THURSDAY, JANUARY 2, 1879

ROBERT DICK OF THURSO

Robert Dick, Baker of Thurso, Geologist and Botanist. By Samuel Smiles, LL.D. (London: John Murray, 1878.)

BLEAK and bare, flat and featureless, the county of Caithness lies apart at the far end of Scotland, separated on one side from the rest of the country by rugged mountains and girdled on the other sides by boisterous seas—an unlovely region of brown moor and black morass, partially redeemed to agriculture along the sea-board, but so swept by storm and salt-spray that trees will not grow, save in a few sheltered spots where they have been carefully screened. The solitary mountain group of Morven and the Scarabins, visible from every quarter, lies at the far southern limit of the county, where it seems rather to be part of the uplands of Sutherland, to which indeed in structure it belongs. One redeeming feature, however, can be claimed for Caithness. It is one which compensates, or even more than compensates for the general monotony. The coast-line is almost everywhere formed by a range of mural precipices, rising here and there to heights of 200 and even 300 feet above the waves. Huge massive quadrangular sea-stacks tower out of the water in advance of the main cliff. The sea, moreover, runs inland in innumerable deep dark clefts or "gyoes," and is ever booming in the far recesses of caves that have been worn out of the solid rock by the chafing tides.

The monotony of scenery corresponds with, and indeed depends upon, the sameness of rock underneath. From one end of the county to the other the same interminable dark-grey flagstones in gently undulating beds underlie the scanty soil and peaty morasses. It is these rocks too which, truncated by the sea, run out boldly into headland after headland, or retire into sheltered bays and there extend in reefs upon the shore.

Over the wide Caithness plains the roads run in straight, unvaried lines for miles together. A curious consequence is alleged to be traceable in the physiognomy of the inhabitants. Two acquaintances, advancing along a road from opposite quarters, begin to recognise each other some time before they can actually meet. The smile of recognition is thus prolonged and fixed, so that the people are said to wear a characteristic Caithness grin.

A more unpromising field for the development of natural history tastes it might seem at first somewhat difficult to find within the compass of these islands. No lover of flowers is attracted to settle where short chilly summers and long damp stormy winters make up the year. And where flowers are few insect life will be scanty. Nor is the assemblage of birds likely to be varied where there is neither bush nor tree on which to perch or nest. The waters of these northern seas offer undoubtedly the greatest prospect of reward to the naturalist. They teem with life. Their plants and animals are cast up on the beach by every storm. Every pool on these rocky shores may be made a subject of patient and delightful study.

It was into the midst of these scenes that in the summer of the year 1830 fortune cast Robert Dick, then a young man of about twenty years. His life had not been altogether a happy one. The son of an officer in the Excise, he had received the ordinary education of a rural district in Scotland, and had shown such aptness at school that there was a proposal to send him to college with a view to his entering one of the learned professions. His father, however, married a second time. Robert's position at home eventually became so uncomfortable that at the age of thirteen he was glad to escape from the paternal roof and become apprentice to a baker in his native village of Tullibody, at the foot of the Ochil Hills. During his school days, and still when employed in distributing bread through the district, he developed an intense love for nature, which remained the master passion of his life. Flowers were his special study in these early years. He knew them in their abode in every bosky dell of his native hills, though as yet he had been able to learn little regarding their scientific names and classification.

With this yearning after wild plants and the scenes amid which they grow Dick came to Thurso (whither his father had already removed) and established himself there as a baker on his own account. His business, however, though he gave very diligent heed to it, did not afford occupation for more than a small part of his time. He was accordingly left with plenty of leisure for making himself acquainted with the natural history of his new home. The sea-shore naturally first attracted him. He wandered for miles along the coast, and collected such shells as he could procure from the beach. But he seems never to have thrown himself with zeal into the study of the marine fauna. His eye, indeed, was ever open, and, with the instinct of a true naturalist, he could recognise the value of facts in departments of knowledge with which he had little practical acquaintance. He ransacked the moors and mosses for beetles, bees, butterflies, and moths, gathering every species and variety he could find, and forming in this way a tolerably complete collection of the insect fauna of Caithness. Eventually he gave himself up, heart and soul, to the flora of the county, traversed on foot every parish and moor from end to end, and formed a herbarium containing not only each species of plant, with its locality and habitat carefully affixed, but also many singular and interesting varieties. He watched the vegetation from season to season, was familiar with the haunts of every species, knew when and where each began to show the earliest buddings, traced out the geography of the flora, and marked on what kinds of soil or rock particular varieties were to be found.

It was not until some years after his settlement in Thurso that he began to look into the rocks of the sea-shore. He stumbled upon scales of fishes imbedded within them, which greatly excited his wonder. Further examination brought to light abundant bones and plates, such as he could not find described in any accessible book. He began to collect these fossils, noting particularly the circumstances under which they occurred in the rock, and endeavouring to work out, as well as he could, their peculiarities of structure.

The way in which Dick found time for long excursions, without in anywise neglecting his business, brings out the wonderful energy and enthusiasm of the man. He would bake his daily supply of bread in the early hours of the morning, have it ready for sale by his faithful housekeeper, and start off himself long before even the earliest riser in Thurso was out of bed. Often would he leave home about midnight, taking advantage of moonlight, and cross the county to reach some special ground for observation by daybreak. Yet he would always get home again in time for the preparation of next day's baking. In this way he would walk sometimes sixty miles or more in a single journey.

Of course such a man could not escape criticism in a small town where everybody knew everybody. And Dick's personal appearance not less than his singular occupations made him a "character" in Thurso. At dusk a tall figure with chimney-pot hat, swallow-tailed coat, jean trousers and travel-stained boots, usually with some bundle of stones, ferns, grass, or what-not, might be seen marching with a swinging pace towards the bakehouse in Wilson's Lane, and the neighbours would watch him as he passed, shrugging their shoulders, and wondering where the poor eccentric baker had been wandering this time. There was no congenial society for him in the place. Though naturally of a sunny hopeful temperament the bitterness of his early life had in some measure soured him; or at least had made him shrink within himself, avoiding the society of others, and finding his companionship among the flowers, mosses and rocks out of doors, and with his books at home. He was a diligent reader, not merely of such books on his favourite pursuits as he could afford to purchase, but of general literature, and in particular of poetry. He had considerable aptitude in quotation and availed himself freely of the gift in his letters. He taught himself drawing, also; turning the acquisition to account not only in the delineation of the objects of natural history which he encountered, but in such excursive subjects as Egyptian antiquities and classical figures, with charcoal outlines of which he would at times adorn the walls of his bakehouse. His artistic taste led him too to procure always the best edition of a book and to put it into the best style of binding.

Dick made Hugh Miller's acquaintance when that eminent writer was at the height of his reputation. There was much in the history and characters of the two men to draw them together. The one had told the whole world his story and had enlisted the sympathy of every reader in the pursuits that had made him famous. Dick on the other hand shrank from notoriety. He told his friend all he knew, showed him all his collections made him welcome to the use of everything, and took him over the ground whence he had quarried many a rare fossil. Such generous help could not but meet with fitting public acknowledgment from its recipient. "He has robbed himself to do me service," said Miller, who fully and frankly stated the nature and extent of his obligations; and then for the first time the geological world heard of the labours of the baker of Thurso. Dick, sitting by his own oven-mouth and reading the allusions to himself in Miller's paper, blushed to find himself thus in print, and begged that he might not be so often mentioned by name: "Leave it to be understood," he writes to Hugh Miller, "who found the old bones; and let them guess who can."

Nevertheless, like many self-taught men, Dick with this avoidance of publicity, united not a little vanity. He was proud of his humble position, and contrasted it with that of the "gentlemen-geologists," who could never do any good work for fear of soiling their clothes. He was proud of his prowess as a pedestrian, losing no opportunity of telling his friends how many miles he had walked and how many hours and minutes he had been on the tramp, while the "gentlemen-geologists" would have been in bed or lazily driving over the ground in gigs. He was proud of his achievements in science, of his power of seizing and sifting facts, of the collections which he had made, of the opinions he had formed. He had indeed much to justify this egotism, though few except the very limited number admitted into his intimacy, knew how much. Hence to ordinary acquaintances, or casual visitors he seemed morose, sarcastic, almost contemptuous. But above these little idiosyncrasies stood out bright and clear, his purity of character, his generous unselfishness, his enthusiasm for nature and his stern conscientious devotion to the daily drudgery of his business. His life was on the one hand a struggle with poverty, and on the other an exuberant communion with the works of God. After fifty-six years of such a life he died, leaving as the result of all his toil just enough of money to defray his debts.

It was well that such a story should be generally known. Dr. Smiles deserves our best thanks for having rescued it from oblivion and thrown it into the form of a volume, made up largely of Dick's own letters. It was fortunate that so many letters could be recovered, for otherwise, as Dick never published anything, and his friends were few, it would have been hardly possible to gather details enough for his biography.

The author has doubtless tried to do his best with the materials at his command, and nobody but he can know the difficulty of his task. But, in spite of the interest of his subject, he cannot be congratulated on having fully sustained in this new venture his well-earned reputation as a biographer. He appears to have taken up the life at intervals sufficiently removed to allow him to forget what he had already written, so that he repeats the same idea, sometimes almost in the same words. We are told three times, for instance, that there is no land between Thurso and Labrador, and twice within the space of three pages that Dick was a favourite with the children of his employer. Dick's habit of Sunday walking is referred to in Chapter XII., and after the lapse of more than a hundred pages it comes up again as if it had never been spoken of before. His baking operations, and how he carried them all on himself, are not likely to be forgotten by any reader of the volume.

More serious fault must be found with the inaccuracies of the book. The author states (p. 98) that "distinguished geologists had asserted that no fossil remains were to be found in the Scotch Highlands," and in support of this assertion he quotes a passage from Conybeare and Phillips's "Geology of England and Wales." The statement is meant to mark the importance of Dick's supposed discovery of fossil fishes in the Old Red Sandstone of Caithness. But a more unfortunate confusion could hardly have been made. In the first place, Caithness is not part of the Scotch Highlands. Geologically and ethnographically it is a portion of the northern lowlands peopled by Scandinavian colonists. Again, while it is true that the rocks of the Scotch Highlands are with rare exceptions unfossiliferous, no geologist for half a century or more has said that those of Caithness are so also. Dr. Smiles, in repeating, amplifies his assertion (p. 245) and blunders still more; for this time he makes Dean Conybeare the author of the astounding statement that "the rocks of *Scotland* are unfossiliferous!" and drags in Sir Roderick Murchison, "who took the statement on trust," and "many writers" as propagating the delusion. A third time he refers to the subject, when (p. 238) he says "Robert Dick discovered numerous remains of fossil fishes in Caithness where distinguished geologists had stated that no fossil fishes were to be found." How he could print these sentences in the same volume with the letter from Murchison given on p. 275, it is hard to understand. In that letter Murchison speaks of himself as an old geologist who had written upon the Caithness fishes thirty-two years before. In truth, that geologist and his companion Sedgwick had found abundant fossil fishes in Caithness and had published an account with drawings of some of them, while Dick was still an apprentice carrying bread among the villages of the Ochils. No fact in Scottish geology was more familiar than that the flagstones of Caithness abounded in fossil fishes. That Dick should have been filled with surprise when he found them, only shows that he had not had opportunities of learning what had already been done in the district.

Again, Dr. Smiles refers to a remark of Sir Charles Lyell's that "very few organic remains had been found in the boulder-clay and especially in the till, throughout Scotland." It would seem as if he were quoting from a letter of Dick to Hugh Miller; for a passage from this letter follows, showing that the writer had found fossils in the boulder-clay almost in every place where he had looked for them. And the reader is left to draw the inference that Dick in testing Lyell's statement by an appeal to nature, had found it to be incorrect. But it remains absolutely true to this hour. The boulder-clay, as a whole, is singularly barren of organic remains. In one or two exceptional places, and Caithness is one of them, it is full of fragmentary marine shells. Dick's observations were quite accurate; but it was not necessary to enhance their importance by showing that they contradicted the published statements of so distinguished a writer as Sir Charles Lyell.

But the most serious defect of all is one with which it is somewhat difficult to deal. Every reader of the book will recognise that its preparation has been a labour of love. Dr. Smiles has wandered over all the scenes of Dick's rambles, has tried to realise as vividly as possible the circumstances and surroundings of the enthusiast's life, has recognised his devotion to the acquisition of knowledge, and has written with the most heartfelt sympathy with Dick's love of nature and the struggles and trials of his position. And yet one feels that into the spirit of the researches which formed the bright side of Dick's lonely life, which cheered him and furnished him with mental food and recreation from beginning to end, the writer of the Life hardly enters at all. It was more than a mere love of nature which carried Dick so buoyantly through his monotonous drudgery; more than the mere pleasure of finding flower, or insect, or fossil in its native habitat, and bringing it home to enrich his collection. We have glimpses of this in his graphic letters, and a reader who knows something of the contemporary history of scientific progress, can read between the lines of these letters and find in them an interest tenfold greater than they can possibly have without this information. The want of such assistance to an ordinary reader must make the letters somewhat monotonous, and give the impression that the book is unnecessarily long. When he reads, for example, Dick's account of his numerous and laborious traverses of Caithness in search of sections of boulder-clay, he will naturally ask the object of these toilsome journeys, what was to be gained from them, and what in actual fact was gained. It would have increased his appreciation of these labours to have learnt something of the problem to the solution of which Dick set himself, and he would have been the better able to realise the eager enthusiasm which led that votary of nature to cross the county on foot at night to get to his boulder-clay scars by daybreak. It would have heightened the reader's respect for the subject of the biography to have been shown how, if the results briefly sketched in the letters now published, had been given in detail to the world a quarter of a century ago, they might have placed the name of Robert Dick among the pioneers of glacial geology.

