THURSDAY, JUNE 12, 1879

EVOLUTION, OLD AND NEW

Evolution, Old and New; or, The Theories of Buffon, Dr. Erasmus Darwin, and Lamarck, as compared with that of Mr. Charles Darwin. By Samuel Butler. (Op. 4.) (London: Hardwicke and Bogue, 1879.)

THE present work will not add to the reputation of the author of "Life and Habit." It is, nevertheless, an interesting and useful book, inasmuch as it gives a pretty full account of the theories and opinions of several authors whose writings are almost unknown to the present generation of naturalists. The sketch of the lives, and the numerous quotations from the works of the celebrated men named in the title page, are instructive and sometimes amusing. Quotations are also given from Mr. Patrick Matthew, Étienne and Isidore Geoffroy St. Hilaire, and Herbert Spencer, illustrating their views on evolution, and giving altogether a fair idea of the progress of modern thought on this important subject. But the main object of the book is to show that all these authors have been right, while Mr. Charles Darwin is altogether wrong; and that the works of the former contain a more philosophical, more accurate, and altogether superior view of the nature and causes of evolution in the organic world than those of the latter.

Mr. Butler finds in all the writers whose views he advocates, opinions which agree more or less closely with those so ingeniously and forcibly developed by himself, and to which full justice has already been done in the pages of NATURE (vol. xix. p. 479). No one can object to his adducing these points of agreement to fortify his own position, or to his arguing that his own hypotheses, thus supported, form an important and even a necessary supplement to the theory advocated by Mr. Darwin. But he goes much further than this, and maintains that the action of external conditions, combined with the desires and habits of animals, are the all-powerful causes of evolution, and that "natural selection," or "survival of the fittest," is comparatively unimportant, and is quite unworthy of the position given to it by Mr. Darwin and his followers. In doing this he not only falls into much confusion as to the phenomena of variation, but indulges in an amount of petty verbal criticism, quite unworthy of the high reputation established by his previous work; and I believe that naturalists in general will endorse the remark in my review of "Life and Habit" (which Mr. Butler has, apparently under the impression that this volume refutes it, placed in a conspicuous position on the fly-leaf of his book), that "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."

In discussing the views and arguments of Buffon, Mr. Butler suggests that the numerous contradictory statements of this eminent writer are due to the necessity he was under of not arousing the enmity of the Church. He therefore adopts the method of directly contradicting himself whenever he has been a little too advanced. Over and over again he points out the evidence of the several families of animals and plants having each had a common ancestor, and he specially mentions the horse and the ass, man and apes, as having been thus derived. But he puts it all hypothetically, and then, to satisfy the Sorbonne and the public, he proceeds thus: "But no! It is certain from revelation that all animals have alike been favoured with the grace of an act of direct creation, and that the first pair of every species issued full formed from the hands of the Creator." These, and numerous other passages quoted, certainly support the theory that many of Buffon's statements are ironical; and that while himself a firm believer in the development of all organisms from common ancestors, he purposely contradicted himself sufficiently to prevent the suppression of his work as being opposed to religion.

Most interesting among the quotations from Buffon, however, are those which show how near he was to seizing upon the idea of "selection" as a means of modifying organisms. Thus he says:—"The dog is short-lived; he breeds often and freely; he is perpetually under the eye of man; hence when—by some chance common enough with nature—a variation or special feature has made its appearance, man has tried to perpetuate it by uniting together the individuals in which it has appeared, as people do now who wish to form new breeds of dogs and other animals." And again, in discussing the origin of our cultivated fruits, &c., he says: "It was only by sowing, tending, and bringing to maturity an almost infinite number of plants of the same kind that he was able to recognise some individuals with fruits sweeter and better than others." Here he clearly recognises the selection of individual variations as the source of varieties, and the necessity for breeding or growing on a large scale, in order to obtain such individual variations as are required. But he never laid hold of this idea with any firmness; for we find him elsewhere dwelling on the influence of change of climate, food, and treatment, as having produced the changes in domestic animals and cultivated plants; especially change of climate while accompanying man in his migrations, and the action of these changes on habits "influencing their natures, instincts, and most inward qualities."

We next come to Dr. Erasmus Darwin, of whose life, writings, and opinions a very interesting account is given, and who is an especial favourite of Mr. Butler on account of his views as to the transmission of memory and habit from parent to offspring, and as to the existence of sensation and voluntary motion in plants, although he laid more stress on imitation and instruction than on inherited habits, and in this departs widely from Mr. Butler. Dr. Darwin anticipated Lamarck in arguing that the transformations of animals "are in part produced by their own exertions in consequence of their desires and aversions, of their pleasures and their pains, or of irritation or of associations; and many of these acquired forms or propensities are transmitted to their posterity." He also had a glimpse of the mode of action of sexual selection; for, speaking of the spurs with which the males of many game birds are armed, and which they use in fighting, he says: "The final cause of this contest among the males seems to be that the strongest and most active animal should propagate the species, which should thence become improved." We cannot see, however, that he had any clear notion of the general action of the law of the

survival of the fittest, nor of the important part it necessarily plays in the accumulation and perpetuation of variations, however these may be caused. In this respect he was probably not so enlightened as Buffon.

Lamarck's writings are very largely quoted and his opinions fully illustrated; and we freely admit with Mr. Butler that, as a thorough and consistent evolutionist, he was not inferior to Mr. Darwin himself. But although he clearly saw the *fact* of evolution, and almost demonstrated the reality of the fact by a variety of arguments and a wealth of observation, yet, so far from adducing any adequate *causes* for evolution, he was actually inferior to his predecessors Buffon and Dr. Erasmus Darwin, since he appears to have had no glimpse of the way in which domestic races have actually been produced by human selection, and still less of the action of the law of the survival of the fittest on animals and plants in a state of nature. Everything he imputes to changed conditions and changed habits, developing new desires in animals and inducing new courses of action. He dwells much on the time required for these changes, and considers that we have a practically unlimited amount at our disposal, remarking that "a time infinitely great *qua* man is still infinitely short *qua* nature."

Lamarck is exceedingly vague in his statements as to the cause and mode of change. After describing the different kinds of locomotion, walking, leaping, flying, swimming, and the great need of these powers of movement to most animals, he adds: "Since, then, the power of locomotion was a matter affecting their individual self-preservation, as well as that of their race, the existence of the want led to the means of its being gratified." He does not seem to have perceived the struggle between individuals of the same species owing to their excessive numbers, but only the struggle between distinct races; as when he says: "The strongest and best armed for attack eat the weaker, and the greater kinds the smaller. Individuals of the same race rarely eat one another; they war only with other races than their own." He also refers to the excessive multiplication of the smaller kinds of animals, and shows how their numbers are limited, but he never observed that the race was thereby invigorated and might even be modified. He sums up his theory in the following three propositions:—

"1. That every considerable and sustained change in the surroundings of any animal involves a real change in its needs.

"2. That such change of needs involves the necessity of changed action in order to satisfy these needs, and, in consequence, of new habits.

"3. It follows that such and such parts, formerly less used, are now more frequently employed, and in consequence become more highly developed; new parts also become insensibly evolved in the creature by its own efforts from within."

These arguments are repeated in a variety of ways, and are applied to explain the origin of all our breeds of dogs and other domestic animals, as well as of all wild species; and he evidently had no notion that though these may be real causes, they would be utterly inadequate to produce any such effects as we see in nature without the accumulating power of natural selection. Mr. Butler, indeed, maintains that this power is implied in Lamarck's reasoning. He maintains "that one of the most important conditions of an animal's life is the relation in which it stands to the other inhabitants of the same neighbourhood—from which the survival of the fittest follows as a self-evident proposition." And he adds: "Lamarck would not have hesitated to admit that, if animals are modified in a direction which is favourable to them, they will have a better chance of surviving and transmitting their favourable modifications."

But it is clear that Lamarck neither saw it nor admitted it; and his theory is therefore radically deficient. And he evidently sees this deficiency himself, for he says that frequent crosses with unmodified individuals will destroy the effect produced, and that therefore isolation is necessary.

We come next to Mr. Patrick Matthew, who in 1831 put forth his views on the development theory in a work on arboriculture; and we think that most naturalists will be amazed at the range and accuracy of his system, and will give him the highest credit as the first to see the important principles of human and "natural selection," conformity to conditions, and reversion to ancestral types; and also the unity of life, the varying degrees of individuality, and the continuity of ideas or habits forming an abiding memory, thus combining all the best essential features of the theories put forth by Lamarck, Darwin, and Mr. Butler himself. The following quotations illustrate Mr. Matthews's views:—"As the field of existence is limited and preoccupied, it is only the hardier, more robust, better-suited-to-circumstance individuals who are able to struggle forward to maturity, these inhabiting only the situations to which they have superior adaptation and greater power of occupancy than any other kind; the weaker and less circumstance-suited being prematurely destroyed. This principle is in constant action; it regulates the colour, the figure, the capacities, and instincts; those individuals in each species whose colour and covering are best suited to concealment or protection from enemies, or defence from inclemencies or vicissitudes of climate, whose figure is best accommodated to health, strength, defence, and support; in such immense waste of primary and youthful life those only come forward to maturity from the strict ordeal by which nature tests their adaptation to her standard of perfection and fitness to continue their kind by reproduction." He then goes on to show how this law tends to the production of almost uniform groups of individuals which we term species, and then adds: "This circumstance-adaptive law operating upon the slight but continued natural disposition to sport in the progeny, does not preclude the supposed influence which volition or sensation may have had over the configuration of the body." This, he says, is a matter to be inquired into, as well as "its dependency upon the preceding links of the particular chain of life, variety being often merely types or approximations of former parentage; thence the variation of the family as well as of the individual must be embraced by our experiments." These, and many other passages, show how fully and clearly Mr. Matthew apprehended the theory of natural selection, as well as the existence of more obscure laws of evolution, many years in advance of Mr. Darwin and myself, and in giving almost the whole of what Mr. Matthew has written on the subject Mr. Butler will have helped to call atten-

tion to one of the most original thinkers of the first half of the 19th century.

The last four chapters of the work are devoted to a critical comparison of the theories of Mr. Darwin with those of Lamarck, Dr. Darwin, and Buffon, greatly to the disadvantage (in Mr. Butler's opinion) of the former. Much of this criticism, however, is merely verbal, and is quite valueless; much of it, also, is founded on a confusion as to the meaning of such terms as "variation" and "variety," and on an inability to grasp the fact of the extent and universality of the individual variations of organisms; while another portion arises from taking the hypotheses of Lamarck as established facts. Of these several classes of unsound criticism we will give a few examples.

Mr. Butler first quotes (p. 339) numerous expressions from the "Origin of Species," referring to our great ignorance of the laws of variation, and our total ignorance of the cause of each individual difference; and then speaks of Lamarck "having established his principle that sense of need is the main direct cause of variation," and that variations thus engendered are inherited, which sufficiently accounts for all the facts. If Lamarck had "established" anything of the kind, Mr. Darwin and all evolutionists would certainly have followed him, but he nowhere proves or even attempts to prove his "principle," but merely states it as an "hypothesis" to account for facts which he saw no other way of explaining. Again, Mr. Butler himself says, that owing to the conditions of life being permanent for long periods—"The thoughts of the creature varying will thus have been turned mainly in one direction for long together; and hence the consequent modifications *will also be mainly in fixed and definite directions* for many successive generations; as in the direction of a warmer or cooler covering, &c. . . . It is easy to understand the accumulation of slight successive modifications *which thus make their appearance in given organs and in a set direction.*" The passages which I have italicised look like statements of fact—of what actually occurs; yet no such facts have ever been made known. If the law thus stated had been sufficiently effective to produce any permanent variations, breeders would sometimes have made use of it. Yet they certainly do not do so, whereas they do systematically and very successfully make use of selection. According to the above theory Australian sheep must have their thoughts constantly turned in the direction of less wool owing to the great heat of the climate, and a much larger proportion of each succeeding generation should have thin and scanty fleeces than occurs in England, especially in the tropical colony of Queensland, which, in proportion to its population, produces as much wool as the other colonies. If Mr. Butler could adduce, on good authority, such a fact as this, he would have some evidence in his favour, instead of which he can only make suppositions. The fantail and pouter pigeons, the crested Poland fowls, and all other strange domestic varieties, have been produced by selection of variations or sports which occurred among animals all subject to the same tolerably uniform conditions; while no proof has ever been given that anything more than very slight changes can be produced and perpetrated by change of conditions unaided by some kind of selection.

Mr. Butler's want of appreciation of what variation and natural selection really are, is shown by his referring to "the fact that *one* in a brood or litter, is born fitter for the conditions of existence than its brothers and sisters"—by his continually laying stress upon Mr. Darwin not having shown "how the individual differences first occur"—by his thinking that because natural selection is not the cause of "variation" it is therefore not the cause of "modification" or of a "variety" or "species"—and by his hardly ever referring to the enormous multiplying powers of animals, and the consequent extermination of a much greater number annually than the whole average living population. In my former article on the works of Mr. Murphy and Mr. Butler (NATURE, vol. xix. p. 477) I have shown how we may look at the whole population of a species at any given time as divisible, with regard to any one of its characters, into a more and a less developed moiety, and I believe that this mode of viewing the question will at once almost entirely remove the coincident-variation-in-the-right-direction difficulty, which forms the great stumbling-block of almost all the opponents of Mr. Darwin.

The difficulty as to the "cause of variation" also disappears from this point of view, for "variation" is seen to be synonymous with "want of perfect identity" between any two organisms, and this is clearly due to the almost infinite complexity of structure and minuteness of parts of all living things and the absolute impossibility that any two can have passed through an identical series of conditions or even had an origin in two identical germs. We see infinite variety arise in the inorganic world where there is a far less complexity of structure or variety of conditions. Even among the sands on the sea-shore no two grains are probably so nearly identical that a good microscopist could not detect a difference; while it is certain that nowhere in the world are there two hills or two rivers with any approach to complete similarity, though the entire process by which many of them have been produced must have often been almost identical. Variation, such as *always* occurs between the individuals of a species, is therefore an ultimate fact of nature which wants no further explanation than that we cannot even conceive it to be otherwise. We may indeed conceive more likeness on the average than actually exists, but we cannot really conceive of *perfect identity* between individuals formed and developed as are animals and plants. We may, on the other hand, seek for the causes of unusual or abnormal variation, and Mr. Darwin has suggested several. It is quite possible that those suggested by Lamarck and Mr. Butler may also be real causes, but they have certainly not been proved to be so; and even if they had they would not in the least affect the law of natural selection which *accumulates and perpetuates variations*, however they may have been produced.

The numerous verbal criticisms or quibbles in which Mr. Butler indulges are quite unworthy of his subject. When Mr. Darwin says, "Variation will cause the slight alterations," Mr. Butler remarks that this is the same as saying "Variation will cause the variations." Again, Mr. Butler maintains that the term "conditions of existence" is identical with or includes "survival of the fittest," which is identical with "natural selection." Therefore, when Mr. Darwin says "natural selection is

the main but not the exclusive means of modification," he must mean "the conditions of existence are the main," &c., &c.; therefore he really agrees with Lamarck, whose opinions he has called "erroneous!" Again, because Mr. Darwin has once used the term *nature*, metaphorically, for natural selection, our author seizes hold of it for a little ridicule, thus: "When, therefore, Mr. Darwin says that natural selection is the most important, but not the exclusive means whereby any modification has been effected, he is really saying that nature is the most important means of modification—which is only another way of telling us that variation causes variations, and is all very true as far as it goes." In the same style the use of the term "by *means* of natural selection" is criticised, and the use of "natural selection" at all, when "survival of the fittest" is admitted to be a more accurate term; and Mr. Butler seems to think that if the latter term were always used, a great deal of the force of Mr. Darwin's arguments would be lost. I venture to assert, however, that every argument can be stated with equal accuracy and effect, using only "survival of the fittest;" but there is this great advantage in using the term "natural selection," that it keeps before the mind the striking analogy and almost identity between the action of man and of nature in modifying species, an identity that was never seen by any of the older writers, but which was first clearly apprehended by Mr. Patrick Matthew, and first fully worked out by Mr. Darwin himself.

In the last chapter Mr. Butler takes the celebrated case of the Madeira wingless beetles to test the respective theories of Lamarck and Charles Darwin, and he could hardly have made a more unfortunate choice. According to Lamarck, he says, when a beetle found the wind taking it in a wrong direction, *which it knew would be fatal to it*, it ceased flying, and thus, by long-continued disuse, gradually lost its wings. Here we have the assumption that such insects as beetles know beforehand that if blown out to sea they will be drowned, an assumption for which not one particle of evidence is adduced, while, as every entomologist knows, pages might be filled with facts proving that insects of various orders do not possess any knowledge of the kind, but year after year go recklessly to their death by myriads.

