THURSDAY, FEBRUARY 12, 1874

A MINISTER FOR SCIENCE

WE are glad to see that the *Times* has at last opened its pages to the question of the propriety of appointing a responsible Minister, whose duty it shall be to look after the interests of Science and of scientific research and education, and take charge of the scientific institutions of the country—institutions whose efficiency is at present sadly crippled from the want of a single responsible head. The whole question could not be better stated than in Colonel Strange's letter which appeared in the *Times* of the 6th inst., and which we consider so admirably to the point, that we gladly reproduce it here. We hope the letter will lead to further discussion, and that whatever Government may hold the reins in the forthcoming Parliament, the important question now raised may meet with serious attention. Colonel Strange's communication runs as follows :—

"No subject affecting the material interests of England is more important at the present day than that discussed at Manchester by Lord Derby, and by yourself in your leader of the 12th ult.

"'Scientific industry' is one of those clever phrases calculated to catch the eye and ear by its novelty, while it expresses what is already well known by other antiquated names. Lord Derby defines it and explains its meaning in a variety of ways ; but throughout his whole speech he is talking, while never naming it, of nothing more nor less than scientific research. The utilisation of redundant natural forces and of waste substances, on which he insists as a primary object of the new movement, is to be brought about by patient, continuous, systematic research, and by nothing else. I own I prefer the old words to the new, but if by using new words old wants come to be recognised and supplied, I shall not complain.

"I, and many who think with me, maintain that scientific research must be made a national business ; that the point at which Science, in most of its leading branches, has now arrived and the problems presented for solution are such as to need for their adequate treatment, permanent well-equipped establishments with competent staffs worked continuously and systematically. Lord Derby truly describes it as a case in which what is 'everybody's business is nobody's business.' We must make it somebody's business. We must make it the State's business. We have tried individual enterprise, which so many hold to be all-sufficient. There is more individual enterprise in England than in any country in the world, and yet we are being rapidly outstripped by nations who, though they encourage private exertion, are wise enough not to rely on it, but to establish a system free from the caprice, the incompleteness, the liability to interruption and cessation incident to all individual labour in whatever field. If asked to describe the system we propose to establish, our reply is in one word, 'completeness.' A steam-engine is a system, composed of many parts, each and all essential to its useful action. Furnace, boiler, cylinder, pistons, connecting rods, beam, and fly-wheel—all controlled by a governor. Tested by the condition 'completeness, what is Lord Derby's new society ? What is any private society ? A mere connecting rod—a most useful link in the system, not to be dispensed with, but still a mere link. Where are the other parts ? Is there a trace of them in England ?

"The first essential to any system is a head. No domestic household, no manufactory, no ship, no army or navy, no public or private establishment of any kind, and these are all 'systems,' can hold its own for a day without a head. But at the present hour there is no head to the science of England. The proposed remedy for this deficiency will have been anticipated as obviously a Minister of State, who shall be responsible to the nation through Parliament for everything connected with the scientific business of the country. For want of this head what have we done ? The various scientific institutions at present maintained by the State are distributed according to the following list, which was correct some time since, but may have undergone recent changes :—1, Royal Observatory, Greenwich, under the Admiralty ; 2, Royal Observatory, Edinburgh, under the Office of Works ; 3, Royal Observatory, Cape of Good Hope, under the Colonial Office ; 4, 5, 6, the Observatories at Madras, Calcutta, and Bombay, under the India Office ; 7, Ordnance Survey of Great Britain, under the Office of Works ; 8, the Great Trigonometrical Survey of India, under the India Office ; 9, Exchequer Standards Office, under the Board of Trade ; 10, the Royal School of Mines, under the Privy Council : 11, British Museum, under 50 irresponsible trustees ; 12, Meteorological Office, governed by an unpaid, and therefore irresponsible, Committee of the Royal Society, under the Board of Trade ; 13, the Royal Botanic Gardens of Kew, Edinburgh, and Dublin, under the Board of Works ; 14, the Geological Survey, under the Privy Council. My list is perhaps not quite complete, but as it stands it shows that we place our scientific institutions under no less than seven different Departments of State, all of which have other matters besides science to attend to. Can anyone pretend there is any trace of a system here ? Is it not a grotesque caricature of State administration ?

"Granted that there must be a Minister for Science—and I am happy to say that those who have given most attention to the question now admit that there must—then the whole of the institutions I have named, besides some others now in existence, and many others that must before long be founded, would be placed under him. This would secure the great object of harmony and unity of parts, of provision for modification and extension, and of definite responsibility to the nation through Parliament, none of which objects are obtainable or seem even dreamt of at present.

"Whether such a Ministry should be created as additional to what we at present possess, or whether some existing Minister should be charged with Science ; whether the Science Minister should not also take Education, Art, and Music under his care ; whether he should not have permanent unparliamentary advisers, and if so on what scale and how constituted, besides many other points, are all extremely important questions, admitting of a great variety of answers ; but compared with the fundamental necessity for a Minister at the head of a Department controlling the whole public scientific activity of the kingdom, they are matters of subordinate detail.

"The Royal Commission on Science, presided over by the Duke of Devonshire, has, for nearly three years, been most assiduously engaged in collecting a body of information of infinite value, and they will no doubt forward many important recommendations on the evidence they have taken; but for my part, as one deeply interested in their proceedings, to which I have contributed largely as a witness, I do not hesitate to say that if they only succeed in obtaining the creation of a Science Minister, that result alone will amply repay the country for the cost of their investigations.

"Let this be done, and we should cease to witness the farce of consulting the Chancellor of the Exchequer about observing eclipses of the sun, the Prime Minister about scientific Arctic expeditions, and the Treasury about tidal reductions. We should perhaps, too, then perceive that overworked Law Officers are not the best managers of a great, or what should be a great, technical Museum, and that fifty irresponsible gentlemen, however eminent individually, ought not to be entrusted with the grandest collection of Art and Natural History in the world. Nor would a wise statesman like Lord Derby fail to perceive, with all science concentrated under one view for his inspection, that a private local Society will prove no match for the complete and powerful State systems of Germany, France, and other Continental nations."

PINK AND WEBSTER'S "ANALYTICAL CHEMISTRY"

A Course of Analytical Chemistry (*Qualitative and Quantitative*). By William W. Pink and G. E. Webster. (London: Lockwood & Co., 1874.)

THIS work forms a volume of Weale's Rudimentary Series, and is advertised "as specially adapted for the use of those students who intend competing in the Advanced or Honours Stage Examinations (Inorganic Chemistry) of the Science and Art Department, also for preparing those intended to sit for the higher class examinations at Colleges, Public Schools," &c. The success which several well-known serial publications of a similarly special nature have deservedly had, appears to have stimulated the publishers of Weale's Series to embark in this enterprise. As the excellence of most of their former publications will be generally admitted, we can only regret that a literary (?) production displaying such deplorable ignorance should ever have found a place in their series. It has rarely been our duty to pass judgment on a more carelessly got-up book. Had it not been advertised as specially adapted for the use of the Science Classes under the Science and Art Department, we might have put it aside with a hearty laugh over the many absurd blunders it contains. Since a practice has, however, sprung up of late to câter for the wants of Science Classes, by printing books (sometimes obtained on commission) privately, and advertising them by means of post-cards, at so many postage stamps a copy, whereby these books manage to escape the eye of the reviewer, and as we fear that much mischief is being done by certain cheap cram-books, strung together with a view to save the teacher as much trouble as possible, our readers will perhaps bear with us if we examine the book before us somewhat closely. If rumour speaks true, some teachers manage to teach chemistry—even analytical chemistry—without ever touching a test-tube or performing the simplest experiments. Questions from previous examinations are eagerly collected and "worked" in the belief that the examiner is sure to give, if not the same questions, at least others of a similar nature. We need not fear giving offence to those earnest and hard-working men, engaged, often on a mere pittance and under most adverse and discouraging circumstances, in imparting a sound knowledge of chemistry in places which would not otherwise be reached by any educational efforts, if we conclude from the course of analytical chemistry before us, that some teachers (Mr. Webster styles himself Lecturer on Metallurgy and the Applied Sciences, Nottingham) are deplorably ignorant of the science they profess to teach.

Beginning on p. 4, we are told that "the term atom is sometimes applied to a compound as well as simple radicals, such as ammonia, hydroxyl, &c.": that "for fixed solids which do not vaporise, the atomic weights are referred to the element lithium, the atomic weight being determined by the amount of heat which any body contains, when it is at the same temperature as lithium, both being the same weight, lithium being considered as seven." On p. 7, "difference of attraction is called the bond affinity, that is, it is assumed that the different atoms possess power, lines of force, or *points of attraction*, called by Dr. Frankland *bonds*." On p. 12, we are informed, that "there are four different forms of notation, or formulæ in present use, two of which are *graphical*, viz. the *glyptic* and *graphic* formulæ. The other two, viz. the empirical and the constitutional or rational, are the symbolic representations." We give it upon the authority of our joint authors, that "Dr. Crum Brown was the first to introduce this form of formulæ, and that it has now been adopted by Dr. Frankland, and generally throughout the kingdom." And on p. 14, we are told, that "students who do not already understand the constitutional formulæ are strongly advised to obtain a complete knowledge of them, not only as an addition to their knowledge, but because the other is now not recognised by many colleges, or *allowed* in many examinations." For fear our authors' inadvertence should lead to further mischief, we may at once state that, to our knowledge, such is not the case, and that the authors are as much in the dark about what is recognised by many colleges or "allowed in many examinations" as they are about chemical analysis.

We can only pick out some of the choicest specimens from the authors' analytical bouquet. Beginning on p. 26, we are told that "deflagration is the arrangement of the crystals of a substance, and is, in ordinary terms, the crackling of a body when exposed to heat;" on p. 28, that "hardly any amount of reading or lecture-hearing can produce a practical analyst, as only practice can make perfect, and therefore the student is strongly recommended to make the experiments himself." We for once entirely agree with the theory, but strongly object to the "practice" of our joint authors. The information on p. 30, that "melted lead cannot be poured even in a cold platinum crucible without spoiling it, and that a drop of lead, tin, or bismuth, falling upon a red-hot platinum vessel invariably makes a hole in it," we owe probably to the sad experience gained by the metallurgical partner in the joint-authorship, and science-students possessing platinum vessels must surely feel thankful for the hint. Great confusion of ideas seems to prevail, however, on the subject of platinum, for we are told on p. 31 that "platinum combines easily with silica and carbon, so that the contact of platinum crucibles with charcoal at a very high temperature must be avoided," together with several other absurd precautions which we will not quote. On p. 33, there figures an apparatus for rapid filtration in an atmosphere of steam, which we have seen before in Normandy's Introduction to his translation of Rose, and which we should have thought had been superseded long ago by more perfect methods of filtration.

As specimens of analytical knowledge (?) we quote p. 57, "hydrochloric acid gives a precipitate on dilution with water (distilled) if $BaCl_2$ or SO_2Ho_2 be present;" p. 38, "dilute sulphuric acid contains more lead, and lead is scarcely soluble in concentrated acid;" p. 42, "A solution of baric chloride must be neutral to test-paper, after precipitation by sulphuric acid;" p. 43, "sodic carbonate must completely volatilise;" p. 46, "hydrofluosilcic acid can be obtained from the chemists in india-rubber bottles."

The analytical tables on pp. 57 to 73 are equally deficient and faulty. We are told to test for ammonia, after having ignited on platinum foil; "a watch-glass becomes corroded on the addition of baric chloride to a neutral solution of salts," "hydrobromic acid turns starch-paper blue," "sodic hydrate," on p. 94, "precipitates light-coloured ferric hydrate which turns dirty-green." Upon heating chlorates, p. 115, "a very violent deflagration ensues." The authors appear never even to have prepared oxygen gas.

The quantitative knowledge displayed by the authors is quite on a par with the choice bits of qualitative chemical information so liberally and innocently volunteered by them. We will not tire our readers, however, by any further quotations, but cannot refrain from firing a parting shot or two by quoting from p. 120, where we are told that "Chlorine is prepared by the mixing of salt, hydrochloric acid and manganic oxide; this last, MnO_2, has no chemical reaction in the last equation;" and from p. 136, on which we are told that "in order to keep the edges of the balance free from rust, it is a very good practice to place inside the case a beaker, half-filled with sulphuric acid or baric chloride." A dialyser is described on p. 171 as "an apparatus having sides and top of gutta-percha, and bottom of parchment, and is used for the separation of urea and other crystallisable salts from urine."

Need we do more than recommend the authors to act upon their own advice (p. 2), and "to speedily endeavour to obtain a complete knowledge of the composition of bodies, and make themselves conversant with the formulæ &c.," of which they exhibit so deplorable an ignorance, before they again venture upon enlightening the public on the subject of chemistry.

THE RACES OF MANKIND

The Races of Mankind: being a Popular Description of the Characteristics, Manners, and Customs of the Principal Varieties of the Human Family. By Robert Brown, M.A., Ph.D., &c. (Cassell, Petter, and Galpin).

THE rapid growth of interest in Anthropology is proved by the appearance, one after another, of popular illustrated works: Mr. J. G. Wood's "Natural History of Man" in 1868-70, an English translation of M. Louis Figuier's "Human Race" in 1872, and just now (though without the date it ought to have on the title-page) this first volume of a work on "The Races of Mankind." Of these, the productive M. Figuier's book is too worthless to say much of, and the comparison lies between the first and last. Both are valuable, and the ground they cover is so far different, that they may be usefully placed side by side in the ethnologist's library. It will be remembered that Mr. Wood's account of Africa occupied the first of his two volumes, so that his account of the races of Asia, America, Polynesia, &c., had to be disproportionately condensed into the second. Dr. Brown, we trust, will be able to keep his scale more uniform. His first volume treats entirely of American races, and he speaks with personal knowledge of the Esquimaux and North-west tribes, compiling as to other tribes with discretion, and generally from not too hackneyed authorities. Such of Dr. Brown's illustrations as are taken from photographs and real drawings are good, and preferable to the too picturesque and imaginative cuts of Mr. Wood's artists. But Dr. Brown inserts some drawings which he had better for truth and good taste have left out. Thus, the Indian scalping his victim at page 68, though no doubt more like the reality than the engraving in vol. ii. of "Schoolcraft," from which it is a kind of rationalised copy, is a piece of sensational make-up; while on the next page a scene of Indians torturing a captive by a slow fire on his stomach, is still more objectionable. At page 284 is a representation of Conibos shooting turtle; this is evidently a fancy picture, and arrows shot at such an angle would glance off the animal's carapace; the arrows should have been shown of heavier make, and so sent up as to fall almost perpendicularly.