But of all this we learn nothing from the memoir. Dr. Smiles sums up Dick's character and points the moral to be drawn from the story of his life. But what was the outcome of these long years of indefatigable labour? Apart from the man himself, what did his work advantage the world? It is, indeed, a worthy thing to have lived a life that may serve as an ensample and encouragement to after generations. Dick did that nobly. But he did more. He felt that he had "done the State some service." Though he never published his knowledge he worked incessantly and freely communicated his stores of information to others. Much of that knowledge died with him. Yet from his letters, his scientific collections, the published references to the assistance freely given by him to fellow-workers in science, and the recollections of his contemporaries, it might have been possible to have given at least an outline of what he had achieved. Such a sketch would have been a fitting tribute to his memory, a recognition of the meaning and value of those long years of solitary toil.

In an interesting and genially written episode, Dr. Smiles sketches the career of another, but still living enthusiast in natural history—Charles W. Peach, who was one of Dick's most intimate friends, worked with him among the Old Red Sandstone fossils, corresponded, argued, battled with him over their respective opinions. But here again the writer's general sympathy with a heroic struggle for the acquisition of knowledge betrays no special interest in or acquaintance with the life-work of his hero. Unwittingly, therefore, he is led to do but scant justice to his subject. From the allusions, for example, in Dick's letters and elsewhere, to a discussion between that dogmatic observer and Mr. Peach regarding fossil wood, no reader could guess what a momentous point in the history of the Old Red Sandstone of Caithness was really in dispute, and how much Peach's observations went to settle it. No one reading the volume, with its account of Dick's hammerings and Hugh Miller's visits and writings, could surmise that in the palæontology of the Old Red Sandstone of Caithness Peach has done far more than Dick, far more than Hugh Miller, more, indeed, than all other geologists put together.

The illustrations of Caithness scenery, plentifully interspersed throughout the book, are well engraved, and, on the whole, very faithful and characteristic. Nothing could be better than the Deil's Brig of Scrabster Bay. We see the very lichens quivering in the gusts that blow for ever through that hideous cleft, and we hear the screams of the northern sea-fowl as they wheel in restless circles from the neighbouring Clett. In transferring the author's sketches to the wood, however, the artists have taken a few liberties which would have roused poor Dick's indignation. Dirlet Castle, which stands on a rock some twenty or thirty feet above the stream, is raised at least 300 feet into the air; and dear old Morven, glorified into a second Matterhorn, is placed just opposite to Dick's contemptuous ridicule of what the books say about the hill—"None of the hills are as big as books make them"—"downright nonsense! Morven is accessible on every side."

ARCH. GEIKIE

TELEGRAPHY

Instructions for Testing Telegraph Lines and the Technical Arrangements of Offices. By Louis Schwendler. (London: Trübner and Co., 1878.)

THE criterion of the good working of a line of telegraph is its freedom from interruption. Interruptions to the communication are technically called "faults," and on our overground lines men are stationed at certain intervals for the express purpose of patrolling these lines and removing defects from them that sooner or later might culminate in faults. Of course accidents, such as those arising from snowstorms and violent winds, cannot be prevented, but most of the interruptions that are met with in practice can by proper supervision be eliminated before they can arrive at such a condition as to interfere with the communication. In telegraphy more than in anything else, "prevention is better than cure," and for many years past all our telegraph engineers who have devoted their attention to the proper maintenance of telegraphs have striven to devise as perfect a method as they can for detecting the presence of faults and for establishing an accurate system of testing.

It is, however, upon our submarine cables, not only in their manufacture but during the process of laying, and whilst subsequently working, that the greatest skill and ingenuity has been employed to devise a perfect system of testing.

The first rational mode of testing our cables was introduced by Dr. Siemens, but Mr. Varley had previously introduced into the service of a Telegraph Company a very elaborate system of testing by the aid of differential galvanometers and resistance coils. Rheostats or resistance coils had been invented by Wheatstone as far back as 1843, and Sir Charles Bright and his brother, Mr. Edward Bright, had introduced them into use on the Magnetic Telegraph Company's system. It was, however, in the telegraph companies' service that the system was to a certain extent perfected, and when all the systems of the different companies were concentrated into the hands of the General Post Office the system became universal for the whole country. We cannot think that Mr. Schwendler, when he asserts that no really practical system of testing had been adopted by any other telegraphic administration than that of India, could have been aware of the perfect system in use by our English administration, and it is a pity that he has not embodied in his book a description of the system in use in England. This perhaps is unnecessary, because it is fully detailed in the "Handbook of Practical Telegraphy," by Mr. Culley, and in the text-book of science on "Telegraphy," by Messrs. Preece and Sivewright. Moreover, there is an excellent little "Handbook of Testing" detailing not only the practice on land lines but on cables also, by Mr. H. R. Kempe, and with another capital little book by Capt. Hoskiaer, on "testing cables," as well as a work on "Electrical Measurements," by Mr. Latimer Clark, leaves very little to be desired on the literature of the subject. Mr. Schwendler really adds little or nothing to our knowledge of the subject, and his book is only valuable as an indication of what has been done in India.

Great strides have been made in the Telegraphic Department in India ever since the accession to power of the lamented Col. Robinson. There is, according to Mr. Schwendler, a large staff of officers available with a first-rate general education and with a strong desire for improvement, and many of them are well trained in conducting physical experiments. It is to be hoped that their education is sufficiently advanced to enable them to follow the rather intricate mathematical developments of Mr. Schwendler. If his book has a defect it is that it is overloaded with mathematical investigations. There is no necessity to appeal to laboured formulæ when simple observations alone are needed to interpret phenomena. The mathematician loves his formulæ as a hen her brood, but the practical man prefers to kick them aside when he can do so and when he can do without them. Now, at p. 16, Mr. Schwendler gives no less than six elaborate formulæ, one of which must be selected for each particular condition to enable the tester to discover the value of any foreign electromotive force that may be in the circuit, the result of what he calls a "natural" current. Now there is no necessity whatever for any formula. The elimination of earth currents in cable and land testing is of daily and constant occurrence, and it is only necessary to compare the deflection upon any galvanometer given by the earth current with a deflection produced by one cell through similar resistance to find its value. Readings by reversals when taken rapidly always give a mean that is approximately true, for an earth current rarely varies so rapidly as to introduce any sensible error. His formulæ for eliminating the electromotive force when measuring with a differential galvanometer simply appal one.

Mr. Schwendler wisely says, "however much testing may become routine by continual practice it *will* always and *should* always partake of something of the nature of a physical experiment which must be conducted with a perfectly clear understanding. Then only can the tester draw the right conclusions from his observed facts; then only can testing become a real benefit to the administration."

Again he says, "We know quantitatively the electrical state of the lines at all hours of the day, and seasons of the year; we are able to localise faults of all kinds very accurately and repair them with despatch; we test all our telegraphic material, and by it have greatly improved its essential qualities; we are not groping in the dark any more—we *measure* and *know*."

It never must be forgotten that testing is in reality a physical experiment, and these physical experiments are being conducted every day throughout the whole of our English telegraph system. Our cable electricians under the guidance of Sir William Thomson have carried this system of physical experiment to a high standard of perfection, and our Indian friends would do well to profit by their teaching.

Mr. Schwendler's explanation of the theory of the bridge is not clear, nor does his use of Kirchhoff's corollaries to Ohm's law much help the student. Indeed it is very doubtful whether his proof that the sensibility of the bridge method is greatest when the branch and the resistance are equal is true. At any rate in our practice we find that the more delicate the galvanometer of the bridge the more sensitive and the more accurate is our test.

The most valuable portion of Mr. Schwendler's book is his abstract of Ohm's classical paper, a translation of which is to be found in Taylor's "Scientific Memoirs," and also in his account of Kirchhoff's corollaries to this law.

The practice generally of line testing and testing for faults contains nothing new, but his chapter on natural currents, showing the effect of polarisation of earth plates and the presence of earth currents, is interesting.

He says, also "*Defective insulation at a few points* in a line is a fruitful source of currents. At all such points polarisation is produced by the working currents, in a manner precisely similar to that of the earth plates, by the same cause already alluded to, and to a degree dependent on the resistance and the position of the faults. These currents will be strongest in rainy weather, when the line is in contact with trees, when the insulators are covered with dew—in fine, under those circumstances which diminish the resistance of faults and promote electrolytic action.

"The stronger the working currents used, and the fewer the defective points, the stronger will be the polarisation currents.

"If these currents become very strong their direction may be reversed by sending for a short time a strong current with zinc to line; and, in such a case, this invariably indicates a single fault in the line or cable." This is a defect which we do not experience in England.

We find that (p. 66) "on all the lines in India positive signalling currents (copper to line) are used in order to have the greatest possible insulation of each line under all circumstances. Now, when measuring the insulation of a line with a positive test current, it is evident that the value obtained must give the insulation much too high, *i.e.*, higher than the line actually has when signals pass through it; because the signalling currents can only have a comparatively small oxidising effect on the line, since only a very small part can escape to earth in the different points of the line, while a positive testing current, the further end of the line being insulated, must all escape to earth at the defective points of the line. Again, when measuring the insulation of a line with a negative testing current, we get a value which gives the insulation of the line much too low, because negative signalling currents are never used. In the absence of any known law, which would give us how much too high the insulation of the line is obtained with a positive testing current, and how much too low with a negative testing current, we can do nothing better than to take the arithmetic mean of the measured values as representing the insulation the line probably has when signals are passing through it. Of this mean it may, however, be said that it must be always somewhat too low, for the very reason that negative signalling currents are never used, and therefore the arithmetic mean again of the *first mean* and the *positive measured value* would represent a value most probably approximating to the one which the line actually has when signals pass, and which alone is of practical interest and consequence to be known."

The latter part of the book is devoted to fault testing, *i.e.*, to the localisation of the positions of faults.

The book itself is a very valuable addition to the literature of the subject, but we doubt whether it will be of any practical use to our English electricians.

OUR BOOK SHELF

Sketches of Wild Sport and Natural History of the Highlands. By Charles St. John. Illustrated Edition. (London: Murray, 1878.)

MANY of our readers must be familiar with the inimitable "Sketches" of St. John, which has long ago achieved the position of a classic for both the sportsman and the naturalist. We do not know of any descriptions of sport to equal those that abound in these pages, in truthfulness, vigour, and genial humour. To the naturalist who loves to know the habits of an animal in its native haunts, the book must be a treasure; and now that Harrison Weir, Whymper, Corbould, Collins, and Elwes have adorned it with their art, the book should become a greater favourite than ever. No artist equals Whymper in his faithfulness to life in drawing animals. Every picture in the book—and there are about eighty of them—is a masterpiece in its way, and an impressive lesson in natural history. We need only say that the engraver is Mr. J. W. Whymper to convince our readers that the artists' charming work has been faithfully and skilfully rendered. No one can read a chapter of the book without being both refreshed and instructed.

LETTERS TO THE EDITOR

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

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

Paradoxical Philosophy

IT is strange to see a writer on philosophy like Mr. S. H. Hodgson, as well as physicists so exceptionally able as Prof. Clifford, and now Prof. Clerk-Maxwell, falling into the same *errors of observation* as more ordinary mortals. Neither the authors of the "Unseen Universe," nor any of the members of the Parodoxical Society, have, so far as I am aware, expressed the notion that the invisible order of things which continuity requires as antecedent to the visible order, is in any sense *material.* They only assume that it must be conditioned. Indeed, the authors of the "Unseen Universe" have expressed this conviction *in the preface to the second edition of their work, in italics, and in language that is not only exceedingly clear, but also extremely strong.*

But it seems to be taken for granted on all sides that a man of science can only imagine a mechanical unseen. This is really very hard.

The analogy (however inadequate) furnished by Thomson's vortex atoms, and the invisible fluid which they postulate, is too good an *illustration* of a novel and difficult conception to be disregarded; but it will have to be laid aside at once, if it can be shown to be necessarily productive of such extraordinary misconceptions even in intellects of the highest order.

HERMANN STOFFKRAFT

Schloss Ehrenberg, Baden, December 25, 1878

Force and Energy

SINCE a year or two back, when Herbert Spencer started, in the columns of NATURE, a discussion as to the real meaning of the word "force," most careful-thinking students of mechanics have probably come to the conclusion that either the use of the word "force" must be discontinued as a physical scientific term, or that it must be defined in a different manner from that adopted almost universally by those "doctors" whose writings seemed to weigh so heavily on the brain of "poor Publius." They all agree in saying that in its physical application the word "force" means that which produces *i.e.*, the CAUSE of change of momentum. It is needless to give quotations. They are all, except one, curiously explicit. Germans, French, and English agree. "So sehen wir diese Aenderung als Wirkung irgend einer in demselben thätigen Ursache an; diese URSACHE nennen wir Kraft." (Ritter's "Mechanik," p. 36). "On donne, en general, le nom de FORCE à la CAUSE quelconque qui met un corps en mouvement, ou seulement qui tend à le mouvoir." (Poisson: "Traité de Méchanique," Introduction, p. 2.) Although in Tait's "Recent Advances" we find on p. 11 "that we have not yet quite cast off that tendency to so-called metaphysics which has so often blasted," &c., &c.; yet on p. 16 of the same book there is reproduced the fine old crusty Newtonian maxim to which Thomson and Tait and Tait and Steele cling with such fond reverence: "force is any CAUSE which," &c. Clerk Maxwell gives no formal definition of force in his "Electricity and Magnetism." On p. 5 he simply gives its dimensions. On p. 83 of his invaluable "Theory of Heat" he defines, "force is WHATEVER changes or tends to change," &c. This is a very ingenious mode of escaping the difficulty by simply giving no definition at all. We are told what the result of force is, but not what force itself is. We are told that force is "whatever," which is not very clear. Jeames would hardly think that justice was done him if we asserted that the complete definition of him was "whatever opens a door," and made no mention of the fact of his humanity or of his grand plush breeches. It is, in fact, a confusion between a statement of the mode of measuring quantitatively the force, and the definition of the force itself. A physical definition should certainly show clearly what the proper way of measuring the quantity is; but this latter is not the definition itself. Moreover, there may be different almost equally good modes of measurement, all leading to the same numerical result. Clerk Maxwell's definition is clear as to *a* mode of measuring force, but furnishes absolutely no information as to the nature of the thing intended to be defined. It, therefore, differs from the others in that they are real metaphysical definitions, presumably comprehensible to those who understand metaphysics, while his is no definition at all. Prof. John Perry, in his book on "Steam," adopts the same device as Prof. Clerk Maxwell, substituting the word "anything" for "whatever." Rankine forms a remarkable exception. He says that "force is an action between two bodies either causing or tending to cause change in their relative rest or motion." Here the word "cause" is used in such a sound, practical, common-sense way that no one could take exception to such use of it, even in a physical definition, and probably "action," as here used, might be explained clearly enough for all useful purposes as "a changing relation" or "a change of relation." Rankine, however, does not take the trouble to do this last.

