaler les concordances de l'écrit de Leibnitz de 1684, avec la théorie antérieure de Fermat. Gauss pensait, comme on le voit dans l'écrit de Sartorius de Waltershausen, que Leibnitz, même de loin, ne doit pas être comparé à Newton."

It is satisfactory to quote Herr Dühring through his reviewer, because the introductory sentence of the extract makes evident to those not already aware of M. Bertrand's historical leanings that it is on Dr. Ingleby's side the eminent French mathematician would give his voice. THOMAS MUIR

High School of Glasgow, March 17

Blue Flame from Common Salt

SOME time ago the question was raised in NATURE (vol. xiii. p. 287) concerning the origin of the blue flame produced when common salt is thrown into a hot fire.

Among the suggestions that were advanced, no one offered the only explanation that is at all feasible, viz., that it is due simply to hydrochloric acid.

The blue flame is not produced by sodium chloride only, but by other chlorides as well. Those I have tried are $BaCl_2$, $SrCl_2$, KCl, AmCl, Hg_2Cl_2, and HCl, the last both in solution and as gas.

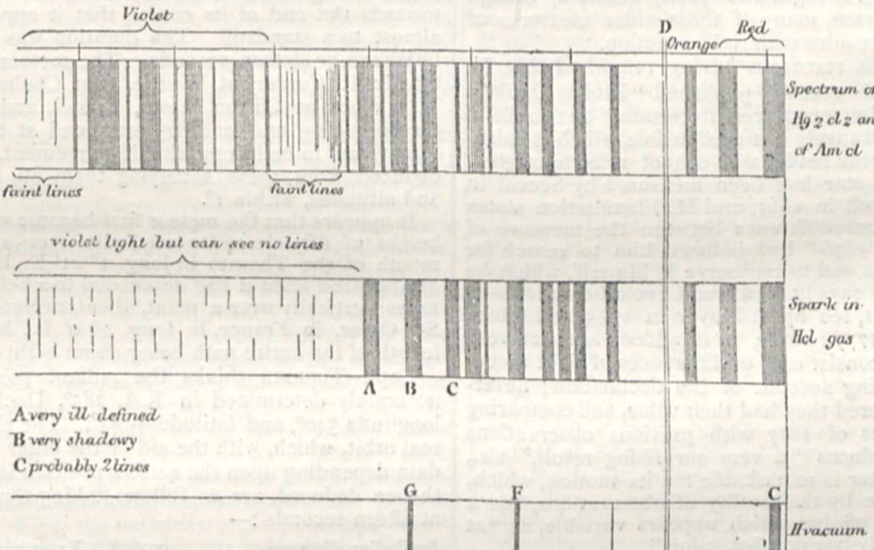
It would be waste of time and space to enumerate all the experiments I have made on this subject ; many of them were for the purpose of proving that neither carbon nor sulphur had any share in the reaction.

One of my methods of obtaining the flame was to burn pure hydrogen from a glass jet, and allow a mingled stream of HCl and NH_3 from two other jets to pass into it. The best source, however, is calomel, heated on wire gauze by a Bunsen burner ; the next best, AmCl.

The spectrum of the "chloride" flame is characterised by a series of double bands in the green, blue, and violet, the least refrangible of each pair being the broadest. The four pairs in the violet are especially prominent. There are two red bands or lines, and one orange. The least refrangible red line occupies the place of the hydrogen line C.

A spark between carbon points in a bottle of HCl gas yields a spectrum similar in appearance to that obtained from a chloride, but I was unable to see any violet bands, only a faint continuous spectrum.

I was able to ascertain that the red lines coincided exactly,



but I cannot affirm with the same positiveness that all the green lines and bands coincide. Some undoubtedly do, but the spectrum of the HCl was so faint, and the spectrum of the chloride so transient, that measurements were very difficult, and I do not pretend to any high degree of accuracy in my delineations of the lines, besides the different conditions under which the two were compared might account for considerable variations. For the HCl I used a coil (capable of giving a 2" spark) with Leyden jar, worked by six Smees. The carbon points were $\frac{1}{8}$ inch apart. The spark was tried both focussed and unfocussed on the slit of a 2-prism spectroscope. When comparing the two spectra side by side it was difficult in some cases to be sure of coincidence, because the flame from the Hg_2Cl_2 would flash out brilliantly for a moment and quite overpower the more feeble lines of the HCl ; it would then disappear entirely, and more chloride would have to be placed on the gauze.

I have no doubt in my own mind that HCl is the cause of the blue flame. I have proved that Cl is a necessary constituent, and I have not been able to get it in the absence of hydrogen (the spectrum of pure Cl is very different), and besides the red H-line is present in both cases, and probably the other two as well. I do not think that the presence of aqueous vapour is sufficient to account for the red line.

I subjoin the spectra *as I mapped them*, but it must be borne in mind that I do not vouch that they are free from error. I intend to photograph them when I have sufficient leisure, and I hope the results will be more definite. I may be able to find violet lines in the spectrum of HCl.

A. PERCY SMITH

Temple Observatory, Rugby, March 15

Unscientific Art (?)

IN NATURE, vol. xix. p. 460, Mr. Buck complains of the drawing in the *Graphic* for December 28, wherein the observer is represented as "sloping the barometer at an angle of about 30° from the vertical," in order to take a reading on a marine barometer by means of the lantern for better illumination. May not the artist be correct, and Mr. Buck have discovered a mare's nest? The barometer may be placed entirely horizontal for reading the scale, *after* the vernier has once been set when the instrument was vertical.

CHAS. COPPOCK

Grosvenor Road, Highbury New Park, N., March 21

OUR ASTRONOMICAL COLUMN

THE DISTANT HERSCHELIAN COMPANION OF γ LEONIS.—In 1861 Prof. Winnecke, writing from Pulkowa, drew attention to a star of the ninth magnitude near the double-star γ Leonis, which M. Otto Struve had found to have an annual proper motion exceeding 0″·5. The star was observed with the Dorpat transit-instrument, on April 12, 1820, and once by Bessel in zone 502, on April 12, 1831, and from these observations compared with two at Pulkowa in April, 1861, and with micrometrical measures from γ Leonis by M. Otto Struve, Prof. Winnecke concluded that the proper motion of the small star with respect to the neighbouring binary, was very nearly 0″·85 in R.A. and 0″·10 in declination, annually. Sir W. Herschel observed a distant companion of γ Leonis, the mean of two angles giving 297°·5 for about 1782·9, with a distance of 111″·4, which he thought was "pretty accurate," though as we are now aware, many of these wider measures of Sir W. Herschel require material correction.

We refer to this star from having remarked that M. Flammarion, in his recently published "Étoiles Doubles et Multiples en Mouvement relatif certain," has made it the subject of a strangely confused article, which is calculated to mislead the reader who cannot refer to original authorities. The star had been measured by Secchi in 1856, and by Powell in 1861, and M. Flammarion states that "the enormous difference between the measure of 1782 and that of 1856" had induced him to search for other observations and to reobserve it himself, which he did, in 1877. He says he had found five observations by Flamsteed in 1691, ten by T. Mayer in 1755, and fifteen by C. Mayer in 1777; these, it is added, are not very precise, for they consist only of differences of right ascension, without taking account of the declination; nevertheless he considered they had their value, and comparing his own measures of 1877 with previous observations separately, he deduces "a very surprising result," viz., that the distant star is remarkable for its motion, which, if one may judge by the totality of observations, has a mean value of 1″·08, but which appears variable, as "at present it certainly has not that value."

The main cause of M. Flammarion's difficulty is his having confounded two quite distinct objects: we have not referred to the work of C. Mayer, but the star observed by Flamsteed, which he more than once calls "Comes γ," and that observed by Tobias Mayer, is really the bright neighbour of γ, or 40 Leonis; Flamsteed did observe the declination, as will be seen in his column "Distantiæ a vertice correctæ;" and Mayer also noted the declination on one occasion, though generally recording only the right ascension. M. Flammarion says he found five observations of Flamsteed in 1691, which is a greater number than we recognise in the "Historia Cœlestis," but there are observations in 1690 and 1692. The zenith distances of γ Leonis and *Comes* on April 6, 1691, and the names of the stars on January 23, 1692, are interchanged in the "Historia Cœlestis." Tobias Mayer's observations do not apply to the year 1755, when his observatory at Göttingen was not yet erected, but to 1756 and 1757, chiefly the former year. Bessel's observation applies to 1831, not 1825, as M. Flammarion assumes.

The star in question is No. 90, in Argelander's valuable treatise, "Untersuchungen über die Eigenbewegungen von 250 Sternen, &c.," where he deduces for the annual proper motion in arc of great circle, 0″·512 in the direction 270°, or the proper motion is entirely in R.A. He observed the star upon the meridian at Bonn, once in 1857 and four times in 1862-63. It was also meridionally observed at Greenwich in 1862. It is No. 234, Hour X., in Weisse's Bessel. Thus we have three stars situate within half a degree, with large proper motions, very divergent, however, in direction:—

	Secular P.M.	Direction.	Authority.
	″	°	
40 Leonis	32·2 ...	229·0 ...	Mädler.
W. B. X. 234	51·2 ...	270·0 ...	Argelander.
γ Leonis	32·3 ...	118·6 ...	Mädler.

A METEOR WITH SHORT PERIOD OF REVOLUTION.—In the very interesting report of the "Luminous Meteor Committee" of the British Association for 1877-78, we find a note by Capt. G. L. Tupman, referring to a fire-ball seen on November 27, 1877, which he considers to have been moving in a nearly circular orbit, with short periodic time. Capt. Tupman observed this meteor from a position about half a mile east of the Royal Observatory, Greenwich; it began as a first or second magnitude star, but suddenly increased in brilliancy and size to a fine bluish white fire-ball ten or twelve minutes in diameter, emitting a train, coloured blue, red, and green, many degrees long. It moved very slowly, so slowly, indeed, towards the end of its course, that it appeared to come almost to a standstill. The duration was considered to be fifteen or sixteen seconds. The meteor was observed by Mr. H. Corder, at Writtle, near Chelmsford, and by Mrs. Ware, at Clifton Down, Bristol, and the positions for beginning and ending, estimated at these stations, were found to be in remarkable agreement, the true path deduced from these satisfying them all, both azimuths and altitudes, within 1°.

It appears that the meteor first became visible at a real height of fifty-six miles vertically over a point off the mouth of the Thames in long. 1° 21′ E., lat. 51° 33′, and disappeared when it had descended to a height of thirteen miles vertically over a point, about twelve miles west of St. Omer, in France, in long. 2° 0′ E., lat. 50° 45′, the length of the entire path being about eighty miles.

Capt. Tupman thinks the radiant point was pretty accurately determined in R.A. 285°, Decl. + 64°, or in longitude 340°, and latitude + 83°. The elements of the real orbit, which, with the aid of the other corresponding data depending upon the earth's position in her orbit, are thence deduced, are as follows, taking the real duration at fifteen seconds:—

Perihelion distance	0·9858	Excentricity ...	0·1568
Longitude of perihelion ...	70° 6′	Inclination ...	15° 0′
,, ,, ascending node	245° 50′	Anomaly ...	− 4° 16′
Semi-axis major	1·1691	Periodic time ...	462 days

Motion—direct.

The precise Greenwich time of the occurrence of the meteor was 10h. 26m.

If the duration of visibility is diminished to 7½ seconds the elements are still very similar to the above; the semi-axis major becomes 1·3785 and the period 591 days. Capt. Tupman remarking that such favourable conditions for inferring the orbit of a meteor may rarely happen, adds, it is sufficient for the establishment of a short periodic time (such as 500 days) that "the meteor moved slowly from a fairly well-determined radiant distant about 90° from the point of the heavens towards which the earth's motion was directed."

We may mention that there is one singular circumstance not alluded to in Capt. Tupman's note: the elements defining the position of the orbit of the meteor have a striking general resemblance to those of the orbit of Biela's comet, in the descending node of which body the earth was precisely situated at the time.

FOSSIL CALCAREOUS ALGÆ

THE very important memoir of M. Munier-Chalmas, "Sur les Algues calcaires appartenant au groupe des Dasycladées Harv. et confondues avec les Foraminifères," which was published in the *Comptes Rendus Hebdomadaires* of the French Academy of Science for October 29, 1877, opens up quite a new or almost a new field of research, which has been followed up by the same author in a note presented last month to the Geological Society of France, "On the genus Ovulites." Though regarded by some of the most eminent palæontologists as a monothalmic foraminifer related to Lagena, the genus Ovulites is herein clearly demonstrated to be neither more nor less than an articulation of a siphonaceous alga having very close affinities to Penicellus.

Ovulites margaritula is described by Messrs. Parker and Jones "as a common Foraminifer of the 'Calcaire grossier.' Shaped like an egg, and when full grown about the size of a mustard-seed, it is one of the most elegant of the fossil forms. The large terminal apertures, moreover, curiously impress upon the mind its resemblance to a 'blown' bird's egg. [Written in 1860; nowadays bird's eggs are not thus blown.] It is the largest of the monothalamous Foraminifera. As a species it appears to have been short-lived. Fully developed in the deposits of Hauteville and Grignon it breaks in at once in the Eocene period. It lingers as an attenuated form in the Miocene beds of San Domingo. A recent Ovulite has not been met with. Scarcely another Foraminifer presents us with a similarly brief history—an undescribed form allied to Dactylopora affording almost the only parallel (namely, *Acicularia pavantina*, d'Arch.)."

In passing it may be noted that without doubt this last-mentioned form is also only a portion of a calcareous alga.