As only the first part of Dr. Brown's work is yet out, it may perhaps be a service to make some suggestions. Native words are sometimes wrongly printed, which gives an air of carelessness to the descriptions they form part of. Thus "inniut" instead of "innuit" (p. 5); "Manco Capas" and "Manià Dello" (p. 119), which appear to be intended for the usual forms, "Manco Capac" and "Mama Oello" (Mr. C. R. Markham would say that "Ccapac" and "Ocllo" are the really proper forms). At page 274, the account of the "couvade," the custom of the husband being put to bed or otherwise treated with reference to his wife's bearing a child, is compiled very inaccurately. Lastly, though references are generally given where long abstracts have been made from books of travel, Dr. Brown seems somewhat apt to make statements and use arguments without due mention of the sources whence he derived them. One consequence is, that he makes himself personally responsible for any blunder in the matter he thus appropriates. Thus, at page 147 a passage is inserted of which the following is part:—"In the Ladrone Islands, the Spaniards found the natives unacquainted with fire; and when Magellan set fire to the huts of the Marian islanders, they looked upon the flame as a living creature which fed upon wood." Unless my memory deceives me, this passage is copied out of Büchner's "Man in the Past, Present, and Future," and has been already commented on in NATURE, not only as embodying statements which have been disproved, but as showing a certain geographical weakness in the writer, who did not know that the Ladrone and the Marian Islands are the same. E. B. T.

OUR BOOK SHELF

Typhoid Fever: its Nature, Mode of Spreading, and Prevention. By William Budd, M.D., F.R.S. Pp. 193. Three plain and one coloured lithograph. (Longmans, 1873.)

THIS handsome volume is a thesis on the question of how typhoid or enteric fever is propagated. Dr. Budd adopts what is known as the contagion theory, and believes that every case of the disease is the result of direct poison, conveyed either by the air or more frequently in water, from the intestine of one patient to that of another. This theory is generally disbelieved by the best medical authorities in London and Paris; but, as Dr. Budd points out, it is not in large towns that the transmission of disease can best be traced. He describes with minute exactness as to time, place, and other important conditions, outbreaks of this terrible disease in secluded country villages, in schools, and other isolated institutions, where he was able to trace the steps of the epidemic from house to house or from room to room. We believe that a candid perusal of these cases will bring the conviction that the theory of contagion is fairly proved. Many of them are at all events almost decisive against the theory that this enteric fever is "pythogenic," *i.e.* is the result of a poison which may be produced by any decomposing sewage under favourable circumstances, without previous contamination from a diseased person. The practical importance of the question is, that if enteric fever only spreads as Dr. Budd and other contagionists maintain, it is possible, and therefore of the utmost importance, to check its propagation. A great part of the book is devoted to this point, and the mode of destroying diseased products is carefully detailed.

One obvious objection to the contagion theory is that it only accounts for the spread, and not for the origin, of the fever. But, as Dr. Budd argues, the same applies to small-pox and every other undoubtedly contagious disease. However the first case came about, no one supposes that fresh ones now arise spontaneously, any more than naturalists who believe that worms and buttercups once came into being for the first time, expect to find a worm appear in a drop of water without an egg, or a buttercup in a meadow without a seed.

The comparison of typhoid disease to the eruption of small-pox, which is revived by Dr. Budd, has been long and deservedly abandoned: indeed the strictly pathological part of this book is the least satisfactory. Notwithstanding a somewhat "drawing-room" appearance, it is no doubt intended for pathologists and physicians to study; and for them we cannot see the advantage of the four illustrations, one of which forms an elaborately coloured frontispiece; they show nothing but what has often been figured before, and is now universally familiar. The style also is now and then too ambitious, suggesting rivalry with the wretched newspaper writing quoted on p. 110 as "lively and facile." On the whole, however, the book is as solid as it is earnest, and may be compared without detriment with Dr. Macnamara's well-known work defending an almost identical theory and practice with regard to the propagation and prevention of Asiatic cholera.

The facts and arguments contained in it will no doubt be duly weighed by the medical profession, and the public will benefit by the result. P. S.

Inorganic Chemistry, Elementary. By Raphael Meldola. F.C.S. (London: Thomas Murby, 1873.)

THE present little volume constitutes one of a series produced by the same publishers as "Science and Art Department Text-books." We must congratulate Mr. Meldola on having produced in a small compass a thoroughly good and sound introduction to the science of chemistry, and it is all the more welcome in these days of "Science Series," when so many badly done "Text-books" are being produced. The information is well and clearly stated, and is sufficiently free from technicalities to be easily understood by the beginner. The book is plainly and well printed, but we cannot congratulate the publishers on the execution of the few and simple woodcuts, every one of which has been spoiled in the cutting. We hope that in a future edition the work will receive better treatment, as a well-done woodcut is a great aid to the beginner in understanding his author's descriptions of various experiments.

LETTERS TO THE EDITOR

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

The Photographic Society

THE sweeping condemnation of the Photographic Society conveyed in an article in NATURE, vol. ix. p. 263, can only have been written under a want of knowledge or misrepresentation of facts. I will not say one word about any dissension which may exist in the Society, but as the statements you have published are calculated to injure the Society very materially, I will ask you, in common justice, to make public the transactions of the Society for the past year, so that the readers of NATURE may judge for themselves whether in a body which does not profess to be a purely scientific one, science is altogether ignored, whether "no man of eminent scientific capacity takes part," and whether the society is altogether beneath contempt as at present conducted. This I ask you to do not only in justice to the society, but to the gentlemen whose names are mentioned below.

1873. January meeting. "The Photographic Operations at the Royal Observatory in connection with Magnetical and other records," by James Glaisher, F.R.S.; "Instantaneous Microphotography," by E. J. Gayer, M.D.

February.—"On the Principles of the Chemical Correction of Object-Glasses," by Prof. G. G. Stokes, D.C.L., Sec. R.S.

March.—"A Contribution to the Early History of Photography," by H. Baden Pritchard, F.C.S.

April.—"Uranium Printing," by John Spiller, F.C.S.; "The Chemical Theory of the Latent Image," by Capt. Abney, R.E., F.C.S., F.R.A.S.

May.—"Improvements in Carbon Printing," by Mons. A. Marion.

June.—"Remarks on three Wet Processes," by Jabez Hughes; "Photo-collotype Printing," by Capt. Waterhouse, B.S.C.

December.—"Photography in the Arctic Regions," by Lieut. Chermside, R.E.

So far as investigations are concerned, I can mention two, at least, now being undertaken by members of the society, touching the process and nature of film best suited for the Transit of Venus observations.

BADEN PRITCHARD, Hon. Sec.

9, Conduit Street, W., Feb. 7

Animal Locomotion

THERE are two or three points in Dr. Pettigrew's new book as to which, perhaps, many of your readers in common with myself would be glad of a little light. First, in speaking of the gannet, he says: "Each wing, when carefully measured and squared, gave an area of 19½ square inches." But how is such an area obtained from the dimensions given? They are: "girth of trunk, 18 inches," *i.e.*, about 5 inches for its width; "expanse of wing from tip to tip across the body, 5 feet," so that each wing would stretch about 33½ inches from root to tip; "across secondaries, 7 inches," and this we may take as about the average width of the wing. Multiplying length of wing by width (33½ × 7), we get therefore an area of 234½ square inches. Similarly Dr. Pettigrew assigns the heron's wing an area of 26 square inches, although the dimensions he gives yield an area of about 311 square inches. A friend of mine has the temerity to suggest that for some reason or unreason Dr. Pettigrew has divided the true area by 12, for so 234½ (if we neglect the half inch) gives just 19½, and 312 instead of 311 gives 26.

But (as a second matter) my friend's notion of Dr. Pettigrew's arithmetic receives some colour from the sentence following the one before quoted, viz., "The wings of the gannet, therefore [each wing being supposed 19½ square inches], furnish a supporting area of 3 feet 3 inches square." So of the heron. Having told us that the area of each wing is 26 square inches, he says, "Both wings consequently furnish an area of 4 feet 4 inches square." Here, surely, square inches have been treated as if they were linear, and only 12 of them instead of 144 reckoned to the square foot!

Once more (as was observed in your review a week or so ago), Dr. Pettigrew maintains, in opposition to all other experimenters, that in flight the downward stroke of the wing is directed forwards and not backwards. Now, to say nothing of the "singularity" of representing the wings in his own case as concavo-convex, and in that of his opponents as flat (much to the detriment of the latter), the whole of Dr. Pettigrew's "mathematical demonstration" of his position is so extremely original that I fear for the uninitiated it is only explaining *obscurum per obscurus.* Would he condescend to accepted methods and prove his case by the parellelogram of forces? As it is, his proof amounts simply to this:—"As the under surface of the wing, which is a true kite, looks upwards and forwards, it tends to carry the bird upwards and forwards." No doubt, if the wing remain still, and the bird have already a sufficient velocity. A kite is sustained or elevated by an extraneous force, either the wind impinging against its under surface, or *vice versâ* when the boy runs. But whence comes the bird's motion, before its wings can act as kites? Dr. Pettigrew nowhere tells us this, but starts with his bird already flying. Thus he says:—"The bird, when flying, is a body in motion. It has already acquired momentum. If a grouse is shot on the wing, it does not fall vertically downwards, as Borelli and his successors assume [Shade of Borelli!], but downwards and forwards. The flat surfaces of the wings are consequently made to strike downwards and forwards, as they in this manner act as kites to the falling body, which they bear or tend to bear upwards and forwards." Here it is unmistakeable that the function of the wings in generating velocity is confounded with their function in directing the velocity already generated; just as if one should confound a steamer's rudder with her screw. The question is, How do the wings generate velocity? In this respect it is immaterial whether the bird is at rest or in motion. But to this there can be but one answer, at least if we are still to believe that "action and reaction are equal and opposite;" the answer that is, that everybody gives but Dr. Pettigrew. The downward motion of the wing is wholly concerned with sustaining or elevating against gravity. A backward movement must carry the bird forward; Dr. Pettigrew's forward movement must make it fly tail first.

JAMES WARD

Trinity College, Cambridge, Feb. 2

YOUR reviewer resorts to very strong language, without, it appears to me, justifying his procedure. In reviewing my volume he exclaims, "Imagine our disappointment on finding that, instead of the work being by the hand of a master, its author is deficient in the knowledge of the first principles of physics, and of the undoubted meaning of some of the most simple terms employed in the science; his argument, if it may be so called, being but little more than a long series of vague and fanciful analogies, incorrectly stated physical facts, and untenable theories." . . . "We must say that we expected better things of Dr. Pettigrew, and regret that he has not, before now, learned that there are errors in his methods and results that cannot be tolerated by a thinking public, which prefers accurate reasoning rather than dogmatic statement, and well-grounded fact to fanciful analogy" (NATURE, vol. ix. p. 221). One would naturally have expected after such announcements an exposure of false theories and a criticism of the nomenclature employed, but Mr. Garrod condescends upon neither. He takes refuge in general statements and implies what he does not attempt to prove.

He states, *e.g.* "that it is at present impossible to obtain from any form of fuel, a sufficient percentage of the potentiality which it possesses for doing work, to work an engine sufficiently compact and light for the wings which it has to drive." Now this is utter nonsense. In 1868 Mr. Stringfellow, of Chard, Somersetshire, exhibited at the Crystal Palace a flying machine which with its engine, boiler, water, fuel, flying surfaces, and propellers only weighed 12 lbs. The engine of this machine exerted the third of a horse power and obtained the 100*l.* prize of the Aëronautical Society of Great Britain as being at once the lightest and most powerful steam-engine ever made.

What bird weighing 12 lbs. can Mr. Garrod inform me exerts a third of a horse power in flying?

This one fact proves that in the ordinary steam-engine we have a power more than equal to the production of flight.

Mr. Garrod takes exception to my statement that "weight when acting upon wings, or, what is the same thing, twisted inclined planes, must be regarded as an independent moving power."

This point will be best illustrated by an example. If a gannet drops from a cliff with *expanded motionless wings* it can sail for an incredible distance, the weight of the body dragging upon the wings, doing the principal part of the work. This is a matter of observation, and the principle may be exemplified by the following simple experiment. If an apparatus composed of two quill feathers stuck in the end of a cork be made to fall from a height it will be found to travel *downwards and forwards* in a curve, the forward curve equalling half the space through which the apparatus descends. Here we have no muscular movement to direct or influence the motion in any way, and it certainly seems to me to afford an explanation of the manner in which mere *weight*, or gravity acting upon weights, may by the aid of wings be made to propel a body from one point to another.

Mr. Garrod proceeds—"After such indications of imperfect knowledge, nothing in the way of mechanical theories could cause surprise, and we are therefore not astonished to find it laid down as the fundamental principle of flight, that *the up-stroke of the wing aids in propulsion*, and that in the down-stroke *the inferior surface of the wing is directed downwards and forwards.*" If Mr. Garrod attempts to elevate a natural wing or an artificial one properly constructed, even in a strictly vertical direction, he will find that it inevitably darts upwards and forwards in a curve and carries the hand with it. In this manner, as experiment proves, *the ascent of the wing aids in propulsion.* If again Mr. Garrod attempts to depress the wing vertically downwards, he will as certainly find that it darts *downwards* and *forwards* in a curve, the hand being carried in the direction specified. The upward forward and downward forward curves, being united as they are in flight, give a *waved track.* If the wings did not dart *forwards* both during their ascent and descent the body of the bird could not be transferred from one place to another in a horizontal waved line which it is. Mr. Garrod is evidently imperfectly informed on the subject of flight, for he inquires "Who can see any close relation between the flight of birds and that of a kite?" The merest tyro in mechanics will, I think, perceive this on a moment's reflection. The kite is pulled *forwards* on the moving air by the string. The kite formed by the wings of a bird is *pushed* forwards on the moving air by the weight of the body.

I do not forget, as Mr. Garrod insinuates, that a kite requires a string. The following passage, written in 1867, will show this. "The wing of a bird acts after the manner of a boy's kite, the only difference being that the kite is pulled forwards upon the wind *by the string* and the hand, whereas in the bird the wing is pushed forwards on the wind by the weight of the body and the life residing in the pinion itself." *

Mr. Garrod's words are—"Dr. Pettigrew seems to forget that a kite needs a string, and yet, backed by his false analogy, *he has the presumption* to quote the experimental verifications and opinions of such able and ingenious thinkers as Borelli and Marey, the authors of the true theory of flight, only to reject them." To one who has experimented on the subject of flight for the last 10 years, the term *presumption* in this sentence sounds strange. One may, I venture to think, without presumption, differ from another after such mature deliberation. Marey's theory of flight, which is nearly, if not identical, with my own, was not promulgated till nearly two years after I had published mine. This point will be fully discussed in the *Athenæum* of Feb. 14. In fact Marey frankly admitted this in a letter to the French Academy of Sciences in reply to a reclamation lodged by me with that learned body.

His words are:—"J'ai constaté qu' effectivement M. Pettigrew a vu avant moi, et representé dans son Mémoire, la forme en 8 du parcours, de l'aile de l'insecte; que la méthode optique à

* On the various modes of flight in relation to aëronautics: Proc. Roy. Instit. of Great Britain, March 22, 1867.

laquelle j'avais recours est à peu près identique à la sienne. . . . Je m' empresse de satisfaire à cette demande légitime, et je laisse entièrement la priorité sur moi, à M. Pettigrew relativement à la question ainsi restreinte." (*Comptes Rendus* for May 16, 1870, p. 1093.)