Hardly less weak than this statement of the Lamarckian theory is the objection to that of Mr. Darwin, which is as follows:—"For Mr. Darwin cannot mean that the fact of some beetles being blown out to sea is the most important means whereby other beetles come to have smaller wings—that the Madeira beetles, in fact, come to have smaller wings, mainly because their large-winged uncles and aunts go away." Though Mr. Butler has tried to put this so as to look like an absurdity, it is strange that he cannot see that it contains an important truth. If the "large-winged" beetles go away, the small-winged remains to breed, and each succeeding generation will have, on the average, smaller wings than the last; and if, so long as any fly at all, the larger-winged continue to "go away," at last none will fly, and then, the wings being unused, will become abortive and rudimentary. As a crucial case, and to compare the power of the two theories as agents of change, let us suppose them both applied to the human inhabitants of Britain. First we will suppose all the men and women above the average height to go away year by year to Australia or elsewhere, while those under the average height remained. Does Mr. Butler doubt that at the end of, say, ten generations, the average height of English men and women would have been considerably reduced? This would be selection pure and simple. Now for the Lamarckian theory. Let all the people be taught (and believe) that to be short is to be beautiful and virtuous, and let all doors and all public vehicles be made low to suit short people and inconvenience tall ones, and moreover, let short people alone be eligible for a number of posts of honour and dignity, there would thus be created a general desire to be short oneself and to have short children, and the Lamarckian principle would be brought fairly into play. Now supposing that no artificial selection of any kind was practised, and that, owing to the prevalence of high moral principle, the health, lives, and affections of tall people were valued and cared for as much as those of their more favoured short fellow-countrymen, does Mr. Butler seriously maintain that at the end of ten generations any perceptible effect would be produced on the average height of the people; or that anything like the same amount of effect would be produced as by the other experiment? But if not, then "selection," whether natural or artificial, is the *main cause* or *means* of modification; the plain reason being that it accumulates differences which actually exist, whereas, by the other mode, you must produce an increase or diminution of these differences by causes which have not been proved to act at all, and which, even if they do produce any effect, can only do so with extreme slowness.

In conclusion, then, we may admit the possibility that the causes of variation adduced by Lamarck, as well as those so well set forth by Mr. Butler in his "Life and Habit," are real causes; we may further admit that some or all of these causes are essential to the origin and development of the more important organs of animals, and that they constitute the chief supplementary agencies the existence of which Mr. Darwin himself recognises; but, even admitting all this, we still maintain that they would be all powerless to effect great or permanent modifications without the accumulating action of natural selection, which may therefore be truly described as the "means" by which alone the "origin of species" has been actually brought about.

ALFRED R. WALLACE

OUR BOOK SHELF

Elementary Arithmetic and How to Teach It. By George Ricks, B.Sc. (London: Isbister, 1879.)

MOST school-books, especially those of an elementary character, are mere poison, and very disagreeable poison too. But Mr. Ricks has supplied us in this volume with really healthy food. We heartily recommend it to all young teachers, and believe, moreover, that many who deem themselves experienced may obtain from it several useful hints. In Part I. the teacher of an infant school is shown how to proceed with his pupils; in Parts II. and III. similar information is afforded to the teacher in a junior school; Parts IV. and V. relate to senior schools; Part VI. is devoted to advanced scholars. We have discovered nothing very remarkable in the latter half of the book; indeed, Mr. Ricks seems to get a little beyond himself as soon as he advances from the juniors to the seniors. This, however, is a matter of small consequence.

It is the elementary part of a subject which is always so badly taught, chiefly because it is a general belief that any one can teach a child. We are, therefore, pleased to welcome in Parts I., II. and III., an exceedingly clear statement as to what sections of arithmetic should be taught, and how they should be taught to young children.

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.*]

A Machine for Drawing Compound Harmonic Curves

IN NATURE (vol. xx. p. 103) there appears an abstract of a paper by Prof. E. W. Blake on a machine for drawing compound harmonic curves. Prof. Blake is doubtless not aware that this machine is based on a plan proposed by Prof. Perry and myself in our paper on "the music of colour and visible motion," read before the Physical Society, November 23, 1878. In that paper, after the description of our own motion-compounder, will be found the following :—"But it is possible that in our new machine we shall adopt a totally different plan, and one which we think is new. If the two extremities of a long rigid rod have parallel motions perpendicular to the rod, the middle of the rod has a motion equal to half the sum of the extremities. Thus the parallel motions of 2, 4, or 2^n points may be compounded. Similarly for 3 points, one-third of the sum of parallel motions is obtained from the centre of a rigid triangular piece of which the points are the corners; so that by bars and frames of simple construction it is easy to get the sum of the parallel motions of any number of pieces."

But I think that this method which we suggested, and which is the one now described by Prof. Blake, is inferior to the roller-cam principle employed by us in the instrument we constructed, and explained to the Physical Society, in that with the latter in its complete form we can alter, while the machine is actually in motion, the *amplitude* as well as the *phase*, by any *desired* amount, of *any one* of the component vibrations. This facility, although not possessed, as far as I am aware, by any other motion compounder, is extremely desirable for the varied description of compound curves, whether these curves be merely intended, as in the ordinary forms of such instruments, to show the resultant of two or more vibrations, or be designed, in accordance with the plan of Mr. Perry and myself, to work on the emotions, in the rendering of a new genus of music, by the varied, yet controlled, motions of the body, or bodies, describing these curves.

W. E. AYRTON

Lightning Conductors

ON the night of Wednesday, May 28, shortly after ten o'clock, my father's house at Caterham, in Surrey, was struck by lightning. We had not noticed any thunder before going to our bedrooms, but shortly after doing so we saw a very vivid flash, followed by thunder after an interval of perhaps five seconds; three or four minutes after this there came with a terrific crash the flashwhich struck the house, and afterwards no more thunder or lightning sufficient to attract our attention while we were intent upon investigating the mischief done.

The house stands high, upon a hill upwards of 700 feet above the sea; it is somewhat higher than any house or other object in its immediate vicinity. Roughly speaking, it may be described as a square block surmounted by a steep tiled roof, the ridge of which runs north and south, and astride upon the ridge stand two chimney stacks of equal height; from one corner of the square block runs away an extension, the roof and chimney stacks of which are upon a somewhat lower level.

Upon the northern chimney stack, at its eastern end, was the lightning conductor, which consisted of the usual hollow rod ($\frac{1}{2}$ inch) at the top, continuous with a flat plaited rope of copper and zinc wire (1 inch in width), carried through glass insulating rings along the slope of the roof, over the rain-water gutter, and down the side of the house into the ground.

The flash first struck the lightning conductor, hurled the rod down, and shattered the chimney-pots and a little of the brick-work; it seems to have followed the chimney-stack down to the ridge of the roof, and there to have divided. That portion of the discharge which passed down the eastern slope of the roof seems to have followed the chain portion of the lightning conductor as far as the rain-water gutter (iron); this it slightly broke, and broke also two panes of glass immediately beneath it, but this portion of the flash could be traced no further, and dead leaves, &c., about the remainder of the lightning conductor would seem to indicate that none had passed down that, at least not any large quantity.

The greater part of the discharge seems to have utterly left the guidance of the conductor, and to have first followed the lead flushing of the chimney stack down the western slope of the roof; a foot or so below the end of the flushing the roof was perforated, and the tiles broken and thrown down; thence, without any disturbance of the intervening tiling the discharge leapt a distance of some 15 feet, perforated horizontally a 9 inch-brick wall, covered on its exterior by weather tiling, and so reached an iron water cistern immediately within this wall. The woodwork about the cistern was started, but not much splintered; thence the discharge passed downwards by way of the water pipes, down two stories to a force-pump in the scullery, and thence, probably, by the pipes, down into the subterranean water-tanks.

But the pump in the scullery was provided with a pipe and tap over the sink, and there was also a small "tell-tale" pipe from the cistern above, designed to show when the cistern had been filled. This also terminated over the sink. Along these some part of the discharge was led, and not being safely conducted away, threw down and shattered the slate about the sink.

The perforation in the brick wall was circular, large enough to easily admit one's finger, and was blackened on its interior; when first found, eight or ten minutes after the occurrence, it was still quite hot.

The practical question that presses for an answer is, what did the lightning conductor do for us? Its selection as the point struck seems as though it brought the discharge upon the house; certainly it, although I believe of the usual construction, was utterly inadequate to carry off, or even direct the course of, the discharge, for a most copious and violently destructive discharge passed altogether from it and down over the opposite or western slope of the roof.

On the other hand, the iron water-tank and its pipe system proved adequate and safe conductors of the electric fluid, which left not the smallest trace of its passage along them until it reached the scullery sink, and there would presumably have done no harm, had it not been for the existence of the tap pipe, which led a portion of it astray. And on both faces of the house the iron gutters and rain-water pipes seem to have proved efficient conductors, for no violently destructive effects were manifested, save the breaking of two panes of glass after the electric fluid reached them. But as I am no electrician I can add nothing useful to the bare narration of the facts. I should add that the soot was very completely and violently ejected into the rooms from the chimneys of the stack struck.

CHARLES S. TOMES

P.S.—Subsequent closer examination of the portions of the lightning-conductor showed that there were traces of fusion of the hollow copper rod at its junction with the rope which had been inserted into it; the sectional area of copper here available for carrying the discharge seems to have been less than that in either the rod or the rope. Slighter traces of fusion existed here and there in the whole length of the rod. In its passage round the water-gutters the discharge cracked each one at its junction with the next segment; that is to say, the iron was cracked wherever the interposition of a little red lead to make the joint tight offered increased resistance to its passage, and the lead flushing of the roof was fused below the perforations in the brick wall, indicating that the whole discharge did not go through the wall to the iron water-tank.

THE effects produced by the recent thunderstorms are so interesting and instructive that I think it worth while to record the results of investigations which I have made personally or had made for me.

On May 31 there was published in the *Times* particulars of the damage done to the church at Laughton-en-le-Morthen, and the conductor was described as a thin corrugated tube of copper made

in sections, apparently fastened together, but in fact disconnected and with bad earth contact.

A disastrous result followed the use of a similar conductor in the case of Mr. Osbaldiston's house near Sheffield, in the storm of Tuesday, the 5th instant, when his house was completely wrecked.

Here the flash, after coming partly down the tube and breaking the worse than useless insulators, left the conductor about nine feet above the ground, and passed through a thick wall to the gas-pipe, behind the drawing-room mirror, which it smashed to atoms, broke some Sèvres vases on the mantelpiece, melted the gas-pipe and set fire to the gas, destroyed the gilded ceiling and cornice, ripped up the floor above, destroyed the furniture, and in short did damage estimated at about 500*l*.

In this case the lightning having left the conductor to go to the gas-pipe shows that the corrugated tube contained too little metal—though it was seven-eighths of an inch in outside diameter, and that the earth contact of the tube was not so good as the gas-pipe.

The next case which I have had examined was Mr. Tomes's house at Caterham, which was struck on Wednesday, May 28, about 10 o'clock P.M. The supposed conductor here was a woven band about an inch wide, made of twelve small copper wires, with two zinc wires interwoven! forming a worse conductor than the copper tube. The top terminal appears to be made of a soft alloy of tin and zinc, with a small steel wire in the centre! This was fastened on to a thin copper tube measuring half an inch outside and about six feet long.

To the lower end of the tube the band was attached, and here again the abominable glass insulators were used. The tube was fixed to a chimney and the band carried over the roof, coming in contact with the iron water spout on its way down. The end of the band was simply struck into the chalky soil to a depth of about a foot, so that it formed a very bad earth contact.

The lightning struck the point (the alloy round the steel has been melted or broken off some time ago, for the steel point is rusty), and passed down the band to the iron water-spout, which went round the eaves of the roof till it came to some lead flushing on the other side of the house away from the conductor; it then went up the lead, which it partially fused, and entered a small water-cistern in the attic, which luckily was connected by an iron pipe leading to a large iron tank under the ground, which thus formed an admirable earth contact. The distance to the conductor to the top water-cistern was about fifteen feet.

The damage done in this case was limited to little more than breaking two panes of glass and the glass insulators, and it proved that the band did not form an efficient conductor.

The third case which I have to report occurred on the same afternoon as that at Sheffield. The church of Saint Marie, Rugby, was struck, and might have been burned to the ground, had not the workmen employed in repairing the spire taken shelter in the church from the coming storm. They had been on the top and saw a dense black cloud approaching, and luckily came down, and had not been long in the church which, at 3 P.M. was so dark that they could scarcely see each other at a distance of a couple of yards, when a terrific crash was heard, and as suddenly the gas under the organ loft was lighted and the woodwork began to blaze. The men got the fire extinguished, and found that the lightning had melted the soft metal gas-pipes at a T-piece joint.

The spire is 212 feet high, and when it was built a conductor of copper-wire rope half an inch in diameter had been fixed. This has been repaired at the top by the present contractor attaching to it about fifty feet of rope of seven-eighths diameter. No insulators had been used, and so far as could be ascertained by a short inspection, the conductor appeared complete.

About half way up the spire there is a peal of eight bells. These have iron wires about one-eighth of an inch diameter leading from the clappers down to the back of the organ loft, but terminating in the spire a short distance from an iron gas-pipe about one inch in diameter fixed against the wall of the organ loft.

In this case I think that the rope conductor carried off part of the flash, and that part came down the bell-wires and went through the wall to the gas-pipe, for part of the stone wall next the organ, about a foot in diameter, was exploded off and thrown into the organ loft. The flash then partly ran along the iron pipe and melted the soft metal pipe, which is three-quarters of an inch diameter and a sixteenth thick, and set fire to the gas, and the remainder of the flash went to earth.

It was fortunate that no one was working the chimes, for if he had he would certainly have formed part of the circuit and been killed. The church, however, escaped with very slight damage.

R. S. NEWALL

Bud-Variation in Bananas

IN my garden there is a large plant (planted about eleven years ago) of a variety of banana, distinguished by purplish stems and petioles, red fruits, and by a very peculiar flavour of the latter. From the centre of this plant, covered by the rotten stems of former years, there are now growing green stems, with green petioles; one of them has already produced fruits, which were green when immature, and yellow when ripe, and the flavour of which I found to be but slightly altered. All the young stems growing from the circumference of the plant are purplish.

May not many of the varieties of bananas have been produced by bud-variation?

FRITZ MÜLLER

Itajahy, April 7

Fertilization of Erica carnea

ALL our Vaccinieæ and Ericaceæ, with tubular corolla, as far as hitherto known, are adapted to cross fertilization by Apidæ; for instance, *Vaccinium myrtillus* and *Vitis idæa*, *Arctostaphylos uva ursi* and *Erica tetralix*. I was, therefore, much struck yesterday by the observation that *Erica carnea* is abundantly visited and cross fertilized by a butterfly, *Vanessa cardui*, but not by a single bee. And, indeed, the colour and structure of this flower corresponds far more to the taste and habits of butterflies than of Apidæ. Like all other alpine flowers adapted to butterflies (*Saponaria ocymoides*, *Silene acaulis*, species of Dianthus, etc.), *Erica carnea* is also of a gay red (pink) colour, and its inclined tubular corolla is so narrowed downwards that its small opening is almost completely occupied by the anthers projecting from it. Hence butterflies which are most distinctly attracted by the colour of this flower, as also by its structure, are alone able easily to insert their thin proboscis into its corolla and to reach its honey.

Wherever in a sunny place of the Albula Valley *Erica carnea* is in full flower, and I observed more than thirty such places yesterday and to-day, *Vanessa cardui* is frequently found visiting and fertilizing it—so frequently that sometimes five or six specimens are visible at the same time.

HERMANN MÜLLER

Bergün, in the Albula Valley, June 3

Early Sun-Spot Records

To the very small number of non-Chinese observations of solar spots prior to the invention of the telescope we may add one, which I find in the voyages of Henry Hudson, published by the Hackluyt Society. He appears to have noticed such a phenomenon on March 21, 1609. Hudson says, "Then we observed the sunne, having a slake, and found our heigth to bee 70 deg. 30 min." A note says, the word slake, as a substantive, seems to be a north country word, meaning according to Brocket "an accumulation of mud or slime from slijck, cœnum, lutum." It will be remembered that there is a paper by Mr. Williams, the late Secretary of the R.A.S., on Chinese observations of solar spots in the monthly notices for April 1873. Mr. Williams's translation records forty-five such between the years A.D. 301 and 1205 inclusive. It will be seen that this list does not correspond with that given in Mr. Hosie's communication to NATURE of June 5. In the above interval Mr. Hosie records twenty-four spots not mentioned in Mr. Williams's paper; and in the latter there are nine recorded that do not occur in the longer list of Mr. Hosie. The number of naked eye records of sun-spots that may be brought to light will never be sufficient to carry back Dr. Wolf's sun-spot periods previous to the introduction of the telescope. There is, however, another Chinese record that it would be interesting to see translated. Mr. Williams, in the paper referred to, says, "these observations are continued in the supplement to Ma Twan Lin's Encyclopædia," and that he had found in the history of the Ming dynasty many observations of solar spots, the latest being November 29, 1638.

SAMUEL J. JOHNSON

Upton Helions Rectory, Crediton, June 9

A Meteor and the Weather in New Caledonia

ACCORDING to my promise to send you accounts of any remarkable meteors that I may see here, I now notify one which appeared yesterday evening, April 13, at 20 minutes past 6 P.M. We were driving slowly home from Ansevata, near Noumea, when a splendid brilliant *pure white* meteor fell from the zenith, about 30°, quite perpendicularly and slowly. It burst into three pieces, and instantly disappeared. From Noumea its direction was due south, and in size it appeared four or five times larger than Venus. We heard no noise; the sound of the carriage wheels grinding on the road would have prevented any but a rather loud one being audible. It was not dark, but twilight.

We have been suffering much from unusual heat, and the atmosphere is surcharged with electricity. Heavy storms brew in the mountains, but we have been free from them here in Noumea. Heavy rain squalls gather to the southward, and on reaching the south point of New Caledonia either divide and run along the mountains on one side or the circling reef on the other, or also pass solidly in either direction, leaving the peninsula of Noumea perfectly dry. E. L. LAYARD

Brit. Consulate, Noumea, April 14

Intellect in Brutes

MR. ROMANES has alluded to some of the peculiarities of my feline pets, but really those are by no means the most striking instances of their intelligence. My wife and I are devotedly fond of our cats, so much so as to afford amusement to our friends, and we are never tired of expatiating on their indications of intelligence. A pedigree book is kept, and any reader of NATURE desirous of possessing a kitten of an intelligent stock is welcome to one on three or four months' notice.