Now clearly a cause is a metaphysical entity, if it is an entity at all, and from the very nature of the difference between metaphysics and physics, a metaphysical entity cannot possibly be made of any use in physical investigations. If, then, the word force is to be usefully employed in physics, it must be defined as something else than a "cause." When we talk of forces, the physical facts the observation of which we think of, are accelerations of momentum; and in his Glasgow lecture Prof. Tait seems half inclined to use "force" and "acceleration of momentum" as synonymous terms. But an acceleration of momentum is a function of one body only; and every one knows that what is mentioned in Rankine's definition is true, namely, that force is a function of two bodies, and can have neither objective nor any other kind of existence except as a relation between two bodies. Seeing that it is so, I beg to lay before your readers for their favourable consideration the meaning of the word force which I have used for several years past. I wish force to be defined as "time rate of transference of momentum." A transference of anything can only take place between one body and another, and in the transference the amount transferred from the first body to the second is necessarily equal to the amount transferred to the second body from the first. This might seem to be such a truism as to be a mere repetition of words; but we must remember that it is the law of motion which the "transcendently lucent" Newton discovered from his extensive physical experience; and, in order to discountenance scepticism, we might add, by way of parenthesis, that during the transference no spilling takes place.

"Poor Publius" might thus get a hint that there is such a physical fact as conservation of momentum which is independent of all formal definitions. If momentum is conserved, *i.e.*, if it has an enduring existence so that at one time there is no more nor less of it than at another, then during a direct transference of some of it from one part of the system in which it is lodged to another part, the amount lost by the one part must evidently be the same as that gained by the other part. Thus an acceleration or time-rate of gain of momentum to one part necessarily implies a simultaneous equal time-rate of loss of momentum from another part, and also a simultaneous equal rate of transference of momentum from that other to the first part. All these three rates have directions inasmuch as they are time-rates of directed quantities. The first is a rate of gain of momentum, which momentum has a certain direction. If that direction be reckoned positive the gain is one of positive momentum, and the acceleration is naturally reckoned as positive. The second is a rate of loss of momentum of the same direction, *i.e.*, a loss of positive momentum which is equivalent to a gain of negative momentum, and therefore this time-rate is naturally reckoned negative. The meaning of this is simply that the proper physical sign to ascribe to acceleration of momentum is the directional sign of the momentum gained. The two opposite signs of the above two rates have given rise to the idea of two equal and opposite forces acting between the bodies. If the forces were located IN the bodies and not BETWEEN them, the phraseology would be consistent with Tait's definition of force as simply "acceleration of momentum." But I do not hesitate to say that this idea of force is quite unnecessarily out of accord with the commonly received notion of force as a mutual action or relation between TWO bodies, because in this view force would distinctly have reference to only one body. If, however, we use force to mean the transference of momentum, there is, of course, only one force between the two bodies. The question is what sign is to be given to this force, and it is not quite easy to answer. Force is in this view a flux, a rate of flow of momentum. This flow takes place in a certain direction, and it is the flow of a directed quantity. Are we to take the direction of the flow or the direction of the momentum that flows, to determine the proper sign of the force? These two directions need not be the same. Thus in a bar subjected to tension the flow of momentum is in the direction opposite to that of the momentum itself. In a bar in compression the flow of momentum takes place in the same direction as that of the momentum. In a mass subjected to shearing stress the direction of the flow is perpendicular to that of the momentum. In the case of the attraction of gravitation between two bodies the direction of the flow of momentum is always the exact opposite of that of the momentum that flows from one to the other in whatever way the two may be moving. In the case of impact if we take the direction of the flow of momentum as that of the perpendicular to the surfaces that touch during impact drawn from the body that loses momentum towards the body that gains momentum, then this direction of flow may make any angle within certain limits with that of the momentum exchanged. If we are to adhere strictly to the ordinary conventions with regard to the directions of forces, it is clear that we would need to consider the transference of momentum which we term force, only with reference to the direction of the momentum transferred, and without any reference to the direction of transference. It is, however, evident that this latter direction is of very great importance in physical investigation, and it is a matter worthy of serious consideration whether or not force should not be considered a two-directional quantity, one into whose definition two directions enter. Impact is a difficult subject, perhaps, just because of the large possible variation of the difference of these two directions. All other forces (rates of transference of momentum), except those involved in impact may be divided into three simple classes corresponding to compression, tension, and shear.

In considering stress, the phrases "transmission of momentum" and "rate of transmission of momentum" are as convenient, perhaps, as the corresponding phrases with "transference" substituted for "transmission."

The most obvious objection to this definition of force is that a force may be applied to a body, and yet it receives no momentum. The objector would probably say that though the force be applied, yet there may be no momentum transferred to the body. But this would be quite wrong, as can be most easily recognised if Prévost's theory of exchanges of heat by radiation and the similar theory for conduction of heat be recalled to mind. A body may quite easily have simultaneously equal amounts of opposite momentum transferred to it. These will balance, and its centre of gravity will suffer no acceleration of velocity. This remark will make it evident that the theory of force gives an easy and unhesitating answer to the much-debated question as to whether there are really such things as unbalanced forces. A transference of momentum between two bodies may just as readily be unbalanced as balanced. Let us consider this balancing of transferences of momentum more particularly. Let a body have momentum transferred to it by the pressure of another body upon a certain portion of its surface. This can be balanced in different ways. It may be balanced by a perpendicular pull applied to a portion of the surface parallel to that to which the pressure is applied, and facing the same way, *i.e.*, on the same side of the body. The directions of the momenta transferred at these two surfaces are the same, but the directions of transference or flow are opposite. Or the pressure may be balanced by an oppositely directed pressure upon a parallel surface facing the other way. In this case the directions of the momenta transferred at the two surfaces are again the same, while the directions of flow are also the same. In all cases when a body is kept in balance by transferences of momentum going on through its different surfaces, it is evident that for any amount of momentum of a given direction transferred into it at one surface an equal amount of momentum of the same direction must be transferred out of it at some other surface. The directions of transference or flow at these two surfaces may be relatively any whatever—they are quite independent. The balance of the body, with respect to the velocity of its centre of inertia, is quite uninfluenced by the directions of the momentum-flows through its different surfaces. But evidently the state of stress and strain throughout the interior of the body depends a great deal upon the relative directions of these flows as well as upon the relative positions of the surfaces.

But, as regards the direction of the momentum, it must be remembered that this depends upon what we arbitrarily choose to be our standard positive direction, whereas the equilibrium of the body acted on certainly does not depend in the least upon that arbitrarily chosen direction. Thus, as in the above example, let the body 2 be kept in equilibrium by the equal and opposite pressures of the bodies 1 and 3 on its opposite faces. The question is whether momentum is being transferred from 1 to 2 and from 2 to 3, the momentum transferred having also this direction; or whether both the flow and the momentum flowing have exactly the opposite direction, viz., from 3 through 2 to 1. If we have a standard positive direction to go by, and if 1 is not in equilibrium, but is being stopped in its motion by impact on 2, then the above question is easily answered at once. But if 1 is in equilibrium as well as 2, we must, in order to answer the question, look beyond 1 to find out the direction of the other transference of momentum, which, along with that between 1 and 2, keeps 1 in equilibrium. If this other transference takes place between 1 and another body which is again in equilibrium, it would be necessary to go back still another step in order to find out in which direction the flow is really taking place. If the whole system of which these bodies form parts is everywhere in equilibrium, *i.e.*, all the parts at rest relatively to each other, we would in this way travel from one body to another in a complete circuit in search of some point which would disclose the real direction of flow, but without ever coming to any such point. Because, following round the circuit, we would again come back to 3 and 2 and 1. The choice of a standard direction as the positive one does not help us in the least to come to even a formal conclusion. We remain, however, sure of two things—first, that there is really a continual flow of momentum taking place all round this circuit in the system; and, secondly, that the direction of this flow is at some places, which we can definitely specify, in the same direction as the momentum transferred, and at some other places, equally easily specified, in the opposite direction. Take as an example a piano. We may suppose the upper horizontal bar of the frame to which the strings are attached to be continually losing upward momentum, which is being continually received by the top parts of the tightened strings. This upward momentum the strings are continually transmitting downwards from particle to particle, and at the foot of the strings it is delivered to the bottom horizontal bar of the frame. This bottom bar transmits the upward momentum horizontally, each section being in shear, to the vertical sides of the frame. The transmission down the wire is in the direction opposite to that of the momentum transmitted; in the horizontal bottom bar the direction of transmission is perpendicular to that of the momentum, through the sides this momentum flows upwards, that is, in the same direction as the momentum itself, and finally, it is transmitted again horizontally through the upper bar to be redelivered to the strings. This explanation is completely satisfactory in accounting for the conditions of strain of the various parts of the piano. But to explain these conditions an equally satisfactory hypothesis would be that a stream of downward momentum is continually circulating through the piano in the same circuit as the above, but in the opposite direction round that circuit. Or again we might suppose two opposite circulations to be continually going on, one of upward momentum and the other of downward momentum. But whichever of the three hypotheses we may adopt, we always have the flow in the string which is in tension opposite to the momentum flowing through it, and the flow through the horizontal bar perpendicular to the momentum, and the flow through the sides of the frames in the same direction as the momentum transmitted.

Which of the three is to be chosen, or is it of any consequence that we should know which should be taken? The question is not one that can be made to have any degree of unreality in appearance by merely measuring the motions relatively to one thing or another. It is not whether the momentum transferred is upward or downward relatively to the centre of the earth, or relatively to the sun or to the stars. It is, what is the direction of this momentum relatively to the centre of inertia of the piano frame itself, whether this relative momentum is directed from one end of the piano towards the other or from that latter to the former, and the answer to this question is quite independent of what we arbitrarily choose to call the absolute velocity of the centre of inertia of the whole structure.

I will venture to say that the correct answer is that there are two opposite streams of equal amounts through the structure. What is meant by equal amounts is, of course, that the opposite rates of transference through any section are numerically equal. The simplest and clearest proof is this very simple one: If there were only one stream circulating in one direction, since from the above it is clear that the momentum flowing along in this stream is at every point of it of the same direction, and since the stream is a continuous steady one, every part of the structure through which this stream flows would have the velocity corresponding to this momentum, and in consequence the centre of inertia of the structure would have a certain velocity in the same direction. The inconsistency of this result with the datum from which we started, namely, that the momentum transmitted was to be measured relatively to the centre of inertia, need not be pointed out. To look at the question in another way, let us only consider what this momentum, this thing that is being transferred from particle to particle, really is, viz., mass multiplied by velocity, and we cannot fail to come in a moment to the conclusion that these streams are simply streams of molecular vibration.

And since each particle maintains constantly the same average position relatively to the centre of inertia of the whole, it is evident that its alternate opposite displacements and velocities relatively to that centre of inertia must be numerically equal. A certain particle has first a certain velocity in one direction, and immediately afterwards has a numerically equal velocity in the opposite direction. This change cannot take place except by its transferring to the next particle the same numerical amount of momentum of one direction as it receives from that particle of momentum of the opposite direction. In this way constant streams run in the two opposite directions, the momentum flowing along one having the opposite direction to that flowing along the other, and equal numerical amounts of these oppositely directed momenta flowing past any given sections of the two streams per unit of time.

As a sort of parenthesis let me give the following symbolical statement of the foregoing. Let V be the velocity of flow of either of these two opposite streams, and μ the mass per unit volume of the material, and v the average numerical velocity of the particles. Then since at any given instant half the particles must be supposed to be moving in one direction, and the other half in the opposite direction, the amount of momentum of one direction passing per unit of time through any section of unit area of the correspondingly directed stream, is $\frac{1}{2} V\mu v$. A numerically equal amount of oppositely directed momentum is flowing per unit of time through the same unit section in the opposite direction. Observe that the material through which these two streams are flowing is in "balance," "in equilibrio." Suppose the one stream to lead out of, and the other to lead into, an unbalanced mass, which mass suppose not to be losing or gaining momentum except by these two streams. By means of the one it loses, say $(+ \frac{1}{2} V\mu v)$ per unit of time. By means of the other it gains $(- \frac{1}{2} V\mu v)$ per unit of time. The amount of positive momentum it transfers to the balanced material for unit of time is, therefore, $+ V\mu v$, and this is the rate of transference of momentum from the unbalanced mass to the balanced material, and through this latter. If the ratio of comparison or extension, *i.e.*, the strain of this balanced material, be called e, then what we usually call its modulus of elasticity is E given by the equation $eE = V\mu v$. If we insert in this expression the proper value of $V = \sqrt{\frac{E}{\mu}}$, the velocity of transmission of longitudinal vibration, we obtain a value of $v = e\sqrt{\frac{E}{\mu}}$, similar to that deduced by De St. Venant for the first stage of an impact, during which a single *unbalanced* wave of momentum is running forward through the body impinged on. But the important point to notice is that the rate of transference of momentum per unit area is the product of a mass per unit volume (μ) and of two velocities (V and v). In unbalanced transmission these two may be in the same direction, in which case the mass being accelerated is in compression, or they may be in opposite directions, in which case the accelerated mass is in tension; or they may be at right-angles, in which case the accelerated mass is in shear. In balanced transmission if in the one stream the velocity of flow is in the same direction as that of the flowing momentum, then also in the opposite balancing stream these two velocities have the same directions and the material is in compression, the strain being double that which would occur if either of these opposing streams existed by itself unbalanced in the material. Similarly for a balanced state of tension and for one of shear.