The next point which Mr. Garrod takes up is the "induced currents" of the wing. I state that "the efficiency of the wing is greatly increased by the fact that when it ascends it draws a current of air up after it, which current, being met by the wing during its descent, greatly augments the power of the down-stroke. In like manner, when the wing descends, it draws a current of air down after it, which, being met by the wing during its ascent, greatly augments the power of the up-stroke." This is simply a statement of fact, and if Mr. Garrod causes a natural or artificial wing to vibrate he will find that the wing takes a greater catch of the air when a down- and up-stroke or an up- and down-stroke are made in rapid succession, than when a single stroke is made either in the one direction or in the other. This point becomes especially clear if a large artificial wing be constructed on the insect type and made to vibrate in a horizontal direction. If such a wing have its anterior margin slightly elevated and made to travel from right to left of the operator it draws after it a current of air which, being met by the wing when it is reversed and made to pass from left to right, acts as an autumn breeze to a kite. The wing literally flies on the current which it creates. It ascends at each thrust and carries the hand of the operator with it. Similar remarks are to be made of the tail of the fish. It is in this way that the *back air* and *back water* are utilised, and herein lies the excellence of the elastic reciprocating screw, as found in Nature, and as contra-distinguished from the rigid rotatory screw employed in navigation.

Mr. Garrod, adducing no proof in refutation of this and similar experiments, states "that these *induced currents* are of no real service in flight, because in their production there is as much force lost as there may be gained from their subsequent employment on the reversal of the action of the wing, if the bird's body has not advanced sufficiently far to be in each stroke beyond the range of their action, which is probably the case." On what authority does Mr. Garrod make this assertion? When a bird flies in still air, the wing of necessity must vibrate. The quicker it vibrates the more marked the reaction obtained from the air, and the greater the elevating and propelling power. The *induced currents* powerfully contribute to this reaction from the fact that the wing and the air are both moving, and moving in opposite directions. This, as explained, is a matter of experiment, and can readily be verified.

Lastly Mr. Garrod attacks my views on muscular movements. Here again he adduces no counter-proof, and, adhering to the old doctrine, contents himself by saying, "We are not ashamed to say that such has always been and still is our idea." This is not saying much. He takes exception to my statement that muscles have a centripetal or shortening power and a centrifugal or elongating power. Can he inform me how the left ventricle of the heart opens after a vigorous contraction, in which all the blood contained in the ventricular cavity is ejected and the ventricle converted into a solid muscular mass, if not by a spontaneous *elongation* of all its fibres?

Edinburgh, Jan. 27 J. BELL PETTIGREW

Specific Gravity of Sea-water

IN reference to Mr. Strachan's letter in NATURE, vol. ix. p. 183, calling attention to the discrepancy between Dr. Frankland's results and my own, permit me to state that they were not obtained from the same series of samples, and that the figures given by Dr. Frankland were, I believe, obtained by the use of a balance on shore, and also that from the way in which his specimens were packed, they were not liable to any appreciable loss by evaporation. They were not, however, taken from that part of the North Atlantic which was examined during the time that I was on board the *Porcupine* in 1869, to which alone my observations refer. My own results were obtained, as stated on p. 503 of "The Depths of the Sea," by delicate glass hydrometers, so graduated that the sp. gr. could easily be read to the fourth decimal place. Two instruments only were employed for the 105 observations made, and though they gave identical results, I had no opportunity of comparing their indications with the results obtained by a balance from the same specimen of water. I may remark here, however, that though the *absolute* results may not be quite correct, the relations between the sp. gr. of surface, intermediate, and bottom waters, pointed out on p. 505 of "The Depths of the Sea," as well as the range of variation, are probably very near the truth, since the same instruments were employed in all the determinations, and at the end of the series they indicated the same as at the commencement, when placed in a test solution, which was preserved for the purpose of detecting possible variations in the instruments themselves.

Clifton, Bristol, Jan. 17 WM. LANT CARPENTER

THE LINNEAN SOCIETY

WE regret to hear of an unpleasant event which took place at the meeting of the Linnean Society on Thursday last (5th inst.). So far as we have been able to gather the particulars they are as follows.

When the usual minutes had been read at the commencement of the meeting, a Fellow of the Society rose in his place and endeavoured to propose a motion reflecting upon the conduct of the President at the preceding meeting. The President (Mr. George Bentham, F.R.S.) ruled that the Fellow was out of order and that his motion could not be put, and requested the would-be mover of it to sit down in his place. In spite of frequent calls to order, however, this gentleman persisted in his endeavours to bring forward his grievances, and to address the meeting. At last Mr. Bentham, finding that his efforts to preserve order were vain, and that the mover of the motion (who had given no sort of notice of his intentions) was backed up by a body of clamorous friends assembled specially for the purpose, quitted the chair and left the meeting-room, followed by the Secretary and all the other members of the Council present.

As the chair of the Linnean Society can only be taken by a member of Council, the meeting thus came to a premature end, much to the disappointment of those who had assembled to hear Mr. W. K. Parker read his paper on the osteology of the woodpeckers.

We regret to have to add that, in consequence of this untoward event, Mr. Bentham has tendered his resignation as President of the Society. But we trust that the Fellows who caused the disturbance will, upon reflection, feel that however much they might have considered themselves aggrieved by the President's decision at the previous meeting, they were not justified in the course they pursued. In all meetings the decision of a chairman upon a point of order is held to be final, at all events for the occasion. More especially should this be the case in a learned society assembled for the discussion of scientific problems, and not for vulgar wranglings and disputes upon immaterial subjects.

We trust therefore that an ample apology will be offered to the President by these gentlemen, and that he will be induced to retain his chair until the approaching anniversary meeting of the Society, when he had already given notice of his intention not to accept re-nomination. The great services which Mr. Bentham has rendered to Science generally and to the Linnean Society in particular, are too well known to the readers of NATURE to render it necessary for us to descant upon them in these columns. The Linnean Society has just acquired a new and most convenient abode in the apartments at Burlington House, recently provided for it by the liberality of the country, and it would be a great misfortune if disunion should succeed in marring the work of those who are now endeavouring to make the Society still more useful and more prosperous than it has been in past times.

POLARISATION OF LIGHT*

IV.

THE phenomena exhibited by selenite are also produced by other crystals, but the facility with which plates of the former substance can be obtained, causes them to be generally used in preference to others. There is, however, a peculiar class of crystals, of which quartz, or rock crystal, is the most notable, which gives rise to effects different from those hitherto described.

* Continued from p. 205.

If a ray of light pass through a plate of quartz which has been cut perpendicularly to the axis, or line parallel to the main planes bounding the crystal, it is as usual divided into two; but the vibrations in each ray, instead of being rectilinear and at right angles to one another, are circular and in opposite directions. That is to say, if the motion of vibration in one ray is directed like the hands of a clock, that in the other is directed in the opposite sense; and the light in each ray is then said to be circularly polarised. The motion of a series of particles of ether, which when at rest lie in a straight line, is circular, and, as in plane polarisation, successive; and consequently, at any instant during the motion such a series of particles will be arranged in a helix or corkscrew curve. The sweep of the helix will follow the same direction as that of the circular motion; and, on that account, a circularly polarised ray is spoken of as right-handed or left-handed, according to the direction of motion. A right-handed ray is one in which, to a person looking in the direction in which the light is moving, the plane of vibration appears turned in the same sense as the hands of a watch. Or, what is the same thing, to a person meeting the ray, it appears turned in the opposite sense, viz., that in which angles when measured geometrically are usually reckoned as positive.

The question, however, which mainly concerns us is the condition of the vibrations after emerging from the plate of quartz and before entering the analyser. In the passage of the ray through the plate the ether is subjected to a double circular motion, one right-handed, the other left-handed; but, as one of these motions is transmitted with greater velocity than the other, it follows that at any given point and at the same instant of time one of the revolutions will, in general, be more nearly completed than the other, or, to use an expression adopted in plane polarisation, there will be a difference of phase. The motions may be represented by two clock hands moving at the same rate in opposite directions, and the difference of phase by the angle between them when one of them is in the position from which angles are reckoned. As both are supposed to move at the same rate, they will have met in a position midway between their actual positions; and if we consider a particle of the ether (say) at the extremity of the clock-hands, it will be solicited when the hands are coincident by forces producing two opposite circular motions. Now, whatever may have been the forces or structural character within the crystal whereby this double circular motion is perpetuated, it is clear that when the ray emerges into air the particle of ether immediately contiguous to the surface of the crystal will be acted on by two sets of forces, one whereby it would be caused to follow the right-handed and the other the left-handed rotation. Each of these may, as is well known, be represented by a pair of forces, one directed towards the centre of the circle, the other in the direction of the motion and at right angles to the first, or, to use geometrical language, one along the radius and towards the centre, the other along the tangent and in the direction of the motion. The two forces acting along the tangent being in opposite directions will neutralise one another, and the resultant of the whole will, therefore, be a force in the direction of the centre. The particle in question, and consequently all those which following in succession serve to compose the entire ray until it enters the analyser, will vibrate in the direction of the diameter drawn through the point under consideration; or, to express it otherwise, the ray will be plane-polarised, and the plane of vibration will be inclined to the plane from which angles are measured by an angle equal to half the difference of phase on emergence due to the thickness of the crystal. The retardation being the same absolute quantity for all rays, will, as in the case of plane polarisation, be a different fraction of the wave-length for rays of different colours, and will be greater for the shorter waves than for the longer. Hence the planes of vibration of the different coloured rays, after emerging from the quartz, will be differently inclined. Each ray will therefore enter the analyser in a condition of plane polarisation; and if the analyser be turned round, it will cross the vibrations of the various coloured rays in succession, and extinguish each of them in turn. Each of the images will consequently exhibit a gradual change of colour while the analyser is being turned; and the tints will be, as explained before, complementary to those which are successively extinguished. For a given plate of quartz the order of the tints will be reversed when the direction of rotation of the analyser is reversed. But it should be here explained that there are two kinds of quartz, one called right-handed and the other left; and that, for a given direction of rotation of the analyser, these cause the colours to follow one another in opposite orders. A similar effect is produced by turning the polariser round in the opposite direction.

The angle of rotation of the plane of vibration for any particular colour varies, as stated above, with the thickness of the plate; while for a given thickness it increases nearly as the square (product of the quantity into itself) of the wave-length decreases. In mathematical language it varies approximately inversely as the square of the wave-length. If this law were accurately true, the product of the angles of rotation into the square of the corresponding wave-lengths (λ) would be the same for all rays. The following are some measurements made by Brock, with a quartz plate one millimetre thick, which show that the law may be considered as true for a first approximation.

Rays	Rotations	Rotations × λ^2.
B	15° 18′	7,238
C	17° 15′	7,429
D	21° 40′	7,511
E	27° 28′	7,596
F	32° 30′	7,622
G	42° 12′	7,842

If the colours exhibited by a plate of quartz when submitted to polarised light be examined by a spectroscope, in the way described when we were speaking of selenite, the spectrum will be found to be traversed by one or more dark bands, whose position and number depend upon the thickness of the plate. But there will be this difference between plane and circular polarised light, that if the analyser be turned round, the bands will never disappear, but will be seen to move along the spectrum in one direction or the other, according as the plate of quartz be right-handed or left-handed, and according to the direction in which the analyser is turned. This is, in fact, identical with the statement made before, that the analyser in its different positions successively crosses the plane of vibration of each ray in turn, and extinguishes it.

This being so, it is clear that a change of colour exhibited by a quartz plate when submitted to plane-polarised light and examined with an analyser, forms a test of a change in the plane of original polarisation. And if the plate be composed of two parts, one of right-handed, the other of left-handed quartz, placed side by side, any change in the plane of polarisation will affect the two parts in opposite ways. In one part the colours will change from red to violet, in the other from violet to red. At two positions of the polariser, or analyser, the colours must be identical. With plates, as usually cut, one of these identities will be in the yellow, the other at the abrupt passage from violet to red, or *vice versâ*. In this case the field appears of a neutral tint, *teinte sensible* or *teinte de passage*, as the French call it, and the slightest change in the plane of polarisation exhibits a marked distinction of colour, one part verging rapidly to red, the other to violet. This arrangement is called a biquartz, and affords a very delicate test for determining the position, or change of position, of the plane of polarisation, especially in cases where feebleness of light or other circumstance interfere with the employment of prismatic analysis.

If the thickness of the plate be such that the difference of rotation of the planes of vibration of the rays corresponding to the two ends of the visible spectrum (or, as it is sometimes termed, the "arc of dispersion") be less than 180°, there will be one dark band in the spectrum; because there can then be only one plane of vibration at a time at right angles to that of the analyser. If the arc of dispersion is greater than 180° and less than 360°, there will be two bands. And so on for every 180° of dispersion.

This mode of examination by means of prismatic analysis is the most accurate yet devised for measuring the angle of rotation produced by circular polarisation; especially if solar light be employed, and the fixed lines used to form a scale of measurement.

The property of circular polarisation is, however, not confined to quartz. Among solids, chloride of sodium is the only other known instance, but among fluids and fluid solutions there are not a few.

The following list is given by Verdet. The angles have reference to the red rays given by a plate of glass coloured with oxide of copper, and are affected with the sign + in the case of right-handed, and with − in the case of left-handed rotation. The length of the column of the solution is in every case one decimetre.

Essence of turpentine . . .	−29°·6
,, lemon . . .	+55°·3
,, bergamot . . .	+19°·08
,, bigarade . . .	+78°·94
,, aniseed . . .	− 0°·70
,, fennel . . .	+13°·16
,, carroway . . .	+65°·79
,, lavender . . .	+ 2°·02
,, peppermint . . .	+16°·14
,, rosemary . . .	+ 2°·29
,, marjorum . . .	+11°·84
,, sassafras . . .	+ 3°·29
Solution of sugar 50 per cent. . .	+33°·64
,, quinine 6 per cent. in alcohol .	−30°.

It will be noticed that the rotary power of all these substances is much less than that of quartz.

A mixture of liquids, one or both of which is active, generally exhibits a rotatory action represented by the sum or difference of their separate powers (a neutral liquid being considered to have a power represented by o); but this law is true only when no chemical action takes place between the elements of the mixture. Saccharine solutions vary not only in the amount but also in the character of their power of rotation; thus cane sugar is right-handed, but grape sugar left-handed.

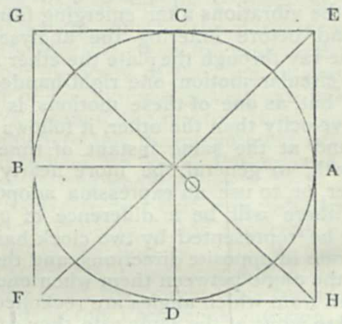
The property in question has been turned to practical use by employing the rotatory power of a saccharine solution as a measure of the strength of the solution. For this purpose a tube containing the solution to be examined is placed between two Nicol's prisms. The simple fact of circular polarisation is proved by a feeble exhibition of the phenomena shown by a plate of quartz cut perpendicularly to the axis. But for accurate measurement various expedients have been adopted. If a biquartz be inserted behind the analyser (the end of the apparatus next the eye being considered the front), then for a certain position of the analyser the two halves will appear of the same colour. When the tube for examination is inserted the similarity of colour will be disturbed; and the angle through which, right or left, the analyser must be turned in order to restore it will be a measure of the rotary power of the fluid.