The earlier memoir, of which the *Comptes Rendus* publishes only an abstract, reminds us that it is not so very long ago (1842) since Prof. Decaisne demonstrated that a number of marine forms known as zoophytes, Corallina, Cymopolia, Neomeris, Penicellus, Udotea, Halimeda, &c., were in reality veritable algæ. But we may remark that Prof. Schweigger, of Königsberg, had, from actual observation of living specimens of several species of these calcareous algæ at Villefranche, come to the same conclusion in 1818 ("Beobachtungen auf naturhistorischen Reisen. Anat.-phys. Untersuchungen über Corallen," Berlin, 1818). To go back to the pre-Linnean times, Ray (1690) described Corallina as "plantæ genus in aquis nascens," and Spallanzani, Carolini, and Olivi even maintained the same against the peculiar reasonings of Ellis, the authority of Linneus, and despite the conversion of Pallas; but so influenced by authority were, apparently, the botanists down to 1842, that a Professor of Botany in the Edinburgh University (Graham) once politely requested the zoologists to keep *their* cryptogamia to themselves, and a Professor of Botany in the Dublin University (Harvey), in the first edition of his "Manual of British Algæ" (1841), did not include any of the Corallines. Since the memoirs of Decaisne and Chauvin, all this has changed, and we imagine that there is now no difference of opinion existing among botanists as to the general affinities of the living forms of calcareous algæ.

M. Munier-Chalmas in his memoir demonstrates that there must be also added to this group a numerous series of fossil forms which the old authors placed among the polyps, and which most of the modern writers on the subject have ranked among the foraminifera. Bosc in 1806 described and figured (*Journal de Physique*, Juin 1806) some fossil organised bodies under the name of *Rétéporites ovoides*, for which bodies Lamarck in 1816 established the genus Dactylopora. "The most singular varieties of opinion have existed," writes Dr. Carpenter in his well-known "Introduction to the Study of the Foraminifera," "as to the true character of these fossil organisms. In separating them generally from Retepora Lamarck still associated them in the same group of supposed zoophytes; this position was also accepted for the genus by De Blainville and Defranc." [It is but justice to De Blainville to point out that he quotes without disapproval the statement of Schweigger, "que les dactylopores et les ovulites ne sont rien autre chose que des articulations d'une grande espèce de cellaire, analogue à la cellaire salicorne"]. "In 1852 Dactylopora was included among the Foraminifera by D'Orbigny, who showed, notwithstanding, by the place he assigned to it, a misapprehension of the real nature scarcely less complete than that under which his predecessors had lain; for he ranks it in his order Monostègues, next to the unilocular Ovulites, and says of it: 'C'est une Ovulite également percée des deux bouts, pourvue des larges pores placés par lignes transverses.' How utterly erroneous is this description will appear from the details to be presently given, yet D'Orbigny's authority has given it currency enough to cause it to be accepted by such intelligent palæontologists as Pictet and Bronn, who in the latest editions of their respective treatises have transferred Dactylopora to the place indicated by him, not, however, without the expression of a doubt on the part of Bronn as to whether the true place of the genus is not among the Fistulidæ in alliance with Synapta and Holothuria—a suggestion that indicates a perversion of ideas on the subject for which it is not easy to account. The complex structure of the organism in question was first described and the interpretation of that structure on the basis of an extended comparison with simpler forms was first given, by Messrs. Parker and Jones in so unobtrusive a manner as scarcely to challenge the attention which their investigations deserve, and I gladly avail myself of the opportunity which the present publication affords to give a fuller account, with the requisite illustrations of this remarkable type, the elucidation of which seems to me not unlikely to lead to a reconsideration of the place assigned to many other organisms at present ranked among Zoophytes or Polyzoa;" and then follow nine pages of a most elaborate description of every ridge and furrow, of every elevation and depression to be met with in any of the so-called species, so that probably no single vegetable cell was ever before so minutely described.

The genus is placed the eleventh in order of the family Miliolida, a family which contains some of the most typical of Foraminifers. "It may be conjectured without much improbability," writes Dr. Carpenter, "that Dactylopora is only the single representative of a group whose various forms filled up the hiatus which at present intervenes between itself and its nearest allies among the ordinary Foraminifera." But, writes M. Munier-Chalmas, "the study and comparison of species of Dasycladus, Cymopolia, Acetabularia, Neomeris, &c., in the herbarium of the museum, and in that of M. Ed. Bornet, who placed without reserve at my disposal his library and collections of these plants, proved to me that the species of Dactylopora, Acicularia, Polytrypa, &c., are decidedly Algæ, very nearly allied to species of the recent genera just quoted, if not identical therewith. The accompanying figures show plainly, for example, that the genera Cymopolia and Polytrypa may be united; for the typical species thereof offer in every respect the same generic characters, and there is even a difficulty to find for them sufficiently distinct specific characters. Under the denomination of '*Siphoneæ verticillatæ*' I unite (1) Those green-spore bearing algæ arranged by Harvey in the family of the Dasycladeæ; (2) All those fossil gener related to Larvaria, Clypeina, Polytrypa, Acicularia, Dactylopora, and Uteria. This group at present contains over fifty genera, which are for the most part to be met with in the triassic, jurassic, cretaceous, and tertiary strata. In the number of those actually living there is a notable falling off, there being not more than the seven following genera :—Dasycladus, Halicoryne, Cymopolia

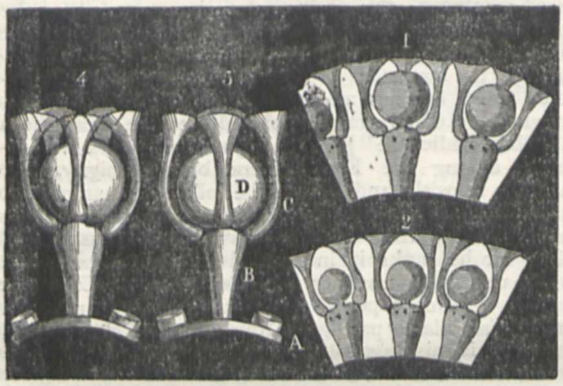


FIG. 1.—Transverse section of a morsel of the calcareous tube of *Cymopolia rosarium*, Lamr., showing the canals which receive the whorl of cellules and the central sporangial cavity. FIG. 2.—Transverse section of *Polytrypa elongata*, Defrance, showing the same portions. FIG. 3.—Part of a whorl of cellules of *Cymopolia rosarium*, separated from the calcareous tube by acid. A, wall of central cellule; B, first row of cellules; C, terminal whorl of cellules, in the centre of which is D, the axillary sporangium. FIG. 4.—Exactly the same parts in *Polytrypa elongata*, obtained from a mould.

(with two sub-genera, Polytrypa and Decaisnella,[1] g.n.), Polyphysa, Acetabularia, Neomeris, and Bornetella,[2] g.n." [Doubtless a few more genera of recent forms yet remain to be described. Thus Chloroclados, of Sonder, appears to be a good and distinct genus allied to Dasycladus.]

"The frond in the *Siphoneæ verticillatæ* is simple or dichotomous ; it consists of a central tubular unicellular axis, around which are arranged the radiary and verticellate ramuli, the exact arrangement of which varies according to the genera and to the species. In most of the species carbonate of lime is found deposited in abundance in the outer walls of the main axis and its ramuli, and this forms around the plant a calcareous envelope, in which is reproduced all the details of its organisation. This mineral coating may consist of one or of two calcareous cylinders. The inner cylinder will be formed by the central axis, and the first row of cells which arise therefrom. The outer cylinder is laid down by the most external of the verticels of cells ; these terminate by a splayed-out enlargement, the lateral edges of which become more or less consolidated with the similar enlargements of neighbouring cells, and by thus causing a reciprocal pressure, very regular hexagonal surface markings are produced. The organs of fructification are themselves surrounded by calcareous material, and assist in the formation of the outer cylinder, a fact easily seen in any section of Cymopolia."

The result of such an organisation is that when the organic vegetable matter becomes destroyed there still remains in those fossil species, which laid down a great deal of calcareous material, as well as in those living species—which lay down more or less of it—a skeleton permeated by canals (rays of the ramuli) and chambers (fructification). This arrangement, which permits of an exact classification of the fossil species, being wrongly interpreted, led even some most distinguished authors to see in these morsels of plants the full organisation of a Foraminifer."

Space will not permit of the table of the twenty-two genera and seven families as detailed in the *Comptes Rendus* being here given, but every botanist will look forward with interest to the promised future detailed contributions of the author on this subject. Here it seems desirable to add that his conclusions are in every particular acquiesced in by one in every way thoroughly able to judge of the facts, Dr. Ed. Bornet, and having written this I feel it almost superfluous to say that on a careful study myself of specimens prepared by M. Munier-Chalmas—for which I take this opportunity of thanking him—I cannot conceive his demonstration as admitting of a doubt.

ED. PERCEVAL WRIGHT

[1] Type, *Dactylopora eruca*, Parker.
[2] Type, *Neomeris nitida*, Harv. MS.

ELECTRICITY AND WATER DROPS[1]

IT has been known for many years that electricity has an extraordinary influence upon the behaviour of fine jets of water ascending in a nearly vertical direction. In its normal state a jet resolves itself into drops, which even before passing the summit, and still more after passing it, are scattered through a considerable width. When a feebly electrified body is brought into its neighbourhood, the jet undergoes a remarkable transformation, and appears to become coherent ; but under more powerful electrical action the scattering becomes even greater than at first. The second effect is readily attributed to the mutual repulsion of the electrified drops, but the action of feeble electricity in producing apparent coherence has been a mystery hitherto.

It has been shown by Beetz that the coherence is apparent only, and that the place where the jet breaks into drops is not perceptibly shifted by the electricity. By screening various parts with metallic plates, Beetz further proved that, contrary to the opinion of earlier observers, the seat of sensitiveness is not at the root of the jet where it leaves the orifice, but at the place of resolution into drops. As in Sir W. Thomson's water-dropping apparatus for atmospheric electricity, the drops carry away with them an electric charge, which may be collected by receiving the water in an insulated vessel.

I have lately succeeded in proving that the normal scattering of a nearly vertical jet is due to the *rebound* of the drops when they come into collision with one another. Such collisions are inevitable in consequence of the different velocities acquired by the drops under the action of the capillary force, as they break away irregularly from the continuous portion of the jet. Even when the resolution is regularised by the action of external vibrations of suitable frequency, as in the beautiful experiments of Savart and Plateau, the drops must still come into contact before they reach the summit of their parabolic path. In the case of a continuous jet the "equation of continuity" shows that as the jet loses velocity in ascending, it must increase in section. When the stream consists of drops following the same path in single file, no such increase of section is possible, and then the constancy of the total stream requires a gradual approximation of the drops, which in the case of a nearly vertical direction of motion cannot stop short of actual contact. Regular vibration has, however, the effect of postponing the collisions and consequent scattering of the drops, and in the case of a direction of motion less nearly vertical may prevent them altogether.

Under moderate electrical influence there is no material change in the resolution into drops, nor in the subsequent motion of the drops up to the moment of collision. The difference begins here. Instead of rebounding after collision, as the unelectrified drops of clean water generally or always do, the electrified drops *coalesce*, and thus the jet is no longer scattered about. When the electrical influence is more powerful, the repulsion between the drops is sufficient to prevent actual contact, and then of course there is no opportunity for amalgamation.

[1] "The Influence of Electricity on Colliding Water Drops." Paper read at the Royal Society by Lord Rayleigh, F.R.S.

These experiments may be repeated with extreme ease and with hardly any apparatus. The diameter of the jet may be about $\frac{1}{20}$ inch, and may be obtained either from a hole in a thin plate or from a drawn-out glass tube. I have generally employed a piece of glass tube fitted at the end with a perforated tin plate, and connected with a tap by india-rubber tubing. The pressure may be such as to cause the jet to rise eighteen or twenty-four inches, or even more. A single passage of a rod of gutta-percha, or of sealing-wax, along the sleeve of the coat is sufficient to produce the effect. The seat of sensitiveness may be investigated by exciting the extreme tip only of a glass rod, which is then held in succession to the root of the jet and to the place of resolution into drops. An effect is observed in the latter but not in the former position. Care must be taken to use an electrification so feeble as to require close proximity for its operation, otherwise the discrimination of the positions will not be distinct.

The behaviour of the colliding drops becomes apparent under instantaneous illumination. I have employed sparks from an inductorium, whose secondary terminals were connected with the coatings of a Leyden jar. The jet should be situated between the sparks and the eye, and the observation is facilitated by a piece of ground glass held a little beyond the jet, so as to diffuse the light; or the *shadow* of the jet may be received on the ground glass, which is then held as close as possible on the side towards the observer.

If the jet be supplied from an insulated vessel, the coalescence of colliding drops continues for a time after the removal of the influencing body. This is a consequence of the electrification of the vessel. If the electrified body be held for a time pretty close to the jet, and be then gradually withdrawn, a point may be found where the rebound of colliding drops is re-established. A small motion *to* or *from* the jet, or a discharge of the vessel by contact of the finger, again induces coalescence.

Although in these experiments the charges on the colliding drops are undoubtedly of the same name, it appeared to me very improbable that the result of contact of two equal drops, situated in the open, could be affected by any strictly equal electrifications. At the same time an opposite opinion makes the phenomena turn upon the very small *differences* of electrification due either to irregularities in the drops or to differences of situation, and is at first difficult of acceptance in view of the efficiency of such very feeble electric forces. Fortunately I am able to bring forward additional evidence bearing upon this point.

When two horizontal jets issue from neighbouring holes in a thin plate, they come into collision, for a reason that I need not now stop to explain, and after contact they frequently rebound from one another without amalgamation. This observation, which I suppose must have been made before, allowed me to investigate the effect of a passage of electricity across two contiguous water surfaces. The jets that I employed were of about $\frac{1}{15}$ inch in diameter, and issued under a moderate pressure (5 or 6 inches) from a large stoneware vessel. Below the place of rebound, but above that of resolution into drops, was placed a piece of insulated tin plate in connection with a length of gutta-percha-covered wire. The source of electricity was a very feebly excited electrophorus, whose cover was brought into contact with the free end of the insulated wire. When both jets played upon the tin plate the contact of the electrified cover had no effect in determining the union, but when only one jet washed the plate union instantly followed the communication of electricity, and this notwithstanding that the jets were already in communication through the vessel. The quantity of electricity required is so small that the cover would act three or even four times without being re-charged, although no precautions were taken to insulate the reservoir.