Considering these reasonings, does it not seem right to make the direction sign of the force, or rate of transference of momentum, the same as that of the product of these two velocities. The sign of the product of two vectors does not depend on the absolute direction of either, or rather it does not depend on the relation of either to what we arbitrarily choose as our standard direction. It depends only on their mutual relation. Thus we get a definite sign for each force not arbitrary, but real. For a compression force the two vectors have different signs, and their product is a multiple of -1. For tension, the two being of the same sign, their product is a multiple of $+1$. For shear, the two being perpendicular, their product is a multiple of $\sqrt{-1}$ or of $-\sqrt{-1}$. If the direction of transference be oblique to that of the momentum transferred, their product is the sum of a scalar and of a vector. In this case we have compound stress, that is a shear compounded with either compression or tension; and, as every one knows, it is usually convenient to consider the scalar and the vector parts separately. The question of the *mode* of transmission of momentum corresponding to these main kinds of stress is one of molecular mechanics, into which there is no need of entering here.

ROBERT H. SMITH

(*To be continued.*)

Leibnitz's Mathematics

IN perusing some old files of NATURE I came upon the following sentence in a letter from Prof. Tait (vol. v. p. 81) in reference to the invention of the Differential and Integral Calculus:—"Leibnitz was, I fear, simply a *thief* as regards mathematics." Prof. Tait has more than once intimated or expressed a similar opinion.

In reply to this imputation Dr. Ingleby says (NATURE, vol. v. p. 122):—"I do not object to the Professor calling a spade a spade; but I assure him that this charge is made just twenty years too late. It is exactly that time since the *last vestige* of presumption against the fair fame of the great German was obliterated. If Prof. Tait does not understand me, or, understanding me, disputes the *unqualified truth* of my statement, I promise to be more explicit in a future letter. But I incline to think the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do the simple act of justice and reparation required of them, viz., publish the letters and papers relating to this controversy, which since that date have slumbered in the secret archives."

Prof. Tait, as far as I know, never responded to the challenge, and I presume there is but one inference to be drawn from his silence.

In a late reading of an account of this controversy from the German standpoint, my interest in the subject has been reawakened, and I feel a strong desire to see the whole question thoroughly ventilated. Such a consummation must surely be wished by every fair-minded man, and in the name of justice I would ask Dr. Ingleby to be more explicit and do what lies in his power to remove the imputation which has been attempted to be fastened upon Leibnitz.

This question will not down at the bidding of any one, and the documentary evidence alluded to by Dr. Ingleby must sooner or later see the light. Let us have the matter at once and for ever definitely and honourably settled. A. B. NELSON

Danville, Ky., U.S.A., November 27, 1878

[It is not to be absolutely presumed that, when a busy scientific man lets pass such challenges, he has given up his point. The question has now lost all but a species of antiquarian interest:—still it is worth clearing up. We might begin by asking Mr. Nelson and other defenders of Leibnitz to explain the very singular appropriation which Leibnitz made of "Gregory's Series" *after* having acknowledged whence he got it.—ED.]

Commercial Crises and Sun-Spots

A SUGGESTION is made by Mr. John Kemp, in NATURE, vol. xix. p. 97, to test the relation of sun-spots to the variation in weight of the cereal grains. Probably the difficulties of giving such a test scientific precision are insurmountable. No doubt these grains do vary in weight from year to year. Of some samples of oats, of crop 1877, contributed by me to the South Kensington Museum, the pound contained 13,642 grains, while the pound of crop 1878 contained 16,870. But there are many varieties of oats, barley, and wheat in general cultivation, each producing grains differing in weight from the others. In an inquiry which I made regarding the weight of the *sterling*, average grains of wheat of crop 1876 from the south of England were found, in an air-dry condition, to weigh as follows: Talavera, 1·01 gr. troy; Chidham white, ·76; Sherriff's bearded, ·86; Kessingland red, ·92; Nursery red, ·76; Trump white, ·81; Red rivet, 1·00; Lammas red, ·89; Hunter's white, ·75. And different ears of a given variety of wheat have grains of different weight. If six or eight culms come up on one stool, the largest ears have the heaviest grains. In general, the larger flower-cups in an ear, contain the heavier grains. Then, there is scarcely such a thing to be found as a crop of one pure variety. Any variety rapidly gets mixed with others. And, supposing that a plot were set aside for a pure variety, year after year, for a few cycles of sun-spots, the mineral conditions would be constantly varying; so that any test by the balance to compare the fruit of one year with that of another, would involve too many unappraisable elements to have a real value.

Prof. Jevons observes that his investigation is "embarrassed by the fact that no inquirer has been able to discover a clear periodic variation in the price of corn." But the quality of corn must be a more immediate effect of solar action than the price. Now, although perhaps not much is to be arrived at from the method suggested by Mr. Kemp, there is another direction in which something might be found, and in which the necessary data already exist. I allude to the records of those Corn Exchanges which contain full details of the measure-weight of every parcel of grain which has been sold in them for several sun-spot periods. I reduced the sales in the Haddington Corn Exchange for the year from July 3, 1868, to June 25, 1869, and found the average bushel-weights as under:—

Wheat ...	27,764 quarters	63·15 lbs. per bushel.
Barley ...	33,022 "	56·85 " "
Oats	16,223 "	43·49 " "

The sales in the Edinburgh Corn Exchange from November 4, 1868, to October 27, 1869, gave the following weights:—

Wheat ...	27,322 quarters	62·84 lbs. per bushel.
Barley ...	35,752 "	56·18 " "
Oats	53,843 "	42·28 " "

Reductions on this larger scale probably eliminate most of the elements of uncertainty. The measure-weight of the cereal grains depends on various factors, one of which is the comparative distension of the epicarp by the inclosed albumen. It is this element which may vary with variations of solar radiation. And if a cycle of measure-weight should be found corresponding with the sun-spot period, a clue might be gained to some unsuspected commercial relationship.

North Kinmundy, Aberdeen A. STEPHEN WILSON

Time and Longitude

I HAVE been much amused at the questions on the above (NATURE, vol. xviii. p. 40), by Mr. Latimer Clark, and the answer (p. 66) by my old friend Capt. J. P. Maclear; the numbers of NATURE for May having only just reached my "out-of-the-world" residence. I suspect Mr. L. C. has had in his mind what I have often had, and with which I have frequently puzzled some "unco guid" Sabbatarians! If it is such a deadly sin to work on Sunday, one or the other of A and B coming, one from the east, the other from the west, of 180° meridian, must, if he continues his daily avocations, be in a bad way! Some of our people in Fiji are in this unenviable position, as the line of 180° passes through Loma-Loma!

I went from Fiji to Tonga in H.M.S. *Nymph*, and arrived at our destination on *Sunday*, according to our reckoning from Fiji, but *Monday*, according to the proper computation west from Greenwich. We, however, found the natives all keeping Sunday. On my asking the missionaries about it they told me that the missions to that group and the "navigators," having all come from the *eastward*, had determined to observe their *seventh* day, as usual, so as not to subject the natives to any future puzzle, and agreed to put the dividing line further off, between them and Hawaii, somewhere in the broad ocean, where there were no metaphysical natives or "intelligent Zulus" to cross-question them! E. L. LAYARD

British Consulate, Noumea, New Caledonia

Hereditary Transmission

I HAVE perused with interest Mr. Edmund Watt's account of the six-fingered family in Dominica, as it recalls to my memory a family showing precisely the same peculiarities in Ceylon, at Point Pedro, the most northerly point of the island, where, twenty-six years ago, I was magistrate.

A family quarrel came before me, and I found, to my great astonishment, that plaintiff and defendant, and all the witnesses, had six fingers on each hand and six toes on each foot! The additional finger or toe was, in each instance, a "little finger" (or toe) inserted in the side of the hand or foot, quite loosely, adhering to the skin, and not part of the skeleton. It might easily have been excised with a pair of ordinary scissors. The parties were all closely related—brothers and sisters, uncles and aunts, nephews, nieces, and cousins—they must have had a common progenitor. It would be easy, and most interesting, to ascertain if any of the family now exist, and, if so, if the supplementary finger has been transmitted to the present generation. A note to the "Resident Magistrate," Point Pedro, would, I hope, produce a reply. If any of the family of my old clerk, Mr. Dehoedt, survive, they would recollect the fact. I think the party came from Panditerripu. E. L. LAYARD

British Consulate, Noumea

"Survival of the Fittest"

IN NATURE, vol. xix. p. 155, Mr. S. F. Clarke's observations on the cannibal habits so rapidly developed by the larvæ of the New England salamanders are cited in illustration of the survival of the fittest. The fact that similar tendencies are invariably betrayed very early in life by the young of the common Mexican Axolotl (*Siredon mexicanum*), numbers of which are annually hatched out in the Brighton Aquarium, may perhaps be of interest. Many of the smaller and weaker ones are bodily devoured by their stronger brethren of the same brood, an inclination which is so marked that systematic over-feeding is resorted to in order to arrest the diminution in the number of specimens.

Brighton, December 27, 1878 A. CRANE

Shakespeare's Colour-Names

IN the very interesting articles and correspondence which you have published on the subject of colour-blindness, it is rather surprising that no one has referred to a passage which, if taken alone, would appear to show that Shakespeare did not know the difference between green and blue. In "Romeo and Juliet" (Act iii., Scene 5), the Nurse says to Juliet, speaking of Paris:—

> "Oh, he's a lovely gentleman;
> Romeo's a dish-clout to him; an eagle, madam,
> Hath not so green, so quick, so fair an eye
> As Paris hath."

What is here called a green eye is evidently what we call a blue one. But Iago ("Othello," Act iii., Scene 3) calls jealousy a "green-eyed monster," using the expression "green-eyed" as a modern might use it, and meaning something very unlike "blue-eyed." These instances appear only to show that in the language of Shakespeare's time the names of colours were used somewhat vaguely. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, December 23

DISCUSSION OF THE WORKING HYPOTHESIS THAT THE SO-CALLED ELEMENTS ARE COMPOUND BODIES[1]

I.

IT is known to many Fellows of the Society that I have for the last four years been engaged upon the preparation of a map of the solar spectrum on a large scale, the work including a comparison of the Fraunhofer lines with those visible in the spectrum of the vapour of each of the metallic elements in the electric arc.

To give an idea of the thoroughness of the work, at all events in intention, I may state that the complete spectrum of the sun, on the scale of the working map, will be half a furlong long; that to map the metallic lines and purify the spectra in the manner which has already been described to the Society, more than 100,000 observations have been made and about two thousand photographs taken.

In some of these photographs we have vapours compared with the sun; in others vapours compared with each other; and others again have been taken to show which lines are long and which are short in the spectra.

I may state in way of reminder that the process of purification consisted in this: When, for instance, an impurity of manganese was searched for in iron, if the longest line of Mn was absent, the short lines must also be absent on the hypothesis that the elements are elementary; if the longest line were present, then the impurity was traced down to the shortest line present.

The Hypothesis that the Elements are Simple Bodies does not include all the Phenomena

The final reduction of the photographs of all the metallic elements in the region 39-40—a reduction I

[1] Paper read at the Royal Society, December 12, by J. Norman Lockyer, F.R.S.

began in the early part of the present year, and which has taken six months, summarised all the observations of metallic spectra compared with the Fraunhofer lines accumulated during the whole period of observation. Now this reduction has shown me that the hypothesis that identical lines in different spectra are due to im-

TABLE I.—FINAL REDUCTION—IRON.

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
	39	
1	0600 / 2	U 3, Zr 5, Yt 4
3	0622 / 4	Va 4
2	0920 / 3	Va 2, Ba 3
3	1010 / 4	Va 4, Pt 3
2	1648 / 2	Co 3
2	1755 / 3	Mn 3, Ce 4
2	1835 / 4	Os 2
1	2700 / 1	Va 2
1	2950 / 1	Mo 3
3	3023 / 4	Ce 4
5	3435 / 4	U 2
3	3475 / 2	Ba 2, Rh 2
3	3628 / 3	Ta 3
2	3975 / 2	Co 3
3	4026 / 3	Va 5
3	4422 / 4	Mo 3
3	4720 / 2	Yt 5, Th 3
2	5012 / 2	Li 2
2	5160 / 2	Ce 3, Ru 3
2	5210 / 3	W 3
2	5423 / 4	U 3, Mo 3, W 4
3	6215 / 3	Yt 5, Ce 3, Di 2
2	6571 / 2	Zr 2
3	6662 / 2	Th 1
1	7555 / 3	Os 2, Ta 4, Cr 2
3	7578 / 4	Di 2
2	7685 / 2	Va 4
2	8083 / 2	Ti 1
1	8320 / 1	Cr 3
3	9520 / 3	Ru 3
2	9750 / 2	Mo 3

TABLE II.—FINAL REDUCTION—TITANIUM.

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
	39	
1	0·00 / 3	Zr / 4
4	0018 / 3	Th / 4
5	1040 / 5	Mn / 4, Ce / 5, Di / 3
2	1360 / 3	Va / 4
5	1915 / 8	Ce / 4
4	2050 / 3	U / 3, La / 3
3	2368 / 2	Va / 3
3	3718 / 5	Th / 4, Ce / 4
2	4775 / 1	Fe / 2
2	5722 / 1	Zr / 1, Rh / 3
4	6175 / 2	U / 3
3	6335 / 2	Di / 3, Ta / 5
2	8083 / 1	Fe / 2
3	8152 / 2	Mo / 3
1	8922 / 1	Mn / 4, Cr / 4
2	9798 / longest	Va / longest

purities is not sufficient. I shall show in detail in a subsequent paper the hopeless confusion in which I have been landed. I limit myself on the present occasion to giving tables showing how the hypothesis deals with the spectra of iron and titanium.

We find short line coincidences between many metals the impurities of which have been eliminated or in which the freedom from mutual impurity has been demonstrated by the absence of the longest lines.