Another method is as follows:—Use a single quartz instead of a biquartz; in front of it place a pair of quartz wedges, with the thin end of one opposite the thick end of the other; the outer surfaces having been cut perpendicularly to the axis. If the plate be right-handed, the wedges must be left-handed, and *vice versâ*. The wedges must be made to slide one over another so as together to form a plate of any required thickness, and a scale connected with the sliding gear registers the thickness of the plate produced. When the tube is removed the wedges are adjusted so as to compensate the quartz plate, and their position is considered as the zero point of the scale. When the tube is replaced, the wedges are again adjusted so as to compensate the action of the fluid in the tube, and the difference of the readings gives the thickness of quartz necessary for the compensation. The rotatory effect of a given thickness of quartz being supposed known we know at once the effect of a thickness of the fluid under examination equal to the length of the tube.

Another method has been based upon the principle of Savarts bands; but sufficient has perhaps here been said to illustrate the principle of the saccharometer.

Circular polarisation may, however, be also produced by other means, namely, by total reflexion, and by transmission through doubly-refracting plates of suitable thickness.

It will perhaps be best to begin with the last. And in order the better to understand the process we must consider briefly the result of compounding two rectilinear vibrations under different circumstances.



Suppose a particle of ether to be disturbed from its point of rest O in a direction O A. The attraction of the particles in its neighbourhood would tend to draw it back to O; and let O A be the extreme distance to which under these attractions it would move. Having reached A it would return to O, and passing through O with a velocity equal to that with which it started under the disturbing force, it would move to a point B equidistant from O with A, but in the opposite direction. And if, as is generally supposed, the ether is perfectly elastic, or that there are no internal frictions or other conditions whereby the energy of motion is converted into other forms of energy, the oscillations or vibrations of the particle between the points A and B will continue indefinitely. Now suppose that while these vibrations are going on, a second disturbing impulse, equal in intensity, but in a direction at right angles to the first, be communicated to the particle. it is clear that the effect on the motion of the particle will be different according as it takes place at the point of greatest velocity O, or at that of no velocity A or B, or at some intermediate point. Our object is to consider the effects under these various circumstances.

A complete vibration consists in the motion from O to A, thence to B, and finally back to O; so that if O be the starting point the passage through A will be removed one-fourth, the passage through O from A towards B will be one-half, the passage through B will be three-fourths, and the passage through O from B to A a complete vibration from the commencement. This being so, suppose that the second impulse be communicated while the particle is at O on its way towards A, then the impulses may be considered as simultaneous and the vibrations to which they give rise will commence together, and the waves of which they form part will be coincident. If the second impulse take place when the particle is at A, the two sets of vibrations or waves to which they belong will have a difference of phase (*i.e.* the first will be in advance of the second) equal to one-fourth of a vibration or one-fourth of a wave-length. If the second impulse take place when the particle is at O on its way to B, the difference of phase will be half; if when it is at B the difference will be three-fourths of a wave-length.

The particle being at O, and subject to two simultaneous impulses of equal strength, one in the direction of A, the other in that of C, must move as much in the direction of C as in that of A, that is, it must move in a straight line equally inclined to both, namely O E in the same figure. And inasmuch as the two impulses in no way impede one another, the particle will move in each direction as far as it would have done if the other had not taken place. In other words, if we draw a square about O with its sides at distance equal to O A or O B, the extent of the vibration will be represented by O E where E is a corner of the square. The complete vibration will then be represented by the diagonal E F in the same way as it was by the line A B in the first instance. If the impulse had been communicated at the instant of passage through O on the way to B, it is clear that a similar train of reasoning would have shown that the vibration would have been in the other diagonal G H. We conclude, therefore, that if two sets of rectilinear vibrations, or plane waves, at right angles to one another combine, then when they are coincident they will produce a rectilinear vibration, or wave, whose plane is equally inclined to the two, and lying in the direction towards which the motions are simultaneously directed. In the figure this is represented by the dexter diagonal. When the two sets of waves have a difference of phase equal to half a wave length, their combination gives rise to a wave represented in the figure by the sinister diagonal.

Suppose now that the second impulse is communicated at the instant when the particle is at A; in other words, that the two sets of waves have a difference of phase equal to one-fourth of a wave-length. At that instant the particle will have no velocity in the direction of A B (for convenience, say eastwards), and will consequently begin to move in the direction of the second impulse, say northwards. But as time goes on the particle will have an increasing velocity westwards and a diminishing velocity northwards, it will therefore move in a curve which gradually and uniformly bends, until when it has reached its greatest distance northwards it will be moving wholly westwards. And as the motion not only will be the same in each quadrant, but would be the same even if the directions of the impulses were reversed, it is clear that the curvature of the path will be the same throughout, that is to say, if two sets of waves of the same magnitude in planes perpendicular to one another, and with a difference of phase equal to one-fourth of a wave-length combine, they will produce a wave with circular vibrations.

If the second impulse be given when the particle arrives at B, that is, if the waves have a difference of phase equal to three-fourths of a wave-length, similar considerations will show that the motion will be circular, but in the opposite direction.

Suppose, therefore, that we allow plane-polarised light to fall upon a plate of doubly refracting crystal cut perpendicularly to the axis in the case of a uniaxal crystal, or in the case of a biaxal to the plane containing the two axes, say a plate of mica which splits easily in that direction; then the vibrations will, as before explained, be re solved in two directions, at right angles to one another. And further, if the original directions of vibration be equally inclined to the new directions, *i.e.*, if it be inclined at 45° to them, the amount or extent of vibration resolved in each direction will be equal. Further, if the thickness of the plate be such as to produce retardation or difference of phase equal to a quarter of a wave, or an odd number of quarter wave-lengths, for the particular ray under consideration; then the two sets of vibrations on emerging from the mica plate will recombine, and, in accordance with the reasoning given above, they will form a circular vibration, left-handed or right-handed according as the retardation amounts to an integral number of three-quarter wave-lengths or not.

It thus appears that a plate of mica which retards one of the sets of waves into which it divides an incident set by an odd multiple of quarter-wave lengths, affords a means of producing circular from plane polarisation. It remains to be shown that, with the same plate in different positions, right or left handed circular polarisation may be produced at pleasure. Suppose tbat the original vibrations are in the direction E F in the foregoing figure; the mica plate will resolve them into the two directions A B, C D, one of the rays, say the first, will be transmitted with greater velocity than the other, and the vibrations along C D will be one-fourth of a wave-length behind those along A B. This will correspond to the case discussed above, and will give rise to a circular vibration in a direction opposite to that of the hands of a clock. Suppose, however, that the plate be turned round through a right angle, so that the vibrations which are transmitted with greater velocity are placed parallel to C D, and those which are transmitted with lesser along A B. The ray whose vibrations are along A B will then be a quarter wave-length in advance, or, what comes to the same thing, they are three-quarters of a wave-length in rear of the others; and this condition of things produces, as explained before, a circular vibration in a direction the reverse of the former. It thus appears that the plate placed in one direction will convert plane into right-handed circular polarisation; and if turned round through a right angle from that position will convert plane into left-handed circular polarisation. A like change from right-handed to left-handed circular polarisation, or *vice-versâ*, may obviously be effected by turning the orginal plane of polarisation through a right angle; so that it shall lie between lines of concurrent instead of between lines of discordant motion.

W. SPOTTISWOODE

(*To be continued.*)

A COMPLETE SPECIMEN OF A PALÆOTHERIUM

FROM *La Nature* we learn that the palæontological collection of the Museum of Natural History of Paris has just been enriched by the addition of a new specimen of very great scientific interest, which is the entire skeleton of *Palæotherium magnum*, imbedded in a large block of gypsum and marl, the whole being exhibited in the anatomical department of the museum.

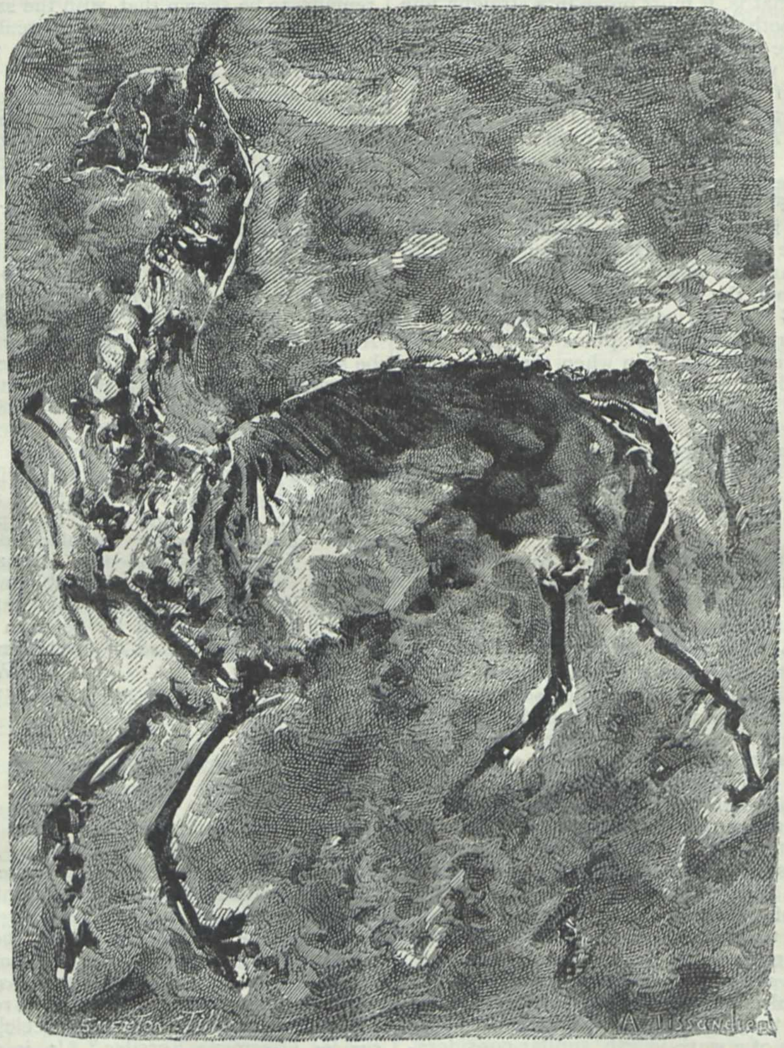
The *Palæotherium magnum*, whose name alone indicates its ancient existence, was first recorded by the great French naturalist Cuvier, in his celebrated "Recherches sur les Ossemens Fossiles." It is an animal which is entirely extinct, without any present representative. Individuals of the species must have been extremely abundant during the period that it existed. Modern zoologists place it among the Perissodactylates, that is to say, with the at present existing rhinoceros, tapir, and horse. It forms part of the fauna which is found abundantly embedded in the deposits of gypsum. All palæontological collections, even the most humble, have for a long time been provided with the remains, or more or less complete portions of this fossil form, but none have yet had the good fortune to obtain a complete skeleton.

The principal result of the examination of the new specimen which we are describing has been to show that until now very inexact notions have been entertained as to what this animal truly was when the proportions and general contour of the tapir were assigned to it, as was done even by Cuvier himself.

Far from being bulky and almost massive, as was thought, *Palæotherium magnum* is now evidently seen to be a very slender animal, with an extremely graceful carriage, with the neck longer than in the horse, and a general contour much on the same type as that of the Llama.

Without attempting a detailed study of its osteological structure, we may mention that *Palæotherium magnum* had a height a little less than that of a middle-sized horse. Three toes are found on each of the feet; the head, much like that of a tapir, had most probably also the rudiment of a trunk; the femur has a third trochanter; the dentary system is composed, in each jaw, of six incisors, two canines, and fourteen molars, these latter corresponding with the same teeth in the rhinoceros.

Palæotherium magnum, like its congeners, of which about a dozen species are at present known, was herbivorous, and without doubt lived in large herds. Its existence carries us back to that age of our earth which is termed the Eocene period, and it is in the middle of that period, which comprises the gypsum deposits or their geological



PALÆOTHERIUM MAGNUM

equivalents, that its remains are discovered, as well as those of all the other species of the same genus.

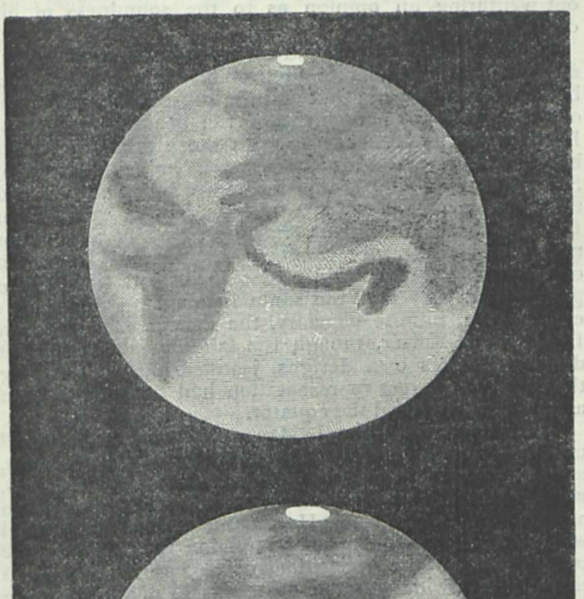
Nevertheless it made its appearance even before the gypsum formation, its presence having been detected in the beds of coarse limestone, which are inferior to and therefore more ancient than that formation.

It is the plaster quarries of Montmartre, Pantin, and La Villette, near Paris, which have for a long time held the privilege of furnishing to palæontologists the numerous remains that are known of this fossil species. The Palæotherium, which forms the subject of this notice, was obtained from a plaster-quarry situated at Vitry-sur-Seine. It was, however, even a few days ago, as we see it to-day, exposed on one side, and on the other encrusted in its stony resting-place in the ceiling of a subterraneous gallery, a little more than four yards high. Only a few have visited it, although M. Fuchs, a civil engineer, the proprietor of the quarry where this magnificent specimen was found, offered to give it to the Museum.

The gift so generously offered was immediately accepted; and Prof. Gervais, with a scientific zeal which ought to be fully acknowledged, occupied himself with the direction of the important task of taking it intact to Paris.

MARS

THE characteristic appearance of this planetary body, long familiar to astronomers, has of late become generally known. Remarkable neither for situation, magnitude, brilliancy, retinue or complexity of arrangement, inferior in each of these respects to some, and in many of them to several of the members of the solar family, one circumstance alone invests it with a peculiar interest—its resemblance to ourselves. Such a resemblance obviously does not exist in the mightier and more nobly attended external planets: the banded skies of two and the strong atmospheric absorption of the two others revealed by the spectroscope, sufficiently show that they belong to classes



MARS IN 1862

mutually indeed dissimilar, but each differing, and perhaps widely, from our own. With the swift and fiery Mercury we can have as little sympathy; and though Venus would offer a more promising analogy, the configuration of her beautiful surface is not well seen or readily interpreted. Mars therefore remains; and while, fortunately for astronomers, he occupies such a position that his features are fairly accessible, they bear an aspect so comparatively intelligible that, whatever may be the case as to our other fellow-subjects in the solar monarchy, we are ready to claim that globe as a close relation of our own, inferior indeed in magnitude and importance, if importance is indicated by an attendant, but arranged in a corresponding manner by the Great Creator as the seat of life and intelligence.