I wish to give one other story of them which seems to show that they are apt to indulge in revenge and to act in systematic co-operation to accomplish it. They are of very cleanly habits, and, save under the circumstances about to be narrated, have never given any trouble in this respect. But some time ago we had a visitor who had a strong and very badly-concealed dislike to them. The dislike was quite mutual. Very soon after the arrival of this visitor the cats became very objectionable on account of messes, and these were concentrated in and near the bedroom occupied by the object of their aversion. Their insanitary proceedings became so pronounced that it almost appeared as if they had invited all their feline friends in the neighbourhood to join in the establishment of a "night-soil tip." No amount of correction, aided by pepper of the most pungent kind, could stop it, and I most reluctantly determined upon a wholesale felicide. The visitor departed, however, before this was carried into effect, and immediately the nuisance ceased, and our cats resumed their original cleanly habits.

LAWSON TAIT

I HAVE perused with interest the admirable summary of the "Animal Intelligence" question by Mr. Romanes. On reading the article in question, it occurred to me that I had at hand some memoranda concerning animal intelligence which bear on the presence, not merely of *abstract* reasoning in dogs, but also upon the presence in dogs of traits of character remarkably resembling those we are accustomed to name "retaliation" and "revenge" in man. I now send you the jottings in question, obtained, I may add, from personal friends. About thirteen years ago, a now deceased medical man residing near Edinburgh, possessed a favourite collie, "Cheviot" by name. The incident I am about to relate, I may mention, was related to me by the son of the gentleman in question, both father and son, along with a perfectly disinterested party, having corroborated the facts. The then provost of the burgh in which "Cheviot" resided, had issued an interdict against unmuzzled dogs during the "dog days," and "Cheviot" submitted with no good grace to the operation of securing his jaws. Frequently "Cheviot's" master and the members of the family spoke in the dog's hearing, in no measured terms of the cruelty of the provost's order. But the end of the "dog days" came, and "Cheviot's" muzzle was removed. On the afternoon of the day of liberation, the provost called on "Cheviot's" master, to say, that in the morning he had heard a dog whining at his front door. The provost opened the door; "Cheviot" was in waiting, his muzzle in his mouth. One look at the provost, and the muzzle was dropped at his feet, "Cheviot" scampering off in the highest glee, as if delighted to have had the opportunity of laying the cause of his grievance at the door of his enemy.

The details were vouched for by the provost himself, also a medical practitioner in the burgh.

Here it seems to me you possess an incident of dog character explicable only on the supposition that there are germs in the canine philosophy of acts and traits fully developed in ours.

Incident number two deals with the doings of a retriever, some four or five years old, who, whilst bearing an implacable enmity to felines at large, had struck up a close friendship with a household cat, which, from kittenhood, had been associated with him. For sanitary reasons the cat was condemned to die. According to the orthodox method, puss was placed in a sack weighted with stones, and carried to the sea, "Keeper," the dog, following in the wake of the procession. The cat was duly thrown into the sea, "Keeper" waited to see if it would rise, but on seeing no signs of his feline friend, he at once dived for the sack, and landed it at an adjacent pier. Being met by the executioners and divining that puss was yet in danger, "Keeper" re-entered the water, sack in mouth, and swam across the bay to a point of safety, and landed his burden. Puss was spared in deference to "Keeper's" anxiety.

I can find still another example of extreme unselfishness in a mongrel dog, who, for some years before the death of an old deaf and blind companion, was accustomed to proceed to his resting-place, and bark in his ear to warn him of the presence near at hand of the milk which the kindly hand of the mistress of the house was accustomed to place for the delectation of both. This proceeding was repeated day by day, with unvarying regularity, and in its nature suggests strongly that the exercise of self-denial—amidst the obvious temptation of an easy acquirement of luxury—has to be placed to the credit account of the canine race. ANDREW WILSON

Edinburgh Medical School, June 6

WITH reference to the article in NATURE of 5th June, permit me to narrate an instance of "abstract reasoning" in a retriever that I was witness to last autumn.

Having shot a hare so slightly as to make it a long chase for the dog (a young one), I watched the retriever follow the hare over the open hills of Aberdeenshire for upwards of two miles until the chase was lost to view under a stone dyke. In a few moments the dog was observed to carry something in his mouth with which he disappeared over the dyke into a turnip field. "He has killed the hare and he is too tired to bring it back, so he is burying it," quoth the keeper, "we shall come up with it in the evening." The day's sport over, we made for the dog's burying ground, but the retriever, if you please, knew nothing about it; and careered wildly about in every direction except the right one. The keeper, Henry Ledingham of Tarland, Aboyne, having a remarkable gift of spotting fallen game, actually put his foot on the very spot among the turnips where the burial had occurred. After immense affectation of surprise the retriever was forced to unearth the hare. The hare, however, was a rotten old carcass of a hare, with no eyes and teeth, that the retriever had picked up and buried to save himself the pains of following the live hare. Perfectly conscious of his misdeed, the dog had given evidence of abstract reasoning in each stage of the transaction.

CHARLES BAILLIE HAMILTON

St. Stephen's Club, Westminster, June 10

I HAVE followed the discussion in your columns on "Intellect in Brutes" attentively, and I maintain that Mr. Henslow's distinction between man's power of abstract reasoning, and the reasoning of animals from objects present to the senses (a faculty they certainly possess, if the theory of deductive reasoning, that all inference is from particulars to particulars be accepted, which, however, cannot be proved), is perfectly valid, in spite of any accidental errors of illustration.

The fact that a cat or a dog subject their food to examination before eating it, does not most assuredly prove the possession of abstract powers of thought in the animal. Mr. Romanes here says:—"The motive of the examination being to ascertain which general idea of quality is appropriate to the particular object examined."

Here he attributes to an animal whose nature he does not fully understand his own process of thought, and this appears to me to be a constant source of error in the investigation of animal psychology. That brutes possess self-consciousness, introspection, imagination, abstract thought, cannot, I think, be proved. The

fact that animals possess faculties differing from those of man is an insuperable obstacle to a perfect analysis of their intelligences.

Name these faculties as you please, call them "inherited habit," "inherited memory," it is perfectly certain that man does not possess them. H. D. BARCLAY

WILLIAM FROUDE

THE death of Mr. W. Froude, F.R.S., is a loss to science that cannot well be estimated. For many years he laboured with great ability and success in a field of research that was beset with difficulties, and had previously been almost barren of results. He was educated at Westminster, and went from there to Oxford, where he distinguished himself in mathematics. After leaving Oxford he became a civil engineer, and assisted Mr. Brunel in railway and other engineering work. He retired from active professional life in 1846, but his love of applied science retained such a hold upon him that he never ceased to occupy himself with important scientific investigations, and the solution of practical problems of peculiar difficulty. His intimacy with Mr. Brunel led to his mind being directed towards the study of those laws of nature which govern the motion of floating bodies. Mr. Brunel had devoted himself, among other things, to the improvement and development of iron steamship construction. In the *Great Western* and *Great Britain* he had made great advances in this direction; while in the *Great Eastern* he showed that iron and steam power could be employed in the production of ships of practically unlimited dimensions, and that by means of these agencies all the advantages appertaining to increased size might be realised.

In designing ships of such exceptional character and dimensions, Mr. Brunel found little to guide him in judging of their behaviour at sea. They were so different to any vessels afloat whose behaviour and qualities might have been ascertained, that he was unable to appeal to experience, while the light of science was so feeble and doubtful as to afford him no aid. Nobody at that time knew anything of the laws upon which a ship's motion at sea depends. There was a large mass of traditional experience, but this was often at variance with fact, owing to phenomena which are familiar to seamen being regarded as absolute, and possessing a reality of existence as well as of appearance; while, as must be obvious, they are only relative in their character, and cannot be accurately defined without making due allowance for the position and motion of a ship, with reference to the sea. The rules and maxims that had been adopted upon such incorrect and distorted data, were either unimportant or misleading; they were of no value. Mr. Froude said, quite correctly, in 1861, that our shipbuilders, while extending their knowledge in other directions, seem to have guided themselves by rhetorical phrases or random speculations in this particular branch of their art, "so that when a new ship is sent to sea, her constructor has to watch her behaviour in a seaway, with as anxious and uncertain an eye as if she were an animal he had bred and was rearing, and hoped would turn out well, not a work which he had himself completed, and whose performance he could predict, in virtue of the principles he had acted on in its design."

Mr. Froude, at the request of Mr. Brunel, commenced in 1856 an investigation into the laws of motion of a ship among waves. This had been previously attempted by D. Bernouilli, Euler, Moseley, and others, but without success. None of these writers had realised the fundamental conditions of the action of wave-water upon a ship, viz., that the direction and intensity of the fluid-pressure at any point is continually changing, and that the direction of pressure is normal to the surface of equal pressure passing through that point. They based their theories upon hypotheses respecting wave-action that were all more or less erroneous, and prevented any useful result being realised. Mr. Froude's method of dealing with the subject was, first of all, to determine the manner in which a wave acts upon a ship; or, in other words, the mode of operation of the agency whose effects he wished to comprehend. In this he was completely successful, and proved in an unexceptionable manner the mechanical possibility of that form of motion known as the trochoidal sea-wave. On the assumption that the motion of each particle on the surface of a wave describes an exact circle, whose diameter is the height of the wave from hollow to crest—which agrees with the results of observation—and that the motions of all particles lie in vertical planes which cut the wave-ridges at right angles, he deduced the theory that the form of the wave would be trochoidal, and that the periodic time would be equal to the time occupied by a heavy body in falling through a height equal to the circumference of a circle whose diameter is the length of a wave. It also followed that all sub-surfaces of equal pressure would be trochoids of the same length as the surface-wave, but of a height which would diminish with the depth in accordance with the equation $\frac{r_o}{r_d} = e^{\frac{\pi}{L}d}$, where e is the base of Napierian logarithms, L the length of the wave from hollow to crest, d the depth of the centre of the circle described by any particle below that of the circle described by the surface-particle, r_d the radius of the circle at the depth d, and r_o that of the circle at the surface. Prof. Rankine also independently deduced the same theory. A striking feature of the investigation was the rapid decrease in the motions of the particles as they are traced to lower depths. Prof. Stokes showed that for all waves of ordinary proportions, the motion at a depth equal to the length of the whole wave from crest to crest is only $\frac{1}{535}$ of that which belongs to a surface particle. The dynamical conditions of wave-water being thoroughly investigated and established, Mr. Froude next proceeded to base upon it a scientific theory of the rolling of ships among waves.

The subject was first brought before the public by Mr. Froude in a paper read before the Institution of Naval Architects in 1861. He stated that he felt some diffidence in bringing forward "what assumes to be a tolerably complete theoretical elucidation of a difficult and intricate subject, which has hitherto been treated as if unapproachable by the methods of regular investigation." He pointed out that the characteristic feature of the dynamical laws to which it would be necessary to refer the movements of a ship when rolling is the gradual accumulation of angle during several successive rolls, the cumulative action thus growing up into a maximum, and then dying out by very similar gradations until the ship becomes for a moment steady, when a nearly similar series of excursions commences and is reproduced; while in reference to the momentary pause, or cessation of motion, it seems clear that it occurs, not because the waves themselves cease, or cease to act, but because the last oscillation has died out at a moment when the ship and the waves have come to occupy, relatively, a position of momentary equilibrium. This is so closely analogous to what happens when a pendulum is subjected to a series of impulses, partially synchronous with its own excursions, that it seemed probable that the laws which govern the latter class of phenomena would be found, *mutatis mutandis*, applicable to the elucidation of the former also. The investigation of the laws of rolling motion, when thus regarded, therefore assumed the form of the inquiry, "What is the cumulative result of the continuous action of a series of consecutive waves operating on a given ship?"

In order to determine this it was necessary first to determine how each individual wave will act upon a ship at each instant of time; or, in other words, "What is the position of momentary equilibrium for a body floating on a wave, and what accelerating force towards that position

will the body experience in terms of her momentary deviation from it?" Mr. Froude has demonstrated both *à priori* and experimentally that to a stabilised particle floating at any point on the upper surface of a wave, the position of momentary equilibrium is that which would place the axis of equilibrium normal to the wave surface at the point where it floats, and that to another similar particle, floating or suspended below the surface, the position of momentary equilibrium is that which would place the axis normal to the sub-surface of equal pressure passing through the point where it is placed. If we take account of the aggregation of particles which a ship displaces, and for which she herself is substituted, and of which she accepts the aggregate dynamic condition, we know that her position of momentary equilibrium must be the mean of the positions belonging to the various particles displaced; and we may assume, with a close approximation to the truth, that this is the position which would place her axis of equilibrium, or her masts, at right angles to one of the wave sub-surfaces of equal pressure.

The sub-surface of equal pressure through the centre of gravity of a ship's displacement may be regarded in theory as a sufficiently close approximation to the effective wave surface; and it follows that when a ship deviates from the normal to this surface the effort by which she endeavours to conform to it depends upon the momentary angle of deviation in the same manner as her effort to assume an upright position when inclined in still water depends on the angle of inclination. Hence her stability or effort to become vertical in still water, measures her effort to become normal to the effective wave surface in wave water. The equations of motion for a floating body, oscillating in still water, which had previously been investigated, could therefore be applied to undulating water by introducing the condition that the position of equilibrium changed with the direction of the wave slope. Mr. Froude was not able, at first, to solve the resulting equation by adopting the trochoidal hypothesis. He therefore substituted the curve of sines for the trochoid, which gave him a form of equation he could deal with. Prof. Rankine afterwards solved the equation obtained by using the trochoid, but the results agreed with those arrived at by Mr. Froude under the conditions to which the investigations applied.

The assumptions made in order to adapt the problem to mathematical treatment were (1) that the ship is rolling passively in the trough of the sea; (2) that she is exposed to a regular series of similar waves; (3) that the waves are so large as compared with the ship that she may be assumed to accept the motion of the part of the wave she displaces; (4) that the variations of apparent weight may be neglected in comparison with the actual weight; (5) that the ship is of such a form as to make her still water oscillations isochronous—this being approximately the form of the old line of battle ships; and (6) that the rolling is unresisted—the effects of resistance in modifying the motion being separately considered.

The equations thus obtained by Mr. Froude, representing the oscillations of a ship among waves as compared with those performed in still water, are most interesting; but our space will not admit of giving a full analysis of them. Their general character may, however, be appreciated if we call attention to some of their most striking features.

One critical case is that of a ship rolling among waves, whose periodic time synchronises with her own time of oscillation. It may be readily deduced from Mr. Froude's fundamental equations that, if it were not for the resistance to rolling caused by surface friction and form, a ship placed broadside on to waves which have her own periodic time, must ultimately roll completely over, however small the wave may be. It is not uncommon to find the length of a half-wave ten times the height. Such waves would increase the angle of roll by 14·1° at each inclination, so that six successive impulses, or three complete waves, passing a ship would produce almost a complete overset. Though this conclusion requires to be greatly limited by introducing the element of resistance, it is obvious that such synchronism of wave-period and ship's-period must produce most formidable effects. Mr. Froude produced the result thus indicated by his theory by direct experiment with floating bodies of such form as would give approximate cases of unresisted rolling. He immersed a sphere to two-thirds of its radius; a prolate spheroid to about the same proportion of its major axis; and a body like a flattened orange was wholly immersed, having only a very narrow neck projecting from it above the water-level, like the stem of a hydrometer. By an ingenious arrangement for regulating the period of the waves it was found that, when the oscillations of the floats and the wave-period were made to synchronise, all the floats were upset after the transit of a very few waves, while a very small change in the natural period of one of the floats, made by slightly altering the position of its centre of gravity, made its behaviour plainly exceptional as compared with the two others. It now refused to be completely overset by the series of waves which would upset the two others almost at the same moment, though it was itself overset by a series slightly quickened or retarded according as its own period was quickened or retarded by the altered position of its centre of gravity, the other two being at the same time released from all danger of capsizing.

Another critical case is when the ship possesses infinite stability; or an infinitely small radius of gyration or moment of inertia. This is not a practical possibility, but is noticed on account of the indication it gives of a ship's tendencies according to the degree in which it may possibly be approached. In this case the ship will be perfectly quick in her movements and will follow precisely the slope of the wave. The movements of a flat board laid on the water are a practical illustration of this condition. The periodic time of such a board may be practically treated as $= 0$; and if a ship could be so constructed as to fulfil this condition, there might be some wisdom in attempting it. It is impossible, however, to construct a ship that will even approximately fulfil this condition, and as an approach to it could only be effected by giving her the greatest possible stability, she would only the oftener meet waves with which she would synchronise and experience the evil consequences of that condition.

One other critical case, the conditions for which are deducible from the equation of rolling motion, is that in which a vessel exposed to a series of waves performs her oscillations not in her own period but in that of the somewhat different wave period. In this case the still water oscillations would not synchronise with the wave period; but a relation subsists which enables the increasing slope of each wave to just counteract the growing inclination of the ship. At the wave hollow and crest a ship under such conditions would be upright: and she would reach her greatest inclination to the vertical when she was in an intermediate position upon the greatest slope of the wave. She would roll so that her masts would always lean towards the wave.

A general feature of the theory as deduced from the equation of rolling motion is that when the natural time of oscillation of a ship and the wave period do not synchronise, and when the rolling has not become permanent, the ship's oscillations will pass through phases analogous to the action of a pendulum when subjected to a series of impulses partially synchronous with its own excursions; and, as we have seen, this deduction is in accordance with the observed phenomena of rolling.