In subsequent experiments the colliding jets, about $\frac{8}{100}$ inch in diameter, issued horizontally from similar glass nozzles, formed by drawing out a piece of glass tubing and dividing it with a file at the narrowest part. One jet was supplied from the tap, and the other from the stoneware bottle placed upon an insulating stool. The sensitiveness to electricity was extraordinary. A piece of rubbed gutta-percha brought near the insulated bottle at once determined the coalescence of the jets. The influencing body being held still, it was possible to cause the jets again to rebound from one another, and then a small motion of the influencing body *to* or *from* the bottle again induced coalescence, but a *lateral* motion without effect. If an insulated wire be in connection with the contents of the bottle, similar effects are produced when the electrified body is moved in the neighbourhood of the free end of the wire. With care it is possible to bring the electrified body into the neighbourhood of the free end of the wire so *slowly* that no effect is produced; a sudden movement of withdrawal will then usually determine the coalescence.

Hitherto statical electricity has been spoken of; but the electromotive force of even a single Grove cell is sufficient to produce these phenomena, though not with the same certainty. For this purpose one pole is connected through a contact key with the interior of the stoneware bottle, the other pole being to earth. If the fingers be slightly moistened, the body may be thrown into the circuit, apparently without diminution of effect. This perhaps ought not to surprise us, as in any case the electricity has to traverse several inches of a fine column of water. On the other hand, it appeared that most of the electromotive force of the Grove cell was necessary.

Further experiment showed that even the discharge of a condenser charged by a single Grove cell was sufficient to determine coalescence. Two condensers were used successively; one belonging to an inductorium by Ladd, the other made by Elliott Brothers, and marked "Capacity $\frac{1}{2}$ Farad." Sometimes even the "residual charge" sufficed.

It must be understood that coalescence of the jets would sometimes occur in a capricious manner, without the action of electricity or other apparent cause. I have reason to believe that some, at any rate, of these irregularities depended upon a want of cleanness in the water. The addition to the water of a very small quantity of soap makes the rebound of the jets impossible.

The last observation led me to examine the behaviour of a fine vertical jet of slightly soapy water: and I found, as I had expected, that *no scattering took place*. Under these circumstances the approach of a moderately electrified body is without effect, but a more powerful influence scatters the drop as usual. The apparent coherence of a jet of water when the orifice is oiled was observed by Fuchs, and appears to have been always attributed to a diminution of adhesion between the jet and the walls of the orifice.

Some further details on this subject, and other investigations respecting the phenomena of jets, are reserved for another communication, which I hope soon to be able to present to the Royal Society; but I cannot close without indicating the probable application to meteorology of the facts already mentioned. It is obvious that the formation of rain must depend very materially upon the consequences of encounters between cloud particles. If encounters do not lead to contacts, or if contacts result in rebounds, the particles remain of the same size as before; but, if the issue be coalescence, the bigger drops must rapidly increase in size and be precipitated as rain. Now, from what has appeared above, we have every reason to suppose that the results of an encounter will be different according to the electrical condition of the particles, and we may thus anticipate an explanation of the remarkable but hitherto mysterious connection between rain and electrical manifestations.

A STUDY IN LOCOMOTION[1]

III.

IT would be very interesting to pass in review the principal epochs of art and trace out the manner of representing horses in motion in various periods of progress or decadence. But such a study would fail to realise its full value unless carried out by an artist.

Col. Duhousset, who joins the skill of the draughtsman to a perfect acquaintance with the exterior of the horse, has essayed a work of this kind, and in a recent publication has pointed out the merits and defects of certain modern artistic performances. M. Duhousset has also gathered together a curious collection of representations of the horse at different epochs of art, and has entrusted to me a few specimens, which I will now exhibit to you. You will see that in a general manner art has progressed, proceeding from simple forms to more masterly delineations.

FIG. 20.—Assyrian bas-relief (British Museum). A horse ambling.

Figs. 20 and 21 represent horses ambling. Has the artist selected this pace because it was in general use at that time? This is scarcely probable; it would seem more likely that he has chosen it because of its extreme simplicity. To seize the moment when the four feet all touch the ground, to repeat in the posterior limbs the attitude of the limbs in front, and, lastly, to represent all the horses harnessed together as keeping exact time in their movements so as to draw them all with a single profile: this is certainly one way of eluding all the difficulties of the situation.

FIG. 21.—Egyptian bas-relief (Medinet-Abou). Two horses attached to a chariot, and ambling.

FIG. 22.—"The Cavalier and Death," by Albrecht Dürer. Horse at the trot.

FIG. 23.—Statue of Henri IV. on the Pont Neuf. Example of the correct trot.

The pace of the *trot*, correctly represented in the Roman epoch, in the equestrian statues of the Balbi

FIG. 24.—Assyrian bas-relief. Foot-pace.

[1] "Moteurs animés; Expériences de Physiologie graphique." Lecture by Prof. Marey at the Paris meeting of the French Association, August 29, 1878. Continued from p. 467.

which adorn the Naples Museum, reappears in the six-

FIG. 25.—Bas-relief in baked earth, of the Volscian epoch (Velletri). Three horses harnessed together, walking at a foot-pace.

teenth century in a painting by Albrecht Dürer (Fig. 22,

FIG. 26.—Captain of the Guards riding at a foot-pace. (Column of Trajan.)

"The Cavalier and Death"). The classic statue of

FIG. 27.—Mule laden with baggage, walking at a foot-pace. (Column of Trajan.)

Henri IV. on the Pont Neuf is an example of the correct trot (Fig. 23).

But the representation of the *foot-pace*, more difficult than the preceding, is rarely faithful. Examples, scarcely satisfactory, are found in all the epochs; take, for instances, Figs. 24 and 25.

The foot-pace is correctly represented in the two figures borrowed from the column of Trajan (Figs. 26 and 27). This column also displays oxen and other animals faithfully represented.

These paces, it should be noted, are a little varied with respect to the instant chosen; almost invariably the horse raises only one fore-foot.

The *gallop* is in general the pace of which the representation leaves most to be desired. Without speaking of contemporary art, I will refer only to the paintings of the two or three last centuries. The horses therein deemed to be galloping are represented in a sort of prancing attitude, posed upon the two hind-feet, and raising the two fore-feet to an equal height. We have seen, by the preceding notations, that this synchronous action of the right and left limbs does not exist.

In the grandest epoch of Greek art we find admirable representations of the gallop. Fig. 28 is an example. The attitude chosen is the first step of the gallop as in Fig. 16. The first step has been taken. The diagonal

FIG. 28.—Frieze of the Parthenon. (Bas-relief remaining at Athens.) Right hand gallop.

limbs which make the second step are approaching towards the ground, and the right hand fore-foot which will make the last step is held high in air.

I have already admired the reproduction in plaster of another bas-relief from the same frieze, in which a galloping horse is represented with equal correctness, and I was led to believe that in the age of Phidias, artists were in possession of the science of paces. But subsequently, in examining the reproductions of the entire frieze, I have become convinced that the results were obtained by a happy chance, for the greater part of the horses are represented in false attitudes, which is all the more to be regretted in contemplating the exquisite elegance of their forms.

It is incontestable that at the present day, artists make great efforts to represent the horse with truthfulness, and many among them succeed. But I will not permit myself to criticise the works of my contemporaries. Such, then, is the graphic method, and such are its numerous applications, extremely varied, and often of enormous importance. In this discourse, the length of which you will excuse, I have only shown you a little corner of the subject, but that will suffice, I hope, to give you a desire to study more deeply, and in its entirety, a method which appears to me to be full of promise, and to the development of which I have already consecrated much effort.

GEOGRAPHICAL EVOLUTION[1]

In the future development of scientific geography one of the main lines of advance will be in the direction of a closer alliance with geology. The descriptions of the various countries of the globe will include an account of how their present outlines came into existence, and how their plants and animals have been introduced and distributed. The principles on which this evolutional geography will be founded have regard to the materials of which the framework of the land consists, to the various ways in which these materials have been built up into the solid crust of the earth, and to the superficial changes to which they have been subsequently exposed. The materials of the land consist mainly of compacted detritus, which, worn from previously existing terrestrial surfaces, has been laid down in the sea. Hence the land, as we now see it, has originated under the sea. But the common belief that over the whole globe land and sea have been continually changing places, and that wide continents may have bloomed even over the site of the most lonely abysses of the ocean, may be shown to be incorrect by a consideration of the character of the sedimentary rocks of the land on the one hand, and of that of the deposits of the sea-floor on the other. The sedimentary rocks, even in the most massive palæozoic formations where they attain depths of several miles, are shallow-water deposits, formed out of the waste of the land and always laid down near land. Nowhere among them, even including the thick organically-derived limestones, such as the chalk, is there any formation which properly deserves to be considered that of a deep sea. Recent researches into the nature of the sea-bottom across the great ocean-basins have likewise shown that the deposits there in progress have no real analogy among the rocks of the land. The conclusion to be drawn from the evidence is that the great ocean-basins have always existed, and that the terrestrial areas have also lain on the whole over those tracts where they still exist.

The way in which the sedimentary rocks have been tilted up and made to lie discordantly on each other shows that the marginal belt of sea-floor near the land has again and again been upraised and worn down. The ocean-basins appear from very early times to have been areas of subsidence, while the continental elevations have been lines of relief from the strain of terrestrial contraction. The land has been subjected to periodic movements of upheaval, sometimes of great violence, whereby not only large areas of sea-bottom were raised into land, but where, as huge earth-waves, lines of mountain-chain were ridged up. During these movements great changes were effected in the structure and arrangement of the rocks in the regions affected, original sedimentary masses being rendered crystalline, and even reduced to such a pasty or fluid condition as to be squeezed into rents of the more solid superincumbent rocks. Volcanic orifices were likewise opened, by which communication was established between the heated interior and the surface. The relative dates of these successive terrestrial disturbances can be satisfactorily determined by stratigraphical and palæontological evidence.

The history of the gradual growth of the European continent furnishes many interesting and instructive illustrations of the principles by which evolutional geography is to be worked out. The earliest European land appears to have existed in the north and north-west, comprising Scandinavia, Finland, and the north-west of the British area, and to have extended thence through boreal and arctic latitudes into North America. Of the height and mass of this primeval land some idea may be formed by considering the enormous bulk of the material derived from its degradation. In the Silurian formations of the British Islands alone there is a mass of rock, worn from that land, which would form a mountain-chain extending from Marseilles to the North Cape (1,800 miles), with a mean breadth of over 33 miles and an average height of 16,000 feet, or higher than Mont Blanc. The Silurian sea which spread across most of Central Europe into Asia suffered great disturbance in some regions towards the close of the Silurian period. It was ridged up into land inclosing vast inland basins, the areas of some of which are still traceable across the British Islands to Scandinavia and the west of Russia. An interesting series of geographical changes can be traced during which the lakes of the Old Red Sandstone were effaced, the sea that gradually overspread most of Europe was finally silted up, and the lagoons and marshes came to be densely crowded with the vegetation to which we owe our coal-seams. Later terrestrial movements led to the formation of a series of bitter lakes across the heart of Europe like those now existing in the south-east of Russia. Successive depressions and elevations brought the open sea again and again across the continent, and gave rise to the accumulation of the rocks of which most of the present surface consists. In these movements the growth of the Alps and other dominant lines of elevation can be more or less distinctly traced. It was at the close of the Eocene period, however, that the great disturbances took place to which the European mountains chiefly owe their present dimensions. In the Alps we see how these movements led to the crumpling up and inversion of vast piles of solid rock, not older in geological position than the soft clay which underlies London. Considerable additional upheaval in Miocene times affected the Alpine ridges, while in still later ages the Italian peninsula was broadened by the uprise of its sub-Apennine ranges. The proofs of successive periods of volcanic activity during this long series of geographical revolutions are many and varied. So too is the evidence for the appearance and disappearance of successive floras and faunas, each no doubt seeming at the time of its existence to possess the same aspect of antiquity and prospect of endurance which we naturally associate with those of our own time. The law of progress has been dominant among plants and animals and not less upon the surface of the planet which they inhabit. It is the province of the biologist to trace the one series of changes; of the geologist to investigate the other. The geographer gathers from both the data which enable him to connect the present aspects of Nature with those out of which they have arisen.

[1] Abstract of an Address given by Prof. Geikie, F.R.S., at the meeting of the Royal Geographical Society on March 24, 1879.

GEOGRAPHICAL NOTES

At a recent meeting of the Board intrusted by the French Government with the care of granting missions for exploring foreign countries, it was decided that none of the regions proposed offered any special field for exceptional services rendered to science. The funds of the Government will be spent neither in exploring Central Africa nor in seeking the north pole, but in excavating Trojan ruins and examining some of the islands of the Asian Archipelago. It was also complained that no qualified traveller had been sent into civilised parts to study the progress of special arts and sciences. Such excursions as the celebrated "Voyage en Angleterre et en Irlande," accomplished by Baron Dupin in 1820 have rendered immense services to French industry, and the memory of it is not extinguished by the sixty years which elapsed. The sending of regular scientific missions abroad was inaugurated in France by the First Republic, for the purpose, not exclusively for cultivating anthropology, but for introducing into France the progress made by the foreign nations.

M. CHARNAY has recently forwarded to the Minister of Public Instruction at Paris a series of communications on the results of his investigations in Java in the summer of last year. He explored the east and west portions of the island, and he claims to have discovered a close affinity between the remains of the civilisation introduced by Hindu Buddhists and that of the ancient Mexican Empire. He also calls attention to the great density of the population of Java. From this island M. Charnay went on to Melbourne, and when last heard from, was engaged in making natural history collections in Queensland and at Thursday Island in Torres Straits.