Such a supposition has been gradually and surely advancing from an early period of telescopic astronomy. The polar whiteness detected by Huygens and Cassini I. as far back as 1672 would naturally suggest the idea of a snowy deposit, which assumed almost the form of certainty, when the elder Herschel showed that its extent was regulated by the Martial seasons, and that it wasted steadily down with the advance of vernal heat. From the obvious division of the surface into brighter and darker portions, the existence of an atmosphere at least would be inferred, so long as they were supposed to be variable; but as the evidence of their general permanence increased under the eye of Herschel I. about a century ago, this impression gave place to the more definite recognition of something corresponding to the outlines of lands and oceans, with occasional variation from atmospheric condensations; and thus by degrees we have been led to acknowledge, in that remote and otherwise unimportant globe, a most interesting counterpart of our own.

This conclusion has not, however, been attained by an uninterruptedly continuous or an uniformly satisfactory process of deduction; and even at the present time it is perhaps not universally received. Schröter referred the darker portions to atmospheric obscuration, a notion which pervaded others of his investigations, not to their advantage; and a more recent observer of considerable ability, the late Prof. Kaiser, of Leiden, whose decease in his 64th year took place July 28, 1872, has, in a very elaborate and interesting report of the work done on the planet at that observatory, expressed his doubts as to the certainty of the more customary inference. Whatever may be our own impressions on the subject, his criticisms and conclusions exhibit so much of the genuine spirit of an impartial student that some notice of them, as they are found in vol. iii. of the Annals of the Leiden Observatory, may be worth the attention of our readers. This observatory, it should be noted, is provided with a Merz achromatic of 7 (French ?) inches aperture, and was therefore, under Kaiser's superintendence, fairly competent for physical researches commensurate with the present demands of science; as it is well known, and indeed especially brought out by the observations we are about to notice, that much larger telescopes are not invariably, or even generally, available in proportion to their magnitude. The addition, in 1872—too late therefore for a share in the professor's observations—of an 8½ inch With-Browning reflector, will hereafter not only afford an interesting comparison of instruments, but if the result corresponds with others obtained elsewhere, will be found a step in advance as regards efficiency.*

In selecting Mars as the subject of special inquiry, Prof. Kaiser laid a solid foundation by consulting every work within his reach, representing or describing the physical aspect of the planet, from the earliest and rudest efforts in 1636 to the elaborate delineations of the present day. No less than 412 drawings thus passed through his hands: upwards of 320 others he could not procure; and the aggregate is doubtless much in defect of the existing total. He did however well in securing so many; more, probably, than any other areographer, if such a word may be allowed. But the result of their comparison and discussion was not as satisfactory as might be wished. The first specimens of representation were of course mere rude attempts. Those of Huygens, however, in 1659, discovered by Kaiser in his "day-book" (of which the most valuable portion was edited by him in 1847) are comparatively well drawn; and Hook, in 1666, caught the true character of what he saw, though Kaiser doubts whether his spots could be as readily identified as has been supposed. We next find Herschel I. taking up the subject

* A curious error on the part of Prof. Kaiser may here be noticed. He has referred (p. 23) to a drawing of Mars by Browning as having been taken with a silvered mirror by Barnes. This gentleman was merely the proprietor of the speculum, which, like the others mounted by that optician, was th work of a most accomplished artist, Mr. With, of Hereford.

in 1777, and continuing his observations till 1783. He first noticed the eccentric position of the two white spots in the polar regions, as well as their diminution from solar action; 5 out of his 31 figures show a broad white band passing obliquely across the disc, and he speaks of changes in the markings from passing clouds and vapours: some of his dark spots can be identified with more recent representations, but not the whole. Of the numerous drawings (217) of Schröter, Kaiser was unable to avail himself, as the Areographische Fragmente, rescued from the disastrous fire at Lillenthal in 1813, were left unpublished at his death. These, however, through the intervention of Dr. Peters of Altona, have subsequently been traced to their safe custody in the hands of Schröter's descendants, and have recently been thoroughly examined by Dr. Terby of Louvain, whose report has been published by the Belgian Académie Royale des Sciences. From the figures contained in this, and another interesting essay by the same astronomer, it appears that many coincidences may be traced between the views of Schröter and other observers, though his preconceived idea of the vaporous nature of the darker features deprived his observations of some of the value otherwise due to them as the results of eminent zeal and perseverance.

Passing by several observers of minor note, of whom Kaiser has given a minute enumeration, we reach the opposition of 1830, which the near concurrence of the aphelion of the Earth and the perihelion of Mars rendered eminently favourable, enlarging the apparent diameter of the latter to 23″·1. The close and systematic investigation then entered upon by Beer and Mädler forms a most important epoch in the progress of areography, and for the first time a series of drawings were executed, little resembling anything previously known, which have ever since been referred to with confidence as a starting point for future inquiries, and which, it might have been hoped, would have set many questions at rest: and so they did; but as Kaiser remarks, later representations have again unsettled points which had been supposed to have been then decided. The comparative failure of the same observers in subsequent oppositions admitted of explanation from the increased distance and altered presentations of the planet; and little advance was made by Mädler in 1841, even with the renowned refractor at Dorpat, to the care of which he had succeeded: the apparent diameter, then, however, was only 15″·1, its minimum, attained in 1837, being 13″·3. Of the near approach in 1845, when the disc was enlarged to 23″·5, no observations seem to have fallen into Kaiser's hands, excepting those of the American astronomer Mitchell. Confining ourselves still to the more important representations, at the expense of doing scanty justice to the Professor's elaborate memoirs, we find that 1854 produced the beautiful designs of Jacob, and 1856 the still more delicate ones of De La Rue, both great advances on anything previously published. In 1858, Secchi, who had been for some years at work at Rome, brought out a series of drawings in much harmony with themselves, but only partial agreement with previous delineations. At length came the favourable opposition of 1862, when the point was taken up by many of the first observers armed with some of the most powerful telescopes in existence; but the result, we regret to add, was very different from what might have been anticipated. Secchi, with his magnificent achromatic, Lassell and Rosse with their colossal reflectors, produced such an unsatisfactory and in part contradictory set of drawings as had never been published before. The weather was not always in fault; and though Mars was rather low, Lassell repeatedly found very sharp definition; Lord Rosse's excellent draughtsman once used a power of 1,200; and the quality of Secchi's instrument and sky compensated to a great extent for his smaller aperture; but then the expected clearing up of difficulties terminated in the annoyance of disappointment. Kaiser assures us that nowhere are there such discrepancies as between Rosse and Lassell, even when the same hemisphere was obviously in sight; it could hardly be imagined that they had the same spot in view: and Secchi is so far from setting the matter straight, that his figures scarcely seem to refer to the same body; and for any purpose of accurate deduction the Leiden Professor felt obliged to put all three aside. He is even induced to say, "the largest telescopes give the worst results, and show themselves very liable to mislead the observer: correct delineations of the celestial bodies require before all things a very practical designer that gives way to no fancies; and such a designer is not apt to possess the most powerful telescopes of the earth." We are here merely reproducing the censor's words, without venturing an opinion as to the soundness of his criticism.

But, fortunately as it would seem for areogrophy, instruments of more moderate dimensions were employed to better purpose during that and the subsequent opposition of 1864; and the agreement of the beautiful designs of Lockyer in the former year with those taken by Kaiser himself, then and in 1864, as well as with those of Schmidt and Phillips, was far more satisfactory: and the same might be said to a considerable extent as to Dawes, whose instrument, however, was of a superior rank.* The difference between Lockyer and Lassell, in one instance, was so wide, that identity of date alone proves that they had the same hemisphere under their eyes; while, on the contrary, the concurrence between Lockyer and Kaiser, though the latter speaks with great diffidence of his own designs, justified him in believing that a pretty correct representation had been attained of a broad girdle round the equator.

The labour which the Leiden professor has bestowed upon a comparison of all the least discordant drawings, and the punctilious accuracy of his protracted discussions, would be little appreciated from so brief a sketch of them as can be attempted here. He was himself so little pleased with the result as to express an opinion that the only safe inference from the oppositions of 1862 and 1864 is, that the art of drawing celestial objects is at much too low a pitch to justify accurate deductions as to their physical character. And this, though it looks like the language of disappointment, and is hardly reconcileable with the striking agreement which he often remarks between the drawings of different observers, or the same observer at different times, seems to have been his deliberate impression. He ascribes the variations in part to the differences of presentation and perspective, in part to faulty delineation; and while he admits that atmospheric condensations may have occasioned small apparent changes, he thinks on the whole that they are evidences of the unsteadiness of our air, leaving almost always an uncertainty as to the minuter spots and shadings, and a want of confidence in the correctness of one's own delineation. The discrepancies among his 412 designs are so "enormous" that no one would believe that they were intended for the same body. These differences arose, however, in part from natural causes. Even in the most favourable case, the spots are only seen with any distinctness or in their true form in the centre of the disc; those lying near the limb being greatly foreshortened and not recognisable in their real shape; and this difficulty is very considerably enhanced by the imperfect transparency of the planet's atmosphere and its frequently strongly illuminated precipitations. The inclination of the axis may vary its position at different times about 60° as regards the spectator, and consequently different oppositions bring before him entirely different features in that central position where alone they can be well observed or drawn.

* It is much to be regretted that only a part of the drawings of this great observer have as yet been published in *fac-simile*. Those given in the Monthly Notices (XXV., 225) omit, as Kaiser remarks, some of the most curious presentations of the globe; and the reproduction of others by Proctor does not profess minute accuracy.

And to these sources of difficulty it might, we think, have been added that, in consequence probably of our study of geography from projections of the globe in which the effect of perspective is designedly counteracted as much as possible, we are apt to have a very defective idea of the amount of apparent distortion which it occasions towards the edges of the visible hemisphere. But even when all this has been allowed for, we find, the professor tells us —nor indeed do we need opportunities as extensive as his to convince ourselves of it—that the differences are much too great to be altogether thus explained; and he concludes that the more conspicuous ones are errors in representation. If there is occasional agreement as to the forms, there is still much risk in referring them to the same object, until it has been ascertained by computation that the presentation of the globe towards the spectators was nearly the same. Fortunately, during any given opposition, the position of the planet's axis shifts but little, and in other oppositions the same presentation recurs from time to time; but instead of the correspondence naturally anticipated, the differences are for the most part, as he expresses it, "enormous." And yet amongst them all, coincidences come to the surface, too remarkable to admit the idea of fortuitous resemblance; and we must suppose that many who have taken pencil in hand have not been sufficiently careful as to form and shading, but have followed arbitrary and perhaps very mistaken impressions, from which nothing but absurd and absolutely contradictory inferences of a physical nature could be drawn. T. W. WEBB

(*To be continued.*)

THE ADMIRALTY CHARTS OF THE PACIFIC, ATLANTIC, AND INDIAN OCEANS *

THESE charts have been compiled by Captains Evans and Hull, of the Hydrographic Departments of the Admiralty, from Maury's pilot charts, Fitzroy's and Fergusson's wind charts, charts issued by foreign Governments, and from the works of Dovè, Neumayer, Buchan, and documents in the Hydrographic Office of the Admiralty. They show for the four seasons the pressure, winds, and temperature over the parts of the globe covered by the sea. January, February, and March are properly grouped together into one season, these being the three coldest months as regards the oceans in the northern, and the three warmest in the southern hemisphere.

The most important piece of new work in these charts is the "isobars," or lines of equal barometrical pressure, which are given for the seasons. These isobars for the sea, taken in connection with Buchan's isobars published in 1868, may be regarded as the first approximation to a complete representation of the earth's atmosphere over both land and sea. We have minutely examined these isobars, comparing them with the large amount of new information collected during the past five years from many places situated on the coasts of the continents, or in islands scattered over the ocean, and can come to only one conclusion, viz., that the greatest care has been taken in their construction. Among the very few cases to which slight exception might be taken is the isobar of 29·7 in. of July, August, and September, drawn to southwards of Japan, which observations do not appear to warrant. It should also be pointed out that a serious omission has been made in not stating how the ship barometric observations were reduced to the mean pressures from which the isobars have been drawn.

We are now in a position to draw one or two general conclusions of great importance regarding the distribution of atmospheric pressure over the ocean. In the ocean, to westwards of each of the continents, there is at all seasons an area, or patch, of high pressure, from 0·10 inch to 0·30 inch higher than is found on the coast westward of which it lies. The distance of the centre of the space of high pressure from the coast varies from 20° to 35° of longitude, the average distance being nearly 30°. The position of the centre of the space varies from about 22° to 35° north or south latitude, or stating it roughly it lies about the zones of the tropics. In these spaces the absolute pressure is greatest during the winter months of the respective hemispheres—a condition of things probably due to the fact that during the winter season of the northern hemisphere the great mass of the earth's atmosphere is disposed about the tropic of Cancer, and during the winter season of the southern hemisphere, about the tropic of Capricorn. The position and shape of the isobars seem to be largely determined by that of the continents adjoining. Thus the rounded form of the southern portion of North America, the bending eastward of the west coast of South America from Payta to Arica, and the form of the north-western part of Africa and its "lie" from S.W. to N.E. are all more or less impressed on the isobars bounding the contiguous spaces of high pressures. These spaces are less prominently marked west of those continents which have the least breadth in lat. 30°; thus the area of high pressure is less marked west of the Cape than it is west of Australia, and still less than to the west of North America. The isobars are much farther apart on the western than on the eastern side of these areas of high pressure; indeed in many cases they are as it were drawn out so as almost to reach the continent lying to westward; and in some cases there is even a tendency towards, or the actual appearance of, secondary areas of high pressure to eastwards of continental masses. This is most distinctly seen to eastward of Australia.

We have dwelt thus particularly on these spaces of high pressures because of their importance in atmospheric physics, but more especially because of their vital connection with prevailing winds and the general circulation of the atmosphere. Out of these high pressures, the wind blows in all directions anti-cyclonically in accordance with the well-known "Buys Ballot's Law of the Winds," of which relation the wind charts before us afford abundant confirmation. Keeping this relation between wind and pressure in view, we have presented in these high pressures the proximate causes of the prevailing winds over the greater portion of the ocean; and through the prevailing winds, the drift currents and other of the surface-currents of the sea; and thereby the anomalous distribution of the temperature of the sea as seen in the Chile, Guinea, and other currents, and the peculiar climates of the coasts past which these currents flow.

The small area of high pressure to the east of Australia may be singled out as perhaps the most interesting of the new facts in the charts. During winter the winds along the east and south of Australia blow inwards upon the interior of that continent, whereas in New Zealand the prevailing winds at the same season are north-westerly and westerly, the directions being thus generally opposite on these two coasts facing each other. The space of high pressure between gives a ready explanation of the direction of these winds, as well as of the heavier rainfall on the west of the South Island of New Zealand as compared with that of the North Island, and of the south-east as compared with the south-west of Victoria.

Like praise cannot be given to the charts of the isothermals of air for January, April, July, and October. In the October chart, the isothermal of 60° cuts the east coast of South America near lat. 27°; now at Monte Video, the mean temperature of October is 61·2°, at Buenos Ayres 61·3°, and at Bahia Blanca, in 38°·4 S. lat. 59°·7; that is, the isothermal of 60° should cut the South American coast 11° of latitude farther to the south. The January isothermal of 60° is drawn passing through New

* "Wind and Current Charts for the Pacific, Atlantic, and Indian Oceans," published at the Admiralty, October 1872, under the superintendence of Rear-Admiral G. H. Richards, C.B., F.R.S., Hydrographer.