The results given by Mr. Froude's equation for unresisted rolling give, so far as character is concerned, a generally correct view of what actually occurs. But, quantitatively, the angles of oscillation indicated are

largely in excess of the truth. A most important practical circumstance is left entirely out of account in the equation, viz., the fact that the oscillations are performed in a resisting medium. The laws of resistance to rolling are not sufficiently well understood to enable their modifying effect to be introduced into a general equation, and a direct and theoretical solution of the question thus attempted. Mr. Froude has shown, however, how in the case of any individual ship an approximate solution may be obtained with certainty by help of data derived from a single experiment with the ship herself, or even with a carefully made model of her, tried in still water. He has also pointed out that if a well-selected series of such experiments were tried for ships of different forms, and the results tabulated, the series of corresponding solutions would enable him to determine, as if *à priori*, what modifications the results of his equation would require for any ship whatever.

The resistance which a ship experiences in oscillating through a given angle among waves is practically the same as it would be if she were performing an oscillation of the same range in still water. If she is set rolling in still water the resistance will rapidly bring her to rest when the force which caused her to roll ceases to act. In the same way, when rolling among waves, the same resistance will reduce the angles of oscillation. It is this circumstance which prevents dangerous angles being reached in the critical case of synchronism, and which, at all times, fixes the limiting position to which a ship will roll. The action of the waves in increasing the angle of roll is balanced by the opposing tendency of resistance to reduce it. The aggregate resistance of a ship to rolling is made up of three parts: (1) That due to the friction of the fluid in moving over the skin of the ship; (2) the direct resistance of the keel and fine parts at the ends to being pushed through the water at right angles, or nearly so, to their planes; and (3) what Mr. Froude calls the "wave making function," or the element of resistance caused by the successive displacements of fluid in rolling which affect the surface as waves, and travel away from the ship, thus abstracting from her the energy they embody.

The aggregate resistance to rolling being of such a character, and containing elements which vary in different ways with the rate of motion of the surface of a ship's bottom, its exact computation in a particular case would be very difficult. Mr. Froude has, however, done much towards enabling a sufficiently close approximation to be arrived at by direct calculation. For practical purposes, however, Mr. Froude's original suggestion of determining the amount of resistance by still water experiments is employed. A ship is set rolling in still water, and upon reaching an angle of sufficient magnitude, she is allowed to come to rest under the action of resistance only. The rate of extinction of her range of oscillation is continuously noted and registered in the form of a curve. It will be obvious that the difference in amplitude of two successive swings measures the aggregate effect of the resistances operating upon the ship, and that by means of a complete curve of extinction a measure of the resistance opposed by her to rolling among waves may be obtained.

This is, necessarily, but a general description of Mr. Froude's theory, and does not include many details and some collateral matter which are of importance in making it quite complete; but it will serve to give an indication of its broad features.

Mr. Froude's theory has met with general acceptance as being a sufficiently close approximation to a correct expression of the laws of motion of a floating body among waves. Some of the departures from absolute accuracy contained in the assumptions necessary to the formation of the theory, such as that wave-water is continually changing its form, and a ship being a rigid body, cannot therefore, strictly speaking, be said to accept the dynamical conditions of the displaced fluid as it accepts the statical conditions of its still-water displacement; and the objection that a sub-surface of equal pressure does not correctly represent the effective wave surface have been criticised; but the criticisms have only shown that the errors thus involved are very small in amount and have no appreciable effect upon the general results of the theory. Mr. Froude has always insisted himself upon the existence of imperfections in his theory, but he has constantly been at work upon their removal, and upon the determination of the tangible amount of error they introduce into the ultimate result.

Mr. Froude's work was undertaken at first with a practical object, on account of Mr. Brunel's requirements, and it has already accomplished great practical results. It has led to the conclusion that ships of long periods are least likely to meet with waves which will cause them to roll heavily, and that the rolling of a ship can be greatly reduced by means of deep-bilge keels. These ideas have been extensively acted upon in H.M. Service. Our heavy armour-clads and other ships of war have been designed in accordance with this theory, so as to have great steadiness at sea, and in many special cases their properties have been determined by Mr. Froude beforehand, so that instead of working in the dark the Admiralty have known what behaviour to expect from a ship after she is built. The effect of bilge-keels in extinguishing rolling was clearly shown by Mr. Froude in 1871, with a model of the *Devastation*. Without bilge-keels the model performed 31½ complete oscillations before coming to rest in still water, after being inclined to an angle of 8½°, but with a bilge-keel equivalent to 36 inches in depth on the full scale of the ship, she came to rest in eight oscillations, and with a bilge keel double this depth she came to rest in four oscillations. The same model when tried among waves which capsized her when no bilge keels were fitted only rolled to an angle of 13½° with 3 feet bilge keels, and to 5° with 6 feet bilge keels. These experiments showed conclusively the effect of bilge keels in limiting the range of oscillation among waves.

The importance of bilge keels was further tested by comparative trials conducted by Mr. Froude off Plymouth in 1872 between H.M. ships *Greyhound* and *Perseus*—both of the same class—the former having bilge keels and the latter being without them. Mr. Froude also conducted an elaborate series of experiments on board the *Devastation* at sea in 1873 and 1875, and, by means of a most ingenious machine of his own construction, obtained continuous automatic records of her behaviour. These records showed (1) the relative inclination of the ship and the effective wave slope at any instant; (2) the inclination of the ship to the vertical at any instant; and (3) the period of oscillation of the ship at any time, that is, the number of seconds occupied in completing the roll from port to starboard, and *vice versâ*. From the diagrams upon which (1) and (2) are traced in the form of curves Mr. Froude deduced, as a differential result, the period and angle of slope of the effective wave surface at any instant, thus determining it with a much greater degree of exactness than that with which the form of a surface wave could be ascertained.

Mr. Froude was constantly checking and correcting his theoretical results by the aid of experiment, and though he had succeeded in fully establishing the true theory of the rolling of ships among waves, he did not rest satisfied with his success, but had recently made arrangements at his experimenting tank at Torquay for proceeding with further important lines of investigation by means of observations upon the behaviour of models in waves mechanically generated.

We have only dealt with Mr. Froude's labours in connection with the question of the rolling of ships in this number, and must reserve a description of his investigations into the laws of resistance and propulsion, which are at least of equal importance, for another occasion.

(*To be continued.*)

THE COLD WEATHER OF LAST WINTER AND SPRING

THE winter and spring just past will be historically memorable for the unprecedentedly cold weather which has been the outstanding characteristic. More intense cold has no doubt been experienced in former years for single nights, or for brief intervals of a few days, than has been recorded anywhere in these islands during the past six months; but for upwards of a century since thermometric observations of the temperature of the air began to be made in Great Britain there has not occurred, so far as these observations show, a tract of weather so cold, as respects duration and intensity combined, as has prevailed during the half year ending with May.

From January, 1764, we have a consecutive series of monthly mean temperatures before us from observations made on the south shores of the Moray Firth and of the Firth of Forth. From this unique and valuable record we give the following periods of protracted low temperatures extending over intervals of from five to ten months, which have occurred in North Britain during the past 115 years, the amount of the depression below the means of the months being at least three degrees:—

Date of Cold.	Duration in months.	Under mean temperature of the months.
		°
February–November, 1782 ...	10	−5·1
January–August, 1799	7	−3·8
October–March, 1799–1800 ...	6	−3·3
November-April, 1807–8	6	−3·5
March–August, 1812	6	−3·4
October–March, 1813–14	6	−3·6
November–August, 1815–16 ...	10	−3·5
January–May, 1838	5	−4·2
January–May, 1855	5	−3·5
December-April, 1859–60	5	−3·0

Of these periods the most intense and, excepting that of 1815-16, the most protracted cold was that of 1782, when, during the ten months beginning with February, the temperature was 5°·1 under the mean of these months, the deficiency being 5°·4 for the five months from February to June, and 4°·8 from July to November. It may be noted that of these ten periods of protracted cold weather none occurred from 1764 to 1781; there were no fewer than seven during the next thirty-four years, and during the sixty-three years which have elapsed since 1816, only three such cold periods have been recorded.

Happily the extraordinary development and extension of meteorological observation which has taken place in late years enables us to define with some precision the distribution of the great cold of 1878–79 over the British Isles. For this purpose we have selected ninety-two places well distributed over the United Kingdom, and calculated their mean temperatures for the six months from December to May, and compared them with Buchan's mean temperatures and isothermals of the British Isles.

From the results thus obtained, it appears that this cold weather was felt in its greatest intensity in Central Enlgand, where, within a circuit roughly defined by a line passing near Stonyhurst, Shrewsbury, Cirencester, Oxford, Audley End, Yarmouth, Kelstern in Lincolnshire, and Durham, the depression of the temperature below the means of the six months exceeded 6°·0, falling to 6°·7 below the average at Cirencester and 7°·4 at Shrewsbury. Large portions of the south of Scotland between the Solway and the Firth of Forth and in Perthshire had also a mean temperature for the period fully 6°·0 under the average. Northwards through Central Scotland as far as Lairg in Sutherland, the depression of temperature was only about 5°·0 below the average; and this appears to have been about the deficiency experienced over central Ireland, falling, however, to 5°·7 at Armagh, and 5°·3 at Lissan, on the west of Loch Neagh. Everywhere round the coast the cold was less intense than in the interior. Temperatures were from a degree to a degree and a half relatively milder along the east coast, and still milder on the west coast; indeed, Shetland, Orkney, the Hebrides, the south of Ireland, Scilly, and the Channel Isles had a mean temperature only from 2°·0 to 3°·0 below the average temperature of the period, so greatly was the conserving influence of the ocean felt on the temperature of places in the west and south during this memorable cold weather.

During these six months, the greatest depression of temperature, absolutely as well as relatively to the monthly means, took place in December and January. If the monthly means be only looked at, the absolutely greatest temperature depression during the period was in December, in the counties of Cumberland and Dumfries, and along the upper reaches of the Tweed and Clyde, with their affluents. Within this region the mean temperature of December, reduced to sea-level, did not rise above 29°·0, falling at some places as low even as 27°·5. The week of intensest cold was the second week of December, when the mean temperature fell at many places in England, Scotland, and Ireland, to from 15°·0 to 18°·0 below the average of the season.

If we look at the monthly mean temperatures of the past 115 years as compared with their averages, with the view of ascertaining the duration of the most protracted periods of cold weather which have occurred during this long interval of years, defining as a period of cold weather an interval of time during which the mean temperatures of the months were continuously under their averages, we find that there have occurred four such noteworthy periods of protracted cold weather, during which the mean temperature of no month included in it rose above its average. These, arranged in the order of their duration, are (1) A period of 19 months, extending from September, 1798, to March, 1800, the mean temperature of this long period being 2°·8 below the average; (2) A period of 17 months from September, 1859, to January, 1861, which was 2°·2 below the average; (3) A period of 15 months, from October, 1815, to December, 1816, which was 3°·0 below the average; and (4) A period of 14 months, from February, 1782, to March, 1783, the mean temperature of which fell 4°·4 below the average of the months. It is thus only too evident that while the cold weather most of us have been suffering from these six months exceeds in intensity any other past period of cold weather in these islands of like duration of which we have an exact and authentic record, the temperature observations of the past 115 years disclose to us tracts of unseasonably cold weather, two or even three times more protracted than the interval which has yet elapsed since the present cold set in with such intensity and persistence in November last.

THE ICE CAVERN OF DOBSCHAU[1]

WHILE on a tour in Hungary last summer I had the opportunity of visiting an ice cavern near the town of Dobschau; the discoverer of the cavern kindly conducted me through it and wished me to make it known to the English public; with this object in view I have written the following short account:—

The cavern is situated to the north-west of Dobschau, and is approached through a narrow winding limestone valley, "the Stracenaer Thal." It has a general direction from west to east in the interior of a mountain whose slope faces north; the descent into it varies from oblique to precipitous, the entrance, which is very narrow, being situated at the highest point of the cavern; the ice consists of innumerable layers frozen together one upon the

[1] Dobschau is situated a little to the south of the Kaschau-Oderberger-Bahn, the nearest station on that line being Iglo.

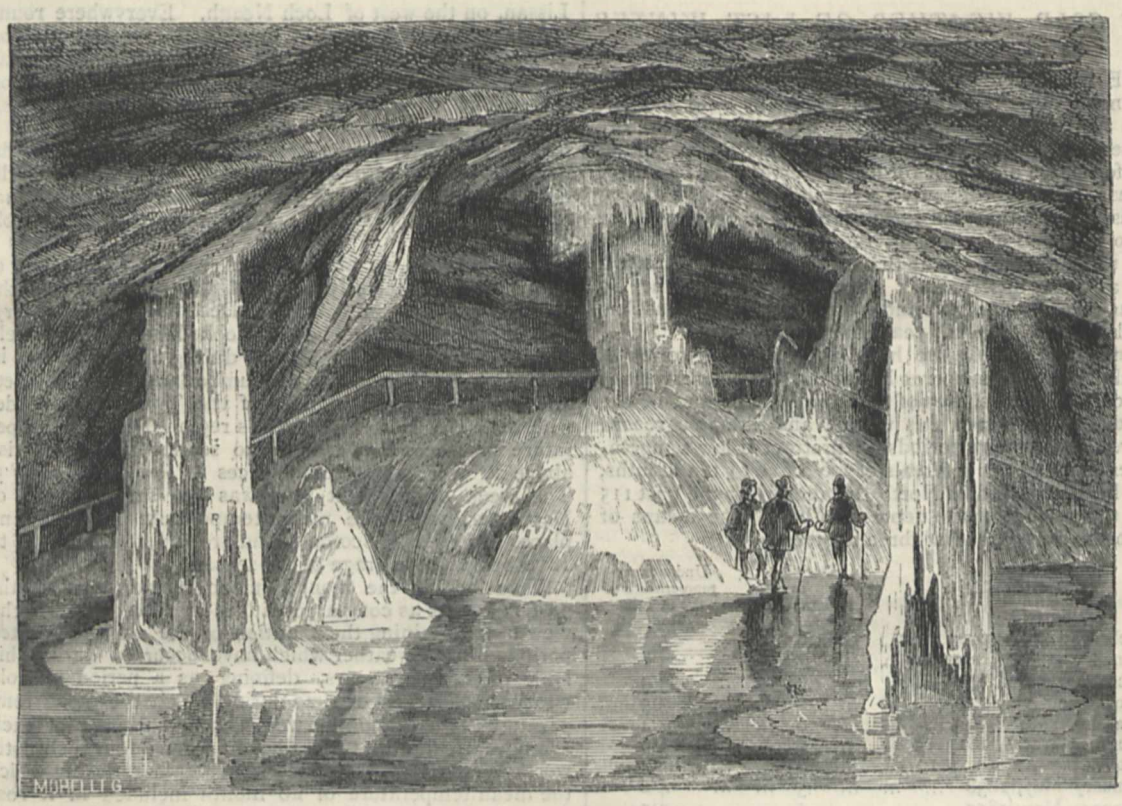


FIG. 1.—Portion of the Great Saloon.

FIG. 2.—The Corridor.

other, and assuming various fantastic shapes. The total surface of ice and rock is 8,874 square metres, that of the former being 7,171 square metres, of the latter 1,703 square metres; the mass of the ice amounts to 125,000 cubic metres.

The cavern consists of two parts, an upper and lower stage; the former is reached immediately after leaving the entrance, its floor being formed of ice, its roof and walls of limestone; it is partially divided into two unequal chambers, the small and great saloons (Fig. 1), by a curtain of rock descending from the roof. The small saloon is not situated on the same level as the great saloon. In the great saloon are three ice pillars (Fig. 1); they are translucent, and down the cylindrical hollow of one pillar there is a continuous though small flow of water; in addition there are numerous other ice ornaments which have received fanciful names. The eastern end of the saloon is contracted into a very narrow corner; at this point there has been a landslip corresponding to a crater-shaped depression in the slope of the Duosa Mountain, in which the cavern is situated. In the small saloon to the right of the entrance is a waterfall composed entirely of ice.

The lower stage consists of a corridor (Fig. 2) following the south side wall of the saloon; the downward prolongation of the dome-shaped rock wall of the saloon forms the south corridor wall, while the naturally formed cross-section of the ice constitutes the northern. The corridor originally consisted of two portions, a right and left wing, separated by a mass of ice; this has now been bored through. The entire length of the corridor is 200 metres.

The right corridor wing is reached from the small saloon by going down a steep flight of steps through a natural opening; the mass of ice whose upper surface forms the floor of the saloon after touching the roof suddenly terminates so as to make a nearly vertical wall to the corridor; the floor of the latter sinks down into the depths below, terminating in a mass of *débris;* this point probably forms a natural outlet for the water.

In the left corridor-wing is a magnificent ice structure termed the Grotto (Fig. 3).

The cooling of the air and the permanent low temperature (the mean of the year being −·86° C.) of the cavern are due to its height and northern aspect, as well as to its narrow opening and contracted exit canal, and to its floor gradually sloping inwards; as a result of this, the water is converted into ice, and the permanence of the latter thus insured.

Through the kindness of Herr Ruffiny, the discoverer, and Dr. Pelech, I have been enabled to obtain the loan of the woodcuts.

W. Bezant Lowe

Fig. 3.—The Grotto.

THE VISITATION OF THE ROYAL OBSERVATORY

On Saturday last the annual visitation of the Royal Observatory was held, when the Astronomer-Royal read his annual report, which refers to a period of thirteen lunations, from the new moon of 1878, May 2, to the new moon of 1879, May 20. We notice some of the most important points in this report:—

Considerable alterations have been made in the great equatorial, so as to make the instrument easy for use with the long half-prism spectroscope. The declination axis being meridionally excentric by 14 inches as regards the polar axis, the observing-chair may be made available for eye-observations and for spectroscopic observations respectively, by reversing the instrument in hour-angle so as to take advantage of this excentricity.