THE death of the last chief of the Belgian caravan has not abated the resolution of King Leopold and the members of the International Committee for African Exploration. A third expedition is to be sent out immediately, it is said, under the guidance of Mr. Stanley. It is also stated that a new Belgian expedition led by Capt. Popelin will soon start for Zanzibar, in order to work out the plan of establishing a chain of stations right across Central Africa, viz., from Zanzibar to the Loango coast. The King of the Belgians will grant the means for this important undertaking.

THE last *Bulletin* of the Société de Géographie Commerciale de Bordeaux contains a brief paper by M. Albert Merle, advocating the exploration of Ferlo, Senegambia. This is a tract of country between the Senegal and the Gambia, marked in our latest maps, "desert country, no water;" it extends from 14° to 16° N. lat., and its interior is quite unexplored. Several travellers have passed along the outskirts of the region, and from their accounts and from native reports, it appears to be covered with thick forests containing many kinds of valuable trees; tobacco, indigo, and cotton also grow there in abundance. Those of its products which are at present turned to account, find their way to the Gambia, but M. Merle's desire is to divert the trade to the French settlements on the north.

M. PAUL SOLEILLET, the French traveller who left St. Louis in Senegal with the intention of reaching Algeria through the Sahara, according to the last intelligence received in Paris by telegraph, had reached Segou, the capital of the negro state of the same name, and he was proceeding onwards. This adventurous man received only 6,000 francs from the Governor-General of Senegal. The Paris Society of Geography, as a protest against such indifference, resolved to send him, when possible, all the money disposable from the travelling and exploring funds.

THE latest news from Dr. Rohlffs' expedition to Central Africa states that one of its members, Baron Leopold von Csillagh, has left the expedition, and will return to Europe after paying a short visit to Murzuk. News from Tripolis states that the presents sent by the Emperor of Germany, and destined for the Sultan of Wadai, have at last arrived there. The latest papers sent by Dr. Rohlffs contain a valuable zoölogical report by Dr. Stöcker, the naturalist accompanying Dr. Rohlffs' expedition, besides a number of astronomical observations.

IN the present demand for accurate information respecting the Zulus and their country, it may not be out of place to call attention to a series of papers which appeared in the *Nautical Magazine* for 1853 and 1854, under the title of the "Loss of the Brig *Mary* at Natal, with Early Recollections of that Settlement." These papers were published anonymously, but were written by Mr. C. R. Maclean, now an official in St. Lucia, who more than fifty years ago spent three years with the famous Chaka, then King of the Zulus, and consequently had the best of opportunities for observing the character of the country and the people.

WE regret to announce the death of Dr. Friedrich Wilhelm Vogler of Lüneberg, well known in Germany as the author of several excellent geographical handbooks. Dr. Vogler was in his eighty-seventh year.

THE King of Portugal has presented to Dr. Oskar Lenz, the well-known African traveller, the knightly cross of the Portuguese Order of Christ.

PROF. BASTIAN, whose severe illness was announced not long ago, is in a fair way of recovery. The indefatigable traveller and ethnographer is at Calcutta and intends soon to start for Batavia.

WE learn from the *Colonies and India* that those who took part in the recent expedition from Wellington, New Zealand, to New Guinea, which proved a failure, intend starting another one. They propose to proceed to Astrolabe Bay, and will take with them two whale boats and a long boat, two horses, some goats, &c. The services of a doctor, geologist, and botanist are to be secured, and a carpenter, gunsmith, and one or two other handicraftsmen are to be invited to join.

NOTES

ON Tuesday morning, in the presence of a small number of his sorrowing friends, the remains of the late Prof. W. K. Clifford were placed in their last resting-place in Highgate Cemetery.

THE following grants have lately been made from the Research Fund of the Chemical Society:—10*l*. to Dr. C. A. Burghardt for an investigation into the constitution of topaz; 20*l*. to Mr. Francis Jones for the investigation of boron hydride; 15*l*. to Mr. F. D. Brown for the study of the theory of fractional distillation; 30*l*. to Dr. Dupré for the estimation of organic carbon in air; and 15*l*. to Prof. T. E. Thorpe for the investigation of albietene, the hydrocarbon of nut-pine.

M. BISCHOFFSHEIM, the well-known French Mæcenas of science, has just returned from Mentone, which he visited with M. Lœwy, the Sub-director of the National Observatory, to examine the practicability of establishing an observatory in his mansion. The site was found to be very convenient in all respects, and M. Bischoffsheim resolved to spend a sum of 900,000 francs for instruments, &c. The work is to begin immediately.

M. ANDRÉ, the well-known eclipse and transit of Venus observer, has inaugurated the publication of meteorological readings taken in the Municipal Observatory established at Tête d'Or, in the vicinity of Lyons. The peculiarity of that establishment is that astronomical and meteorological observations are conducted *pari passu* with the same zeal. It is the only place in France where the schemes organised by Leverrier, at Paris, are practised.

THE Select Committee of the House of Commons appointed to inquire into the subject of the lighting of towns by means of electricity, and to which the Liverpool Lighting Bill was referred, has determined to go into the general question, settle the principle, and then leave the thirty-four private Bills which ask for powers to light by electricity to be dealt with by the regular Committees of the House of Commons. The inquiry will commence on the 31st inst. As the evidence will be lengthy and the committee will probably report late in the Session, it is expected that no powers will be granted this Session for lighting by electricity.

THE Werderman light was tried by M. Becquerel in his lectures on electricity, delivered at the Conservatoire des Arts et Métiers on March 19. This apparatus, which will be tried very shortly in Paris, has been introduced into France by Dr. Cornelius Herz. Six Werderman lights were arranged round the chair of the professor and burned with the utmost regularity every time they were lighted. The opinion of M. Becquerel was very favourable indeed; he insisted upon the presence of a microscopic arc giving an exceptional brilliancy to the flame. He reserved the question of cost in comparison with gas, and confined himself to making a comparison between Werderman and his competitors. M. Jamin has presented to the Academy of Sciences a system of his own, which has been described in the *Comptes rendus*, and will be tried by the Jablochkoff Company. Other systems are said to be preparing so that the partial failure of *bougies* is giving rise to a renewed electric light agitation in Paris. The members of the Municipal Council have determined to give fair play to any rational experiments.

As the result of the experiment of lighting the Holborn Viaduct with the electric light it has been found that the cost is seven and a half times that of gas, while the illuminating power is seven times greater. It has been decided not to continue the experiment.

A VALUABLE contribution to the marine zoology of the coast of the United States is furnished by a paper by Prof. H. E. Webster upon the chætopod annelids of the Virginia coast. This contains the result of several years' observation on the coast of Virginia by Prof. Webster, adding many new species to our hitherto published lists.

THE death of Prof. Yarnell, a much esteemed member of the scientific corps of the United States Naval Observatory, took place in Washington on February 27, at the age of sixty-two. The annual volumes of the Observatory contain a great many important memoirs by Prof. Yarnell, and he had just completed another at the time of his death.

IN the Report for 1878 of the "Observations of Injurious Insects," which has been drawn up by Miss E. A. Ormerod and her fellow-workers, and recently published (London: West, Newman, and Co.), a good many facts interesting alike to the agriculturist, entomologist, and meteorologist are recorded. The thanks of all interested in the preservation of food-crops are due to Miss Ormerod for her persistent efforts to promote a knowledge of, and a means to diminish the ravages caused by insect pests, and to all those who have opportunities to assist, such information if sent to Miss Ormerod, Dunster Lodge, Spring Grove, Isleworth, will be sure to be utilised. For the guidance of fresh observers the points required to be observed are pointed out, those particularly wished for being as follows: "1. With regard to weather; a very few lines as to general state through the year, such as any marked succession of warm or cold days, of great rainfalls, or drought. 2. Any observations as to the spread of common crop-insects from common crop weeds. For instance, with regard to observations of charlock and blackthorn in connection with turnip fly and gooseberry caterpillar. These two plants supply food or shelter for two insects that certainly come under the head of 'pests.' Their presence is either agriculturally bad or of little use, but they keep up the supply of successive insect generations in safety, because little noticed on these worthless growths. 3. Observations as to infested farm stores and seeds might throw much light on the intermittent appearance of some destructive insects. Thus the wheat-midge *Cecidomyia tritici* is kept safe in the larval state during winter in neglected chaff-heaps, and the red clover weevil may be seen in legions creeping from the recently-stored clover. The amount of loss from this insect has been observed for more than eighty years, and still *Apion apricans* is at work in the clover as hard as ever." Miss Ormerod further points out that often the larvæ and pupæ are both contained in the seeds, and consequently sown with them, and she further remarks that we shall probably find the key to great devastations in what were originally small appearances. "Each note of information," we are reminded, "even if incomplete in itself, will or may probably join on to those of other observers, and thus the circumstances which give rise to insect ravage be gradually more and more clearly made out, till we may hope, if not entirely to check the evil, at least to mitigate it greatly." The special points of interest in the report under consideration are: 1. The spread of the turnip fly in localities where charlock was most prevalent, and attention is drawn to the desirability of eradicating, as far as possible, this food-plant of the insect. 2. The effect of rain or dew in diminishing the spread of the pest by reducing its powers of locomotion; and 3. The observations regarding the destruction of insects by birds. One observer mentions that in the neighbourhood of Plymouth migratory insectivorous birds were very abundant, especially starlings, who congregated in such enormous quantities that the flocks coming in from all quarters to roost in the evenings so completely filled the roosting-trees as to constitute quite a sight. The report altogether is one of much value, and its circulation will, we doubt not, create that interest in the subject which the promoters desire.

MISS E. A. ORMEROD, who has done so much in the field of economic entomology, has recently contributed a brief but interesting paper, entitled "Notes on Economic Entomology," to the Watford Natural History Society. This paper may well be taken as a companion to the "Notes of Observations of Injurious Insects" for 1878.

THE *Chemist and Druggist* gives the following account of an experiment in opium-smoking, made by Dr. Miclucho Maclay upon himself during his stay in Hong Kong:—The experiment was made at the Chinese Club, where every convenience for smoking opium is to be found. Dr. Clouth, of Hong Kong, took the necessary observations, and his notes are recorded below. These may be summarised as follows:—Herr Maclay was in normal health, and had fasted eighteen hours before commencing the experiment. He had never smoked tobacco. Twenty-seven pipes, equivalent to 107 grains of the opium, used by the Chinese, were smoked in two and three-quarter hours, at tolerably regular intervals. The third removed the feeling of hunger caused by his long fast, and his pulse rose from 72 to 80. The fourth and fifth caused slight heaviness and desire for sleep, but there was no hesitation in giving correct answers, though he could not guide himself about the room. After the seventh pipe the pulse fell to 70. The twelfth pipe was followed by singing in the ears, and after the thirteenth he laughed heartily, though without any cause that he can remember. Questions asked at this time were answered only after a pause, and not always correctly. He had for some time ceased to be conscious of his actions. After the twenty-fifth pipe questions asked in a loud tone were not answered. After the last pipe had been smoked he remarked, "I do not hear well." Forty minutes later there was a slight return of consciousness and he said, "I am quite bewildered. May I smoke some more? Is the man with the pipe gone already?" Fifteen minutes later (4.55 P.M.) he was able to go home, and then retired to bed. He woke the next morning at 3 A.M., and made a hearty meal, after his fast of thirty-three hours. During the next day he felt as if he had bees in a great hollow in his head, as well as a slight headache. The organs of locomotion were first affected, next came sight and hearing, but Herr Maclay is very positive that there were no dreams, hallucinations, or visions of any sort whatever.

IT is well known from the equable temperature of the Fiji Islands and the favourable nature of the soil of many parts that the colony is well adapted to the cultivation of various useful plants that require only to be introduced to thrive. These matters have recently been discussed in a little pamphlet published at Levuka, on the agricultural prospects of Fiji. That the productive powers of these islands is very great is here fully exemplified. It is shown that tropical produce of all kinds is capable of being grown on an extensive scale, so that the resources of the islands might be made highly remunerative. Sugar-cane, coffee, tea, cinchona, and cocoa are the principal staples advocated. Sugar is looked upon in a most favourable light; some parts of the islands, both in richness of soil and climate, as well as in extent, are spoken of as extremely favourable for growing and maturing the cane; so much so as to make all well-wishers of Fiji look for the time when sugar will be made in the islands and "exported by the hundred thousand tons and to the value of millions of pounds sterling." Regarding coffee we learn that the Government have sent large supplies of seed into the interior of Viti Levu to form coffee gardens for the natives. The plants are described as having an extremely healthy appearance. Tea and cinchona could both be grown successfully in Viti Levu over an extent of country roughly estimated at about one hundred square miles. Though many valuable timber trees exist in the islands it is suggested that several well-known Indian trees such as teak, saul, sissoo, toon, and ebony, as well as mahogany, rosewood, and others should be introduced. It is to be hoped that as the resources of Fiji, including those of the forests, become developed, no undue sacrifice of timber will be effected, but on the contrary the trees will be carefully preserved or replanted as others are cut down.

THE class of substances whose fluorescence does not follow Stokes's law, and so which do not emit rays of less refrangibility than the existing rays, has lately been enlarged by Prof. Lommel by addition of one of two new fluorescing substances. That is anthracene blue, an etheric solution of which fluoresces olive green very strongly; it is excited extremely weakly by the blue and the greater part of the violet rays, but very strongly by the orange-green and yellow-green. The second new fluorescing substance is bisulphobichloranthracenic acid, the etheric solution of which gives superficially a beautiful blue, and the interior a greenish fluorescence. It obeys Stokes's law.

THE French Minister of Public Works has not yet answered the inquiries made by M. Giffard as to the probability of the Cour de Tuileries being at his disposal up to the end of September, in order to organise a new series of captive ascents. But M. Giffard, willing to give the preference to his native city, has rejected the advantageous offers made by the German company offering to work his captive balloon, and to pay him a royalty of 33 per cent. on the gross receipts.