Evidences of Celestial Dissociation

It is five years since I first pointed out that there are many facts and many trains of thought suggested by solar and stellar physics which point to another hypothesis—namely, *that the elements themselves, or at all events some of them, are compound bodies.*

In a letter written to M. Dumas, December 3, 1873, and printed in the *Comptes Rendus*, I thus summarised a memoir which has since appeared in the *Philosophical Transactions.*

"Il semble que plus une étoile est chaude plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques.[1]

"Ainsi nous avons :

"1. Des étoiles très-brillantes où nous ne voyons que l'hydrogène, *en quantité énorme*, et le magnésium ;

"2. Des étoiles plus froides, comme notre Soleil, où nous trouvons :

H + Mg + Na
H + Mg + Na + Ca, Fe, . . . ;

dans ces étoiles, pas de métalloïdes ;

"3. Des étoiles plus froides encore, dans lesquelles *tous les éléments métalliques sont* ASSOCIÉS, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des métalloïdes et des composés.

"4. Plus une étoile est âgée, *plus l'hydrogène libre disparaît;* sur la terre, nous ne trouvons plus d'hydrogène en liberté.

"Il me semble que ces faits sont les preuves de plusieurs idées émises par vous. J'ai pensé que nous pouvions imaginer une '*dissociation céleste*,' qui continue le travail de nos fourneaux, et que les métalloïdes sont des composés qui sont dissociés par la température solaire, pendant que les éléments métalliques monatomiques, dont les poids atomiques sont les moindres, son précisément ceux qui résistent, même à la température des étoiles les plus chaudes."

Before I proceed further, I should state that while observations of the sun have since shown that calcium should be introduced between hydrogen and magnesium for that luminary, Dr. Huggins' photographs have demonstrated the same fact for the stars, so that in the present state of our knowledge, independent of all hypotheses, the facts may be represented as follows, the symbol indicating the spectrum in which the lines are visible.

Hottest Stars	Lines of	H + Ca + Mg
Sun ...		H + Ca + Mg + Na + Fe
Cooler Stars		— — Mg + Na + Fe + Bi + Hg
Coolest ...	Fluted bands of	— — — — — — Metalloids

Following out these views, I some time since communi-

[1] This referred to the old numbers in which Mg = 12, Na = 23.

cated a paper to the Society on the spectrum of calcium, to which I shall refer more expressly in the sequel.

Differentiation of the Phenomena to be observed on the Two Hypotheses

When the reductions of the observations made on metallic spectra, on the hypothesis that the elements were really elementary, had landed me in the state of utter confusion to which I have already referred, I at once made up my mind to try the other hypothesis, and therefore at once sought for a critical differentiation of the phenomena on the two hypotheses.

Obviously the first thing to be done was to inquire whether one hypothesis would explain these short line coincidences which remained after the reduction of all the observations on the other. Calling for simplicity' sake the short lines common to many spectra *basic lines*, the new hypothesis, to be of any value, should present us with a state of things in which basic molecules representing bases of the so-called elements should give us their lines, varying in intensity from one condition to another, the *conditions* representing various compoundings.

Suppose A to contain B as an impurity and as an element, what will be the difference in the spectroscopic result?

A in both cases will have a spectrum of its own;

B as an impurity will add its lines according to the amount of impurity, as I have shown in previous papers.

B as an element will add its lines according to the amount of dissociation, as I have also shown.

The difference in the phenomena, therefore, will be that, with gradually increasing temperature, the spectrum of A *will fade*, if it be a compound body, as it will be increasingly dissociated, and it *will not* fade if it be a simple one.

Again, on the hypothesis that A is a compound body, that is, one compounded of at least two similar or dissimilar molecular groupings, then the longest lines at one temperature will not be the longest at another, the whole fabric of "impurity elimination," based upon the assumed single molecular grouping, falls to pieces, and the origin of the basic lines is at once evident.

This may be rendered clearer by some general considerations of another order.

General Considerations

Let us assume a series of furnaces A . . . D, of which A is the hottest.

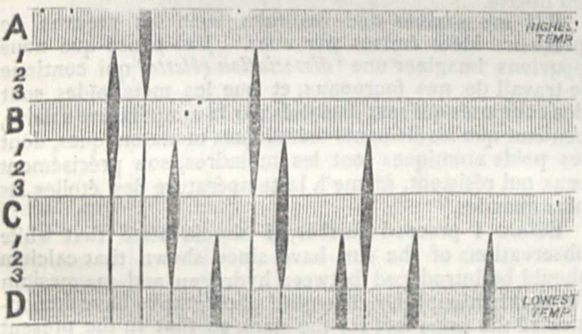

FIG. 1.[1]

Let us further assume that in A there exists a substance α by itself competent to form a compound body β by union with itself or with something else when the temperature is lowered.

Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be the only one visible in B, as the spectrum of the assumed elementary body α would be the only one visible in A.

A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

Now if into the furnace A we throw some of this doubly compounded body γ we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β, and finally α would exist alone, and the spectrum would be reduced to one of the utmost simplicity.

This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ, and δ all higher, that is, more compound forms of α, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

In short, the line of α strong in A is *basic* in B, C, and D, the lines of β strong in B are *basic* in C and D, and so on.

I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into α is far below that stage, although higher than B.

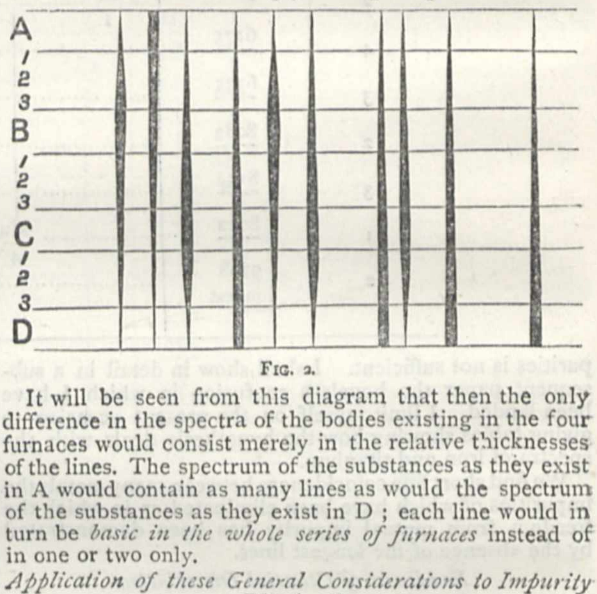

FIG. 2.

It will be seen from this diagram that then the only difference in the spectra of the bodies existing in the four furnaces would consist merely in the relative thicknesses of the lines. The spectrum of the substances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; each line would in turn be *basic in the whole series of furnaces* instead of in one or two only.

Application of these General Considerations to Impurity Elimination

Now let us suppose that in the last diagram (Fig. 2) the four furnaces represent the spectra of say, iron, broken up into different finenesses by successive stages of heat. It is first of all abundantly clear that the relative thicknesses of the iron lines observed will vary according as the temperature resembles that of A, B, C, or D. The positions in the spectra will be the same, but the intensities will vary; this is the point. *The longest lines, represented in the diagram by the thickest ones, will vary as we pass from one temperature to another.* It is on this ground that I have before stated that the whole fabric of impurity elimination must fall to pieces on such an hypothesis. Let us suppose, for instance, that manganese is a compound of the form of iron represented in furnace B, with something else; and suppose again that the photograph of iron which I compare with manganese represents the spectrum of the vapour at the temperature of the furnace D. To eliminate the impurity of iron in manganese, as I have eliminated it, we begin the search by looking for the longest and strongest lines shown in the photograph of iron, in the photograph of manganese taken under the same conditions. I do not find these lines. I

[1] The figures between the hypothetical spectra point to the gradual change as the spectrum is observed near the temperature of each of the furnaces.

say, therefore, that there is no impurity of iron in manganese, but although the longest iron lines are not there, some of the fainter basic ones are. This I hold to be the explanation of the apparent confusion in which we are landed on the supposition that the elements are elementary.

Application of these Considerations to Known Compounds

Now to apply this reasoning to the dissociation of a known compound body into its elements—

A compound body, such as a salt of calcium, has as definite a spectrum as a simple one; but while the spectrum of the metal itself consists of lines, the number and thickness of some of which increase with increased quantity, the spectrum of the compound consists in the main of channelled spaces and bands, which increase in like manner.

In short, the molecules of a simple body and a compound one are affected in the same manner by quantity in so far as their spectra are concerned; *in other words, both spectra have their long and short lines*, the lines in the spectrum of the element being represented by bands or fluted lines in the spectrum of the compound; and in each case the greatest simplicity of the spectrum depends upon the smallest quantity, and the greatest complexity (a continuous spectrum) upon the greatest.

The heat required to act upon such a compound as a salt of calcium so as to render its spectrum visible, dissociates the compound according to its volatility; the number of true metallic lines which thus appear is a measure of the quantity of the metal resulting from the dissociation, and as the metal lines increase in number, the compound bands thin out.

I have shown in previous papers how we have been led to the conclusion that binary compounds have spectra of their own, and how this idea has been established by considerations having for a basis the observations of the long and short lines.

It is absolutely similar observations and similar reasoning which I have to bring forward in discussing the compound nature of the chemical elements themselves.

In a paper communicated to the Royal Society in 1874, referring, among other matters, to the reversal of some lines in the solar spectrum, I remarked[1]:—

"It is obvious that greater attention will have to be given to the precise *character* as well as to the position of each of the Fraunhofer lines, in the thickness of which I have already observed several anomalies. I may refer more particularly at present to the two H lines 3933 and 3968 belonging to calcium, which are much thicker in all photographs of the solar spectrum [I might have added that they were by far the thickest lines in the solar spectrum] than the largest calcium line of this region (4226·3), this latter being invariably thicker than the H lines in all photographs of the calcium spectrum, and remaining, moreover, visible in the spectrum of substances containing calcium in such small quantities as not to show any traces of the H lines.

"How far this and similar variations between photographic records and the solar spectrum are due to causes incident to the photographic record itself, or to variations in the intensities of the various molecular vibrations under solar and terrestrial conditions, are questions which up to the present time I have been unable to discuss."

An Objection Discussed

I was careful at the very commencement of this paper to point out that the conclusions I have advanced are based upon the analogies furnished by those bodies which, by common consent and beyond cavil and discussion are compound bodies. Indeed, had I not been careful to urge this point the remark might have been made that the various changes in the spectra to which I shall draw attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line, due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular grouping of hydrogen as that which gives us two green lines only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green lines of hydrogen without the red one.

I submit, therefore, that the argument founded on the overnotes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures is, as I shall show, as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other.

Inquiry into the Probable Arrangement of the Basic Molecules

As the results obtained from the above considerations seemed to be so far satisfactory, inasmuch as they at once furnished an explanation of the *basic lines* actually observed, the inquiry seemed worthy of being carried to a further stage.

The next point I considered was to obtain a clear mental view of the manner in which, on the principle of evolution, various bases might now be formed, and then become basic themselves.

It did not seem unnatural that the bases should increase their complexity by a process of continual multiplication, the factor being 1, 2, or even 3, if conditions were available under which the temperature of their environment should decrease, as we imagined it to do from the furnace A down to furnace D. This would bring about a condition of molecular complexity in which the proportion of the molecular weight of a substance so produced in a combination with another substance would go on continually increasing.

Another method of increasing molecular complexity would be represented by the addition of molecules of different origins. Representing the first method by $A + A$, we could represent the second by $A + B$. A variation of the last process would consist in a still further complexity being brought about by the addition of another molecule of B, so that instead of $(A + B)_2$ merely, we should have $A + B_2$.

Of these three processes the first one seemed that which it was possible to attack under the best conditions, because the consideration of impurities was eliminated; the prior work has left no doubt upon the mind about such and such lines being due to calcium, others to iron, and so forth. That is to say, they are visible in the spectra of these substances as a rule. The inquiry took this form: Granting that these lines are special to such and such a substance, does each become basic in turn as the temperature is changed?

I therefore began the search by reviewing the evidence concerning calcium and seeing if hydrogen, iron, and lithium behaved in the same way.

(*To be continued.*)

[1] *Phil. Trans.*, vol. clxiv., part 2, p. 807.

ZÖPPRITZ ON OCEAN CURRENTS

I SEND you a translation by a friend of an important contribution to the theory of ocean currents by Prof. Zöppritz, of Giessen, which has recently appeared in the *Annalen der Hydrographie und Maritimen Meteorologie*. The mathematical part of the subject has been published in the *Annalen der Physik* for April last, a translation of which will be found in the *Philosophical Magazine* for September.

One of the main objections urged against the theory that ocean currents are due to the impulse of the winds is that the winds can, it is alleged, produce only a *surface drift*, whereas many of the currents extend to great depths. I have always maintained that this objection is totally erroneous; that if the surface of the ocean be impelled forward with a constant velocity by the wind or by any other cause whatever, the layer immediately below will be dragged along with a constant velocity somewhat less. The layer underneath this second layer will in turn be also dragged along with a velocity less than the one above it. The same will take place in regard to each successive layer, the velocity of each being somewhat less than the one immediately above it, and greater than the one below it. In this manner the surface velocity may be transmitted downwards to any depth. This conclusion has now been demonstrated by Prof. Zöppritz, in the following paper, to be perfectly correct. JAMES CROLL

Though for a long time the majority of seamen and geographers have firmly held the opinion that the great equatorial ocean currents derived their origin from the trade winds, yet, so far as I know, no attempt has yet been made to treat the physical problem of the propagation of surface-velocities downwards through a very thick stratum of water, with the means presented by the theory of the friction of fluids, as elaborated within the last thirty years. Such an attempt is all the more demanded as many authors have lately denied that surface-forces could set the sea in motion to any considerable depth. At the same time the most groundless assumptions have been set forth as to the depth of such drift-currents.