With regard to the numerous group of minor planets, the Berlin authorities have most kindly given attention to the Astronomer-Royal's representation, and we have now a most admirable and comprehensive ephemeris.

The meridian observations of stars are directed primarily to the determination, with the greatest attainable accuracy, of the places of 215 fundamental stars observed throughout the day and night, and these are supplemented by observations of stars taken from a working catalogue of about 2,500 stars. About 1,300 of these stars were observed in 1878, forming a larger annual catalogue than usual.

As might have been expected, the Astronomer-Royal's report on the weather of the past year is not satisfactory. After a fine autumn, the weather in the past winter and spring has been remarkably bad. More than an entire lunation was lost with the transit-circle, no observation of the moon on the meridian having been possible between January 8 and March 1, a period of more than seven weeks. Neither sun nor stars were visible for eleven days, during which period the clock-times were carried on entirely by the preceding rate of the clock. The accumulated error at the end of this time did not exceed 0s.'3.

"During the past year, spectroscopic observations have been almost entirely suspended, in order that the reductions of accumulated photographic observations might proceed more rapidly. The sun's chromosphere has been examined on seven days only, and, on five of these, prominences were seen. Two measures of the displacement of the F-line in the spectrum of *a* Virginis, and 26 of the *b* lines in the spectra of 6 stars (3 of them not previously examined), as compared with the corresponding lines of hydrogen and magnesium, have been made. In five of these, though the stars were of the second magnitude only, a dispersive power equivalent to fifteen prisms of 60° was used. These observations were checked by reference to the F or *b* lines in the spectrum of the moon or of the sky. The spectrum of Brorsen's comet has been examined with the half-prism spectroscope, and that of the eclipsed moon on 1868, August 12, with the single-prism spectroscope.

"Photographs of the sun have been taken on 150 days, and 228 of these have been selected for preservation. The photographs show a complete absence of spots on 121 days out of 150; and on comparing them with those of the preceding year, when there was an absence of spots on 66 days out of 156, it appears that we have not yet passed the minimum.

"The following are the principal results for magnetic elements in the year 1878:—

Approximate mean westerly declination	18° 49'.
Mean horizontal force ...	3·905 (in English units). 1·801 (in Metric units).
Mean dip	67° 37' 10" (by 9-inch needles). 67 38 12 (by 6-inch needles). 67 38 59 (by 3-inch needles).

"The magnetic reductions for the years 1865 to 1876 are nearly completed. The results are exhibited in the form of annual and monthly mean curves of diurnal inequality, as in preceding investigations, the abscissæ showing the variation of magnetic declination, and the ordinates that of horizontal force, throughout the twenty-four hours. The annual curves of diurnal inequality are now complete for the period of thirty-six years, from 1841 to 1876; and from the great length of this series of observations," the Astronomer-Royal goes on to say, "all made on the same system and with similar instruments, most important inferences may be drawn, both as to the laws of diurnal inequality in general and its changes in different years and seasons, and as to the connection between magnetic phenomena and sun-spots. These annual curves show a well-marked change in close correspondence with the number of sun-spots. About the epoch of maximum of sun-spots they are large and nearly circular, having the same character as the curves for the summer months; whilst about the time of sun-spot minimum they are small and lemniscate-shaped, with a striking resemblance to the curves for the winter months. We think with the Astronomer-Royal that it may be worthy the consideration of the Board whether the whole of these results, with any modifications that experience suggests, should not be printed and circulated as a separate volume.

"The monthly curves, 1865-76, have been formed for three periods of four years each, corresponding roughly to periods of minimum, maximum, and mean of sun-spots, and the whole series stands thus (the general character of the curves being added):—1841-47 (curves) mean; 1848-57, small; 1858-63, large in summer, small in winter; 1865-68, small; 1869·72, very large; 1873-76, mean. The maxima of sun-spots occurred in 1848, 1860, and 1870, and the minima in 1844, 1856, and probably in 1879.

"The connection between changes of terrestrial magnetism and sun-spots," the Astronomer-Royal states, "is shown in a still more striking manner by a comparison which Mr. Ellis has made between the monthly means of the diurnal range of declination and horizontal force, and Dr. R. Wolf's "relative numbers" for frequency of sun-spots. It appears from this that not only is there a general correspondence in the two sets of phenomena, but that the minor irregularities of the sun-spot curve are reproduced in the curves of diurnal magnetic range; and further that the well-marked annual inequality in the latter is itself variable, being greatest at the time of maximum of sun-spots and least at that of minimum. It will be an interesting inquiry, when sun-spots become more numerous, to determine whether the present paucity of earth-currents is connected with the fewness of spots."

The Astronomer-Royal, by these researches, has endorsed the work we owe to Sabine, Broun, Stewart, and others.

The Report states that the Westminster clock has not been quite so well regulated as usual. During the period to which this Report refers, its error exceeded 1s. on 77 days; on 15 of these it was between 2s. and 3s., on 4 between 3s. and 4s., and on 1 day it exceeded 4s.

With regard to the last Transit of Venus results, the past year has been occupied in putting reports and calculations in a shape adapted to eventual printing of the account of the whole enterprise. With regard to the Transit of 1882, the Astronomer-Royal informs us that the general impression appears to be that it will be best to confine observations to simple telescopic observations or micrometer observations at ingress and egress, if possible at places whose longitudes are known. For the first phenomenon (accelerated ingress) the choice of stations is not good; but for the other phenomena (retarded ingress, accelerated egress, retarded egress), there appears to be no difficulty. The adoption of a south-polar station seems to be practically abandoned.

With regard to the numerical lunar theory, the Report states that the incessant pressure of the business of the observatory has prevented the Astronomer-Royal advancing so rapidly as he had hoped. "The solar perturbing forces are all computed to 10^{-7} in all cases, and to 10^{-8} and 10^{-9} in those cases in which large factors are introduced by theory as necessary for obtaining the correction to tabular coefficients from discordance of computed deductions (lunar places on one side, with solar forces on the other). The lunar places with the same arguments, first computed to 10^{-7}, are now extended to 10^{-8} or 10^{-9}, for the powers of radius vector. The computations of the same kind for the other assumed elements (longitude and latitude) are not begun. In regard to the discordance of annual equation, to which I called attention in the last Report, I suspend my judgment. I have now discussed the theory completely; and, in going into details of secular changes, I am at this time engaged on that which is the foundation of all, namely, the change of

excentricity of the solar orbit, and its result in producing lunar acceleration."

With reference to the practicability of reducing the extent of the printed volume of "Greenwich Observations," the report concludes with some of the suggestions that have been made by certain members of the Board. The introduction, it has been suggested, might to some extent be stereotyped. No reduction, it is thought, should be made, in regard to the details of meridional and altazimuth observations.

To the strong appeal made for extension of the spectroscopic observation of stars, in reference to their motion in the line of sight, the Astronomer-Royal has given a tacit response by the modification of the S. E. Equatorial, so as to facilitate that extension. "The tendency of external scientific movement," he remarks, "is to give great attention to the phenomena of the solar disk (in which this observatory ought undoubtedly to bear its part). And I personally am most unwilling to recede from the existing course of magnetical and meteorological observations. All these, however, are inferior in importance, with regard to the question now before us, to the extent of printing the original details of astronomical observations."

"The general tendency of these considerations is," the report concludes, "to increase the annual expenses of the Observatory. And so it has been, almost continuously, for the last forty-two years. The annual ordinary expenses are now between two and a half and three times as great as in my first years at the Royal Observatory. I would fain flatter myself that the value of its results has increased in a greater degree."

NATURAL SCIENCE DEGREES AT OXFORD

DR. ODLING, replying in the *Times* to Canon Liddon's letter, referred to in NATURE, vol. xx. p. 132, maintains that unless some little Greek is considered absolutely essential to a liberal education, there can be no ground for refusing a degree in arts to students who, though unacquainted with Greek, are familiar with such like studies as geometry, arithmetic, and astronomy, which equally with grammar, dialectics, and rhetoric, have been counted from time immemorial among the liberal arts. And assuming the compulsory modicum of Greek now brought up by mathematical and natural science students to be a non-essential element of their liberal education, as certified to by a degree in arts, how can a degree in arts be hereafter refused to advanced students of either of these subjects who, while still bringing up Latin, shall in future offer a considerable amount of German, together with some amount of both mathematics and natural science as a substitute for the present modicum of Greek?

Dr. Odling, rather than degrade science by awarding its graduates an inferior degree, seems disposed to retain the little modicum of Greek at present required for a pass; he would have been contented with Canon Liddon if the existing arrangements had been undisturbed. His objection is not to the incubus of Greek, but to the slur about to be put on natural science.

Canon Liddon replies that, in speaking of the educational advantages of Greek, he was in part thinking of the *minimum*. He believes Dr. Odling mistaken in thinking that the new degree was intended as anything but an honourable distinction. No one could suppose, he believes, that the majority of the present Council could be unfriendly to physical science. The statute appeared to him to be drawn almost exclusively in the interests of natural science students, and with a view to relieving them of an uncongenial study.

In a subsequent letter Dr. Odling quotes a passage from a lecture delivered by Dr. Whewell some twenty-five years ago. In the course of showing that the great contributions made to intellectual education by Greece, Rome, and modern Europe in succession have been geometry, jurisprudence, and physical science respectively, he wrote as follows:—

"Our intellectual education now, to be worthy of the time, ought to include in its compass elements contributed to it by every one of the great epochs of mental energy which the world has seen. . . . A mind well disciplined in elementary geometry and in general jurisprudence would be as well prepared as mere discipline can make a mind for most trains of human speculation and reasoning. . . . But however perfectly the habits of deduction may be taught by these studies, such teaching cannot, according to the enlarged views of modern times, compose a complete intellectual culture. . . . As the best sciences which the ancient world framed supplied the best elements of intellectual education up to modern times, so the grand step by which, in modern times, science has sprung up into a magnitude and majesty far superior to her ancient dimensions should exercise its influence upon modern education, and contribute its proper result to modern intellectual culture."

Happily the further discussion has been postponed until Michaelmas Term; by that time it is hoped that some method will be found by which natural science will be honoured without hurting the feelings of any one. We may state that the Council of the Cambridge Senate recommend to the Commissioners that power be given in the statute to recommend degrees in science (B.Sc., M.Sc., D.Sc.).

ON SOME MARINE ALGÆ[1]

THE successor of Harvey in the Chair of Botany in the University of Dublin has taken, as his eminent predecessor did, the algæ for the principal object of his study. In 1877 Dr. E. P. Wright published in the *Transactions* (vol. xxvi. Science) of the Royal Irish Academy two memoirs, one on a green unicellular alga (*Chlorochytrium Cohnii*) parasitic in the mucous tubes of some diatoms, in *Polysiphonia urceolata*, and in *Calothrix confervicola;* the other on a parasite deprived of chlorophyll (*Rhizophydium Dicksonii*), which develops itself in the cells of an Ectocarpus, and which has been taken, at least in one case (*E. crinitus*, Harv.), for the fruit of an Ectocarpus. This present year the *Transactions* of the same Academy contain two additional memoirs by the same author, which are accompanied by three coloured plates drawn by Tuffen West. The latter memoirs seem to me to be conceived in a spirit, and executed after a manner, which one does not always meet with in the writings of the British algologists. Dr. Wright has studied the living plants, an innovation on which he cannot be too much congratulated. One would only wish that his exact and minute observations on the development of organs, a subject in which he has shown himself at home, had been joined to an experimental determination of their functions, a determination which morphology by itself is powerless to declare to us. It is easy to prove this latter statement by a few instances taken from authors whose abilities have been placed beyond all doubt. Thus it has been said over and over again—Naegeli himself believes it ("Algensysteme," p. 134, Pl. 1, Fig. 34; 6)—that the heterocysts of the Nostocs are reproductive bodies, while experiments the most easily made prove that they are nothing of the sort. Cramer ("Phys. syst. Untersuch. über die Ceramieen," Heft 1, p. 125) has mistaken the antheridia of Bonnemaisonia for young cystocarps. Dr. Wright has, as we will show further on, been himself the victim of a similar error. Morphology is not like the spear of Achilles; it does not heal the wounds which it makes.

Griffithsia setacea is a pretty, red alga, well known to

[1] From the French of Ed. Bornet.

all collectors of marine plants. It is composed of branched filaments made up of large cylindrical cells placed end to end in a single row. When it is in fruit, the filaments are furnished with short branches terminated by a globular involucre, in the interior of which are ranged the reproductive bodies. How are these formed, and how do these filaments and appendages grow? What modifications do the cell-contents experience during this formation and growth? These are the points explained with a good deal of clearness by Dr. Wright in the first of the two latter memoirs referred to (on the cell-structure of *G. setacea*, and on the development of its antheridia and tetraspores). Referring to the memoir itself for details, I would only call attention to a peculiarity noticed in the development of the involucre. The rays which compose it take their origin in a circle from the penultimate cells of particular ramuli, formed by a small number of cells and slightly club-shaped at their superior extremity. These rays are not all at once free. Detached from the protoplasmic mass on which the apical cell reposes, they for a long time increase underneath the common membrane which clothes the frond, and they are only made free somewhat later on by the rupture of this membrane. First of all figured, but very imperfectly by Derbes and Solier, well represented from life by Thuret, this peculiar disposition is shown by Dr. Wright as made clear by the use of reagents, and it would appear to be equally met with in the genus Pandorea, recently described by J. Agardh.

In following from their first appearance the development of the reproductive organs on the rays of the involucre, Dr. Wright observed that the cells destined by their origin and their position to form the tetraspores, did not all comport themselves in the same manner. Some of them produced the ordinary four spores, but in the interior of the others globular cells arose provided with a beak, from which there came out colourless corpuscles, wonderfully like the antherozoids of the Florideæ. The resemblance of these bodies to species of Olpidium did not escape Dr. Wright, but struck by their constant presence on the specimens which he examined, by the regularity with which they appeared on determined points of the involucre, he thought they might be regarded as the antheridia of *Griffithsia setacea*, and here he has overlooked the fact that true antheridia, of the ordinary type in the Florideæ, had long since been described and figured in this very species by Thuret (*Ann. des Sc. Nat.* 3 ser. Bot. Tom. 16). On this occasion Dr. Wright, however, records an observation as new as interesting, viz., that he has seen the corpuscles as they left these wrongly imagined antheridia perform movements after the manner of amœbæ.

In the second of the two memoirs, having for its title "On the Formation of the so-called 'Siphons,' and on the Development of the Tetraspores in Polysiphonia," the author describes with much care the method of the formation of the frond in *Polysiphonia urceolata*, and very exactly proves the relationship existing between the "tube central" and the "siphons," and between the siphons themselves. For a great part he therein only confirms the results of those preceding him in such investigations, for the history of the development of the frond in Polysiphonia has been almost exhausted by the works of Naegeli, Kny, and Magnus. I am almost afraid that an analysis of these minute details would inspire the reader with that horror which, according to Naegeli, such morphological researches bring with them to the systematic botanists, but I cannot bring myself to omit extracting the following passage, in which some curious vital phenomena are incidentally described by Dr. Wright, as he found them to exist in the cells of Bryopsis.

"Under the influence of some local irritation, which must not be enough to injure the cell wall of the specimen under examination, the denser portion of the protoplasm will often be found to draw itself from the upper part of these cells. As it does so, the very conspicuous chlorophyll granules will be seen to be drawn together until they become pretty tightly packed. There is an apparent rounding off of the upper portion as it gets drawn down in the tube of the cell wall, and under a low power of the microscope this convex surface seems pretty sharply defined; but turn on a high quarter of an inch or an eighth of an inch objective, and a very remarkable phenomenon will present itself—for there will then be seen a mass of pseudopods not easily to be forgotten and difficult to describe under any other name; they stream away from below the apex of the cell wall, converging downwards until they are lost in the centre of the convex margin of the withdrawing mass of protoplasm. Here they are broad, while towards the apex of the cell they disappear through their very tenuity. Coursing down along these pseudopods, very minute granules can be, on careful focussing, detected; these are ultimately lost in the denser protoplasmic mass which engulphs them. This streaming goes on for a while, until all the protoplasm of a certain density is drawn into the lower mass; this then finally rounds itself off and forms an independent cell wall in front, which of course will be below the former growing point of the cell. There is apparently no plastic protoplasm remaining above this—no small disc even of homogeneous mucilage to be seen; all the viscid protoplasm seems to have gone to the rear, and it would appear as if the upper portion should now become sphacelated—perhaps disappear—and a new apical growth proceed from below it; but this is not so; there is life in the front still; it goes on growing as before, and in process of time it will be found to leave in its rear dense chlorophyll-bearing protoplasm, and so on through the several layers until the *punctum* itself is, as before, reached."

OUR ASTRONOMICAL COLUMN

BIELA'S COMET.—As bearing upon the possible return of Biela's comet during the latter part of the present year, it will not be out of place if we here summarise the results of an investigation made by Prof. Oppolzer in 1873, on the possible connection of the comet discovered by Mr. Pogson at Madras on December 2 previous, with Biela's comet and the great meteoric shower of November 27, 1872. It will be remembered that the comet in question was found in consequence of a telegram sent by Prof. Klinkerfues to Madras immediately after the meteoric display, to the effect that Biela's comet had "touched the earth" on the evening of November 27, and urging Mr. Pogson to search for it near the star θ Centauri. From the Madras observations on the nights of December 2 and 3 (the only occasions on which the weather was favourable), as they were first approximately reduced, Oppolzer derived the following data:—

1872, December 3·0 M.T. at Berlin.