AT 12.35 A.M. on the 22nd inst. an earthquake traversed Northern Persia, taking a direction from Tabreez to Zendjan and Mianeh, and shocks continued with more or less severity until Sunday, the 23rd. Several strongly-built houses were thrown down at Mianeh, and in others large rents were made in the walls. The most serious damage, however, appears to have been occasioned in two villages off the road, about four farsachs from Mianeh, named respectively Tark and Manan. These were totally destroyed, and of the 500 inhabitants in the one case and the 600 inhabitants in the other, only a few are reported to have been saved. Mianeh is situated in north lat. 37° 27', east long. 47° 43'.

IN a report from the Philippine Islands we learn that in the towns of Molo and Javo, both situated close to each other, and distant about three miles from Yloilo, it is very rare to enter a house that has not its loom at work, so large a trade is done in weaving not only in the towns themselves but all over the province. The principal fibre used is that of the pine-apple, and some of the articles manufactured, such as shirts and dresses, are of considerable merit and sell at high prices. In weaving China silk in colours is intermixed with the pine-apple fibre, for the purpose of giving stripes to the dresses and shirts. The value of the Chinese silk so imported varies at from 200,000 dol. to 400,000 dol. (£40,000 to £80,000) per annum.

THE Teplitz thermal question may be considered as being solved in the most satisfactory manner. The Spring Committee established by the Austrian Government declared that the quantity of recovered water is 2,224 cubic feet per hour, which is sufficient for supplying all the thermal establishments in existence before the catastrophe. The temperature has not been altered in any sensible manner. It appears that altogether the catastrophe may be considered as having been in some respects useful. The actual quantity is one-third more than the sum of the several sources which were used before the catastrophe.

WE have received the first number of a new American journal —*Useful Arts*—edited by Mr. J. A. Whitney. It contains a great deal of miscellaneous industrial information, mostly referring to patents.

Gardening Illustrated is the title of a new cheap "weekly journal for town and country."

A METEOROLOGICAL work, entitled "Ergebnisse fünfzigjähriger Beobachtungen der Witterung zu Dresden," with an introduction on meteorology, the atmosphere, meteorological instruments and observations, has just been published by Dr. Adolf Drechsler, the director of the Royal Physico-Mathematical Institution at Dresden.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by Miss Sandford; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. George Eggar; a Chinchilla (*Chinchilla lanigera*) from South America, presented by Sir Chas. Smith; a Greater-spotted Woodpecker (*Picus major*) European, presented by Mr. H. Laver; a Sumatran Rhinoceros (*Rhinoceros sumatrensis*) from Sumatra, a Tabuan Parrakeet (*Pyrrhulopsis tabuensis*), a Stair's Dove (*Phlogœnas stairi*) from the Fiji Islands, deposited; a Pied Wagtail (*Motacilla yarrelli*), a Reed Bunting (*Emberiza scheniclus*) European, purchased.

SPECULATIONS ON THE SOURCE OF METEORITES[1]

I HAVE recently read M. G. Tschermak's most interesting memoir, "Die Bildung der Meteoriten und der Vulcanismus."[2] I am not competent to offer any opinion on the mineralogical questions involved in his discussion, but the numerous arguments he has adduced appear to me to justify his conclusion that "the meteorites have had a volcanic source on some celestial body." These arguments are briefly as follows:—

Meteorites are always angular fragments even before they come into the air.

Most meteoric irons have a crystalline structure which, according to Haidinger, requires a very long period of formation at a nearly constant temperature. This condition could only have been fulfilled in a large mass.

Many meteoric stones show flutings resembling those seen on terrestrial rocks and which are due to the rubbing of adjacent masses.

Other meteoric stones show a joining together of several fragments analogous to breccia.

Many meteoric stones are composed of very small particles analogous to volcanic tufas.

After glancing at the old theory of the volcanoes in the moon and rejecting as untenable the supposition that meteorites have any connection with ordinary shooting stars, Tschermak concludes—"We may suppose that many celestial bodies of considerable dimensions are still small enough to admit of the possibility that projectiles driven from them in volcanoes shall not return by gravity. These would really be the sources of meteorites." Similar views having been put forward by Mr. J. Lawrence Smith and other authorities it is not unreasonable to discuss the following problem.

[1] Read at the Royal Irish Academy, January 13.

[2] "Sitzungsberichte der mathematisch-naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften," Wien, 1875. Band lxxi., Abtheilung 2, pp. 661–674.

If meteorites have been projected from volcanoes, on what body or bodies in the universe must those volcanoes have been located?

Let us first take up a few of the principal celestial bodies *seriatim* and consider their claims to the parentage of the meteorites. We begin with the sun. It has been abundantly shown that there exists upon the sun tremendous explosive energy. It is not at all unlikely that that energy would be sufficiently great under certain circumstances actually to drive a body from the sun never to return. We might therefore find upon the sun adequate explosive power for the volcano, but the projectiles are here the difficulty. There are a number of circumstances (notably the breccia-like appearance of some meteorites) which show conclusively that the meteorites have beeen torn from rocks which were already nearly, if not quite, solid, and as it seems in the highest degree improbable that rocks of this nature should exist in the sun, we may conclude that the sun has not been the source of the meteorites.

Can the meteorites have come from the moon? Owing to the small mass of the moon the explosive energy required to carry a body away from the moon is comparatively small. Can such a body fall upon the earth? *To simplify questions of this kind we shall suppose various disturbing influences absent.* We shall suppose that the projectile is discharged from a volcano on the moon with sufficient velocity to carry it therefrom. We shall then omit all account of the disturbing influence both of the sun and moon on the projectile, and we shall suppose that the projectile is really revolving round the earth as a satellite. This projectile will fall upon the earth if its distance from the earth's centre when in perigee be less than the radius of the earth (augmented, perhaps, by the thickness of the earth's atmosphere). It should however be observed that *if the projectile once escaped the earth it would never fall thereon*, hence the question as to whether the moon can be the source of the meteorites now falling appears to be connected with the question as to whether the lunar volcanoes are *now* active. But it is generally believed that the lunar volcanoes are not now active to any appreciable extent (even if the suspected indications of recent change were thoroughly established). It follows that even if the moon has been a source of meteorites in ancient times, we no longer receive a supply from that quarter. There is of course just a possibility that projectiles from the moon which have been revolving round the earth as satellites in elliptic orbits ever since their ejection may, under the influence of *the disturbing causes previously excepted*, gradually change their orbits until they become entangled in the atmosphere and descend as meteorites. It therefore appears to be not quite impossible that even still a meteorite which had its origin in the moon in past ages may occasionally tumble on the earth.

Passing from the sun and the moon let us now bring under review some of the other celestial bodies and see how far they will fulfil the conditions of the question. Is it possible that the meteorites can have been projected from the surface of a planet? In order to get over the difficulties of the great initial velocity which would be necessary to overcome the gravitation of a large planet, it seems natural to inquire if a volcano placed upon one of the small planets could accomplish the task.

It is clearly impossible that a projectile should ever fall on the earth unless the orbit of the projectile cuts the plane of the ecliptic in a point which lies in the narrow ring between 8,000 and 9,000 miles wide which the earth traces out on the ecliptic, but if a meteorite with an elliptic orbit intersect this ring, then, in the lapse of time, it may happen that the earth and the meteorite meet at the intersection of their orbits, in which case of course the long travels of the meteorite will come to an end.

We shall therefore consider the circumstances under which it would be possible to discharge a projectile from the surface of a planet (say Ceres), so that the projectile shall intersect the ecliptic in the ring we have just referred to. The planet being small the initial velocity that would be required to carry a projectile from its surface presents no difficulty; perhaps an ordinary cannon would be sufficient *so far as the mere gravitation to the planet is concerned.* But when we consider the necessity that the projectile must be driven through the ring we have been considering, a vastly more powerful instrument would be required.

Ceres is moving in an orbit (supposed circular and in the ecliptic) with a velocity of about eleven miles per second. A projectile discharged from Ceres will have an actual velocity which is compounded of the velocity of Ceres, with the velocity which is imparted by the volcano. But simple dynamical considerations show that if the projectile have an initial velocity *perpendicular to the radius vector*, differing from about eight miles per second, it can never intersect the ring, no matter in what direction it be discharged.[1] The volcano on Ceres must therefore be adequate to the abatement of the velocity perpendicular to the radius vector from eleven miles per second to eight miles per second, *i.e.*, *the volcano must be at the very least adequate to producing an initial velocity of three miles per second.* As this is quite independent of the additional volcanic power requisite to carry the projectile away from the attraction of Ceres, it is obvious that after all there may be but little difference between the volcano which would be required on Ceres, and that (of six mile power) which would project a body away from the surface of the earth for ever.

Admitting, however, that a volcano of sufficient power were placed upon Ceres, would it be likely that a projectile driven therefrom would ever cross the earth's track? This is a question in the theory of probabilities, and it is not easy to state the problem very definitely. If the *total* velocity with which the projectile leaves the orbit of Ceres be less than eight miles per second, then the projectile will fall short of the earth's track; on the other hand, if the *total* initial velocity exceeds sixteen miles per second, the orbit in which the projectile moves will be hyperbolic, and though it may cross the earth's track once, it will never do so again. Taking a mean between these extreme velocities we may investigate the following problem:—Suppose that a projectile is discharged from a point in the orbit of Ceres in a *random* direction with the *total* initial velocity of twelve miles per second, determine the probability that the orbit of the projectile shall cross the earth's track. When this problem is solved in accordance with the calculus of probabilities it is found that the chances against the occurrence are about 50,000 to 1, *i.e.*, out of every 50,000 projectiles discharged at random from a point in the orbit of Ceres, only a single one can be expected to cross the earth's track.

It is thus evident that there are two objections to Ceres (and the same may be said of the other minor planets) as a possible source of the meteorites. Firstly, that notwithstanding the small mass of Ceres, a very powerful volcano would be required; and secondly, that we are obliged to assume that for each meteorite which could ever fall upon the earth, at least 50,000 must have been ejected.

It thus appears that if the meteorites have been originally driven from any planet of the solar system, large or small, the volcano must from one cause or another be a very powerful one.

There is, however, one planet of the solar system which has a special claim to consideration. On that planet it is true that a volcano would be required which was capable of giving an initial velocity of at least six miles per second; but *every* projectile launched from that volcano into space would, after accomplishing an elliptic orbit round the sun, dash through the track of the earth, and again pass through the same point at every subsequent revolution. It is not here a case of one solitary projectile out of 50,000 crossing the earth's track, but every one of the 50,000 possesses the same property. The planet of which we are speaking is, of course, the earth itself. If in ancient times there were colossal volcanoes on the surface of the earth which had sufficient explosive energy to drive missiles upwards with a velocity sufficient to carry them away from the earth's surface, after making allowance for the resistance of the air, these missiles would then continue to move in *orbits round the sun*, crossing at each revolution the point of the earth's track from which they were originally discharged. If this were the case, then doubtless there are now myriads of these projectiles moving through the solar system, the only common feature of their orbits being that they all intersect the earth's track. It will, of course, now and then happen that the earth and the projectile meet at the point of crossing, and then we have the phenomenon of the descent of a meteorite. This theory, that the meteorites have originated in the earth, was so far as I know first put forward by Dr. Phipson. Mr. J. Lawrence Smith in a letter I received from him some months ago inclines to the same view as at all events one of the probable sources.

It is well to note here the great difference between the lunar theory of meteorites and the terrestrial theory. For the lunar theory to be true it would probably be necessary that the lunar volcanoes should be *still* active. In the terrestrial theory it is only necessary to suppose that the volcanoes on the earth *once*

[1] Disregarding an obvious exception.

possessed sufficient explosive energy. No one supposes that the volcanoes on the earth at present eject the fragments which will constitute future meteorites, but it seems probable that the earth may be now slowly gathering back in these quiet times the fragments she ejected *in an early stage of her history*.

Assuming, therefore, that the meteorites have had a quasi-volcanic origin on some considerable celestial body, I am led to agree with those who believe that most probably that body is the earth.

ROBERT S. BALL

RECENT RESEARCHES ON ABSORPTION SPECTRA

THE numerous absorption spectra of soluble substances which have been described hitherto, have referred as a rule to the solutions of the substances, and but rarely to the solid substances themselves. It is true that certain differences were remarked between the spectra of certain solutions, those of uranium and didymium salts, for instance, and the spectra of the solid salts; yet, on the whole, these differences were so slight that it was generally believed that the spectra were essentially the same. On the other hand experiments had shown that the spectra of solutions differed according to the dissolving medium; indeed Herr Kundt established the fact that the absorption band of a substance in solution lies the nearer to the red end of the spectrum the stronger the dispersion of the dissolving medium. In these experiments the fact seems to have been overlooked that when changing the dissolving medium often the whole character of the spectrum is changed, so that comparison with the former one becomes extremely difficult. Close investigation of these differences was therefore an important desideratum, both for the theory of absorption spectra as well as for practical absorption spectrum analysis.

In the Monthly Report of the Berlin Academy of Sciences, Herr Vogel has recently published the results of such investigations, to which he was led by the remarkable differences between the spectra of solid and those of dissolved substances which he had observed in the case of certain pigments.

For the examination of these absorption spectra Herr Vogel used instruments of but moderate dispersion, which allow of an easier survey of the whole spectrum, and consequent judgment of its general character, than is the case with strongly dispersing spectroscopes. The absorption spectra of solid salts and pigments were obtained from thin layers of these substances, prepared upon glass plates, through evaporation of a few drops of solution. Herr Vogel reproduces the spectra he observed on two plates, which at once show not only *the differences in the spectra of one and the same solid substance and its solution*, but frequently an *extraordinary coincidence in the position of the absorption bands belonging to totally different substances* (for instance, in nitrate of uranium and permanganate of potash). Of several substances, such as iodine, hyponitric acid, and indigo, the spectrum of the vapour is also given for comparison, and in most cases the aqueous, alcoholic, and some other solution of each substance has been examined.