The essential principle of the theory of the internal friction of fluids is that when a plane stratum of water is moved forward, by any cause, in its own plane with a given velocity, the adjoining stratum cannot remain at rest, but, in consequence of its molecular cohesion experiences an impulse to move in the same direction. And if the velocity of the former stratum be continuous the latter assumes a velocity which tends to approximate constantly to the given velocity. This second stratum now exerts the same influence on a third adjoining stratum that it had to suffer from the first, and sets it in motion in the same direction. The third stratum draws with it in a similar manner a fourth, a fourth a fifth, and so on. The propagation of the velocity is only bounded by the limits of the fluid itself. If these limits consist of a solid plane parallel to the strata, then the propagation of the velocity will cease only at this point, *i.e.*, between the last liquid stratum and the first solid stratum.

The law according to which two neighbouring strata of velocities mutually influence one another has already been demonstrated by Newton, and the accelerating force exerted by the friction has been assumed as independent of the pressure and proportional to the difference of velocity. The later theory of the friction of fluids carries out this fundamental hypothesis as to the propagation of velocity between strata of the same medium which lie at an indefinitely small distance ξ from one another, and have accordingly only an indefinitely small difference of velocity Δ, inasmuch as it makes the acceleration produced by the friction, at the plane in which the strata meet, proportional to the quotient $\Delta : \xi$. The factor k, by which this quotient must be multiplied in order to give the acceleration, is called the *Coefficient of Internal Friction*.

The Newtonian hypothesis can be applied likewise to those parts of the bounding-surfaces of the fluid (where it is in contact with other bodies) which may possess independent motion. Here the acceleration produced by the limiting medium (which may be solid, fluid, or gaseous) is proportional to the difference of velocity which may in this case be finite. The factor of the proportion is called the *Coefficient of External Friction*. If the bounding body is a solid or even a fluid, then the fluid may wet it, that is, the stratum of fluid touching the limiting body may cling so fast to that body as to assume the same velocity. The coefficient of external friction is in this case infinitely great. This is the case between wood and water, glass and water; and, on the other hand, not so between glass and quicksilver.

The theory founded on this simple hypothesis has been subjected to the most varied experimental tests, and has, on the whole, been found to agree with the facts, so that the hypothesis may be regarded as proved.

In order to apply this theory to ocean currents, the simplifying presupposition has been made that the ocean is a mass of fluid contained between two horizontal planes at the distance h from one another, but in other respects unbounded. On the surface of this mass of fluid a wind of uniform strength and direction is acting at all times, while the under-surface wets a solid plane, the sea-bottom, and is therefore always at rest.

We must not, however, look on the action of the moving air on the surface stratum of the water as proceeding according to the Newtonian hypothesis; it will act in this way only so long as the surface remains level.

But the wind produces waves and acts on them according to quite different laws. One fact of experience is available here, viz., that the surface-stratum of the ocean under the influence of a uniform wind, moves in the direction of the wind with a constant velocity dependent on the strength of the wind. If, therefore, we place on the velocity of the water at the surface the condition that it has a value w_0 at all times given, everywhere uniform, and of uniform direction, then the problem of the determination of the internal velocity becomes soluble.

But the simplifying presuppositions here assumed are almost realised in the central equatorial regions of the great ocean; the solution of the problem becomes, therefore, of deep interest.

The following are the chief results of the solution:—

If for an infinitely long time the surface-stratum has been kept at an unchanging velocity, then the whole mass of water is in a steady state of motion, *i.e.*, a state which no longer varies according to the time. The velocity w is then dependent only on the depth x beneath the surface, and diminishes in proportion as the depth increases, till at the bottom it reaches *zero*. This relation is expressed by the formula

$$w = w_0 \frac{h - x}{h}.$$

Naturally it is presupposed that no other causes, *e.g.*, displacing currents, affect the motion of the deeper strata. If these deeper strata are kept by any foreign cause whatsoever in steady motion in a direction exactly opposite to the assumed motion, then at some point between the highest and the deepest strata there lies a plane where the velocity = 0. If this plane lies at the depth h_1, then in the mass which lies above it the velocity follows the formula

$$w = w_0 \frac{h_1 - x}{h_1},$$

and is therefore in the same condition as if the strata that lie beneath were a solid mass.

It is specially noteworthy that the velocity is independent of the coefficient of friction, *i.e.*, that in the state of motion that prevails after an infinitely long time the distribution of the velocity is the same in a thin fluid like water and in a thick fluid like syrup. In the fixed state of motion the influence of friction is shown by the participation of all the strata in the motion which is imparted from without to the surface alone. Dependence on the coefficient of friction takes place only on the consideration of motions that vary with the time, and affords a measure for the depth of penetration of a surface-impulse within a given time.

The formula which gives the velocity at the depth x of a mass of water originally at rest when for the time t the surface has been kept at a constant velocity w_o, has naturally a less simple form than the formula which was found for steady motions. (The formula is the same as that which determines the propagation of heat in a solid wall whose one side is kept at a temperature $w_o°$, whilst the other remains at 0°.) From this formula results the simple law that any velocity whatever between o and w_o prevails at different times at depths which are related to one another as the square roots of the times. I have used the formula to compute the time that a point at the depth of 100 metres requires to attain half the surface velocity, *i.e.*, $\frac{1}{2}$ w_o. The coefficient of friction of the water was assumed according to O. E. Meyer's determination, at 0·0144, in which centimetres and seconds are the units of calculation. The result was that 239 years are required for the layer of water 100 metres deep to assume the half of the surface velocity. If it be asked what length of time is required for one-tenth of the surface velocity to penetrate to that depth, the answer is 41 years. Accordingly, the same velocities will be attained at a depth of 10 metres after 2·39 and 0·41 years respectively. In a more viscous fluid the resulting numbers would be smaller.

These numbers are well calculated to give an idea of the slow rate at which changes of motion are propagated downwards. For the numbers computed for the propagation of a given surface motion, hold likewise for the penetration of a change of the motion from the surface downwards, whose influence is simply added to the already existing motion. A steady current, therefore, whose velocity diminishes linearly according to the depth, will sustain only an extremely slight alteration (except in the strata nearest the surface) from passing changes of motion that affect the surface, *e.g.* from contrary winds or storms. There will prevail, rather, at every deep-lying point of this current, a mean velocity that changes only very slightly according to the time, and which is determined by the mean velocity at the surface. This latter velocity has the direction of the prevailing wind, according to whose strength it varies by a law that cannot be more accurately settled.

If the surface velocity varies periodically according to the time, as is the case with all winds that depend on seasons and the hours of the day, then, after this periodic state has lasted an infinitely long time, the velocity at all depths is a periodic function of the time of similar period, but such that the amount of variation decreases rapidly according to the depth and that the occurrence of the maxima and minima is delayed proportionally to the depth. At a depth of 10 metres the amount of the yearly oscillation is already diminished to less than $\frac{1}{13}$th; at a depth of 100 metres it is beyond observation; at this depth the velocity is that corresponding to the steady state when the mean annual velocity is given to the surface. When the depths decrease in arithmetical proportion, the amounts of the oscillation decrease in geometrical proportion such that at four depths x_1, x_2, x_3, x_4, which stand in the relation

$$x_4 - x_3 = x_2 - x_1;$$

the amounts D_1, D_2, D_3, D_4 stand in relation

$$D_4 : D_3 = D_2 : D_1.$$

A maximum and the following minimum of the annual oscillation always exist at the same time at a vertical distance of 11·9 metres.

To give a conception of the time that a constant surface-velocity which begins at the time $t = o$ requires, in order to bring the interior of an ocean 4,000 metres deep, which was previously at rest, to the state of steady motion, the following numbers will serve:—After 10,000 years there prevails at the half-depth, *i.e.*, at $x = 2,000$ metres, just the velocity $0·037w_o$. Since, according to the already-stated formula, in the steady state the velocity $0·5w_o$ must prevail at this point, it is easily seen how far the ocean is still removed after 10,000 years from the steady state. After 100,000 years the velocity at the depth stated is already $0·461w_o$, therefore very near the definitive value. After 200,000 years it differs only by two units in the third decimal place.

Among the results we have found, particular emphasis is to be laid on two, which seem more or less to contradict the views which have prevailed up to this time. In the first place, the steady motion arising in the interior of an unlimited stratum of water from an unvarying surface velocity makes itself felt with linearly decreasing velocity down to the bottom. Hitherto the view frequently expressed was, that the influence of surface currents, *e.g.*, the drift caused in the tropical ocean by the trade winds, reached only to very moderate depths. Secondly, it was found that all variations according to time, whether periodic or aperiodic, of the forces acting on the surface, propagate themselves downwards with extraordinary slowness, the periodic in very quickly decreasing amount. Taking both statements together, it follows that the movement of the chief part of a stratum of water exposed to periodically varying surface forces is determined by the mean velocity of the surface, and that the periodic variations are observable only in a comparatively thin surface stratum. From this it is obvious that hitherto the influence of the friction was undervalued in one direction, in so far, namely, as it was believed that its influence need not be considered as penetrating so deep, but in another direction it was overvalued, as too great an influence was wont to be ascribed to friction in respect of the propagation of varying current motions Its effect was also very much overvalued in another point, viz., in respect of the action of a bank on a stream flowing along it. If, I repeat, the whole surface is kept at a constant velocity, then also in the current bounded at the side the distribution of velocity in the steady state is independent of the co-efficient of friction. Beyond that, the influence of the banks on the distribution of velocity is exceedingly slight.

A further result is that two steady currents flowing parallel to one another, but in opposite directions, in a fluid-stratum of constant depth, may very well graze one another without mutual disturbance. Their surface of division is then a vertical plane parallel to their direction in which the velocity 0 prevails, and which, therefore, stands to each current in the relation of a solid bank.

We have already shown numerically how extraordinarily slow the velocity existing at the surface is propagated downwards when the interior was previously at rest. Hence it may be concluded, *vice versâ*, that when every point of the whole mass of fluid has at a given moment a given velocity varying according to the depth, and when from the same moment onwards the surface remains at rest, the effect of this initial state vanishes equally slowly, *i.e.*, the ocean passes into the state of rest with the same slowness with which in the first case the surface-velocity was propagated into the interior. In fact the formulæ show that the times for the increase and decrease of the same fraction of the given velocity are expressed by the same number.

If from some cause or other strong currents had been generated in the ocean, say 10,000 years ago, these currents would certainly not have as yet disappeared, but would still be the chief agents in determining the movement of the ocean at great depths, supposing that the earth were completely covered by an ocean of the uniform depth of 4,000 metres.

The interruption by continents and islands of irregular form will contribute to weaken the effect of these former states of motion, not so much through the increased friction on the ocean-bed as through the reflex currents, which arise everywhere, crossing and impeding one another. But it must be observed after the above numerical proof of the extremely slow spread of local alterations of motion over the interior mass, that the difficulties of an exact computation must not be shirked, on account of the traditional expression: "Friction quickly uses up all these velocities."

It would be possible to determine by observations whether effects of former movements are still present in the ocean. There would be required for this purpose only comparative current-observations at the most varied depths, to be applied in the central parts of the great equatorial currents and of the region of calms. Yet, however, we dare not hope to be able to detect small remnants of interior motion with the same certainty with which the effect of the former high temperature of the earth, which disappears according to the same law, could be detected by subterranean observation of temperature, were one able to penetrate deep enough with the thermometer into the earth's crust.

The above computations give us also an idea how distant must be the time of the initial state. What a long time, for example, must we imagine the trade winds to have been blowing with their present extent and strength in order to be justified in assuming that the present state of motion of the equatorial currents is steady. For that about 100,000 years are needed, supposing we postulate a mean depth of 4,000 metres and do not take into account the deadening influence of continents and islands which must somewhat diminish that number. Every initial state, whatever it may have been, vanishes finally, and gives way to a steady state, only the time varies which is required to diminish the originally arising velocity to any required degree of smallness.

OUR ASTRONOMICAL COLUMN

THE MELBOURNE OBSERVATORY.—The thirteenth official Report of the Board of Visitors of the Melbourne Observatory, with the annual statement of the Government Astronomer, is before us. Mr. Ellery reports that the new building to contain the magnetical and meteorological instruments registering continuously by photography is completed. The staff of the Observatory now consists of the director, with a chief assistant (Mr. White) and three junior assistants. The transit-circle is found to be inadequate for modern requirements, and the Board of Visitors lay stress upon the necessity of providing an instrument of greater pretensions, to enable Melbourne to co-operate effectively with European and American observers; the Sydney Observatory being already in possession of a very superior meridian-instrument, and one having been ordered, it is understood, for the observatory under the direction of Mr. Todd at Adelaide, it is hoped that a new transit-circle may soon be provided for Melbourne, and it is suggested that the necessary appropriation, about 1,200*l.*, might be made in two annual votes, as two years will be required for the completion of the instrument.

The great reflector, though reported to be working satisfactorily, the mirrors retaining an excellent polish, and no marked signs of deterioration being visible, is occasionally subject to trifling derangements of its mechanism. Unfortunately the publication of the work with this instrument, the drawings of nebulæ, has been delayed by the loss of the gentleman who copied the drawings on stone. The drawings, however, now only require printing, and their publication is not likely to be long retarded. Mr. Ellery refers to the miscellaneous observations made during the year to which his report relates (to June 30, 1877), including observations of D'Arrest's comet of short period, determination of positions of stars used by Mr. Gill during his expedition to Ascension, measures of southern double stars and of the polar and equatorial diameters of Mars, and of Saturn's ring. With regard to the use of the great reflector it is mentioned, "Out of 326 available nights 150 were unfitted for observation from unfavourable weather, bright moonlight interfered on 32, while 49 were occupied with visitors, which, together with about 20 nights during which the telescope was under repair, or which were unavailable from other causes, left only 75 nights upon which observations could be made." From the observations made during the year upon 77 of the smaller nebulæ in Sir John Herschel's "General Catalogue," it is gathered that while the actual aspect of many conforms precisely with Herschel's description, others are so considerably changed as to be only recognisable by their position. The only change detected in the great nebula about η Argûs, since the drawing in March, 1875, has been "a break or separation in one of the branches on the preceding side."