Comet's geocentric longitude (λ)	... ...	223° 15′·6
,, ,, latitude (β)	... ...	− 20 10·0

And the unit of time being a mean solar day,

$$\frac{d\lambda}{dt} = +187''{\cdot}0, \quad \frac{d\beta}{dt} = +46''{\cdot}3.$$

At a subsequent time Mr. Pogson published more accurate positions of the comet than those at first communicated, which would give the following similar data, differing, it will be seen, in no material degree from those adopted by Oppolzer:—

1873, December 3·0 M.T. at Greenwich.

λ ... 223° 21′·1 β ... −20° 8′·6 $\frac{d\lambda}{dt}$ ... +189″·9 $\frac{d\beta}{dt}$ ... +46″·4

It had soon been found, as might have been expected, that no satisfactory conclusion could be arrived at by comparison of Michez's elements of Biela's comet with

the observations, proportionally small variations in the elements producing greatly magnified effects upon the geocentric place and geocentric motion, in consequence of the close proximity of the comet.

Oppolzer describes his method of calculation in No. 1,938 of the *Astronomische Nachrichten*, to which we must refer the reader, as an outline of it would unnecessarily extend this note. He makes three assumptions as to the distance of the comet from the earth and deduces three orbits for comparison with the orbit of Biela's comet, as follow :—

	(A)	(B)	(C)	Orbit of Biela.
Distance assumed ...	0·04	0·08	0·12	—
	° ′	° ′	° ′	° ′
Mean anomaly	− 4 54·4	− 5 6·8	− 5 4·8	—
Long. of perihelion ...	128 48	141 9	151 50	109 45
,, ascending node	247 38	244 34	242 12	245 50
Inclination	9 14	10 28	11 46	12 22
Angle of excentricity..	51 36	54 17	56 49	48 48

It must be added that Oppolzer pre-supposes the comet moving in an orbit with same semi-axis major as that of Biela, the corresponding mean daily motion being 530″·1; hence with the above mean anomalies on December 3·0, the dates of perihelion passage on the three hypotheses would be respectively January 5·3, January 6·7, and January 6·5.

The similarity of these systems of elements is striking; only in the longitude of perihelion are there comparatively large differences, which Oppolzer observes, may not appear so noteworthy when it is remembered that Michez's elements do not include the effect of perturbations from 1866 to 1872, nor those which might just have resulted from the presumed exceedingly close approach of the comet to the earth on the night of the meteoric shower. The great difference of nearly three months in the perihelion passage, however, he regarded as against the identity of the object with Biela's comet, though from the anomalies which the disintegration of the comet might have occasioned, this circumstance might not really possess all its apparent signification. His general conclusions may be stated thus :—It may be asserted with confidence that assuming the distance of Pogson's comet from the earth December 3·0 to have been within the limits 0·04 and 0·12 of the earth's mean distance from the sun, we are led to elements which show a remarkable resemblance to those of Biela's comet, as well as with the course of the great shower of meteors on November 27, 1872. When the distance is much increased we find materially different elements, and the greater distance cannot be regarded as improbable; in this, Oppolzer remarks, lies in his opinion the weakest point of the argument, and only by observations at a future time can a certain conclusion be attained. Nevertheless he considers the striking coincidences following on arbitrary assumptions, taken together, militate strongly in favour of the approximate correctness of his assigned distance. Thus there seems to be under the above suppositions as to the comet's distance, a most remarkable connection with the meteor-shower of November 27. If, as a rough approximation, it is assumed that the comet at 8 P.M. on that day touched the earth, and further, that the differential daily variation of the distance within the $5\frac{2}{3}$ days was equable, an hypothesis, which in the case of a contact, will not differ much from the truth, there will be deduced from the three values, for the distance of the comet on December 3, respectively 0·061, 0·071, and 0·080; comparing these values with those assumed, it is seen at once that an agreement is established with the final value, when the distance = 0·07 nearly. This result Oppolzer urges as highly deserving of note, and in his opinion almost demonstrates the connection of the swarm of meteors with the comet. On the supposition that the true values of the elements must be sought between the systems (A) and (B), considering further that the earth on November 7·3 was in 65°·9, heliocentric longitude, and that the comet if it gave occasion to the meteor-shower must have been situate near its descending node, so the longitude of the node by this criterion would be 245°·9, a value which also falls between the limits (A) and (B).

Further, if the distance of the comet from the earth is calculated from the above elements for the time of the meteor-shower, the following series is formed :—

A	B	C
0·024	0·009	0·053

and it is seen that the assumption of 0·07 for the distance on December 3, leads to a very close approximation of the comet to the earth at the time of the shower. Calculating now the comet's radius-vector for November 27·3, the three systems give logarithmically—

A	B	C
0·0042	9·9950	9·9908

while the log. distance of the earth is 9·9940. Consequently with elements A and B the comet is a little outside the earth's orbit, and with elements C it would occupy a position within it. At the first glance it will appear probable that necessarily the last relation must have place, or the comet would certainly have been detected ere it reached its least distance from our globe. On the one side, from the uncertainty of the data for calculation, the results may be considerably in error, on the other there may be some probability that the comet was visible in the southern hemisphere, and we might have received intimation that a comet of great brightness and with rapid motion was there recognised. Calculating from the three systems of elements the geocentric place, there result—

	A	B	C
λ	67°	110°	180°
β	+11	− 75	− 25

so that, in fact, with the system B, which appears to approach nearest the truth, the circumstances of visibility for the southern hemisphere would be favourable.

Weighing all these circumstances, Oppolzer thought it must be granted that Pogson's comet stands with high probability in intimate relation with the meteor-shower of November 27, and that it is possible the observed object was one of the heads of Biela. That the second head was not found, is not decisive against this, since the same, on account of close proximity to the earth, might have been situate in an entirely different quarter of the heavens, and besides, from its greater relative distance, might have been considerably fainter, so as easily to escape detection. Thus, at the time of writing his paper on the subject, Oppolzer was of opinion that the connection of Biela's comet with Pogson's object and the meteor-shower was by no means to be regarded as improbable.

GEOGRAPHICAL NOTES

THE *Golos* publishes a telegram, dated the 13th of May, from the celebrated Central Asian traveller, M. Prjevalsky, formerly a colonel in the Russian army. At that time he was on the river Buluguna. He had marched 600 versts from Saisan along the river Urumtsu, and would immediately set out for Chemi through the southern Altai mountains. All the members of his expedition were in good health.

THE Alexandria correspondent of the *Daily News* sends some details of Major Serpa Pinto's recent journey across Africa from Benguela to Durban. He tells us little that has not been already made known, and we shall look with eagerness for Pinto's promised work. Science has evidently had considerable attention rom Major Pinto during his journey. He has brought home a collection of 1,800 plants and "a superb collection of birds and insects." Astronomical and meteorological observations have been taken along the route, and several volumes of notes made, with maps. The Coando, which flows into the Zambesi,

is stated to have a length of 600 miles. It says much for the enthusiasm, if not for the knowledge, of the *Daily News* correspondent that he places Major Pinto in the "first rank of African explorers."

THE International African Association have received letters from MM. Cambier and Dutrieux down to March 16. They state their intention of remaining at Tabora till the end of the *masika*, or rainy season, which commonly ceases at the beginning of May. M. Cambier says that he has established friendly relations with the Arabs, and that he has ample resources for the next year without further supplies being sent. He also advises having forwarded an entomological collection made by Dr. Dutrieux. Though intelligence respecting this unfortunate expedition is remarkably vague, it may be hoped that we shall before long hear of their having done some real work, as they are now well advanced into the interior, have ample supplies, and the proper travelling season before them. The Association's second expedition will probably not be long before they start for the interior, as MM. Popelin and Van den Heuvel were to arrive on May 29 at Zanzibar, whither they have been preceded by M. Dutalis, who has already been engaged in a preliminary examination of the River Wami.

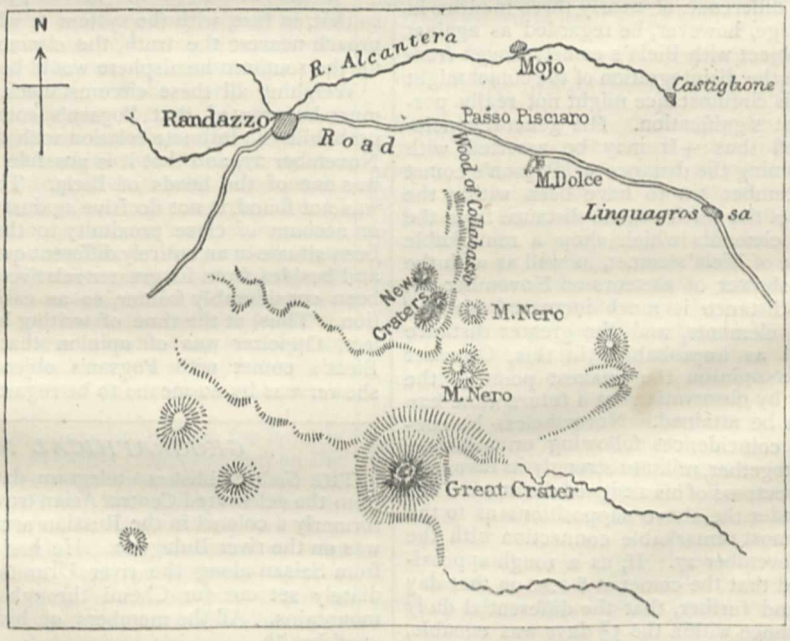
M. DE VILLIERS, the new Governor-General of French Cochin China, was Director of the Interior in Algiers under General Chanzy. He is the author of a dictionary of all the Algerian tribes and sub-divisions of tribes. This valuable work was published some years ago at the expense of the French Government.

THE current number of *Les Missions Catholiques* contains an account by Père Schmitt of a journey to Loango, in Western Africa.

THE new number of the *Annales de l'Extrême Orient* contains the first instalment of Dr. J. Harmand's "Notes de Voyage en Indo-Chine," illustrated by a map and copies of Khmer inscriptions. This is followed by part of a paper on New Guinea, in which is embodied much information respecting the Karons, the Kebars, and the Amberbaks. There are also some remarks by R. Friederich on the archæology and iconography of Java.

THE ERUPTION OF ETNA

ON the night of Sunday, May 25, loud bellowings were heard by the dwellers on the northern slopes of Etna. Towards the morning of the 26th these increased, and about midday a dense cloud of smoke was seen to issue from the side of the mountain below the great crater, apparently half way between Randazzo and Linguaglossa. This cloud increased, and on the 27th the mountain was rendered invisible, and an effect like that of an eclipse resulted. A rain of fine black ash, "like powdered emery," fell for miles around, and was so thick that Capo di Schiso could not be seen from Taormina, a distance of two miles. This black rain continued all day, accompanied by thundering noises from the mountain. No exact information could be procured concerning the position of the centre of disturbance, because no one could approach the new craters. During the night of the 27th the ashes continued to fall, and "huge fires could be seen looming through the black clouds"—no doubt the reflection of the molten lava on the smoke above it. It was reported in Piedemonte, a village on the north-



east flanks of Etna, that three craters about a mile apart had opened at the points of a triangle, about six miles above Passo Pisciaro, a posting station nearly midway between Randazzo and Linguaglossa. Lava was said to be flowing in a valley to the north of the Val del Bove. On the 28th a great stream of lava was seen from Taormina to be descending the mountain in the direction of Randazzo, "while from the new craters great balls of fire were thrown high in the air, and burst into showers of fire like gigantic rockets, accompanied by thundering explosions." On May 29 the lava was still flowing, but the shower of ash was diminished. The facts, as above stated, were witnessed by an Englishman living in Taormina, 800 feet above the sea, at the north-eastern termination of the flanks of Etna, about fifteen geographical miles from the new craters.

Daily bulletins in the newspapers have given us the history of the eruption since May 29. It is to be regretted that these have not been more concordant. Many times has the lava stream reached the bed of the river Alcantara, according to the telegrams, and often the next day has it been a kilometre distance. A telegram from Rome dated

June 3, and published in the *Times* for June 4, asserted that "the lava has still half a metre to run before reaching the Alcantara." The previous telegram asserted that the lava had run eleven kilometres from the craters, had rolled into the Alcantara, and had obliterated the Commune of Mojo; while the telegram of the following day made the stream 350 yards from the Alcantara. The fact is, that part of Etna is not thickly populated; distances are often guessed at; the new craters are not easily reached; and the shower of ashes prevented accurate observation, hence the discrepancies. But by a careful comparison of the telegrams, with the *Dettagli sull' eruzione dell' Etna*, issued by the Prefect of Catania at frequent intervals, the broadside sheets entitled *Guasti dell' eruzione dell' Etna*, and the letter written, on May 29, from Taormina, to the *Times*:—by comparing these with the fine map (scale = 1·266 inch to the mile) of the Italian Stato Maggiore, some of the discrepancies disappear, and a just estimate may be formed of the position of the new craters.

All accounts agree in placing the new craters near Monte Nero, but unfortunately there are two minor cones near together which bear the name of Monte Nero. We are helped out of this difficulty, however by the statement that the new craters are 1,900 metres (6,232 feet) above the level of the sea, that the higher of the two Monte Neros is far above this level, while the lower of the two has a little to the west of it a space marked by contours 1,900 metres. Here, accordingly, we shall place the new craters without hesitation.

The great crater, Randazzo, and Linguaglossa, form the three points of a nearly equilateral triangle, within which at present the eruption is completely confined. Linguaglossa is 12 miles from Randazzo, and 11 from the great crater, while Randazzo is only 10 miles from the crater. The new craters are 5 miles from the great crater, 7¼ from Randazzo, 7 from Linguaglossa, 7 from Mojo, 6½ from the River Alcantara, and 5 from Passa Pisciaro.

The lava has devastated the wood of Collebasso, and has crossed the main road at Passo Pisciaro, destroying the bridge there. Several vineyards have been destroyed, and if the bed of the Alcantara is invaded, the water supply will be cut off from a large tract of fertile land. The lava stream at Passo Pisciaro is about half a mile broad and 100 feet in depth. On May 30 it flowed at a rate of one metre per minute.

The last bulletin received to-day (June 6) from Catania dated June 1, 10 A.M., ends as follows:—"L'eruzione continua al solito. La lava verso il fiume dilatasi sempre, e scende insensibilmente. Stanotte un nuovo braccio investi la vigna di Salvatore Cimino, che quasi distrusse, producendo un danno di circa trentamila lire. La casina prospiciente sullo stradale versa in imminento pericolo." Signor Silvestri of Catania, together with two Germans, have penetrated as near as possible to the new craters. Silvestri, together with Prof. Blaserna of Rome, and Prof. Gemellaro of Padua, have been appointed to report on the eruption, in the interests of vulcanology. It was asserted in the telegram of June 6 from Messina that the stream of lava is only 100 metres from the Alcantara, and that it is advancing at a rate of fifteen metres per hour. Loud rumblings and dense smoke proceed from the new craters.

During the last few days the telegrams have stated that the eruption is diminishing, and that although the lava has slowly progressed, it has not yet reached the Alcantara. Some curious errors have been propagated in the newspapers. Thus the *Times* correspondent in Naples, writing under the date of June 2 (published June 10), asserts that "the side on the north-west is rent in two, and the fiery mass is ejected to the height of 1,900 metres, or considerably more than a mile." The real facts are that the new craters stand at an elevation of 1,900 metres above the sea; while a fissure which does not extend over even one-half the north-west side of the mountain, has been formed near Monte Nero. It has been asserted that saline mud has recently been ejected; also that the craters emitted on June 2, 450 cubic metres of lava per minute; also that the principal lava stream has a front of 800 metres, and that it has flowed for six or seven miles, But in regard to any exact statements, it will be preferable to wait for the report of Professors Blaserna, Gemellaro, and Silvestri; or at least for the very detailed account of the eruption, which is sure to appear in the next number of Prof. de Rossi's *Bolletino del Vulcanismo Italiano*. G. F. RODWELL

NOTES

WE understand that by permission of the Statistical Committee of the India Office, the new tide-predicter, which has been constructed for the Survey Department by Mr. E. Roberts (*Nautical Almanac* Office), will be exhibited at the closing meeting of the Royal Society on the 19th inst. The instrument, although not yet out of the makers' hands, is sufficiently complete to show its entire working; in fact, the tide-curves for the year 1880 for Bombay and Kurrachee, have been already run off, and the results are now being tabulated for printing. Specimen tide-curves of the Southern Indian, Pacific, and North Atlantic Oceans, the English Channel, and the Mediterranean, will also be exhibited to show the universality of the system of prediction by the instrument.

AMONG those on whom the honorary degree of LL.D. was conferred in the Senate House at Cambridge on Tuesday were Mr. Justice Grove, Dr. W. Spottiswoode, Prof. Henry J. S. Smith, Prof. T. H. Huxley, and Mr. H. C. Sorby.

PROF. HUXLEY has been elected a corresponding member of the Paris Academy of Sciences, in the section of Anatomy and Zoology, in succession to the late Prof. von Baer; and M. Schiaparelli in the Section of Astronomy, in place of the late M. Tisserand.

THE death is announced, on the 9th inst., of Dr. Moore, who for more than forty years has filled the office of Curator of the Botantic Gardens, Glasnevin, Dublin. He was a native of Dundee, and commenced the study of botany under the late Dr. Mackey, Curator of the College Botanical Gardens, whose place his eldest son, Dr. F. W. Moore, now fills. He was for some time employed on the geological survey of Ireland before he was appointed Curator of the Royal Dublin Society's Gardens at Glasnevin. He pursued the study of botany with great ardour, not only at home, but in various parts of the Continent. Among his works were "Notices of British Grasses," "Irish Hepaticæ," and "Irish Mosses."