Without entering into the highly interesting details for which we must refer our readers to the original paper, we confine ourselves to stating the results of Herr Vogel's researches, which are the following: 1. Considerable differences exist between the spectra which a substance gives in the solid, liquid, or dissolved and gaseous state. Characteristic bands which are shown in the spectrum of one state are either not reproduced in that of the other (this is the case with chrome alum, chloride of cobalt, iodine, bromine, naphtaline red, fuchsine, indigo, cyanine, aniline blue, methyl violet, eosine, carmine, purpurine, alizarine, santaline), or they reappear in a different position, or different intensity (examples: nitrate of uranium, permanganate of potash, hyponitric acid, alcanna red). Sulphate of copper and chlorophyll show the same absorption both in the dissolved and in the solid state.

2. The spectra given by the same substance when dissolved in different media are the same in some cases (purpurine in alcohol or sulphide of carbon, aldehyde green in water or alcohol, methyl violet and indigo-sulphuric acid in water or amylic alcohol); in other cases they differ only in the position of bands (chloride of cobalt, fuchsine, coralline, eosine and iodine green in aqueous or alcoholic solutions); and again in others their character is totally different, so that no point of coincidence remains (iodine in sulphide of carbon or alcohol, naphtaline, aniline blue, purpurine, hæmatoxyline, brasiline in water or alcohol).

3. The rule established by Kundt, viz., that the absorption bands of a body in solution lie the nearer towards the red end of the spectrum the greater the dispersion of the dissolving medium is in the region of the bands, is not confirmed in many cases; on the contrary, in some instances the absorption bands move towards the blue in a solution of greater dispersion (nitrate of uranium and blue chloride of cobalt in water and alcohol); in other cases their position remains unaltered for various media (hyponitric acid in air and benzol, indigo-sulphuric acid and methyl violet in water and amylic alcohol, aldehyde green in water and alcohol, purpurine in sulphide of carbon and alcohol). In some cases a great difference in the sense of Kundt's rule becomes apparent, while in others for the same spectral region but a very trifling one appears, according to the nature of the pigment (coralline and fuchsine). Indeed it happens sometimes that certain bands are in the same position with different dissolving media, while others which are *simultaneously* visible are displaced (nitrate of uranium in water and alcohol, oxide of cobalt in glass and in water, protonitrate of uranium in neutral solution and in a solution of oxalic acid, chlorophyll in alcohol and ether).

4. The position of absorption bands in the spectra of solid and dissolved bodies may be only exceptionally deemed characteristic for any certain body. Totally different bodies show absorption bands in exactly the same position (solid nitrate of uranium and permanganate of potash in the blue; naphtaline red and coralline in the yellow; indigo, aniline blue, and cyanine in the orange; aldehyde green and malachite green in the orange). Closely related substances sometimes show remarkable differences in the position of their bands under prefectly equal conditions (solid uranium salts).

5. The rule set up for absorption spectra, "each body has its own spectrum," can be admitted only with great restrictions. The great number of polychromatic substances show different colours and different spectra in the solid state, according to the direction in which they are observed. Most other bodies show different spectra in the solid state from those of their solutions, and in the latter case again different ones according to the dissolving medium, and the question arises which of all these spectra is the body's "*own*" spectrum.

The most important difference of the spectra of elements in a state of incandescent vapour, the position of the spectral lines, ceases to be characteristic in the case of absorption spectra of liquid and solid bodies. In the latter spectra, however, the characteristic differences shown by the spectra of incandescent vapours cannot be expected. It is known that metals, which give such remarkably different spectra in the state of incandescent vapours, all give qualitatively the same spectrum as incandescent liquids or solids, viz., a continuous one; for this reason the absorption spectra of these bodies cannot show any remarkable characteristic differences, whatever quantitative differences may become apparent with regard to the absorbed colours. If these well-known facts show that already with regard to elements the laws applying to the spectra of gases do not apply to those of liquids and solids, then Herr Vogel's investigations prove that in the case of compound bodies simple relations between the spectra of their different aggregate states are still less frequent and occur only exceptionally.

The analysis of absorption spectra therefore is based not so much upon the recognition of the position of the absorption bands of a substance, as upon the *changes in the spectra* of the same body which take place under the influence of various dissolving media and reagents. Thus cyanine and aniline blue dissolved in alcohol give a very similar spectrum, dissolved in water a totally different one. The absorption bands of oxyhæmoglobine disappear with reducing agents; those of carmine, which are in a similar position, do not; the band of brasiline disappears when acetic acid is added to the solution, that of fuchsine does not, &c., &c.

The position of bands becomes more characteristic for the recognition of a body, if the latter shows *several* absorption bands. But even here we should go too far if from the accidental coincidence in the position of bands of two different bodies we were to draw conclusions regarding any similarity or chemical identity between them (this has indeed been done in certain cases, particularly with blood and chlorophyll). A conclusion regarding such similarity or identity is only justified if *the same bands show equal intensities and analogous changes under the influence of the same reagents.*

INTELLECT IN BRUTES

We have received so many letters on this subject that we are compelled to content ourselves with giving the following extracts:—

The Rev. George Henslow comments as follows on some of the cases already adduced:—

I would not assert that what I call "practical reasoning"—that is, reasoning applied to objective facts directly apprehended by the senses—is fundamentally different from "abstract," *i.e.*, with no objective fact immediately present to consciousness; but they certainly do represent two stages in our own mental development.

E. H. Pringle's account of "Bully" shows nothing beyond what is common to dogs in practical cunning (*i.e.* reasoning) with objective things, (1) his "lady" friend, (2) best road to take so as not to be seen, (3) the person to be avoided, (4) E. H. Pringle's eye to be eluded. In the only point where abstract reflection was really required Bully failed, just as a child would, *viz.*, in shamming sleep. For, it is not enough *to lie down* and *shut one's eyes*. This is all a child is told to do, or is conscious of doing on going to sleep. The relaxation of every muscle follows spontaneously. Hence, as children do not think of this when pretending (since it requires reflection) they can so easily be detected by some rigidity of the muscles in the face or in breathing which at once betrays them. I have known a child overdo it by screwing up its eyes in order to appear *very fast asleep!* Exactly so, too, Bully was totally unable to think of the importance of putting his ears in repose.

Dr. Rae's dog only strengthens my case, for it clearly associated the bell with a *particular* maid, whereas a reasoning human being would have *generalised* that since the maid was in the room, the bell could be rung for some one else. Hence the dog proved its impotency in all power of generalisation, which is a pure form of abstract reasoning.

Dr. Muirhead's donkey was solely concerned with immediate sense-objects, the gate and the cows, and required no abstract reflection. That horses and donkeys can discover how to open gates is by no means uncommon.

Lastly C.M.'s cat and Mr. Belsham's kitten are the only cases I have yet seen which show a *primâ facie* evidence against the distinction I propose to draw. But, that a half-grown kitten could go through such a process of reflection analogous to what I gave for a hypothetical untaught dog ringing a bell, would be so astounding, that all possible explanations must be eliminated first, before it is credited.

Is it not far more probable that the cat and the kitten discovered by accident that the door was opened when a knock was made, and that this discovery arose from the common habit of cats to play with anything suspended within their reach? That animals discover facts, and then use them, will not be disputed, like the dog that, on discovering a stream carried him down too far on swimming across it, ran a mile up stream ever afterwards to allow for the current. Again, that animals mimic, as do parrots and apes, is common enough, but they do not know why they do it. A monkey might knock at a door after seeing a man do it, but, I believe, could have no similar motive as the man, until (like the kitten) it should accidentally discover for itself what the real use was, or else unless it be taught to do it.

In re rats gnawing pipes. I have just heard of a mouse gnawing through a gas-pipe. May it not be accounted for by the fact that, although the upper incisors of a rodent, by working on the lower, keep the chisel-like ends in order; yet this may be assisted by gnawing wood, lead, or other hard substances? Does not this account for rabbits, though well fed on cabbage and bran, &c., still persisting in gnawing their hutches?

I will, in conclusion, give another case to illustrate the want of abstract reflection: this time in a lady (aged thirty), whose mental powers were curiously arrested. Looking at the picture of a shark in the sea, with a pig in its mouth, in "Masterman Ready," and knowing that the pig had been dropped from the wreck to see if it would swim to shore, she *naïvely* asked, "Is the shark carrying the pig to the shore?" The idea of the shark eating the pig would only arise from the abstract reflection on the habits of sharks, which was not suggested by the story; the single objective fact present to her mind was that "the pig had to get to the shore."

Mr. Arthur Nicols writes:—

I cannot understand practical *reasoning*, but a practical result of reasoning upon either simple or abstract ideas is intelligible. Can we conceive any human being reasoning more correctly than a dog did in the following instance?—Towards the evening of a long day's snipe-shooting on Dartmoor, the party was walking down the bank of the Dart, when my retriever flushed a widgeon which fell to my gun in the river, and of course instantly dived. I said no word to the dog. He did not plunge in after the widgeon *there*, but galloped *down* stream about fifty or sixty yards, and then entered the water, and dashed from side to side —it was about twenty or thirty feet wide—working up stream, and making a great commotion in the water, until he came to the place where we stood. Then he landed and shook himself, and carefully hunted the near bank a considerable distance down, crossed to the opposite side, and diligently explored that bank. Two or three minutes had elapsed, and the party was for moving on, when I called their attention to a sudden change in the dog's demeanour. His "flag" was now up, and going from side to side in that energetic manner which, as every sportsman knows, betokens a hot scent. I then knew that the bird was as safe as if it was already in my bag. Away through the heather went the waving tail, until, twenty or thirty yards from the bank opposite to that on which we were standing, there was a momentary scuffle; the bird just rose from the ground above the heather, the dog sprang into the air, caught it, came away at full gallop, dashed across the stream, and delivered it into my hand. Need I interpret all this for the experienced sportsman? The dog had learned from long experience in Australia and the narrow cañadas in the La Plata that a wounded duck goes down stream—if winged, his maimed wing sticks out, and renders it impossible for him to go up—and will invariably land, and try to hide away from the bank. But if the dog enters at the place where the bird fell, the latter will go on with the stream for an indefinite distance, rising now and then for breath, and give infinite trouble. My dog had found out all this long since, and had proved the correctness of his knowledge times out of number, and by his actions had *taught me* the whole art and mystery of retrieving duck. His object—I say, without a doubt, because I had had numberless opportunities of observing it—was to flurry the bird and force it to land by cutting it off lower down the stream. Then assuming, as his experience justified him, that the bird had landed, he hunted each bank in succession for the trail, which he knew must betray the fugitive.

Mr. A. Petrie writes:—In my own family we had a tabby cat, who, when turned out, would let herself in at another door by climbing up some list nailed round it, then pushing up the click-latch, pushing the door, with herself hanging on it, *away* from the post, so as to prevent the latch falling back into its place, and then dropping down and walking back to the fire. I knew a Skye terrier, who, being told to carry a fishing-rod, carefully experimented along its length, to find its centre of gravity, then carried it on till his master came to a narrow path through a wood. Here Skye considered, dropped the rod, took it by the end, and dragged it under him lengthwise, till the open road was gained, when he took the rod by the centre of gravity again, and went on. This could not be a copy of human actions, but the result of original reasoning.

Mr. Henry Cecil gives the following on the authority of the late Mr. Dawes the astronomer:—

Being busy in his garden, and having a large bunch of keys in his hand, he gave it to a retriever to hold for him till he was at liberty. Going into the house soon after he forgot to reclaim the keys. The remembrance of what he had done with them only returned to him when he required to use them in the evening. He then recalled that he had given them to the dog, and forgotten to take them again. Calling him, and looking impressively in his face, he said, "My keys! fetch me my keys." The dog looked wistful and puzzled for a moment, and then bounded off to the garden, his master following. He went straight to the root of an apple-tree, scratched up the keys, and brought them. May we not fairly put into words the dog's train of reasoning thus: "My master has given me these keys to hold; he has forgotten them; I cannot carry them all day; but I must put them in safety where I can find them again?"

Mr. W. S. Chamberlayne writes that many years ago, taking an afternoon ride through a wood in the Bahamas, he came to a gate which was kept closed by a small iron hoop hung over a post and the end of the gate. To open the gate he leant over his horse's neck and lifted up the hoop, shutting the gate and replacing the hoop when he had passed through. On returning from his ride the gate was still shut, when, to his surprise, his horse, without any hesitation, took the hoop in his mouth and tried to lift it off the gate. He, however, was not successful in his efforts, and Mr. Chamberlayne had to finish the operation for him, but the exhibition of memory was certainly remarkable.

Mr. T. B. Groves, of Weymouth, sends the following account given to him by a relative, a gentleman well known in the district, and who would be everywhere accepted as a trustworthy and competent observer:—

In the wine-cellar two vessels, one an open earthen jar containing hazel-nuts, the other a wooden sieve, tub, or something of the kind, full of wine-corks, stood side by side. It was observed that the nuts were gradually diminishing, owing to the depredations of mice; but after a time this seemed to have altogether ceased, and it was inferred that the difficulty of egress had caused the mice to abandon the enterprise as soon as the level of the nuts had reached a certain depth from the mouth of the jar. Matters so remained for some little time; but afterwards, on visiting the cellar, it was found, to the owner's great surprise, that his nuts had now entirely disappeared, and in their place were discovered the corks! The only explanation that could be suggested was this: that the mice, *reflecting* on the difficulty of making their exit from the partially-emptied jar, had *conceived* and carried out the plan of providing for their escape by dropping into the jar from time to time sufficient corks to enable them to make a safe retreat with their plunder.