Observations of the satellites of Uranus were made on sixteen nights, and on the same number of nights the satellites of Mars (the announcement of the discovery of which had been telegraphed to Mr. Ellery by Sir George Airy) were unsuccessfully sought for; the failure to find these objects with certainty and ease Mr. Ellery considers "somewhat unaccountable," but the reader will hardly need to be reminded that there are other cases where the large reflectors have not proved so adequate for work as the large refractors: sooner or later, at Melbourne or elsewhere, we hope to see a large instrument of the latter class applied to the survey of the southern heavens: the real astronomical work in the northern hemisphere, the more precise micrometrical measures and more delicate observations falling to the task of the practical astronomer, have been, as yet, pre-eminently due to the use of the refractor.

BIELA'S AND HALLEY'S COMETS.—There are near approximations between the orbits of these bodies not far from points which were first roughly indicated by Littröw, in a communication to the Vienna Academy in 1854, entitled "Bahnnähen zwischen den periodischen Gestirnen des Sonnensystemes." In heliocentric longitude 39° 25′ (equinox of 1836) the distance between the two orbits is 0·032 (the earth's mean distance from the sun = 1), and in 200° 51′, the distance is as small as 0·011. At the former point the true anomaly of Halley's comet is − 94° 9′, with the elements of 1836, and that of Biela's − 71° 17′; at 200° 51′ the true anomaly of Halley's is + 104° 59′, and of Biela's + 90° 2′; we see then that on the last return of Halley's comet to these parts of space, though its orbit approached so near to that of Biela's, there was no near approximation of the two bodies. It will be remembered that Biela's comet also passes very near to the orbit of Tempel's comet 1866 I., and consequently to the track of the November meteor-stream.

GEOGRAPHICAL NOTES

AMONG the geographical notes in the January number of the new periodical issued by the Royal Geographical Society we find some interesting information regarding the work to be done by Mr. Keith Johnston's East African Expedition. He is instructed to gather data for constructing as complete a map as possible of the route, and to make all practicable observations in meteorology, geology, natural history, and ethnology, with the view of rendering as exact as possible the information obtained regarding the region, its inhabitants, and products. As special subjects of investigation he is to observe and note the routes best adapted for future more extensive communication, and to spare no efforts in examining the range of mountains seen by Mr. E. D. Young and by Capt. Elton and his party, at the north-east end of Lake Nyassa, ascertaining their extent and elevation, and the condition of the routes or passes over them. The practicability of constructing a line of telegraph from north to south through the region is also to be inquired into. If Mr. Johnston should succeed in reaching Lake Tanganyika he is directed to pay special attention to facts bearing upon the extraordinary rise in its level in very recent times, as stated by Mr. Stanley. Besides making accurate measurements, Mr. Johnston is recommended to institute inquiries as to whether the rise may not be periodical, or the result of a succession of years of excessive rainfall; but in the event of its proving continuous he is to investigate with care the causes and results of so remarkable a phenomenon. This note is followed by a summary of the survey arrangements of the Afghanistan Expedition, which promise to add much to our knowledge of the unknown tracts of country on our north-west frontier. The information contained in the remaining notes has already been placed before our readers in our own columns. The maps in the present number are those of the Fly River, New Guinea, from Signor D'Albertis' survey, of the Sulimani Mountains, on our Afghan frontier, illustrating an article by Mr. C. R. Markham, and of the routes of the Swedish and Dutch Arctic Expeditions.

THE International African Association at Brussels have recently received intelligence that MM. Wautier and Dutrieux, with 360 porters, had left Mpwapwa on October 15 to rejoin M. Cambier. On October 27 they were at Mvumi, in Ugogo, where a letter from M. Cambier reached them, announcing his arrival at Kasisi, which is two days' march from Urambo, the capital of King Mirambo, of Unyamwesi. They are now travelling in company with M. Broyon, Mirambo's son-in-law, who is said to be taking up a large convoy to Ujiji for the English missionaries, and under his able guidance and advice it may be hoped that they will escape similar misfortunes to those which they have experienced in the past.

INTELLIGENCE has been received at St. Petersburg that Prof. Nordenskjöld's steamer *Vega* is ice-bound on the Siberian coast.

IN the last number of the *Tour du Monde*, M. Alfred Marche, the former companion of M. Savorgnan de Brazza, in his explorations of Western Africa, concludes his admirably illustrated chapters, entitled "Voyage au Gabon et sur le Fleuve Ogooué."

PROF. KIEPERT, the eminent geographer, has recently expressed his opinion regarding the alleged return of the Amu Darya (or Oxus) into its ancient bed, and consequently becoming a tributary to the Caspian instead of the Aral Sea. The Professor remarks that all statements made hitherto, even as far back as those of the old Roman writers, are simple speculations, proving nothing else but merely the existence of a dry river-bed in the direction indicated. He thinks it a matter of course that, in the event of unusual accumulation of water in the Oxus, this bed may be filled with water for many miles' distance, and, during the few centuries for which we possess reliable data this event has happened so often, that the present recurrence need not in any way have given rise to so much talk and discussion.

MR. E. F. IM THURN, of the British Guiana Museum, paid a visit, in October and November last, to the Kaieteur Fall of the Potaro River, for the purpose of testing whether it was rightly described by its discoverer, Mr. Barrington Brown, as "one of the grandest falls in the world," as well as to prove the truth of his (Mr. im Thurn's) idea that such a place ought to be a rich treasure-ground for a collecting naturalist. He professes to be disappointed with the fall because it is neither so high as the Yosemité Fall nor so broad as Niagara. But he visited it when the water was at its lowest volume, and yet, when looking at it from above, he confesses that the fall is one of "splendid and awful beauty." Altogether we infer that Mr. Brown's description is essentially correct, especially when the river is at its fullest. The country on the road to and about the fall is described as of matchless beauty, and evidently it is a splendid field for a naturalist. The fall can be reached with comparative ease in a few days from Bartica Grove.

AT the November meeting of the Russian Geographical Society, Admiral Krusenstern described the results of his journey to Siberia in 1876 to investigate the possibility of connecting the basin of the Petchora with that of the Ob, and thus open a continuous water-way from Europe to Siberia. He reports favourably on the practicability of the scheme. The scientific results of the journey are topographical surveys, levellings of the principal parts of the route, a whole series of astronomical determinations, and a large addition to our knowledge of a region still little known.

THE last number of the *Zeitschrift* of the Berlin Geographical Society contains an elaborate paper by Herr G. Hartung on the formation of valleys and lakes. There is also a valuable paper by the late Saharan explorer, Erwin von Bary, on the character of the vegetation of Aïr; besides a large map of the African river Quanga, the result of the exploration of Herr Otto Schütt. The last two numbers of the *Verhandlungen* of the same Society contain some important papers. Prof. Karsten gives some data on the problem of ocean currents, and Dr. Tietze describes the results of his exploration of the volcanic Mount Demavend, to which we referred in a previous number. Dr. Hartmann has some interesting observations on the distribution of deep-sea animals. It will thus be seen that this Society regards geography as embracing a very wide field of research, and in this respect is a model that might with advantage be followed by other geographical societies.

A COMMITTEE has been formed at Berlin with the object of founding a "Central Union for Commercial Geography and the Furtherance of German Interests in Foreign Countries." The Society hopes to enter into friendly relations with all German and foreign geographical societies.

"BOSNIEN in Bild und Wort," is the title of an interesting work by Amand von Schweiger Lerchenfeld, just published by Hartleben, of Vienna. The geographical publications of this firm are of particular excellence, and the present work is a fair case in point. It contains some twenty charming drawings from the artistic pen of J. J. Kirchner, illustrating the most interesting parts of the province which has played so prominent a part in the past year's history. The text is carefully written, clear, and to the point. Altogether the work is an acceptable addition to geographical literature.

THE MARQUESS OF TWEEDDALE, P.Z.S.

IT is with extreme regret that we have to chronicle the death, after a three days' attack of bronchitis, on the morning of December 29, of Arthur Hay, ninth Marquess of Tweeddale, F.R.S., and President of the Zoological Society of London. Born in 1824, the second son of Field-Marshal the late Lord Tweeddale, K.T., a veteran of the Peninsula and other campaigns of "the Great War," Lord Arthur Hay at an early age entered the army, as befitted the godchild of the grand English captain, and obtained a commission in the Grenadier Guards. But the ordinary guardsman's life in times of peace was inadequate to his aspirations, and reaching the rank of captain, he was soon after appointed aide-de-camp to Sir Henry (afterwards Lord) Hardinge, then Governor-General of India, and in that capacity accompanied his chief through the ever-memorable campaign of the Sutlej. After the English arms had triumphed in the conquest of the Punjab, Lord Arthur was attached to a mission, the details of which, we believe, have never been made public, to some of the tribes bordering upon our northern frontier, and in discharge of that duty reached places unvisited by any European traveller since the days of Moorcroft. Lord Arthur's services in India and the adjacent countries lasted over several years, in the course of which time his attention was attracted by their rich and little-known fauna, and he not only formed the acquaintance, but assiduously cultivated the friendship of two of the greatest Indian zoologists of the time—Jerdon and Blyth—of whom he became an apt pupil, fishes and birds being particularly the objects of his pursuit. Returning home at length he resumed his regimental duties, and on the outbreak of the Russian war, in 1854, he accompanied the expeditionary force first to Turkey and thence to the Crimea, taking part in the operations which ended in the fall of Sebastopol. Soon after the conclusion of peace he left the army, and his old zoological tastes, which had been growing slack, returned to him more strongly than ever. On the death of his eldest brother, Lord Gifford, he became heir to his father's honours and estates, and assumed the courtesy title of Lord Walden, by which, perhaps, he will be most generally recognised, for under that designation he published the greater part of his contributions to zoology, and under it he succeeded the late Sir George Clerk as President of the Zoological Society, performing the duties of that office with a singular amount of dignity and urbanity. For several years he continued to live in a cottage he had built for himself at Chislehurst, and there he began to form an ornithological library and collection on a scale almost unattempted hitherto in this country, though the collection was supposed to be limited to Indian, or at least Asiatic, specimens. On the death of his father, at a very advanced age, in 1876, Lord Walden inherited the Scottish peerage and estates, and thenceforth his home was mainly the old ancestral seat of Yester, near Haddington, where he entered, with the energy natural to his character, upon the life of an agriculturist; in this respect following the example of his father, who had long since turned his sword into a ploughshare, and had earned the reputation of being one of the most scientific farmers in that part of North Britain, which is the head-quarters of scientific farming.

The late Lord Tweeddale was a frequent and, when occasion required, a powerful writer. Most of his acknowledged communications are to be found in the *Journal* of the Asiatic Society of Bengal, the *Ibis*, and the *Proceedings* and *Transactions* of the Zoological Society, but it is believed that his anonymous contributions to the public press were still more numerous, though these were seldom on scientific topics. He married twice: first, the daughter of the late Count Kielmansegge, for many years the popular Minister of Hanover at this Court, who died in 1871, and secondly, a daughter of Mr. Mackenzie of Seaforth, who survives him.

One word must be said of Lord Tweeddale's generosity. No reasonable project for the advancement of zoology in any of its branches was ever started but he was ready to support it liberally. His loss will be deeply felt by a wide circle of his brother ornithologists, and the Zoological Society will find it very difficult to replace him in its presidency, a post which seems to require a peculiar position of scientific and social rank.

NOTES

We are happy to state that at the end of the last legislative session the French Central Bureau of Meteorology obtained from the National Exchequer a sum of 120,000 francs, required for the organisation of the services which were decreed in the month of June. A semi-monthly paper will be issued by the Bureau summarising the results of observations during that period. The work of normal schools, which had been suspended during two or three years, will be resumed and published as in former times.

The French Minister of Public Works has prepared a most important decree, which was signed on December 20 last. For the execution of the great works which have been voted by the French Parliament, an auxiliary corps of Ponts-et-Chaussées engineers has been created. The members of this newly created body will enjoy the same privileges as the government engineers who have been trained at the Polytechnic School. The consequence is that the privileges of that celebrated establishment are practically at an end, and the principle that office should be given to the fittest irrespective of their origin has a fair chance of becoming an axiom of the French administration.

The first part of a posthumous work by Prof. Poggendorf on the History of Physics has been sent us by Messrs. Williams and Norgate. It will be completed in three parts and will contain much interesting matter collected by the late eminent physicist during his long career as lecturer at the Berlin University. We have also received the first part of the "Publications of the Astrophysical Observatory of Potsdam," containing observations of sun-spots from October, 1871, to December, 1873, by Dr. Spörer.

From *Science News* we learn that Mr. Alex. Agassiz left Cambridge (U.S.) on December 1 for a second dredging-trip in the West Indies on the Coast Survey steamer *Blake*. The specimens secured by him are divided among scientific men in Europe and America, who work them up, while many of them go into his own Cambridge collection. This year he will cruise between the Windward Islands and the coast of South America, having spent last winter in the Gulf of Mexico.

The prominence given to science is a noteworthy feature in the annual summaries for the past year which appear in most of our newspapers.

We have much pleasure in drawing our readers' attention to the following circular concerning a Society for the Collection of South African Folk Lore. The circular explains itself, and we trust that those of our readers who are interested in the subject will subscribe to the periodical which it is desired to start:—"The existence, among the aboriginal nations of South Africa, of a very extensive traditionary literature, is a well-known fact. Not a few stories forming part of this literature have been written down; and as in some of them terms occur which no longer appear to be used in colloquial language, and the meanings of which are, in many instances, not fully understood, there is no doubt that we meet in them with literary productions of great antiquity, handed down to the present generation in a somewhat similar manner to that in which the Homeric poems reached the age of Pisistratus. But European civilisation is gaining ground among the natives, and within a few years the opportunities for collecting South African folk-lore will be, if not altogether lost, at least far less frequent than they are now. This would be a great loss to 'the science of man,' particularly as there is much which is exceptionally primitive in the languages and ideas of the South African aboriginal races. There are not a few missionaries and other Europeans in South Africa who have ample opportunities for collecting South African folk-lore. Some of these, however, are not aware of the importance of such collections, and those who are would be greatly encouraged in the task of making them, if a channel for their speedy publication existed. In the hope of contributing towards the collection of South African traditionary literature, a Folk-Lore Society is in course of formation at Cape Town, which already includes members in distant parts of South Africa. The publication of a small periodical every second month is also proposed by the Society. The annual subscription to this periodical will be four shillings, exclusive of postage. Folk-lore intended for publication in it should be accurately written down in the language and words of the narrator, and a translation into English, or some other well-known European language, added. Further information regarding facts illustrative of native life or native literature will also, whenever practicable, be published. Intending subscribers to the projected periodical are requested kindly to send in their names and addresses, stating the number of copies required by them, to the secretary of the South African Folk-Lore Society, care of Miss L. C. Lloyd, Cape Town."