IT is proposed immediately to establish a zoological station on the Aberdeen coast, in connection with the natural history laboratory of the University, similar to those already instituted for the Universities of Paris, Vienna, and Leyden. The objects of such a station are:—1. To supply the laboratory with fresh animals for purposes of teaching and research. 2. To enable students to become practically acquainted with natural history, and to afford them opportunities of advanced study and independent research, during the vacations. 3. To afford means for the exhaustive study of the marine fauna. For the establishment of such a station on the smallest possible scale it is necessary to have—(1) A movable shed or house with suitable fittings; (2) a large fishing boat and a small two-oared boat; (3) nets and dredges; (4) aquaria glassware and miscellaneous apparatus; (5) the services of a fisherman and a boy for part of the year. For the purchase of boats and apparatus a sum of 250*l*. is required,

and for wages, &c., at least 75*l.* annually. Lord Roseberry has given a donation of 50*l.*, and other subscriptions raise the total sum already obtained to 180*l.* It is not creditable to this wealthy country that it possesses no zoological station, and we trust our readers will do what they can to assist in raising the moderate sum required. Subscriptions may be sent to Mr. G. J. Romanes, 18, Cornwall Terrace, Regent's Park, N.W.

A NEW work is announced by Prof. Boyd Dawkins, on "Early Man in Britain and His Place in the Tertiary Period." In this the results of geological and archæological research, so far as they relate to the history of man in this country, will be placed before the reader in a connected narrative. Man will be treated as the central figure in the tertiary period, and the various changes in geography, climate, and living forms which preceded his arrival in Britain, will be examined, as well as those changes in his environment which took place after he appeared in Europe. His antiquity, his relation to the glacial period, and to existing peoples, and his manner of life, will be discussed, as well as the distribution of the Iberic and Celtic races, their manners and customs, their progress in civilisation, and the extent to which they were influenced by the civilised nations of the Mediterranean. It is the principal object of this work to give a picture of man isolated in Britain, from his first arrival down to the Roman invasion. It will be largely illustrated with maps and engravings. The work will be published by Messrs. Macmillan and Co.

MR. RUDLER has succeeded to the place of the late Trenham Reeks, in the Geological Museum, Jermyn Street.

IN an appreciative article on Sir Henry Bessemer, *à propos* of his knighthood, the *Times* gives some striking statistics to show the vast advances made in the production of steel since the adoption of the Bessemer process:—"Prior to this invention the entire production of cast steel in Great Britain was only about 50,000 tons annually, and its average price, which ranged from 50*l.* to 60*l.* per ton, was prohibitory of its use for many of the purposes to which it is now universally applied. In the year 1877, notwithstanding the depression of trade, the Bessemer steel produced in Great Britain alone amounted to 750,600 tons, or fifteen times the total of the former method of manufacture; while the selling price averaged only 10*l.* per ton, and the coal consumed in producing it was less by 3,500,000 tons than would have been required in order to make the same quantity of steel by the old or Sheffield process. The total reduction of cost is equal to about 30,000,000*l.* sterling upon the quantity manufactured in England during the year; and in this way steel has been rendered available for a vast number of purposes in which its qualities are of the greatest possible value, but from which its high price formerly excluded it. During the same year the Bessemer steel manufactured in the five other countries in which the business is chiefly conducted—namely, the United States, Belgium, Germany, France, and Sweden—raised the total output to 1,874,278 tons, with a net selling value of about 20,000,000*l.* sterling. The works in which these operations were carried on were eighty-four in number, and represent a capital of more than three millions. According to the calculations of Mr. Price Williams, who has made the endurance of rails a matter of careful study, the substitution of Bessemer steel for iron for this purpose alone will produce a saving of expenditure during the life of one set of steel rails on all the existing lines in Great Britain of a sum of more than one hundred and seventy millions sterling. It may safely be said that there is no other instance in history of an analogous impetus to manufacture, or of an analogous economy, being the result of the brain-work of a single individual; still less is there an instance of such results being realised while the inventor was living to enjoy the fruits of his labours, and able to work in fresh directions to increase the benefits which he had already conferred upon his country and upon mankind."

THE enterprising Birmingham Natural History and Microscopical Society have again arranged a marine excursion, this time to Falmouth, during next month, on a somewhat similar principle to those to Arran, which proved so successful in the last two years. Facilities will be afforded for dredging excursions and for land excursions to investigate the botany and highly interesting geology of the district. The botany of the district is very peculiar and interesting, and the geology is unique in the British Islands, Kynance Cove and the Lizard being within easy reach of day excursions from Falmouth. During the summer season a most interesting series of observations may be made on the microscopic larval forms of marine life (hydroids, echinoderms, annelids, &c.), which abound in the sea, and may at this time readily be taken by the tow net. A small steamer will be chartered, which will economise time and add to personal comfort. The marine fauna of the Cornish coast is exceedingly rich and varied. The time for the excursion will be from the 5th to the 14th or 21st of July.

PROF. VIRCHOW has returned to Berlin from Asia Minor, where, as our readers are aware, he had taken part in Dr. Schliemann's excavations. The learned professor was received with great honours at Athens. The Medical Faculty of the High School of that city presented him with the honorary doctor's diploma, and the Medical Society of Athens elected him an honorary member.

IN digging a channel in the neighbourhood of Lake Neuchâtel, a lacustrine canoe, very nearly seven metres long, has been found. It has been placed in the Cantonal Museum.

IN addition to the list of quite recent earthquakes we gave last week, we now have reports of two more. At Aachen (Aix la Chapelle) several shocks were felt on May 26, soon after 8 P.M., which seemed to proceed in the direction from west to east; and at Idstein (in the Prussian province of Nassau) a violent shock occurred on May 27, about 1 A.M.

M. DE LESSEPS has accepted the chairmanship of a committee for arranging the commemoration of the eighteenth centenary of the great eruption of Vesuvius in 79, when Pliny lost his life, and Pompeii and Herculaneum were destroyed. According to the most trustworthy records it was on the 23rd of August that this unexpected event took place.

M. CLAMOND, the inventor of a thermo-electric pile, has succeeded in producing a current strong enough to work a Serrin regulator with tolerable success. The expense is only 7 kilogrammes of coals per hour, and the appearance of the battery reminds one of an ordinary furnace.

THE Paris Academy of Meteorological Ascents has inaugurated the series of its aërial excursions. The first took place at St. Mandes and the second at Arcueil on the occasion of the opening of the École Laplace. The ascensionists propose to take photographs from the car in order to ascertain the position of the balloons and make a verification of the laws of barometric height. The original idea of this difficult operation may be attributed to Leverrier. Each of these ascents will be followed by the publication of diagrams and scientific results obtained. They are prefaced by a lecture, given by a member of the association, on the practice of aëronautics.

FROM June 7 up to the end of the month the exhibition of Beaux Arts at the Paris Palais de l'Industrie will be lighted every night by electricity. The motive power is supplied by 262 Jablochkoff electric lights. 120 have been distributed in the

gardens where statues are exhibited; 142 in the saloons where pictures are suspended on the walls. The 120 candles are surrounded by opaline globes, which diminish the total effect, but the general illumination is satisfactory. The other 142 have been placed in translucent glass spheres, which leaves the light its original force. The appearance of the pictures is splendid and the general impression is exceedingly favourable. It is supposed that the garden could well be arranged shortly according to the same system, and the illumination will be unrivalled in brilliancy. The partial extinctions are very few and generally very easily repaired. The motive force is supplied by four steam-engines placed in a shed at a distance, and is estimated at 300-horse power (seventy-five for each motor). The consumption of coal and other expenses are very small in comparison with the receipts which were more than 200*l.* for the first evening. It is divided in two equal parts, one for the Government and the other for the Jablochkoff Company. But the speculation cannot be said to be a paying one owing to the expenses of installation. The length of insulated wire is about 42,000 yards at a shilling each, and the other expenses in proportion. But it is supposed that the scientific exhibition will inherit the bargain and the electric fixtures, which are said to be worth 20,000*l.* will be in constant use up to the Month of November.

From Friedländer and Son, of Berlin, we have received two very full Catalogues of works in Geology and Geognosy, which we commend to all interested in these subjects.

In reference to a statement in the *Globe* that the prediction of an earthquake to happen on or about May 21 was falsified, a correspondent, Mr. Frank Barnard, writes that on that day he felt the movement of an earthquake at Hastings, at 12 noon. He describes it as a quivering of the ground, slight, but too palpable to pass unheeded, communicating a quiver to his whole frame such as he never felt before. He forwards us a cutting from his sketch-book, on which he pencilled at the time a record of the occurrence.

In reference to Mr. Hosie's article in last week's Nature, on Chinese Observations of Sun-Spots, a correspondent writes that Mr. Sayce, in his "Babylonian Literature," shows that more than 4,000 years ago it was recorded in the library of Nineveh that the sun was spotted on the first day of the Chaldean year, "from which," says a *Times* reviewer, "we may infer the presence of an unusually large spot." We may, our correspondent thinks, infer more—the presence of spots.

Mr. R. Etheridge has written to the *Times* on a boring made by the New River Company at Ware in Herts, in which it was found that the Gault at a depth of 800 feet rests upon the Upper Silurian rocks (the Wenlock Shale), richly fossiliferous, dipping at an angle of 40 deg., but to what point of the compass is not at present known. The *Geological Magazine* for June reprints the letter and appends a list of the fossils found in the cores of the Wenlock.

When King Victor Emmanuel took possession of Rome he left the Roman Observatory in the hands of the late Father Secchi, out of respect for his exceptional merits. When Father Secchi died, the Pope appointed his successor, who took possession of the establishment and refused to leave the place. He has been expelled, however, *manu militari*.

The Roman Alpine Club has decided to send an excursion to Etna, which will leave Rome on July 1. Intending excursionists are directed to address the Secretary of the Roman section of the Italian Alpine Club before the 15th instant, as preparations are required for the comfort and safety of the excursionists. We believe that any one, irrespective of nationality, can join the excursion. The travelling expenses from Rome to Catania will be diminished by one-half, owing to the liberality of railway and steamer companies. The duration of the excursion will be at least seven days.

We have received several numbers of the *Naturalist*, the journal of the Yorkshire Naturalists' Union, from which we see that the many societies composing that union are as active as ever. A most interesting series of excursions has been arranged for the summer.

M. E. Morel, who has been Belgian Consul at Shanghai for many years, has informed the *North China Herald* that his Government, with the view of bringing the manufacturers of Belgium into greater prominence, contemplate the establishment at that port of a permanent museum in which specimens of all descriptions of Belgian manufactures and produce will be exhibited. It is believed that about 1,000*l.* per annum will be expended on this object.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mr. J. Beech; a Golden Eagle (*Aquila chrysaëtos*) from the Western Hebrides, presented by the Earl of Dunmore, F.Z.S.; a Red and Yellow Macaw (*Ara chloroptera*) from South America, presented by Miss C. Cattlin; two Common Kingfishers (*Alcedo ispida*), British Isles, presented by Mr. W. W. Cobb; a Common seal (*Phoca vitulina*), British Isles; three Javan Peafowls (*Pavo spicifer*) from Burmah, deposited; three Maned Geese (*Bernicla jubata*) from Australia, purchased; an Eland (*Oreas canna*), a Canadian Beaver (*Castor canadensis*), a Great Kangaroo (*Macropus giganteus*), a Red Kangaroo (*Macropus rufus*), a Bennett's Wallaby (*Halmaturus bennetti*), born in the Gardens; four Amherst Pheasants (*Thaumalia amherstiæ*), three Egyptian Geese (*Chenalopex ægyptiaca*), bred in the Gardens.

SCHLIEMANN'S TROJAN EXCAVATIONS

The *Times* of Tuesday contains several letters from Dr. Schliemann, describing the researches he has been recently making in the Trojan country in company with Dr. Virchow of Berlin, and M. Burnouf, of Athens. They endeavoured to ascertain the geological character of the plain of Troy, by sinking shafts in different parts between Mount Hissarlik and the Hellespont. Dr. Schliemann states:—"We obtained everywhere the same result—viz., below the clay soil a thick layer of coarse or fine river sand, and below it the very compact dark-brown clay of the plain. But the most important result was obtained by the shaft we sunk in the Stomalimne, mentioned by Strabo, which is an easily recognisable swamp, situate between the mouths of the rivers; it slopes abruptly from the plastic clay of the plain to a field of sand which is nearly on a level with the sea. Excavating there, we found below the layer of sand, which is hardly an inch thick, a layer of plastic clay, about 16 inches thick, which is perfectly the same as in the plain, and below it a dark-blue sand containing putrified vegetable matter, which can leave no doubt that here existed a swamp. The upper part of this layer of blue sand is exactly on a level with the sea and with the adjoining inlet, the water of which is brackish and has no current. Having dug in this blue sand a large hole two feet deep, we saw the water filtering from all sides through the sand and soon filling the hole completely, and thus the water's surface was on a level with both inlet and sea; but this water was sweet and drinkable. In no one of our shafts sunk elsewhere did we discover the slightest trace of the sea having ever sojourned there; everywhere we found only the produce of sweet water. Thus it is evident that the soil of the plain of Troy has been produced by sweet water, and that this deposit is anterior to the existence of both the Scamander and the Simois, the more so as the modifications produced by these rivers are but very slight." Therefore he maintains the theory that at the time of the Trojan war the sea formed a deep gulf in the Plain of Troy, that the later Ilium (Hissarlik) was too near the Hellespont, and no space left for the great deeds of the "Iliad"; that consequently the two cities

could not possibly be identical—is now exploded and can never be revived again. Both Professor Virchow and Mr. Burnouf have accepted Dr. Schliemann's theory that the immense bed of the small and insignificant rivulet Kalifatli-Asmak, which has no running water except during the inundations in winter—that this river-bed, which has in many places a breadth of from 660 ft. to 825 ft. —is identical with the ancient bed of the Scamander.

Dr. Schliemann then describes the results of his excavations on the gigantic tumuli called Udjek-Tepé and Besika-Tepé. He now inclines more and more to the conviction that these and other immense Trojan tumuli were no real tombs, but mere memorials erected in commemoration of some great event. "They certainly existed at the time of Homer, who mentions four of them (those of Batieia, Æsyetes, Ilus, and Achilles) as real tombs, no doubt because, in his time, similar conical tumuli were erected at Sardis and elsewhere on the Asiatic coast over the ashes of great men."

In continuing his excavations at Troy, Dr. Schliemann found two other treasures of Trojan gold jewellery, and Ilium, he states, appears now exhausted. Dr. Virchow and M. Burnouf are astonished at the monstrous quantity of bricks, which solely occur in the burnt city. The three explorers are convinced that these bricks must have been slightly burnt in an oven before being used for building.

Mr. Sayce, in a letter in yesterday's *Times*, states that the markings on a fragment of pottery, a *fac-simile* of which was sent him by Dr. Schliemann, are rude attempts to imitate cuneiform characters on the part of a potter who was unacquainted with the meaning of the latter. "As is well known, the specimens of Phœnician art found in the Mediterranean frequently bear rude representations of Egyptian hieroglyphics figured for the sake of ornament, and grouped in such a way as to show that the artist had not the faintest idea of their signification. If my view of the markings on the piece of pottery discovered by Dr. Schliemann is correct, it becomes certain that some kind of prehistoric intercourse was carried on between the Troad and the populations who employed the cuneiform system of writing, and since the remains found at Hissarlik show little or no trace of Assyrian or Phœnician influence, the intercourse must be assigned to the older Babylonian period. Five years ago I suggested that some of the designs on the terra-cotta disks from Hissarlik might be rude imitations of designs on archaic Babylonian cylinders. I may add that Mr. Newton, to whom I showed the *fac-simile* sent me by Dr. Schliemann, thought my view of it very possibly right."

In connection with the above the *Times* states that a recent number of the *Norddeutsche Zeitung* stated that the Chinese Ambassador at Berlin, Li Fangpao, well known in his own country as a great scholar, has lately read as Chinese the inscription on a vase found by Dr. Schliemann in the lowest stratum of his excavations at Hissarlik, and figured on p. 50 of the introduction to his "Troy and Its Remains." Li Fangpao is quite confident that the unknown characters, which recur again and again on the Trojan antiquities, especially on the terra-cotta whorls, are those of his native tongue, and gives as the purport of the inscription, that about B.C. 1200, three pieces of linen gauze were packed in the vase for inspection.

With reference to this Mr. Sayce states that the authority of the Chinese ambassador, high as it is, will never persuade any one acquainted with the characters of the Cypriote syllabary that the characters found on some of the objects from Hissarlik are Chinese. As was stated in a *Times* leader, they belong, Mr. Sayce repeats, to the curious syllabary which seems to have been used on the coasts of Asia Minor and in the islands of the Ægean before the introduction of the simpler Phœnician alphabet, and which continued to be employed in conservative Cyprus down to a late date.

ON THE ORIGIN OF THE SOLAR PROTUBERANCES

WE take the following from a communication made by Herr Spörer to the Berlin Academy on November 7 :—

"Simultaneously with the minimum of sun-spots the protuberances have been also insignificant hitherto, but since the middle of this year (1878), while the spot minimum continues to last even far beyond expectation, important protuberances have yet appeared, and among them some from which important deductions may be drawn with tolerable certainty.

"According to my observations of the year 1871 I had distinguished two classes of protuberances, viz., the ordinary hydrogen protuberances, and the flame-like protuberances which are remarkable on account of their intensity and pointed forms. In the latter ones, apart from the H lines and D_3, the magnesium lines may be easily recognised even with the smaller 5-inch telescope, which was then and is now at my disposal; other lines are less easily recognised. When Secchi agreed with me in this division of protuberances he chose the name of 'metallic' ones for the second kind, because the lines of metallic elements principally appear in them, apart from the H lines and D_3.