Mr. R. Howson sends us the story of a terrier-like dog of no particular breed, named Uglymug, who had a poodle for companion. Whenever Uglymug saw signs of a family meal being laid out, he inveigled the poodle into a labyrinthine shrubbery under pretence of seeking for rats, and when the latter was fairly intent on its game, Uglymug sneaked back to enjoy all by himself what he could get from the family table.

V. I. writes:—The following instance will show that in the case of the mule intelligence has a limit. We had a mule who could take the staple out of a gate and open it (he never shut it). This mule used to go to the water-butt, turn the brass tap, and drink, but never turned the water off. Common sense would have forbidden a human being neglecting such a precaution.

MR. E. PARFITT, of the Devon and Exeter Institution, writes of a favourite cat:—She would frequently come and sit near the door opening into the library of the institution. The door only divides my house from the library; puss would place herself here mostly at dinner-time, and, as I am informed, not before; she would wait here until she heard my footsteps down the library; she would then proceed directly to the kitchen, and inform the servant, either by mewing or looking up into her face. She would then come to me and tell me in her way that she had ordered dinner. I have seen her scores of times trotting along the passage to the kitchen, when I have opened the library door, to inform the servant that I was coming. How Topsy ascertained the time to proceed to the door I do not know, except that she saw that dinner was preparing; but how did she know the time it would be ready and the time that I was expected to come in?

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Association for promoting the Higher Education of Women in Oxford is to open two halls in Oxford in October for the reception of lady students. One of these is to be an "Academical house on the principles of the Church of England," and in the other, "Somerville Hall" (after Mrs. Somerville), care will be taken that members of different religious denominations are placed on the same footing. The charges in the latter will be considerably lower than in the former.

TWO Combe Exhibitions of 35*l*. each will be open for competition in May next at Trinity College, Oxford. Candidates will be at liberty to offer classics, mathematics, chemistry, and physics, a period of history, or any two or more of these branches of study. There is no fixed limit of age. The examination, which will be combined with the ordinary Matriculation examination, will commence on Tuesday, May 20, at 9 A.M. Names, with subjects offered and testimonials of good conduct, to be sent to the president not later than May 12.

THE Astronomer-Royal continues to give evidence of his intense desire for the promotion of sound mathematical training, and has published to the Cambridge Senate his views as to the papers set in the Smith's Prize examinations of recent years, classifying the questions set, and showing that several subjects, more valuable, in his opinion, to men of science and to students, than all the others together, have had no questions set upon them. Among these are attractions, higher dynamics, perturbations, figure of the earth, thermodynamics, waves and tides, sound, physical optics, &c. He says very pertinently: "The use of an examination is to test the power of a candidate to command the application of mathematics when required. The use of publication of examination is to guide students in the subjects recommended for their study. The guidance which too many of these subjects intimate is this: that clever and abstruse algebra, without any reference to its benefit as an application of a tool to other purposes, is the *summum bonum*." He believes this guidance is against the instincts of many residents at Cambridge and the desires of undergraduates.

PROGRESS is evident at Cambridge in response to the memorial we recently referred to against the compulsory study of Greek by all undergraduates. Very few votes prevented reform years ago; no doubt the claims of science students and of liberty for all will now be more fairly listened to. The syndicate on the subject includes Dr. Humphry, Professors Liveing and James Stuart, and Mr. Todhunter, and thus the real interests of mathematics, physics, biology, and medicine, as regards the education of students, as well as the progress of science, will be sure of recognition.

THE examiners in the Cambridge natural sciences tripos this year are Dr. Humphry, Prof. Bonney, Mr. J. F. Walker, (Lecturer on Chemistry at Sidney Sussex College), and Mr. Yule, of Magdalen, Oxford, the foregoing being re-appointments, and Prof. Liveing, the Rev. J. W. Hicks (University Demonstrator of Chemistry, and Lecturer on Botany at Sidney Sussex College), Mr. W. Garnett, Demonstrator of Experimental Physics, and Mr. F. M. Balfour; the latter three are fresh nominations.

IN the last Cambridge Local Examinations (December, 1878), among 626 senior boys there were 92 candidates for the chemistry paper and 44 for practical chemistry; of 997 senior girls, 29 took the paper and only 4 the practical examination; 21 boys and no girls entered for experimental statics and dynamics, &c., 38 boys and 24 girls for heat, 30 boys and 4 girls for electricity. The result is that only 3 boys, 2 from the Liverpool Institute and 1 from Newton College, Devon, obtained the mark of distinction in the section "natural philosophy," in which all these subjects are included; and no girls. It should be added that a pass may be attained on two of these subjects, and only three in all may be taken by any candidate. Is it possible to show more strongly the lack of attention to and interest in the elementary forces of nature in English schools and by English parents? These are boys and girls between sixteen and eighteen years of age, most of them supposed to be ready, or almost ready, to leave school and take part in the battle of life. Among 3,329 junior boys and 1,483 junior girls, 423 boys and 13 girls took the chemistry paper, 169 boys and 1 girl the practical chemistry, 76 boys and no girls statics, &c., 178 boys and 12 girls heat. Seven boys, the majority from Liverpool College, and no girls, obtained distinction. We do not become further consoled by finding that 15 senior boys and 79 senior girls took zoology, 11 boys and 177 girls botany, 24 boys and 150 girls geology; for girls have no *more* right to a scientific training than boys. Most likely, however, boys and their teachers will seek to know more of the life and the past history of the globe when they find that girls can really hold their own in and enjoy these studies, and look with amazement on men for being so unwilling to learn or teach them. Among the juniors, 75 boys and 148 girls took zoology, 45 boys and 238 girls botany. These numbers, however, represent no great attainments as yet, for the standard of passing is very low; severity would only kill the tender growth. But evidently there is in secondary schools little belief in the educative and attractive power of the study of nature. Why is it not considered that *mathematicians* are fostered by neglect and hindrance? It appears to be thought capital training to produce physicists and naturalists. Really, conservatism and unwillingness to take a little trouble are the enemies.

A REVISED schedule of subjects in natural science for the *ordinary* B.A. degree at Cambridge has just been issued. This is for the third or final examination, and a pass in one subject is sufficient to give a degree. Is it supposed at Cambridge that a year is to be fully employed in dealing with botany in an *elementary* manner? The schedule says the questions (all elementary) will include the description and classification of plants; the form, structure, and development of stem, root, leaf, flower, and fruit; inflorescence, cross-fertilisation, germination, and nutrition. Twenty-one "natural orders" are specified for special attention, including one cryptogamic group, Filices. This seems a vague syllabus, not likely to encourage the study of botany. How much and how little knowledge of physiology and histology will satisfy such terms as "nutrition," "structure"? No doubt the present is better than the old in omitting to insist on technical terms, some of them antiquated. But cannot more definite requirements be suggested for ensuring some practical insight into vegetable life on the part of the man who is to be stamped as an elementary botanist? Surely the best way is to let the knowledge be good, and sound, and practical, as far as it goes, giving some training in scientific method, and capable of further development in after life. We believe many would welcome a change giving the ordinary B.A. for a lower standard of attainment in the first part of the natural science tripos, thus doing away with the recognition of *dilettante* work in a single subject as a sufficient basis for a B.A. degree. A very satisfactory schedule is presented for zoology, requiring a knowledge of the anatomy of certain selected principal types, as well as the characters of orders, and the comparative anatomy and functions of the systems and organs as exemplified in the animal kingdom. Further, the general development of the embryo chick, the leading facts and conclusions respecting the geographical distribution of animals are included in the subjects. The schedule is to be discussed next Saturday.

THE Cambridge Council of the Senate has framed a draft statute to carry out the grace passed in December last in favour of the appointment of a general Board of Studies, representative in character, to report upon the proposals of each special board of studies as they arise, and so aid in holding the balance among the various interests concerned. The draft statute provides that the new Board shall consist partly of persons appointed on the nomination of the Boards of Studies, but abundant freedom is left to the senate to add other members and to vary from time to time the composition, mode of appointment, and duties of the new board.

AT the next meeting of the Governors of Addenbrooke's Hospital Mr. J. W. Cooper will propose: That a memorial be presented to Her Majesty's Commissioners for the University of Cambridge, under the seal of the Governors, representing that Addenbrooke's Hospital is extensively used as a place of study by the Medical Students of the University; that it is essential in the interests of the Medical School that it should not cease to be a recognised place of medical study; and, further, that as large endowments have been left to various colleges for the promotion of medical study, some adequate endowment should be made for Addenbrooke's Hospital out of the funds at the disposal of the Commissioners. There cannot be much chance of success for such a proposal unless it be made more definite. The hospital can only properly benefit by educational endowments by being the locus of the study and appliances of research in therapeutics, sanitation, and pathology.

GEOLOGICAL students at Cambridge will have plenty of work provided for next term. Prof. Hughes will give one course on the geology of the neighbourhood of Cambridge, and another stratigraphical course, beginning with the Permian, and ascending. Prof. Bonney will continue his lectures on elementary physiography, and will give weekly demonstrations on microscopic lithology. Mr. Tawney will be demonstrating the principal genera of fossil invertebrata; and both he and Dr. R. D. Roberts will give practical instruction in petrology. Lectures begin April 25. The first geological excursion of the term is fixed for Saturday, May 3.

MR. THOMAS W. BRIDGE, B.A., of Trinity College, Cambridge, and Demonstrator of Comparative Anatomy in the University of Cambridge, has been appointed to the Professorship of Zoology at the Royal College of Science at Dublin, vacant by the resignation of Dr. Leith Adams, F.R.S.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 13.—"The Contact Theory of Voltaic Action," No. III. By Professors W. E. Ayrton and John Perry. Communicated by Dr. C. W. Siemens, F.R.S.

The authors commence by referring to the experiments that had been made prior to 1876, on the difference of potentials of a solid in contact with a liquid, and of two liquids in contact with one another, and they point out that:—

1. The earlier experiments were not carried out with apparatus susceptible of giving accurate results.

2. Owing to the incompleteness of the apparatus assumptions had to be made not justified by the experiments.

3. *No direct* experiments had been performed to determine the difference of potential of two liquids in contact, with the exception of a few by Kohlrausch, using a method which appeared to the authors quite inadmissible as regards accuracy of result.

In consequence of this great vagueness existed as to whether the contact difference of potentials between two substances, when one or both were liquids, was a constant depending only on the substances and the temperature, or whether it was a variable dependent upon what other substance was in contact with either. Some authorities regarded it as a variable, Gerland considered he had proved it to be a constant, but first, the agreement of the value of the electromotive force of each of his cells with the algebraical sum of the separate differences of potential at the various surfaces of separation, and which was the test of the accuracy of his theory, was so striking, and so much greater than polarisation, &c., usually allows one to obtain in experiments of such delicacy, that one could not help feeling doubtful regarding his conclusions; secondly, his apparatus did not allow of his experimenting with two liquids in contact, consequently he could not legitimately draw any conclusion in this latter case. And although Kohlrausch had made some few experiments on the difference of potentials of liquids in contact, still since he employed moist blotting-paper surfaces instead of the surfaces of the liquids themselves, the authors considered for that reason alone, if for no other, that his results did not carry the conviction the distinguished position of the experimenter might have led them to anticipate.

They therefore designed a method and an apparatus for carrying it out, by means of which they could measure the difference of potentials, in volts, at each separate contact of dissimilar substances in the ordinary galvanic cells, from which they could ascertain whether the algebraical sum of all the contact differences of potential was, or was not, equal to the electromotive force of the particular cell in question. From the results they obtained, and which are given in Papers Nos. I. and II., *Proc.* Roy. Soc., No. 186, 1878, they concluded within the limits of their experiments that if $\overline{AB}$, $\overline{BC}$, $\overline{CD}$, &c., were the contact differences of potential measured *separately* of the substances A in contact with B, and neither in contact with any other conductor, B in contact with C, &c., then, any one or more of the substances being solid or *liquid*, if any number $A, B, C \cdots\cdots K$ were joined together, and the electromotive force of the combination $\overline{AK}$, measured, the following equation was found true:—

$$\overline{AK} = \overline{AB} + \overline{BC} + \overline{CD} + \cdots\cdots + \overline{JK},$$

which proved that each surface of separation produced its effect independently of any other.

Their method, by which any single contact difference of potentials was measured, was as follows:—Let 3 and 4 be two insulated gilt brass plates connected with the electrodes of a delicate quadrant electrometer. Let 1 under 3, and 2 under 4 be the surfaces whose contact difference of potential is to be measured; 3 and 4 are first connected together and then insulated, but remain connected with their respective electrometer quadrants. Now 1 and 2 are made to change places with one another, 1 being now under 4 and 2 under 3, then the deflection of the electrometer needle will give a measure of the difference of potentials between 1 and 2.

The apparatus employed by the authors in the present investigation is then explained in detail, and it is shown how, by improving on their earlier form, they have removed a difficulty which formerly existed, and which prevented their previously experimenting on pairs of substances having very different weights, such as a vessel of mercury and a sheet of metal.

The authors explain that the results they have obtained in this investigation have divided themselves into three groups:—

1. The contact difference of potentials of metals and liquids at the same temperature.

2. The contact difference of potentials of metals and liquids when one of the substances is at a different temperature from the other in contact with it—for example, mercury at 20° C. in contact with mercury at 40° C.

3. The contact difference of potentials of carbon and platinum with water, and with weak and with strong sulphuric acid.

They mention, however, that they give only the results under head No. 1 in the present communication, reserving those they have obtained under heads Nos. 2 and 3 for a future occasion.

Then follow arranged in the order in which they were obtained from January to May, 1878, some 150 results of experiments (each result given being on the average the mean of ten observations), representing the contact differences of potential of nine solids and twenty-one liquids. The authors explain that many of the results they obtained are not mentioned in the paper, having been rejected on account of inaccuracies arising from the great delicacy of the experiments. These remarks especially apply to the authors' attempts to measure the contact difference of potentials between a liquid and a paste—for example, mercury and mercurous sulphate paste, great difficulty being introduced by the extremely thin layer of water on the surface of the paste acting inductively instead of the paste itself. They mention that this difficulty is a very good example of the inaccuracies that must have been introduced by former experimenters using a moist blotting-paper surface instead of the surface of the liquid itself.