Zealand near lat. 40°, and the isothermal of 50° near Dunedin; now the mean temperature of January at Southland situated at the extreme south of New Zealand is 57°·6, and at Duned in (550 ft. high) 57°·5; in other words, the isothermal of 60° and not that of 50° ought to pass near Dunedin. Dr. Hector's meteorological reports during the past seven years place this beyond all doubt, and it is unfortunate that the summer climate of this important colony of Great Britain should have been so misrepresented as to appear to be colder than that of Iceland, and altogether insufficient for the ripening of wheat, barley, and other cereals. The July isothermal of 90° is represented as having its eastern extension at the entrance to the Persian Gulf in 57° E. long. Now Murray Thomson's and Blandford's meteorological reports show that the isothermal of 90° extends eastward to about 77° E. long., so as to embrace the Punjaub and the upper tributaries of the Ganges to the west, being thus 20° farther east than is represented on the chart.

The truth is, that, excepting for the months of January and July, there have been no isothermal charts of the months for the whole globe yet published which do not contain many gross errors similar to those we have pointed out. The time is surely not far off when a committee of the British Association, or some competent authority, will take up this subject, and give us a set of new isothermal lines laid down from all data which the great expansion meteorology has received of late years has made available.

The two charts showing the isothermals of the sea for the extreme months, February and August, and the chart showing the surface currents of the ocean, are very valuable. A supplementary chart showing the currents south and east of Asia during the monsoon season is also given. We should suggest for the second edition of the Charts, that charts of the surface currents for both February and August should be given for the whole globe, it being only thus that these important aids to navigation can be adequately presented.

It was pointed out in NATURE some years ago that the prevailing winds and surface currents of the Atlantic are all but absolutely coincident. These Charts enable us now to extend the remark to the prevailing winds and surface currents over all the oceans. Keeping out of view the deep-water currents of the sea to which Carpenter has given so much attention, it is now placed beyond all doubt that it is to the winds we must look as the prime movers of oceanic currents.

MR. GARROD'S NEW CLASSIFICATION OF BIRDS

AT the scientific meeting of the Zoological Society, on Tuesday, February 3, Mr. A. H. Garrod introduced a new Classification of Birds, based mainly on the disposition of their muscles and other soft parts. The following is an abstract of his paper:—

The osteology of birds, judging from the unsatisfactory state of their classification in the present day, is not sufficient in itself as a basis for distinguishing the mutual relations of the different families and genera; and as the peculiarities in the soft parts are very constant, they deserve more consideration than they have hitherto received. The researches of Hunter, Nitzsch, Macgillivray, Owen, and others, have brought to light many facts in visceral anatomy and pterylosis, all of which are of great value in classification. Sundevall is the only ornithologist who seems to have made any generalisations respecting myology, and these have an important bearing on the subject.

My method of work, Mr. Garrod went on to say, has been the following:—After having carefully dissected a few birds that are known to be but distantly related, a comparison of the notes on the individuals examined showed that there were important myological differences between them. Further dissection of species related more or less intimately, indicated broadly the relative value of the peculiarities that were found, when taken in connection with the most approved classification of the present day; and as observations became more numerous the relative importance of the facts observed was more easy to estimate. The muscles which have, on account of their marked tendency to vary in the class Aves, attracted the most of my attention, are all situated in the thigh, and they are five in number: (1) the *femoro-caudal*, which runs from the linea aspera of the femur, near its head, to the sides of the tail vertebræ; (2) the *accessory femoro-caudal*, which runs parallel to the last, and behind it, from below the femur-head to the ischium; (3) the *semitendinosus*, which crosses the first-named muscle superficially, and arises from the lower part of the ischium, to be inserted into the inner side of the tibia-head; (4) the *accessory semitendinosus*, which arises from the distal end of the linea aspera, and joins the fibres of its larger namesake obliquely just before their insertion; (5) the *ambiens*, that peculiar slender muscle which arises from just above the acetabulum, and after running obliquely through the ligamentum patellæ, joins the tendon of the flexor perforatus digitorum. My observations on these five muscles have been made on more than 500 species of birds, including more than 600 specimens, and the results are recorded in a tabular form, in a paper now in course of publication in this Society's Proceedings. For the present, no more attention need be paid to these muscles themselves, but only their presence or absence considered; therefore, to simplify description, a myological formula will be employed which indicates all the facts required in a very precise manner. Calling the first four of the above-mentioned muscles, A B X and Y, respectively, and omitting from the formula thus based, the symbol or symbols which represent any that are deficient, it is clear that a bird, like the common fowl for example, which possesses them all, would be represented by ABXY; and the eagle, in which the femoro-caudal is alone present, by A; whilst the sparrow, which only wants the accessory femoro-caudal, must have the formula A X Y; and the duck, which only lacks the accessory semitendinosus, is represented by A B X. By this means it is possible to make important statements respecting the myology of any bird in a very concise form, which gives great facility towards the comparison of different species. It must here be mentioned that individuals of a species and species of a genus do not vary among themselves in the muscles under consideration. The following table gives the myological formula of the different families of birds, as far as my dissections enable me to go, the only important types omitted being Eurypyga, Psophia, Todus, and Bucco. They are arranged in an order to be subsequently explained, and the presence or absence of the ambiens-muscle is indicated by + or — after each formula:—

TABLE I.

Struthionidæ B X Y +	I.
Casuariidæ { A B X Y — B X Y —	*Picidæ* { *A X Y* — *A X* —
Tinamidæ A B X Y +	
Palamedeidæ A B X Y +	*Ramphastidæ A X Y* —
Gallinæ (excl. Turnix) { A B X Y + B X Y +	*Capitonidæ A X Y* — *Upupidæ A X Y* —
Rallidæ A B X Y +	*Bucerotidæ A X Y* —
Otididæ B X Y +	*Alcedinidæ A X* —
(Incl. Cariama and Serpentarius.)	
Phœnicopteridæ B X Y +	
Musophagidæ A B X Y +	II.
Centropinæ A B X Y +	
Cuculinæ A X Y +	*Passeres* { *A X Y* — *A X* —
Psittaci A X Y ±	
Anatidæ A B X +	*Trogonidæ A X* —

Sphenisciḍæ A B X +
Colymbidæ A B X +
Podicipidæ B X —
Procellariidæ { A B X Y + ; A B X + ; A X +
Ciconiidæ A X Y +
Cathartidæ { A X Y + ; X Y +
Ardeidæ { A X Y — ; X Y —
Phalacrocoridæ A X +
Phaethontidæ A X Y +
Fregatidæ A +
Falconidæ A +
Strigidæ A —
Gruidæ A B X Y +
Charadriidæ { A B X Y + ; B X Y +
Laridæ A X Y +
Alcidæ A B X —
Columbæ A B X Y ±

Meropidæ A X Y —
Galbulidæ { *A X Y —* ; *A X —*
Caprimulgidæ A X Y —
Steatornithidæ X Y —
Coraciidæ A X Y —
Momotidæ A X Y —

III.

Cypselidæ A —
Trochilidæ A —

On looking at the formulæ in the above table it will be seen that there is a tendency to similarity in those that are placed in juxtaposition; and further, that the presence or absence of the ambiens muscle, indicated by the signs + and —, is more constant than the other characters. Thus, among the *Cuculidæ*, the *Picidæ* and *Ardeidæ*, the ambiens does not vary whilst one or other of the rest is inconstant. There are more reasons than the above for assigning primary importance to the ambiens muscle, which depend on the nature of the tip of the oil-gland and the cæca of the intestine. For, with but few exceptions, those birds which possess the ambiens muscle have cæca to the colon and a tuft of feathers on the oil-gland, whilst those in which the ambiens muscle is absent, have either cæca and a nude oil-gland, or a tufted oil-gland and no cæca. The true relationship of the exceptions is, however, indicated by other collateral characters, the most important of which is the presence or absence of the accessory femoro-caudal (B); that muscle being never found in those birds in which the ambiens is always absent, so that any bird with it developed, is certainly related to those in which the ambiens is present. These facts lead me to propose the division of the class Aves into two primary sub-classes,—the *Homalogonati*,* in which the ambiens is present, and the *Anomalogonati*† in which it is always absent. The former of these are printed in the above table in Roman letters, the latter in italics.

It may be asked, why, on the above principles, are the Ardeidæ and the Strigidæ placed with the Homalogonatous birds, especially as the latter have a nude oil-gland? The position of the latter of these two families is no doubt uncertain, but the sum of characters is in favour of the places assigned to it.

Next, respecting the most important sub-divisions of the Homalogonatous, and the Anomalogonatous birds. Taking the latter first, because they are fewer in number, and more clearly separable, they are found to fall naturally into three well-defined orders:—(1) those in which the oil-gland is nude and the cæca of the intestine present; (2) those in which the oil-gland is tufted and the cæca are absent; and (3) those in which the oil gland is nude and the cæca are absent. These three sections of the Anomalogonatous birds are indicated in Table I. by the corresponding numbers, the Picidæ heading the first, the Passeres the second, and the third comprising the Macrochires only. To most ornithologists the not unreasonableness of this arrangement will be fairly apparent.

* With the knee normal; that is, with the ambiens crossing it.
† With the knee abnormal; that is, with the ambiens deficient.

TABLE II.

Class AVES

Sub-class HOMALOGONATI

Order I. *GALLIFORMES*

Cohort (α) STRUTHIONES
Family 1. *Struthionidæ*
Sub-fam. 1. Struthioninæ
„ 2. Rheinæ
„ 2. *Casuariidæ*
„ 3. *Apterygidæ*
„ 4. *Tinamidæ*
„ (β) GALLINACEÆ
Family 1. *Palamedeidæ*
„ 2. *Gallinæ*
„ 3. *Rallidæ*
„ 4. *Otididæ*
Sub-fam. 1. Otidinæ
„ 2. Phœnicopterinæ
„ 5. *Musophagidæ*
„ 6. *Cuculidæ*
Sub-fam. 1. Centropinæ
„ 2. Cuculinæ
„ (γ) PSITTACI (†)

Order II. *ANSERIFORMES*

Cohort (α) ANSERES
Family 1. *Anatidæ*
„ 2. *Spheniscidæ*
„ 3. *Colymbidæ*
„ 4. *Podicipidæ*
„ (β) NASUTÆ
Family 1. *Procellariidæ*
„ 2. *Fulmaridæ*
Sub-fam. 1. Fulmarinæ
„ 2. Bulweriinæ

Order III. *CICONIIFORMES*

Cohort (α) PELARGI
„ (β) CATHARTEÆ
„ (γ) HERODIÆ
„ (δ) STEGANOPODES
Family 1. *Phæthontidæ*
„ 2. *Pelecanidæ*
„ 3. *Phalacrocoridæ*
„ 4. *Fregatidæ*
„ (γ) ACCIPITRES
Family 1. *Falconidæ*
„ 2. *Strigidæ*

Order IV. *CHARADRIIFORMES*

Cohort (α) COLUMBÆ
„ (β) LIMICOLÆ
Family 1. *Charadriidæ*
„ 2. *Gruidæ*
„ 3. *Lariдæ*
„ 4. *Alcidæ*

Sub-class ANOMALOGONATI

Order I. *PICIFORMES*

Family 1. *Picariæ*
Sub-fam. 1. Picidæ
„ 2. Ramphastidæ
„ 3. Capitonidæ
„ 2. *Upupidæ*
„ 3. *Bucerotidæ*
„ 4. *Alcedinidæ*

Order II. *PASSERIFORMES*

Family 1. *Passeres*
„ 2. *Bucconidæ* (?)
„ 3. *Trogonidæ*
„ 4. *Meropidæ*
„ 5. *Galbulidæ*
„ 6. *Caprimulgidæ*
„ 7. *Steatornithidæ*
„ 8. *Coraciidæ*
Sub-fam. 1. Coraciinæ
„ 1. Momotinæ
„ 3. Todinæ (?)

Order III. *CYPSELIFORMES*

Family 1. *Macrochires*
Sub-fam. 1. Cypselinæ
„ 2. Trochilinæ

The Homalogonatous birds must be divided upon a different basis, and their myological formulæ here come into service. Before going further it is necessary to show that the habits of the species are not the cause of their myological peculiarities in most cases, though probably in some they do affect them. The Heron and the Swallow have the same formula, and yet how different their habits? the same may be said of the Owls and the Swifts; the Kaleege and the Flamingo. The Auk and Guillemot, however, are most probably but distantly related to the Ducks and Penguins if the peculiarity in the nasal bones has the importance that I assign to it; nevertheless, the muscles of their legs agree more with them, than with the other Schizorhinal birds. By a glance at Table II., the manner in which the Homalogonati may be best subdivided according to the facts that I have been able to bring forward, may be obtained. Commencing with the orders, the *Galliformes* include all those birds related to the Fowls; and notwithstanding the high opinions to the contrary, I cannot feel justified in separating the Struthious birds away from this group. It is not difficult, after having seen the formula of the Musophagidæ and Cuculidæ (Table I.), to recognise that these families have nothing to do with the Anomalogonatous birds, although they are peculiar in the former having no cæca, and the latter a nude oil-gland. The Psittaci also cannot be placed anywhere else.

The *Anseriformes* all agree, with the exception of the Storm-Petrels, which are also otherwise difficult to place, in wanting the accessory semitendinosus (Y), and in having the great pectoral muscle very elongate. The whole family of petrels are exceptions in this point also, and may have to be put in the next order, amongst the Ciconiiformes.

The *Ciconiiformes* include amongst them the Accipitres, but myology is in no point more clear than with regard to the unnaturalness of that family as at present defined. Every Eagle, Hawk, true Vulture, and Owl, has for formula A. The Secretary Bird, which is generally placed with them, is represented by B X Y; from which it is seen to be as different from them as it can possibly be. This shows that the position of *Serpentarius* must be changed; that it is not a raptorial bird at all; and that, as in formula and general appearance it resembles *Cariama*, it must be placed near it and the Bustards. Similar arguments indicate that the Cathartidæ are not true Accipitrine birds, but must form an independent family, though still in the same order as the Falcons.

The *Charadriiformes* all possess the peculiar nasal arrangement which I have termed Schizorhinal. The *Turnicidæ* and *Parridæ* are included with the *Limicolæ*, and the *Pteroclidæ* with the *Columbæ*.

The justification of many of the smaller divisions of the above orders will be seen by comparing the myological formulæ, and by a review of the osteological, pterylographical, and visceral arrangement of each.

In any attempt at classification on new facts, it must be remembered that there must be great inequality in the importance of the results arrived at in each order as freshly defined. In one family there may be a uniformity in a particular structure which is greater than could possibly have been expected; whilst in another the previously constant character may be one of the most uncertain. For instance, the left carotid artery is alone present in all the Passerine birds that have ever been examined; but amongst the Bustards the Great Bustard has two, Denham's only the right, and *Tetrax* only the left. Therefore it is not to be wondered at that myology is equally uncertain in its indications sometimes, though on other occasions its teaching is most decided. In the above attempt at a new arrangement, it has been my endeavour to bring forward the results of observations made during a considerable time, with the facts obtained from previous work always kept prominently in the foreground.