"No doubt we may suppose that many of the ordinary hydrogen protuberances originate through *storms* forcing up the hydrogen sea in gigantic *whirls* and *waves*, and I have also succeeded in finding examples confirming this, as the changes observed took place entirely after the manner corresponding to our water-spouts; this, however, does not exclude that many of the hydrogen protuberances originate through eruptions from the interior of the sun's body. We are still more inclined to look upon the flame-like protuberances as *eruption* products. I had considered also whether for the explanation of these, electricity might not be taken into account, particularly since then the quick shooting up and the quick change of the formations would not have to be explained merely by the motion of masses, and the over-great velocity would not be so surprising. Indeed, observed zigzag lines of communication between neighbouring flame-like protuberances had suggested the thought of *electric discharges*.

"The thought that bright protuberances do not start from the surface, nor yet from the hydrogen envelope, but rather that they are formed only at a certain height, therefore that perhaps at the low temperature existing at greater heights, *chemical combinations* take place, and that only by these the intense flaring up is produced—this thought may doubtless not be designated as a new one, but no forms of protuberances have been published yet which would decidedly favour this view."

Herr Spörer then quotes from numerous Italian publications a considerable number of protuberances which appeared *perfectly detached* from the surface, and says he could considerably augment these examples from his own observations. But the objection may be made to these cases that the formations described are only the remains of larger ones which previously had their origin at the sun's surface. This objection is perfectly justified since observations have proved often enough that brightly luminous protuberances became partly obscured, and particularly that the foot of a protuberance disappeared while the upper part remained visible.

It is necessary therefore to adduce such examples where the proof may be furnished with certainty that a luminous formation which is observed detached from the surface, did not originate *at* the sun's surface. Now Herr Spörer together with Herr Kempf has observed protuberances of this nature in July and August last year; he deferred their publication in the hope of being able to obtain more examples; this hope was annihilated for the present through the beginning of the unfavourable season, and hence the observations which had been made were made known. There were altogether three observations of this kind, which Herr Spörer describes and represents by illustrations; here we must confine ourselves to reproduce the one case which was the most favourable one to his theory, and which was observed on July 22 from 5h. 30m. until 6h. 50m.

The protuberance appeared in lat. 35° – 40° south, and attained a height of 46″, or 34,000 kilometres. First of all it was seen in one point in the shape of an eruption, which, by the action of storms, was partly deviated to the left. In this direction an arc appeared, which expanded further to the left (possibly always in consequence of storms), until later on it touched the sun's surface, and thus spanned a dark segment. Intensely bright rays were remarkable about 5h. 47m. when they first appeared, and which proceeded from the highest part of the arc segment in a vertical direction, and quite detached from the solar surface. The direction of the rays did not permit the idea that they might have been torn off from the protuberance on the right by the action of storms perhaps, or otherwise. Then a larger mass was formed by these rays, whereupon extremely quick and varied changes took place, the exact observation of which was impossible, since the slit of the spectroscope could not be widened and therefore only a part of the protuberances could be observed at a time. At 6h. 12m. the complete arc was seen, the denser part over the middle had become a little looser, but glistening points were again present, standing perpendicular upon the arc; the formation had assumed larger dimensions at 6h.

23m. To the right a new ray had formed, which attained a much greater height than the other parts of the protuberance, viz., to 61", or 46,000 kilometres, and which lasted only for a short time.

It might perhaps be thought possible that the vertical rays over the middle of the segment and the larger protuberances originating there, had yet been in connection with the sun's surface, the dark segment having formed the foreground, so that dark and dense gases of this segment might have hidden the parts behind them from view. But then the segment ought to have appeared as a sun spot later on, if it had not happened to appear at the very extreme limb. On account of the enormous dimensions a spot of this kind could not have disappeared entirely until the following day, on July 23, therefore, at least some remains should have been observed really as a spot. But there were not any spots on the sun's disk neither on July 23 nor on the following days, except two small spots on the northern hemisphere on July 26. At the same time we might remark that in the high southern latitude of the protuberance so large a spot has never appeared before; this we know by experience.

The examples of July 24 and of August 9 are equally important, if indeed less grand in proportions. In both cases the protuberance, seen detached from the solar surface, could not be due to the action of storms, but, like the one described above, rather appeared to favour the hypothesis of the *origin* in higher regions.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Museums and Lecture Rooms Syndicate have issued their thirteenth annual report, in which they give details of the progress made in the buildings in course of construction. They report that they have no hope that the necessary expenditure upon the various departments of the museums can again be brought within the limits of the existing allowance for their maintenance. Having regard to the present state of the University finances, they have directed a curtailment of expenditure in all directions. This economy, however, although unavoidable at present, will, if persisted in, lower the standard of scientific education in the University, for it will be impossible to maintain the departments in their present state of efficiency. They hope that at no distant date the University will be in a position to increase the fund to a sum sufficient for their proper maintenance on the enlarged scale rendered necessary by the erection of the new buildings and the increased number of students. Appended to the report are the reports of the various professors, the superintendent of the Museums of Zoology and Comparative Anatomy, the Strickland Curator, and the Trinity Prælector of Physiology on the conditions of their several departments.

A MEETING of the curators of the University of Edinburgh was held on Friday, the 6th inst., with reference to the vacancy in the Chair of Mathematics caused by the death of Prof. Kelland, and it was resolved to hold another meeting early next month as to the appointment of a successor. Meantime, it is requested that the applications of intending candidates, accompanied by any testimonials desired to be submitted, be forwarded by Saturday, July 5. The emoluments, which are derived from a fixed endowment and from class fees, amount to between 1,200*l.* and 1,500*l.* per annum. Any details desired can be obtained from the Secretary to the Senatus Academicus.

THE ninth Annual Report of the Wellington College Natural History Society has to record a good deal of satisfactory work done, as is evident, indeed, from the very full lists and reports appended in the various departments. We should like to see the recommendation adopted given to those Fellows who do not play cricket, that they should devote themselves in the afternoons to the observations of birds and insects. A very fair proportion of the members, however, seem to do work.

THE new university building of Marburg was inaugurated on May 29 in the presence of Dr. Falk, the German "Cultus-minister."

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 15.—"On the Capillary Phenomena of Jets," by Lord Rayleigh.

In this paper are given the results of observations and their discussion on water issuing under varying pressure from orifices of various shapes.

Geological Society, May 28.—Henry Clifton Sorby, F.R.S., president, in the chair.—Edward Garlick was proposed as a Fellow of the Society.—The following communications were read:—On the endothiodont reptilia, with evidence of the species *Endothiodon uniseries*, Owen, by Prof. R. Owen, C.B., F.R.S. The author referred to the characters assigned by him to his *Endothiodon bathystoma*, which had the alveolar borders of both jaws toothless, perhaps covered with horn during life, as in the Chelonians; whilst within this border there were three series of teeth both in the palate and the mandible. He next described a new species, under the name of *Endothiodon uniseries*, founded upon the fore-half of a skull, having only a single row of teeth in the palate, a character which may prove to be of generic importance. The author finally discussed the relationships of this genus, which he regarded as belonging to the order Anomodontia, and as showing, like *Oudenodon*, traces of derivation from *Dicynodon* in the presence of caniniform processes in the upper jaw. The development of teeth interior to the alveolar margins in both jaws was to be regarded as a character of family value, and the author remarked upon the interest of the continuance of a common ichthyic and batrachial dental character in exceptional cases among the reptilia up to the establishment of the crocodilian type, above which, in the vertebrate series, calcified palatal teeth no longer appear.—Note (third) on *Eucamerotus*, Hulke, *Ornithopsis*, Seeley, = *Bothriospondylus magnus*, Owen, = *Chondrosteosaurus magnus*, Owen, by J. W. Hulke, F.R.S.—Description of the species of the ostracodous genus *Bairdia*, M'Coy, from the carboniferous strata of Great Britain, by Prof. T. Rupert Jones, F.R.S., and James W. Kirkby. The long persistence of the genus *Bairdia*, from the silurian period to the present day, and its essentially *marine* character, were first noticed; also the relatively rare occurrence of any species of *Leperditia*, *Beyrichia*, and *Kirkbya* (associates of *Bairdia* in carboniferous strata) in fresh-water or estuarine beds. *Carbonia*, on the other hand, was confined to the fresh or brackish waters in which the coal-measures were formed. The difficulty of defining the species of *Bairdia* from carapace-valves alone, without limbs and soft parts, and the possibility of several genera being grouped under this head, were mentioned. The species of *Bairdia* described and figured in this paper were, it is believed, all that have been found in the British carboniferous rocks, with the exception of M'Coy's *B. gracilis*. Two of Count Münster's Bavarian *Bairdiæ*, from Hof, have not yet occurred with us; neither have four of Dr. d'Eichwald's Russian carboniferous species, nor the Australian *B. affinis*, Morris. Including these, there are twenty-three known carboniferous species of *Bairdia*. Seven of these are recurrent in the overlying permian limestones, which have yielded twelve species of this genus. With six silurian forms, there are altogether thirty-four recorded palæozoic species of *Bairdia*.—Report on a collection of fossils from the Bowen River coal-field and the limestone of the Fanning River, North Queensland, by R. Etheridge, jun., F.G.S.—On a fossil *Squilla* from the London clay of Highgate, part of the Wetherell collection in the British Museum, by H. Woodward, F.R.S.—On *Necroscilla Wilsoni*, a supposed stomatopod crustacean from the middle coal-measures, Cossall, near Ilkeston, Derbyshire, by H. Woodward, F.R.S.—On the discovery of a fossil *Squilla* in the cretaceous deposits of Hâkel, in the Lebanon, by H. Woodward, F.R.S.—On the occurrence of a fossil king-crab (*Limulus*) in the cretaceous formation of the Lebanon, by H. Woodward, F.R.S.

Zoological Society, June 3.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—The Secretary exhibited and made remarks upon two volumes of original drawings of the birds of India, which had been deposited in the Society's Library by Brigadier-General A. C. McMaster. The volumes contained about 270 figures of the birds of India, most of which had been drawn by soldiers in General McMaster's house at Secunderabad.—Mr. Sclater exhibited and made remarks on a small collection of birds forwarded to him by Dr. A. Döring, of the University of Cordova, in the Argentine Republic.—Mr. W. Ottley gave a description of the blood-vessels of the neck and head of the ground hornbill.—Mr. Edward R. Alston read a paper on the specific identity of the British martens, in which he pointed out the distinguishing characters of *Martes sylvatica* and *M. foina*, and showed that the former species only is found in this country.—Messrs. Sclater and Salvin gave an account of the birds collected by the late Mr. T. K. Salmon in the State of Antioquia, United States of Columbia. Mr. Salmon's collections were stated to have been very extensive, having been the product of

some five or six years' assiduous collecting, and to have contained altogether about 3,500 specimens of birds, which were referable to 469 species.—Mr. G. French Angas gave an account of the land-shells collected by the late Dr. W. M. Gabb, in Costa Rica. The collection was stated to contain examples of forty-two species, of which ten or twelve were believed to be new to science.

Anthropological Institute, May 27.—Mr. E. Burnett Tylor, D.C.L., F.R.S., president, in the chair.—A paper by Mr. Hodder M. Westropp was read, entitled notes on fetichism, in which the views of Prof. Max Müller on the subject of fetichism as expressed in his late lectures on the development of religion were combated, and Mr. Westropp advanced an opposite theory of his own. Having stated all the points of difference between himself and Prof. Max Müller, the author explained his position thus:—that those who believe in a primordial fetichism must take it for granted that human beings passed through a rude and primitive phase when their minds were naturally and instinctively endowed with certain vague ideas of spirits and ghosts, which seem to be the spontaneous outgrowth of minds in a rude and primitive condition in all countries and in all ages. In the same way it must be taken for granted that all human minds have passed through a state of infancy.—A paper was also contributed by Mr. J. Matthew, of Little River, Victoria, on the Kabi dialect of Queensland. Although the Australian dialects are usually spoken of as agglutinative, the author considered that if it were possible for a language to be classed as isolated, although having the majority of its words composed of two syllables, such a language is Kabi.

Mineralogical Society of Great Britain and Ireland, June 3.—Prof. T. G. Bonney in the chair.—The election of fourteen Corresponding Members, eight Ordinary Members, and two Associates, was announced.—The following papers were read:—On abriachanite, a new Scottish mineral, by Prof. M. F. Heddle and Dr. W. H. Aitken.—On haughtonite, a new mica, by Prof. M. F. Heddle.—On brechite and xantholite from Scotland, by Prof. M. F. Heddle.—On christophite from St. Agnes, Cornwall, by J. H. Collins, F.G.S.—Minerals from Japan, by John Milne.—On some gold occurrences, by the Rev. J. Clifton Ward, F.G.S.—Additional note on penwithite, by J. H. Collins, F.G.S.—Measurements of angles of basaltic columns from the Giants' Causeway, by Professors Jellett and O'Reilly.

PARIS

Academy of Sciences, June 2.—M. Daubrée in the chair.—A telegram from the Emperor of Brazil stated that Tempel's comet had been seen on the 24th and 25th ult.—On the magnetic impenetrability of iron, by M. Jamin. The experiments prove that a given current through coils magnetises a bar much less when the latter is inclosed in a tube than when exterior to the tube (placed beside it); and the tube is more magnetised in the former case than in the latter. 6 mm. of concentric thickness of iron suffices to arrest almost completely the magnetic effect of an exterior spiral.—On the ultra-violet limit of the solar spectrum, by M. Cornu. Taking fine days, a collodion of constant composition, and a constant time of exposure, very comparable observations are had. The extent of the spectrum diminishes with the altitude of the sun, which tends to prove that the limitation is due to absorption by the atmosphere. The furthest limit obtained was at wave-length 293; this was only twice, on June 24 and August 18, 1878, about noon. M. Cornu discusses the causes of error, the conditions of extending the limit, and the variation of the limit with the height of the place of observation. The limit is pushed back one-millionth mm. in wave-length as you rise 663·3 m. (a small gain; becoming only 6 millionth mm. for 4,000 m. altitude, or about half the difference between winter and summer).—On alkaline amalgams, and on the nascent state, by M. Berthelot. A thermal study of the reducing action of these amalgams on organic compounds. They always liberate, in hydrogenising reactions, more heat than free hydrogen would, the respective excesses for amalgams of sodium and potassium being 32·8 and 27·5.—On stannpropyls and isostannpropyls, by MM. Cahours and Demarçay.—On the quantity of nitric acid contained in the water of the Nile before and after flood, by M. D'Abbadie. Having observed that thunderstorms accompanied most of the Ethiopian rains, he thought traces of the nitric acid formed would be found in the river at Cairo. Samples were taken before, during, and after the flood time, at about two months' intervals from July 10. The several numbers (for nitric acid) were, unexpectedly, 0·01, 0·0038, and 0·002 grammes per litre (of the last there is some doubt). He thinks the flooding of the Nile offers abundant material for study.—On the origin of sounds in the telephone, by M. Du Moncel. In most experiments showing that speech may be reproduced simply by a magnetic core surrounded by a helix, a microphonic transmitter with battery has been used. M. Du Moncel now shows the effect may be had with the induced currents of the Bell telephone, (made by Breguet). The receiver was a small strongly magnetised piece of watch-spring, fixed to a deal board, and surrounded by a small helix of fine wire. The diaphragm in the telephone, serving as armature, exalts the magnetic force of the bar, and is itself affected by the vibrations of the telephonic case fixed to one of the ends of the bar.—On the inter-oceanic maritime canal, by M. de Lesseps. He recounts the steps leading to the recent decision of the Congress; speaks hopefully of the work to which he is designated, and announces his purpose to make an appeal in all countries for a capital of 400,000,000 francs.—On the lines of sodium vapour, by Mr. Lockyer. Sodium vapour, after long distillation in vacuo, no longer gives the line D near the metal.—Widmanstaetten figures on artificial iron, by Prof. Lawrence Smith. He obtains them by heating a siliciuret of iron on lime with the gas blowpipe; the metallic button, after cooling, is placed in warm nitric acid.—M. Schiaparelli was elected Correspondent in Astronomy in room of M. Tisserand, and Prof. Huxley in Anatomy and Zoology in room of M. de Baer.—On a mode of transformation of ruled surfaces, by M. Mannheim.—Solar observations during the first quarter of 1879, by M. Tacchini. Of 35 days utilised, 32 figure without spots and cavities. The mean frequency per day was 0·33 (relative proportion greatest in January); this is less than in 1878, showing extension of the minimum. The hydrogenic protuberances also show diminution; they are nearly all in the northern hemisphere; the faculæ in the equatorial zone.—New arrangement for increasing the sensibility of the vibrating plate in the telephone, by M. Decharme. This consists in fixing the plate by its centre point alone, instead of by its border.—On a combination of alumina with carbonic acid, by MM. Urbain and Renoul. The composition (very unstable) is alumina, 52, carbonic acid, 11, water, 37; a hydrated sub-carbonate of alumina. Prof. Smith stated that the mineral, *Dawsonite* found at Montreal, is composed of carbonate of alumina and soda.—Influence of the pneumogastric and action of digitaline on the movements of the heart in sharks, by M. Cadiat. Digitaline acts as poison directly on the heart, causing tetanisation of the ventricle and diastole of the auricle.—On evolution of the embryo in eggs subjected to incubation in warm water by M. Dareste. He got such evolution to some extent in repeating the experiment of Reaumur (whose results were negative).—On a case of trichinosis observed in a young hippopotamus of the Nile, which died in captivity, by M. Haeckel.—M. Delecheneau described a modification of the phonograph.

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