A large number of discordant results were obtained in March, 1878, and their explanation led to the interesting result that the apparent contact difference of potentials between a metal and mercury, as measured inductively, varied much with small additions of temperature. The investigation of this apparent change of contact difference of potentials with temperature led to a consideration of the contact difference of temperature of mercury with air, since, of course, in all these inductive experiments two air contacts are included in the result.

It has usually been thought that the differences of potential of liquids in contact with one another were so small as to be almost inappreciable in comparison with the differences of potential of metals in contact; but the authors have ascertained, among other results, that strong sulphuric acid in contact with distilled water, solutions of alum, copper sulphate, and zinc sulphate, has a measured difference of potentials of 1·3 to 1·7 volts, or an electromotive force more than twice as high as that of zinc and copper in contact. And hence the great importance of an apparatus that can directly measure the difference of potentials of two liquids in contact.

Zoological Society, March 18.—Prof. St. George Mivart, F.R.S., vice-president, in the chair.—The Secretary called the attention of the meeting to the herd of Japanese Deer (*Cervus sika*) in the park of Viscount Powerscourt, at Powerscourt, in Ireland, now about eighty in number, and gave an account of their introduction and history, from particulars supplied to him by Lord Powerscourt.—A communication was read from Dr. G. Hartlaub, containing the description of a new species of Barn Owl, from the island of Viti-levu, which he proposed to call *Strix oustaleti*.—Mr. Edward R. Alston read a paper on female deer with antlers, showing that these weapons are not unfrequently abnormally developed in fertile females of certain species of *Capreolis* and *Cariacus*, and giving reasons for believing that in the ancestral forms of deer they were probably common to both sexes.—Mr. Sclater made remarks on some of the rarer parrots living in the Society's Gardens. The whole series of this group in the Society's collection was stated to consist of 170 individuals belonging to ninety-eight species.—A communication was read from Prof. Garrod, F.R.S., containing notes on the visceral anatomy of the Tupaia of Burmah (*Tupaia belangeri*). The cæcum coli in this animal was stated to be small, whilst in a specimen of *T. tana* it was ascertained to be wholly wanting. —A second communication from Prof. Garrod contained notes on the anatomy of *Helictis subaurantiaca*, in the course of which he showed that the hippocampal gyrus of the brain is partly superficial in this animal, which is not the case in any other carnivorous animal yet recorded.

Linnean Society, March 20.—William Carruthers, F.R.S., vice-president, in the chair.—The Rev. G. E. Commesford Casey was elected a Fellow of the Society.—A paper by Mr. Fred. Smith, on new aculeate hymenoptera from the Sandwich Islands, collected by the Rev. T. Blackburn, was read. The author states the general aspect of the series is certainly North American, with mixture of a few South American forms. The ants are most diverse in character, some being cosmopolitan in range. The house ant of Madeira is common, and the little European ant (*Ponera contracta*) unexpectedly turns up here.—Some observations on the reproduction of ferns, by Mr. T. R. Sim, were also read by the Secretary in the absence of the author. Among the great collection of living ferns at Kew a marked feature is the large number of species that regularly bear adventitious buds. Of a thousand species there grown barely fifty are ever found without buds, and some forms produce them regularly, though the normal forms do not. The above number seems very high when compared with Phanerogams, where adventitious buds, with some few exceptions, may be said never to be normal. Among viviparous ferns the contrary obtains, and the buds are always on the same part of the plant in all the individuals of a species. *Polystichum angulare*, for example, bears a bud on the rachis in the axil of almost every pinna on the lower part of the frond, in some all up the rachis. Some Aspleniums produce them on the veins of the upper surface of frond, but never directly through from a sorus. Great variety in position, however, is manifested in different genera and species where budding occurs, various examples of which the author gives. Where buds become detached, considerable difference obtains as to size and stage of separation, whereof many instances are pointed out and other curious instances of deviations related by the author. In commenting on the subject, Sir J. D. Hooker stated his belief that ferns at Kew were more bulbiferous than in their natural state, possibly from more constant nutrition and warmth. —The fifth contribution to the ornithology of New Guinea, namely, recent collections from the neighbourhood of Port Moresby, was read by the author, Mr. R. Bowdler Sharpe. The interesting series dwelt on were obtained by Mr. Kendal Broadbent, and usefully compare with those got by Signor Albertis from the Fly River. A parrot of the genus *Aprosmictus* closely resembles one from the Fly River, but nevertheless is specifically distinct, offering thus a parallel case to the crowned pigeons, *Gonra albertisi*, inhabiting Port Moresby, and *G. sclateri*, the Fly River. At present the affinities of the South-Eastern species seem to be with those of Australia, a few to those of the Aru Islands.—Mr. W. T. Thiselton Dyer exhibited *Helichrysum vestitum*, a perennial everlasting, from the Cape of Good Hope.

Anthropological Institute, March 11.—Mr. E. Burnett Tylor, D.C.L., F.R.S., president, in the chair.—The president read a paper entitled "The Geographical Distribution of Games," in which attention was called to the games of Polynesia and America as proving that a drift of civilisation from Asia reached these regions before they were known to Europeans. The draughts played in the Sandwich Islands and New Zealand were not our modern game, but apparently some variety related to the ancient classical game (which is alive in Egypt to this day). It may have reached the South Sea Islands from Eastern Asia, together with kite-flying, at which they were experts, and which they perhaps had before the comparatively modern time when it reached England.

Royal Microscopical Society, March 12.—Dr. Beale, F.R.S., president, in the chair.—The following papers were read:—Contribution to the knowledge of the British Oribatidæ, by A. D. Michael, F.R.M.S.—The development and retrogression of fat cells, by G. and F. E. Hoggan.—Microscope with swinging sub-stage and improved motions, by J. Beck, F.R.M.S. —The use of osmic acid in microscopical preparations, by T. J. Parker, F.R.M.S.—Other papers by Prof. Keith, Mr. Tolles, and Mr. Crisp were taken as read, or postponed in consequence of want of time.—The new $\frac{1}{18}$ oil immersion objective by Zeiss was exhibited, with remarks by Prof. Abbe on the Stephenson homogeneous immersion system.—A large number of objects were exhibited illustrative of the papers read and otherwise, together with microscopes and apparatus by Mr. Crisp.—Lord Justice Bramwell and six other gentlemen were elected Fellows.

Photographic Society, March 11.—James Glashier, F.R.S., in the chair.—Papers were read: On coloured glass suitable for the developing-room, and on the employment of quinine as a substitute, by Capt. Abney, R.E., F.R.S., who, in illustration of his paper, exhibited photographs of the solar spectrum taken through various stained glasses, and stated that a combination of cobalt and stained red glasses secures immunity from the actinic action of light, and that collodion-films on both sides of a glass, stained with either magenta, aurine, or chrysoidine, practically are also non-actinic. Quinine, he found, cuts off the ultra-violet rays, and no others.—Mr. C. Bennett, on the gelatine emulsion process, enforced the fact that the extreme sensitiveness of his process was produced by the long time he allowed the gelatine and salts to emulsify or ripen before eliminating the bromide and silver not taken up.—Mr. W. Wainwright, jun., note on Bennett's gelatine emulsion process; also, Mr. Howard Grubb exhibited some new forms of stereoscopes, one designed to exhibit pictures of a much larger size than ordinary.

Institution of Civil Engineers, March 11.—Mr. Bateman, president, in the chair.—The paper read was on movable bridges, by Mr. James Price, M. Inst. C.E.

PARIS

Academy of Sciences, March 17.—M. Daubrée in the chair.—The following papers were read:—On an electric burner and blowpipe, by M. Jamin. Two carbons are supported vertically abreast, hinged below, and drawn together at the top by a spring. A current is sent up one (A), down the other (B), then round a rectangular circuit inclosing the two, and passing first round A; by current attraction the carbons are drawn apart, and the arc appears at the top and descends gradually, consuming one or both carbons. When the action of the rectangle is sufficient, the arc driven beyond the points is like a gas flame, and M. Jamin receives it on a piece of lime, magnesium, or zirconium, getting intense light. It is also so hot as to fuse the lime, and the author recommends it as a blowpipe to chemists and physicists.—On a meteorite belonging to the group of eukrites, which fell on July 14, 1845, in the Commune of Teilleul (Manche), by M. Failli.—M. Larrey communicated a letter from M. Tholozan, Persia, on the plague, which he shows to have sprung up and died out in certain localities in the absence of restrictive measures. The French Government have sent Dr. Zuber to Astrakhan to study the disease.—On a new type of anomalous stems, by M. Cornu. This relates to supplementary cortical ligneous bodies anastomosed together, in certain *Sempervivum* and *Crassula*; their *rôle* seems to be to strengthen the fragile stems when they have to bear a large inflorescence.—On the amyloid granules of the yolk of eggs, by M. Dareste. He urges reasons for thinking the granules starch, and not lethicine (as has been affirmed). They are difficult to study.—On the correspondence between Chladni's acoustic figures and liquid systems produced on vibrating circular plates, by M. Decharme. It was stated that the Italian Society of Natural Sciences had formed a service of antiphylloxeric vedettes, to survey vineyards, and report the first suspicions of the disease.—On a new catadioptric telescope, by MM. Paul and Prosper Henry. The tube of a reflecting telescope is hermetically sealed by means of a thin crown glass lens of the same size as the mirror, very slightly concave, and not detracting from the optical power of the instrument. The instrument has given remarkable results.—Demonstration of the convergence of a double series met with by Lamé in his researches in mathematical physics, by M. Escary.—On the integration of a differential equation, by M. Halphen.—On the determination of the imaginary roots of algebraic equations (concluded), by M. Farcas.—On a system of light signals permitting the determination of differences of longitude between different stations not connected electrically, of a triangulation of parallel or meridian, by M. Liais. This system has been adopted in Brazil. M. Liais shows the advantage of making rhythmic signals commanded by a clock, and received at the other station by a chronographic inscription. In this way there is no variable personal equation to be concerned about. The point is to make a screen, with aperture, beat seconds (*e.g.*, by a clock commanding an electro-magnet) so as to give an instantaneous appearance of light each second. The receiving-station may either register with a chronograph or (better) compare directly the clocks of the two stations by the method of coincidences; a screen, with aperture, passing before the objective of the telescope, and the light seen only when the two clocks are in coincidence.—On the distribution of heat on the sun's surface; results of the first series of observations at the Imperial Observatory of Rio de Janeiro, by MM. Cruls and Caille. These researches fully confirm the results got by Secchi, though there are some differences as to absolute value of radiations. The absolute radiation of the whole disk was estimated at $\frac{22}{100}$, the absorption, $\frac{78}{100}$.—Determination of the approximate value of a coefficient relative to the viscosity of water, by M. Geoffroy.—New experiments on telephones without a diaphragm, by M Ader. He gets better results than with an ordinary telephone from a thin piece of wire (with small helix round it) fixed at one end in a wooden board (a microphonic speaker being used), better if the two ends are in contact with metallic masses. Voice is reproduced, too, from a mere bobbin without core, if the windings are very free. He supports M. du Moncel's view that the sounds in the telephone are from contractions and elongations of the magnetic rod.—M. Du Moncel described some observations in the same sense.—M. Resis presented a note on a hydro-electric telephone, in which the variations in intensity of the current are reproduced by variations in resistance of a liquid column, which serves as receiver (without any electro-magnetic organ).—On new combinations of hydrochloric acid with ammonia, by M. Troost. The two specified are the tetra and hepta-ammoniacal chlorhydrates.—Combinations of phosphuretted hydrogen with cuprous chloride, and its determination in gaseous mixtures, by M. Ribau.—On the crystalline form of combinations of stannmethyls and their homologues, by M. Hiertdahl.—On a new process of treatment, by the dry way, of iron and copper pyrites, by M. Simonin. This relates to Mr. Holloway's method.—On the state in which precious metals are found in some of their combinations; ores, rocks, products of art, by MM. Cumenge and Fuchs.—On the constitution of coal, by M. Guignet. He treated powdered coal with phenol, nitric acid, &c. With the latter he obtained, *inter alia*, *trinitroresorcine* (oyxpicric acid), probably from resinous or waxy matters retained in the coal. No resorcine was found.—On alcoholic fermentation, by MM. Schutzenberger and Destrem. Yeast prevented from developing and multiplying still retains its power of decomposing sugar; and yeast acting on sugar deassimilates more nitrogen than that kept in presence of water, but without sugar and oxygen.—On the determination of glucose in the blood, by M. Cazeneuve. He criticises Bernard's method (by cupropotassic liquor) as inexact, and thinks the study of glycæmia should be taken up again when a more precise method is acquired.—On the derivatives of normal methyloxybutyric acid, by M. Duvillier.—Analysis of some fodders, and observations on damage done to Italian beans by weevils, by M. Grosjean.—Comparative evolution of the male and female genital glands in the embryos of mammalia, by M. Rouget.—On the non-excitability of the grey cortical substance of the brain, by M. Couty.—Note on the history of peduncular expansions, by M. Bitot.—On the nature of the albumen of hydrocele, by M. Bechamp.—Experimental researches on a leptothrix found during life in the blood of a woman attacked with grave puerperal fever, by M. Feltz.—On the modifications of the physical properties of starch, by M. Musculus.—On ferruginous particles observed in dust brought by a blast of sirocco to various parts of Italy, by M. Tacchini. This was in February. He thinks the phenomenon of so-called *meteoric* spherules must in many cases be attributed to this phenomenon.—Morphology of the dental follicle in vertebrates, by MM. Legros and Magitot.—Pathogeny and treatment of intermittent convergent strabism, without operation, by use of mydriatics and myosics, in children, by M. Boucheron.

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