NOTES

IN a Congregation held at Oxford on Feb. 10, Prof. H. Smith introduced a statute providing that the certificate of the examiners appointed under the authority of the Delegates of the Examination of Schools, when given in Greek, Latin, and Elementary Mathematics, be accepted in lieu of Responsions. He represented that in Mathematics the standard would be higher than in Responsions; in Greek and Latin it would be equal, owing to the requirement of translation of "unseen pieces." The candidate would also have to pass in some other subject. It was therefore inconceivable that the idle should select the Schools Examination as the easier. The standard would be kept up by the employment of the same class of examiners as in other University examination. The preamble of the statute was accepted.

DR H. ALLEYNE NICHOLSON, Profe sor of Natural History in University College, Toronto, has been appointed to the Professorship of Zoology in the Royal College of Science, Dublin, vacant by the resignation of Dr. Traquair. Prof. Nicholson is known as the author of many papers on the Graptolites, and as a writer of several text-books of zoology.

THE Smith's Mathematical Prizes have been adjudged to Mr. Walter W. R. Ball, second wrangler, 1874, and Mr. George Stuart, B.A., Emmanuel College, Cambridge, bracketed fourth wrangler, 1874.

A MEETING of those who have signified their interest in the formation of the new Physical Society will be held on the 14th inst., at 3 o'clock P.M., in the Physical Laboratory, South Kensington.

A GENERAL meeting of the Provisional Committee for the establishment of the Scientific Societies Club was held on Jan. 29 at the Westminster Palace Hotel, when an organising committee was appointed with a view to the early opening of the club. The number of "original members" is nearly complete, 231 gentlemen having given in their names. Among the Provisional Committee we notice the names of Dr. Gladstone, Prof. Lawson, and Prof. Morris, and others known to Science.

AT the meeting of the Paris Academy of Sciences on Feb. 2 the place of Correspondent of the Astronomical Section, left vacant by the election of Sir George Airy to a Foreign Associateship, was filled up. M. Tisserand obtained 25 votes and M. Stephan 23. The former was therefore elected. At the same meeting the Academy, sitting in secret committee, received the report of the committee appointed to select candidates for the Chair of Embryology at the College of France. M. Balbiani was placed first, M. Gerbe second. The election was announced for the 9th inst.

WE have just received from Mr. Gerard Krefft the cast of a fossil specimen of extreme interest. It is that of one of the teeth of an extinct species of *Ceratodus* found with the usual *Diprotodon* remains in the alluvial deposits of the Darling Downs district of Queensland. This able naturalist has named the fish indicated by this fossil in honour of the present Colonial Secretary of Queensland, *Ceratodus palmeri*. It is larger than the corresponding tooth—the left upper dental plate—of *C. forsteri*, the enamel being rather coarser and the surface more undulated than that of Forster's fish. In the specimen under consideration, three of the prongs are perfect, being three-fourths of an inch in width. Mr. Krefft mentions that the existing fish is called "Jeevine," and not "Barramundi;" also that it never goes ashore, and is not caught, as supposed by some, with hooks baited with frogs.

THE *Academy* has been favoured by Dr. Kirk with the following private telegram, which he received from Brigadier-General Schneider, C.B., Her Britannic Majesty's political Resident at Aden, with reference to the news of the death of Livingstone. Dr. Kirk considered that the details given in the telegram as published concerning Livingstone's death and the embalming of his body presented so many doubtful points which required clearing up, that he was anxious to ascertain whether Cameron had convinced himself of the accuracy of these reports by personal examination of the messengers who, it is said, preceded Livingstone's dead body to Unyanyembe, and among whom was Chumah, his servant; or whether the reports had come to his ear, before Chumah himself reached Unyanyembe, in the usual untrustworthy and exaggerated native manner. He therefore telegraphed to General Schneider; but, as will be seen by the reply from General Schneider, it cannot be ascertained at present whether Cameron actually saw Chumah. The evil tidings may have preceded him by some days; and there is nothing for it but to wait the receipt of Cameron's written advice:—"General Schneider to Dr. Kirk.—Aden February 2, 5.15 P.M.—Captain Prideaux merely says Chumah went ahead and gave intelligence to Cameron."

THERE has been instituted by the French Government, under the Minister of Public Instruction, a Commission of Scientific and Literary Voyages and Missions. The object of the Commission, we learn from *Les Mondes*, is (1) to discover what are the most useful scientific and literary enterprises; (2) to examine the projected voyages and missions proposed to the Minister; (3) to study the programmes of these missions, to give detailed instructions to those who undertake them, and to carry on correspondence, if necessary, during the voyage; (4) to examine, on their return, the works on which the voyagers have reported, and prepare their publication in a record of Missions, when that is founded; (5) to name to the Minister such voyagers as may be worthy of honourable reward after the completion of their enterprise; (6) to appeal to the various administrations to concentrate on certain enterprises all the resources at the disposal of the state. The Under-Secretary of State is President of the Commission, and M. Beulé Vice-President; while, among the members are, MM. Felix Ravaisson, Conservator of the Louvre Museum, Leon Renier, Chevreul, Milne-Edwards, D'Avezac, President of the French Geographical Society.

THE *Paris Journal* gives a curious account of an hotel situated in the *Rue des Petites Ecuries*, which has a *clientèle* of living phenomena. It is an hotel of the lowest order, which was fitted up by a French barman for housing extraordinary creatures. The *homme chien* and his son Fedor lived there for some time. The giant of Folies Bergeres (8 ft.) dwelt there. He was an intimate friend of a dwarf whom he carried in his arms every evening, when taking his daily promenade after dark. There are also a good many acrobats and lion-tamers admitted into the house. Mdlle. Christine, the double sisters, were not a lodger; they had an agent of their own, an Englishman. Most of these curious specimens of humanity are placed under the direction of the hotel-keeper, who procures engagements for them at certain prices, according to their *demerits*, and directs them either to some of the minor theatres, concert-halls, or to the booths erected at suburban fairs. A *Table d'hote* of the Petites Ecuries Hotel, where all these strange creatures come together, is the most extraordinary sight in the whole town.

THE sale of several works on the book-stalls at railway-stations. has been prohibited by the Minister of the Interior, Amongst these we notice "Les Ballons du Siege," by M. W. de Fonvielle, who, as it is known, escaped from Paris in a balloon during the investment of Paris, and delivered lectures in London. M. W. de Fonvielle, who was just returning from London when the prohibition was issued, has written to the minister in order to ascertain the real facts of that extraordinary decision.

DR. A. ERNST prints (unfortunately in Spanish) under the title "La fécula y las plantas farináceas del nuevo mundo," a list of 100 plants of the New World which yield starch, with detailed accounts of the more important ones.

IN the discussion which followed, Sir Bartle Frere's address, at the opening of the African Section of the Society of Arts, Mr. Hyde Clarke read a letter from Lieut. Maurice, Private Secretary to Sir Garnet Wolseley, dated "Head Quarters, Yancoomassie," from which we take the following extract; it may prove of some interest to students of the Science of language:—"A somewhat curious piece of word-coining, which has fallen under our notice here, may interest you in connection with the broader aspects of the subject of which you write. The Ashantees having experience of our rockets only as they come to them in destructive form at the end of their journey, call them by the sound they make, 'Schou-schou,' or something of the kind. The Fantees, on the other hand, adopt bodily into their Language our own names for those things which they have not seen before. Thus to the Houssa or the Fantee, in speaking to one another, our rockets are named rockets, while their enemies call them schou-schou. It is possible that as war has not been in savage times an uncommon condition of mankind, analogous causes for different names having been adopted by different nations may have been not unfrequent in the past."

THE Council of the Statistical Society have founded a bronze medal, under the title of "The Howard Medal," to be presented to the author of the best essay on some subject in "Social Statistics," a preference being given to those topics which Howard himself investigated, and illustrated by his labours and writings. The title of the Essay to which the Medal will be awarded in November 1874, is as follows:—"The state of Prisons, and the condition and treatment of Prisoners, in the Prisons of England and Wales, during the last half of the eighteenth century, as set forth in Howard's "State of Prisons," and work on 'Lazarettos.'" The Essays must be sent to the Assistant Secretary of the Society on or before September 30. The competition is open to any competitor, providing the Essay be written in the English language.

WE called attention last week to the course taken by the Perthshire Society of Natural Science, in reference to the present election of Members of Parliament. The Society sent questions to the candidates for the City and County of Perth, relating to the appointment of a responsible Minister of Education, to State help for Science, and to the promotion of scientific exploration expeditions, such as that of an Arctic expedition. The Liberal candidates sent no reply; the Conservative candidates sent favourable answers. The following is the reply of Sir William Stirling Maxwell, the Conservative candidate for the County of Perth:—

"In reply to your letter, I beg to say that I have long been of opinion that the Existing Education Department, and all our Public and Literary Institutions should be placed under the general supervision of a responsible Minister. In Parliament I was generally inclined to favour the expenditure of money for scientific objects when the Government thought proper to sanction them; and an Arctic expedition, and various researches, unremunerative in a pecuniary sense, might fairly fall into the list of such objects."

IN June last year Prof. O. C. Marsh, the discoverer of *Dinoceras and Brontotherium*, started on a five months' geological expedition to the Rocky Mountain regions and the Pacific coast, to study, as he had done on previous occasions, the Cretaceous and Tertiary formations, which are there so rich in vertebrate remains. From Fort McPherson, Nebraska, they proceeded to Niobrara under the escort of two companies of United States cavalry, which were indispensable on account of the hostile position of the Indian tribes. Among the other places visited were Fort Bridger, Wyoming; Idaho and Oregon; Colorado and Kansas. The expedition was very successful, and the collections procured were large, containing many new forms. It is much to be regretted that no English geologists have accompanied Prof. Marsh, as most of the fossils peculiar to the regions he is exploring, are quite unknown in this country, except from descriptions.

THERE will be held at Christ Church, Oxford, on Saturday February 28, an election to a Junior Studentship in Physical Science, tenable for five years from the day of election. It will be of the annual value either (1) of 100*l*. (inclusive of an allowance for room rent), if the Governing Body shall so determine; or (2) of 85*l*. (also inclusive of an allowance for room rent), which sum may be raised to the larger sum above-named after the completion of one year's residence, if the Governing Body shall so determine. Candidates must call on the Dean on Wednesday, February 18, at 1.30 P.M. The examination will follow at 2 P.M. Candidates must not have exceeded the age of 20 on the 1st of January last, and must produce certificates both of the day of their birth, and of good character. Papers will be set in Chemistry, Physics, and Biology; but candidates will not be expected to offer themselves for examination in all these subjects.

PROF. COPE has recently explored the beds of the late tertiary formation, called Pliocene, as it occurs in north-east Colorado. He discovered twenty-one species of vertebrata, mostly mammals, of which ten were new to science. Four are *carnivora*, six horses, four camels, two rhinoceroses, one a mastodon, &c. The most important anatomical results attained are that all the horses of the formation belong to the three-toed type, and that the camels possess a full series of upper incisor teeth. The discovery of a mastodon, of the *M. ohioticus* type, constitutes an important addition to the fauna. One of the horses is distinguished by its large head and slender legs, much longer than in the common horse. A full account of these results will shortly appear in the report of Dr. Hayden's Geological Survey of Colorado.

THE additions to the Zoological Society's Gardens during the past week include a Suricate (*Suricata zenic*) from S. Africa and a West African Python (*Python sebæ*), presented by Mr. J. H. Coonley; a Feline Douracouli (*Nyctipithecus felinus*) from Brazil, presented by Mr. G. Hollis; a common Kingfisher (*Alcedo ispida*), British, presented by Mr. A. Yates; a Collared Fruit Bat (*Cynonycterus collaris*), an Axis Deer (*Cervus axis*), and a Molucca Deer (*C. moluccensis*), born in the Gardens; two De Fillippi's Meadow Starlings (*Sturnella de filippi*) from Rio de la Plata, received in exchange.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Jan. 22.—"On the Physiological Action of the poison of *Naia tripudians* and other Indian Venomous Snakes," II., by Drs. Brunton and Fayrer.

The results of these investigations show that the poison of the cobra is similar in its action to that of Ophiophagus, Bungarus, and other Colubrine snakes, whilst that of Daboia is similar to the virus of Echis, the Trimeresuri, and other viperine snakes, the chief difference between them being the greater tendency in the viperine poison to cause hœmorrhage or more severe local symptoms. The blood of animals killed by the viperine snakes generally remains fluid after death, whilst that of animals killed by colubrine snakes form a firm coagulum.

The conditions caused by the action of the poison are illustrated by the symptoms manifested by man and the lower animals. The Cobra, Ophiophagus Hydrophidæ, and Bungarus are all very deadly. The Viperine Daboia, and Echis are scarcely less so, whilst the Indian Crotalidæ, such as the Trimeresuri, are much less so. A series of experiments is detailed which illustrate the physiological action of the virus on the nervous system, the muscles, the blood, the respiration, the circulation, and the function of excretion, and also the mode in which death is produced.

The fatal action is shown to be due (1) to arrest of the respiration by paralysis of the muscular apparatus, by which that function is carried on. (2) Or by rapid arrest of the heart's action, in cases where the poison has found direct entry by a vein, *e.g.* the jugular. In such, death is almost instantaneous, and the heart is found to have ceased to beat when in systole. The physiological import of this is very interesting and important, and it was demonstrated by Dr. Brunton, who explained its probable mode of action in certain ganglionic centres in the heart; a subject which gave rise to some discussion; (3) or death may be due to a combination of arrest of respiration and of the heart's action; (4) or it may be due where the quantity of poison is small, or its quality less active, to secondary causes of the nature of other septicæmia, a purely pathological question not discussed.

The mode in which paralysis of respiration (the ordinary form of death) is induced, has been most thoroughly investigated, and it may be said that the question is now settled.

The virus absorbed into the blood either by inoculation into the areolar tissue, or by application to a mucous membrane, affects the cerebro-spinal nerve-centres, the nerves and their peripheral distribution, more especially of the motor nerves. The sensory nerves are less and later affected, and the intelligence generally latest of all, and slightly. The complete loss of it, and the convulsions which precede death, is mainly caused by the circulation of venous blood, the result of the impeded respiration.

Muscular force and co-ordination are gradually lost; paralysis and asphyxia being the evidence thereof. In ordinary cases, the heart goes on beating vigorously long after apparent death, and with artificial respiration, may be kept up for many hours.

The investigations recorded, were made with cobra and daboia poison, sent to England from Bengal in the dried state, a condition in which it resembles gum arabic, and its activity is great. The animals experimented on were dogs, cats, rabbits, guinea-pigs, fowls, pigeons, small birds, frogs. Its action on all these, and the mode in which functions and tissues are affected, are recorded in detail, as well as the extent to which the action of the poison is modified when introduced through different channels.

It has now been clearly shown that the poison acts, when introduced into the stomach, or when applied to a mucous or serous membrane. The idea that it was only effective when injected directly into the blood, is erroneous. It is, no doubt, more certainly and rapidly fatal when it enters the blood direct.

It is also shown that it may be eliminated by the excreting organs, and that there is, therefore, reason to hope that life may be saved if it can be artificially sustained long enough to admit of complete elimination being accomplished, as in the case of curare poisoning; but from the more complex action of the cobra poison this remains a subject of doubt.

By artificial respiration the circulation has been maintained, both here and in India, by Dr. Ewart and Mr. Richards, for many hours; and in one case, after complete paralysis had occurred, symptoms of reaction and elimination were obtained; but no complete recovery has yet occurred. The doubt still remains whether the nervous system that has sustained so much damage, is capable of ever resuming its functions, even though elimination be complete.

The so-called antidotes appear to be inert; all that have been submitted to trial, including the intra-venous injection of ammonia, have failed to have any satisfactory effect. Artificial respiration has certainly prolonged life, and partial recovery has followed, but no life has actually been saved by it.

The microscopic appearances of the blood are described, but no very remarkable change was observed beyond crenation of the corpuscles or diminished aggregation into rouleaux. Chemical examination of the blood and its gases is still needed and further analysis of the poison is desirable.

It is shown that the activity of the poison is scarcely impaired by drying, excepting perhaps so far as regards its *local* action.

Dilution with water, glycerine, liq. ammoniæ, and liq. potassæ did not destroy its activity, nor did coagulation by boiling in the ordinary way. The boiling for half-an-hour under a temperature of 102° C. seemed to destroy the activity of one specimen which was injected into a bird.

The poison acts on all life, on the lower and higher vertebrata, the invertebrata, and even on vegetable life; for it retards, although it may not arrest the germination of seeds. But it acts most vigorously on the warm-blooded animals.

The most remarkable fact connected with it is that it has little or no effect on poisonous snakes. They can neither poison themselves nor their congeners; or if at all, very slightly so, whilst the poison acts rapidly and fatally on innocent snakes, lizards, fish, and mollusca.

With reference to the means of preventing death, it may be said that those that mechanically prevent the entry of the poison into the circulation by means of the ligature, excision, or cautery are the most reliable, but that they are only so when applied immediately.

No means that offer any hope of benefit should be neglected, and it is possible that stimulants such as alcohol and ammonia may be useful; and in some cases, where the poisoning has been severe but not fatal, do good and even determine recovery where death would have otherwise resulted. The so-called antidotes, however, beyond any actions of this kind that they may possess, are apparently quite inert.

Transfusion of blood is alluded to, but the experiments hitherto proposed have not met with success. A more perfect way of accomplishing it may be more successful.

Zoological Society, Feb. 3.—Dr. E. Hamilton, vice-president, in the chair. The secretary read a report on the additions that had been made to the society's menagerie during the month of January, 1874, amongst which were specially noticed a female Water-Deer (*Hydropotes inermis*), a pair of Pink-headed Ducks (*Anas caryophyllacea*), and a Dusky Monkey (*Semnopithecus obscurus*), acquired by purchase, and two Vulturine Guinea-fowls (*Numida vulturina*), presented by Dr. J. Kirk.—An extract was read from a letter addressed to the secretary by Mr. Luigi M. L. Albertis, containing an account of a new species of kangaroo, of which he had lately obtained a living specimen from New Guinea, and which he had proposed to call *Halmaturus luctuosus*.—Dr. Cobbold communicated the second part of a series of papers entitled "Notes on the Entozoa;" being observations based on the examination of rare or otherwise valuable specimens contributed at intervals by Messrs. Charles Darwin, Robert Swinhoe, Charles W. Devis, the late Dr. W. C. Pechey, Dr. Murie, and others.—Mr. Garrod read a paper in which he proposed a new classification of birds, details of which will be found in another page.

Chemical Society, Feb. 5.—Prof. Odling, F.R.S., president, in the chair.—The secretary read a preliminary notice on the action of benzyl chloride on the camphor of the Lauraceæ (*Laurus camphora*), by Dr. D. Tommasi.—Dr. C. R. A. Wright had a paper on the Isomeric Terpenes and their derivatives: Part III. On the essential oils of wormwood and citronelle; being a detailed account of his experiments on these substances, a preliminary notice of which was communicated to the society some time since.—The other communications were a preliminary notice on the perbromates, by M. M. Pattison Muir, F.R.S.E.; and on the coals from Cape Breton, their cokes and ashes, with some comparative analyses, by Henry How, D.C.L. The latter paper giving the amount of coke produced by slow and quick coking, from the main seam coal of Sydney mine, Nova Scotia, and the Lingan coal, also analyses of the ashes left by these coals.

Royal Microscopical Society, Feb. 4.—Anniversary meeting.—Chas. Brooke, F.R.S., president, in the chair. The report of the council and the treasurer's statement of accounts were submitted and adopted, and the officers and council for the ensuing year were elected. The president delivered an address, and concluded with obituary notice of Fellows deceased since the last annual meeting. The following gentlemen were elected as officers and council. President—Chas. Brooke, F.R.S. Vice-Presidents—Dr. Braithwaite, F.L.S.; J. Millar, F.L.S.; W. Kitchen Parker, F.R.S.; F. H. Wenham, C.E. Treasurer—J. Ware Stephenson, F.R.A.S. Secretaries—H. J. Slack, F.G.S.; C. Stewart F.L.S. Council—J. Bell, F.C.S.; F. Crisp, B.A.; Dr. W. J. Gray; J. E. Ingpen; S. J. McIntire, H. Lee, F.L.S.; W. T. Loy; Dr. H. Lawson; H. Perigal, F.R.A.S.; A. Sanders; C. Tyler, F.L.S.; T. C. White. Assistant Secretary—Walter W. Reeves.

Royal Horticultural Society, Jan. 21.—Scientific Committee.—A. Smee, F.R.S., in the chair.—The Rev. M. J. Berkeley sent portions of holly stems pierced by the larva of the wood leopard moth (*Zeuzera Æsculi*).—Prof. Thiselton Dyer exhibited a small branch of *Vitis gongylodes* from the Victoria House at Kew. The end appeared to have been broken off, and the adjacent internodes had (apparently in consequence) swollen into a mass like a small cucurbitaceous fruit.—Prof. Lawson remarked that an Indian vine (*Vitis quadrangularis*) ordinarily had the internodes swollen, though not to anything like the same extent.—A conversation then arose as to the production of aërial roots by vines.—Mr. Worthington Smith, F.L.S., detailed the results of a series of experiments made with the object of ascertaining how far perfectly sound potatoes can be contaminated by contact with infected ones.—Mr. Andrew Murray, F.L.S., made some remarks on interesting plants suitable for horticulture which he had met with in the Rocky Mountains.

General Meeting.—Mr. W. A. Lindsay, secretary, in the chair.—Prof. Thiselton Dyer made some remarks on a parasitic fungus, which was proving exceedingly destructive to hollyhocks. It has been identified by Berkeley in this country, and subsequently by Durieu de Maisonneuve, in France as *Puccinia Malvacearum* of Montagne; it was first described from specimens collected in Chili by Bertero.

EDINBURGH

Geological Society, Dec. 18, 1873.—On some points in the connection between Metamorphism and Volcanic action, by Prof. Geikie, president. After adverting to his previously published views regarding the connection between the protrusion of granite and ordinary volcanic rocks, the author proceeded to point out that the facts were probably capable of a wider interpretation. The metamorphism of large areas was well known to be intimately related to the contortion and plication of rocks, highly metamorphosed regions being those where the rocks had undergone the most intense pressure and crumpling. Heat would necessarily be evolved in the process of compression, and might have been in some parts sufficient actually to fuse the rocks. Such fused portions were probably recognisable in the masses of granite, syenite, porphyry, and other so-called igneous rocks so common in metamorphosed regions. These views were shared by many able geologists of the present day. The author, referring to the recent memoir of Mr. Mallet, pointed out that such conditions as those indicated by the facts of metamorphism were eminently suggestive of the probability that volcanic action had accompanied metamorphism. The extensive crumpling of the rocks of a region indicates a weak part of the crust of the earth through which the internal heat would for a time be more easily transmitted to the surface, while the effect of that crumpling would be greatly to increase the store of heat out of which volcanic energy arises. Hence both by the access given along the line of weakness to the internal heated mass of the earth, and by the increased temperature due to the contortion, water finding its way downward from the surface would encounter conditions eminently favourable for the production of volcanoes. If this speculation has any ground of truth, we should expect to find some evidence of the association of volcanic masses with wide tracts of metamorphism. Without travelling beyond our own country, we seem to have corroboration of it all along the flanks of the highly-contorted, and, over the Highlands, intensely-metamorphosed Silurian hills. The author then gave some details as to the probable thickness of rock under which the present metamorphosed rocks of the Highlands lay at the time of their metamorphism, and showed that it was probably comparatively small. They were in great measure, if not entirely, metamorphosed before the time of the Lower Old Red sandstone. But the process of metamorphism was no doubt a very prolonged one, and we should therefore be prepared to find proofs of its progress at widely separated periods. It is now well known that low down in the Old Red sandstone of the Midland Valley of Scotland enormous sheets of felspathic lavas and tuffs occur, forming such chains of hills as the Saidlaws, Ochils, and Pentlands. No earlier traces of volcanic action have yet been met with in Scotland, but these masses prove that when that action began it was developed upon an enormous scale. The author believed the inference might with much probability be drawn that this vast effusion of volcanic material was a consequence, or it might even be to some extent an accompaniment, of the crumpling and metamorphism of the older Silurian rocks. He drew attention to the way in which these volcanic rocks bordered the Silurian areas on both sides of the broad lowland valley, and to the numerous remarkable bosses of granite, syenite, and porphyry by which the Silurian tracts were pierced. That many of these bosses were formed by the actual fusion of the stratified rocks themselves seemed to him highly probable. But he held also that some of them marked the lower parts of the actual orifices out of which the volcanic materials of the Old Red sandstone had issued. He alluded especially to the singular rounded or dome-shaped hills of granite, felstone, and quartz-porphyry by which the Silurian uplands of the southern counties are dotted, and which, from their general form and their relations to the surrounding stratified rocks recall some of the characters of true volcanic "necks." The sheets of lava and tuff have been preserved in the broad lowland valley owing to faulting and subsidence, while they have been removed from the surrounding hills by denudation, so as to uncover the roots of the pipes or funnels from which they were emitted. After the enormous masses of volcanic materials erupted during the period of the Lower Old Red sandstone, the underground forces gradually declined in vigour, and as the author had shown, became reduced in Permian times to the production of a few small cones scattered over the midland valley, and down the valley of the Nith. The remainder of the paper was devoted to the Tertiary volcanic rocks of the western Highlands. The author showed that in Skye, Raasay, and Mull, masses of granite and quartz-porphyry were associated with the volcanic rocks in such a way as to suggest a community of origin. Even at a distance from the main mass of the basalt plateaux, granite occurred which was almost certainly of Tertiary date. The picturesque granite of Arran, for example, which had long been known to be at least post-carboniferous, he now firmly believed to be of the same age as the terraced hills of Skye and Mull, that is, younger in date than the soft clays on which London is built, and it appeared to be associated with actual *coulées* which had, in some cases, suffered an enormous denudation like that of the Scur of Eigg. He had not yet been able to show that the renewed and prodigious outburst of volcanic action in Tertiary times had been associated with the metamorphism of any wide region, and perhaps no data are obtainable to throw light upon this question. But the extravasation of granite rocks at several places seemed to indicate that metamorphism had taken place, and at least showed, as Mr. Jukes long ago pointed out, that molten granite might be associated with true volcanic action, though it did not reach the surface as granite.—On fossil cones from the Airdrie black-band ironstone, by G. A. Panton.—Notes on the geology of India, by Andrew Taylor.

MANCHESTER

Literary and Philosophical Society, Jan. 27.—R. Angus Smith, F.R.S., vice-president, in the chair.—"On a Source of Error in Mercurial Thermometers," by Thomas M. Morgan, Student in the Laboratory of Owens College. While engaged in distillation, the thermometer, which was placed in a Wurtz tube so that the column of mercury was entirely surrounded by the vapour of the distilling liquid, was found after some days to indicate three degrees too little—a discrepancy caused by volatilisation from the surface of the column of mercury and condensation on the upper part of the tube. By causing the mercury to flow to the end of the tube and back, the condensed portion was gathered up and the correct temperature indicated. It has since been observed that after each day of distillation, with liquids boiling between 60° and 100° C., a quantity of mercury equal to 1° or 1°·5 volatilises.—"Notes on fossil Lithothamnia (so-called Nulliporæ)," by Arthur Wm. Waters, F.G.S. These attain their greatest development in the Leithakalk, a miocene formation which is principally, in some cases almost entirely, composed of these algæ. But they are in no way confined to the Leithakalk, being also very abundant in the eocene, especially the upper division; the so-called granit-marmor, or Bavarian marble, a nummulitic formation, is very largely composed of this concretionary-looking body. In North Italy it abounds in the eocene formations which are so largely developed in the Veronese and Vicentin. In many places the formation is some hundred feet, much more than half composed of the Lithothamnium. It occurs abundantly in Hungary and Switzerland. The so-called pisolithic limestone of Paris is according to Gümbel about eight-tenths stone algæ; also M. Mario, Astrup; the pleiocene of Castel Arquato; and in fact it seems to be found in most of the tertiaries on the Continent; it is further found in the chalk at Maestricht, and in the jurassic sponge beds at Schwabenbergs. The object of this paper is to draw attention to the great masses of these bodies and the importance of always noticing their occurrence in geological formations, since it should be a very material help in regard to the climate, and the conditions of the coasts and currents, besides being of great stratigraphical assistance; nor is it of less importance to note carefully the growth of recent ones, for only through a knowledge of the present can we interpret the past.

PARIS

Academy of Sciences, Jan. 26.—M. Bertrand in the chair.—The following papers were read:—Note on magnetism, in answer to M. Gaugain, by M. J. Jamin.—Direct demonstration of the equation $\int \frac{dQ}{T} = o$ by M. A. Ledieu. This was a continuation of the paper read at the last session of the Academy by the same author.—Note on the Rhone irrigation canal, by M. A. Dumont.—Several papers on the action of water on lead, were received.—Organogenesis compared with androgenesis (*l'androcée*), by M. Ad. Chatin. This part of the paper dealt with the *Saxifrages* and *Crassulaceæ*.—On the lateral solfataras of the Chili volcanoes, and on certain new minerals, by M. L. Domeyko.—On the history of the question as to the passage of birds through the air, by M. A. Penaud.—On the shocks of earthquake at Nice, by M. Prost.—Determination of the pluckerian numbers of envelopes, by M. H. G. Zeuthen.—On the apparent orbit and period of revolution of the double star ζ Herculis, by M. Flammarion.—On the variable state of voltaic currents, by M. P. Blazerna. This was an answer to M. Cazin's recent remarks on the subject.—On a new saccharometer and a new method of obtaining an absolutely monochromatic sodium flame, by M. Laurent. The latter object is attained by interposing a cleavage plate of a crystal of potassic dichromate between the polariser and the flame. This absorbs nearly everything but the yellow light of the flame.—Researches on the flow of liquids through capillary tubes, by M. A. Guerout.—On a new laboratory balance, by M. Deleuil.—On ethylic oxalurate and oxamethane cyanurate, by M. E. Grimaux.—On the grafting of dental follicles and of their constituent organs, separately by M. Legros and Magitot.—Remarks on M. Martin's paper on the comparison of the anterior member of the "*Montotremès*," with those of birds and reptiles, by M. E. Alix.—Note on the ammoniacal fermentation of urine, by M. A. Lailler.—On the pretended emission of ozone by plants, by M. J. Bellucci. The author had made a number of comparative experiments on this subject. He found the colouration of the test paper to be due to the combined action of light and moisture.

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