ACCORDING to a report made by Prof. Palmieri an interesting application of the microphone to volcanic phenomena has just been made by Prof. Michele Stefano de Rossi, who during a series of experiments extending over several months and made at his seismic observatory at Rocca di Papa in the Albanese Mountains, has found that the present eruption activity of Mount Vesuvius could be perceived through the microphone even at that enormous distance. Prof. de Rossi, in order to continue his experiments, has recently stayed with Prof. Palmieri at the Vesuvius Observatory, and they have together visited the crater of the Solfatara near Pozzuoli, where the subterraneous work of the volcanic forces became so very evident to the sense of hearing, that a considerable amount of fear was caused amongst those present at the experiments. Prof. de Rossi will publish an account of his researches in his serial *Il Vulcanismo*.

EARTHQUAKES are reported from Seefeld (Tyrol) on December 14 at night, where the shock came in the direction from north to south, and from Luxemburg on December 15 at 11 A.M. where six or seven distinct oscillations were noticed.

NEWS from the American Republic of San Salvador states that the volcanoes of Santa Ana and of Izalco are in eruption. The eruption of the former had been anticipated for some time past (see NATURE, vol. xix. p. 86), and seems to be of particular violence.

"AN English Manufacturer" makes a strong appeal in yesterday's *Times* on behalf of the introduction into this country of the decimal system in weights, measures, and coinage. It is long since we showed the absurdity of our present systems, and the necessity for the introduction of something more scientific. But the "Manufacturer" shows that by our want of any international system, such as prevails among other nations, the trade of this country seriously suffers. We hope this aspect of the question will be urged upon the Government by all interested, and that a much more radical reform will be instituted than what has been attempted in the recent most unsatisfactory Weights and Measures Act.

THE director of the Vienna Geological Institution, Counsellor Franz von Hauer, has been nominated "Officier de l'Instruction publique" by the French Minister of Public Instruction.

M. W. DE FONVIELLE sends us the following details concerning the recent electrical observations at Montsouris Observatory:—Electrical observations are registered regularly at Montsouris seven times a day, according to a proper scale of variations, and with a Thomson electrometer. These observations are made by M. Descroix, under the general direction of M. Marie Davy, the director of the observatory, and the results are recorded daily in the Paris *Temps*. The series from the beginning of the month of December offers some notable peculiarities. The frost in Paris set in on December 7, and from that date to the 22nd there were fifteen days of continued cold. Only once, on the 18th, a thaw was for a few hours imminent, but the snow was not melted in the observatory grounds. During the whole of that period not less than 105 careful readings were taken and registered, but not a single one of these readings exhibited the least negative tendency. The variations were very few, and the sign + was always recorded. This high positive state of tension was observed in spite of a number of variations in the pressure of the air, which was almost always under 760 mm., and sometimes so low that the forecast published by the Bureau Central announced "approaching rainfall." The maximum was on the 19th, during a heavy cold fog; it was so large that the instrument was thrust out of balance, and the record of the number is wanting. The tension then exceeded 200 Daniell cells. From that time the scale of comparison was altered, so that the range of the instrument was enlarged. In consequence of this observation it was suggested that the real thaw, or change of weather would not set in without the previous appearance of negative tension. Instructions were given by M. Marie Davy to test this suggestion by a careful examination of the electrical circumstances attending this lengthened period of unprecedented cold and the future thaw which would put an end to it. The thaw set in in France on the night of December 25-26, at an hour varying according to the circumstances of the several localities. The electrical readings at Montsouris were found positive on the 25th during the whole of the day, but the mean value was greatly diminished, and the readings very unequal. On December 26 at six in the morning, negative readings were taken and registered for the first time since December 7. It must be added that under the circumstances the Thomson electrometer kept at Montsouris is not considered by M. Descroix as exhibiting the exact numerical value of the tension of the air, but merely its kind and the general progress of the phenomenon. This reservation has been made in a correspondence with Signor Palmieri, the director of the Mount Vesuvius Observatory, on the occasion of some strictures passed on the location of the Thomson electrometer used in these observations.

No general meeting of the Association for the Improvement of Geometrical Teaching will be held in January, 1879. Considerable progress has been made by the sub-committees appointed in January, 1878, and draft syllabuses will soon be submitted to members of the Association.

M. VALENTIN has been snowed up during more than fourteen days in the observatory on the top of the Puy-de-Dome, where he takes the meteorological readings. The telegraph connecting Puy-de-Dome with Clermont laboratories being out of order no telegram has been received from him for a lengthened period. No anxiety is felt for his safety, he having been well furnished with provisions and fuel. A similar accident has befallen General de Nansouty, the director of the Pic du Midi Observatory. His telegraphist having descended to Bagnères was unable to ascend again and the General was left to his fate. As he is rather old and of delicate health heroic efforts were made by the peasantry to reach him, which they did on December 24; the telegraphic line was repaired and telegrams recorded as usual in the *Bulletin International*. General de Nansouty refused to relinquish his post, and he is spending his winter as usual on one of the highest peaks of France.

THE *Neue Wiener Zeitung* states that an electric light has been tried on a locomotive on the Vienna Railway system. The apparatus was designed by Mr. Whitehead, the inventor of the celebrated torpedo, and is said to have worked satisfactorily.

A CORRESPONDENT writes to us that in looking through some of the drawings and prints, &c., of Old London, belonging to Mr. J. E. Gardner, F.S.A., of Park House, he came upon the following interesting handbill:—

London, 1775

Proposals
for a
Short course of lectures
on
Fossils
by
Emanuel Mendes da Costa.

The course will consist of only
TWELVE LECTURES.

A public Introductory lecture will be given *gratis* to any one who chuse to come.

To begin *on Wednesday*, 7 *June*, *at noon*, at the Author's appartments at a shoemaker's opposite Arundel Street in the Strand and the future Lecture Hours will be determined by the subscribers.

The conditions are
One Guinea the course.
To be paid on Subscribing.
Single lectures at two shillings and sixpence each.

Subscriptions are taken in at Mr. Elmsley's, Bookseller, opposite Southampton St., Strand; Mr. White, bookseller in Fleet-street; Mr. Humphreys, dealer in shells and other curiosities in St. Martin's Lane, near Charing Cross; and by the author at his said appartments.

N.B.—The Introductory lecture will be repeated on Thursday evening at six o'clock.

IT is proposed to hold an anthropological exhibition at Moscow in the coming summer, together with a general meeting of anthropologists from all parts of the world.

WE have on our table the following works:—"History of the Steam Engine," R. H. Thurston, Kegan Paul and Co.; "Études et Lectures sur l'Astronomie," Camille Flammarion, G. Villars, Paris; "Catalogue des Étoiles Doubles et Multiples," Camille Flammarion, G. Villars, Paris; "Sport and Work on the Nepaul Frontier," "Maori," Macmillan and Co.; "Mathematical Problems," J. Wolstenholme, Macmillan and Co.; "The Fairy-Land of Science," Arabella B. Buckley, E. Stanford; "Das Leben," Philipp Spiller, Gerstmann, Berlin; "Wanderings in Patagonia," Julius Beerbohm, Chatto and Windus; "Natural History of Victoria," Frederick McCoy, Trübner; "Fourth Annual Report of the Imperial Mint;" "Extra Physics and the Mystery of Creation," Hodder and Stoughton; "From Kulja Across the Tian Shan to Lob Nor," Col. N. Prejevalsky, Sampson Low and Co.; "The Heart of Africa," Dr. Georg Schweinfurth, Sampson Low and Co.; "The Philosophy of Science, Experience, and Revelation," John Coutts, F. Pitman; "The Native Flowers and Ferns of the United States," Parts 13, 14, 15, 16, Thomas Meehan, L. Prangola, Boston; "The Principles of Light and Colour," Edwin B. Babbitt, Trübner and Co.

THE additions to the Zoological Society's Gardens during the past week include a White-whiskered Paradoxure (*Paradoxurus leucomystax*) from East India, presented by Mr. W. G. Wilson; a Common Barn Owl (*Strix flammea*), British, presented by Mr. W. Davies; a Common Coot (*Fulica atra*), British, presented by Mr. F. H. O'Donoghue; two Philantomba Antelopes (*Cephalophus maxwelli*) from West Africa, two Egyptian Jerboas (*Dipus ægyptius*) from Egypt, purchased; three River Jack Vipers (*Vipera rhinoceros*) from West Africa, deposited.

CIRCULATING DECIMALS

THE properties of circulating decimals mentioned by Mr. R. Chartres and by Mr. E. P. Toy in NATURE (vol. xviii. pp. 291, 541) are particular cases of very general laws relating to the periods of circulating decimals of which, as they are not stated with any approach to completeness in any work on arithmetic with which I am acquainted, it may be worth while to give a brief explanation.

Consider the process of converting a vulgar fraction into a circulating decimal; take for example $\frac{1}{39}$. The work is—

$$
\begin{array}{rl}
39)\ 1{\cdot}00 & (\cdot\dot{0}2564\dot{1} \\
78 & \\
\hline
220 & \\
195 & \\
\hline
250 & \\
234 & \\
\hline
160 & \\
156 & \\
\hline
40 & \\
39 & \\
\hline
10 &
\end{array}
$$

which may be more concisely and better arranged thus:—

$$
\begin{array}{rl}
39)\ 1 & (\cdot\dot{0} \\
10 & 2 \\
22 & 5 \\
25 & 6 \\
16 & 4 \\
4 & \dot{1}
\end{array}
$$

10, 22, 25, 16, 4 being the remainders and the corresponding quotient figures being written at the side. From this it is clear that—

$$\tfrac{1}{39} = \cdot\dot{0}2564\dot{1},\ \tfrac{10}{39} = \cdot\dot{2}5641\dot{0},\ \tfrac{22}{39} = \cdot\dot{5}6410\dot{2},\ \tfrac{25}{39} = \dot{6}4102\dot{5},$$
$$\tfrac{16}{39} = \cdot\dot{4}1025\dot{6},\ \tfrac{4}{39} = \cdot\dot{1}0256\dot{4},$$

and the numbers 1, 10, 22, 25, 16, 4 form a cycle such that if we divide any one of them by 39 we obtain the others as remainders in this order, and all the fractions give rise to the same period, though the beginning is made in each case at a different place in the period.

The following are three other divisions arranged in the same manner:—

$$
\begin{array}{rl}
39)\ 2 & (\cdot\dot{0} \\
20 & 5 \\
5 & 1 \\
11 & 2 \\
32 & 8 \\
8 & \dot{2}
\end{array}
\qquad
\begin{array}{rl}
39)\ 38 & (\cdot\dot{9} \\
29 & 7 \\
17 & 4 \\
14 & 3 \\
23 & 5 \\
35 & \dot{8}
\end{array}
\qquad
\begin{array}{rl}
39)\ 37 & (\cdot\dot{9} \\
19 & 4 \\
34 & 8 \\
28 & 7 \\
7 & 1 \\
31 & \dot{7}
\end{array}
$$

The four divisions thus give the values of the periods of the fractions $\frac{1}{39}, \frac{2}{39}, \frac{4}{39}, \frac{5}{39}, \ldots \frac{38}{39}$, *i.e.*, of all the proper fractions in their lowest terms, having 39 as denominator. In this case, therefore, there are four distinct periods, or, say, four periods each containing six figures; one of these, viz., that to which $\frac{1}{39}$ belongs, may be called the leading period.

In general if q be any number prime to 10, and if all the proper fractions in their lowest terms having q for denominator be converted into decimals there will be f periods each contain- a digits, and a and f will be connected by the relation $af = \phi(q)$, where $\phi(q)$ denotes the number of numbers less than q and prime to it. If q be a prime, $\phi(q) = q - 1$.

It is to be observed that if we divide r and $q - r$ respectively by q the digits of the periods will in the two cases be complementary, *i.e.*, the sum of each corresponding pair will be 9. Thus in the case of 39

$$\tfrac{1}{39} = \cdot\dot{0}2564\dot{1} \qquad \tfrac{2}{39} = \cdot\dot{0}5128\dot{2}$$
$$\tfrac{38}{39} = \cdot\dot{9}7435\dot{8} \qquad \tfrac{37}{39} = \cdot\dot{9}4871\dot{7}$$

and $9 + 0 = 9$, $7 + 2 = 9$, &c. Also, the sum of each pair of corresponding remainders is q; *e.g.*, in the divisions for $\frac{1}{39}$ and $\frac{38}{39}$, $\frac{2}{39}$ and $\frac{37}{39}$, the sum of each pair of corresponding remainders is 39.

If, as in the case of 39, the remainder $q - 1$ does not belong to the leading period, the periods may be arranged in pairs, the periods in each pair being complementary to one another. If the remainder $q - 1$ does belong to the leading period, each period will contain an even number of digits, and the first half and second half of each period will be complementary. Thus, for $q = 73$ there are nine periods: $\dot{0}136\,986\dot{3}$, $\dot{0}273\,972\dot{6}$, $\dot{0}410\,958\dot{9}$, &c., and in each the two halves are complementary. If there is but one period corresponding to q, of course the remainder $q - 1$ must belong to this period, so that in this case the two halves are always complementary. Returning to the period of $\frac{1}{39}$, we see that it is such that if we multiply it by 4 we obtain the same period, only beginning with the last digit, that if we multiply it by 16 we obtain the same period beginning with the last digit but one, and so on. Thus, from knowing that the last figure of the period is $\dot{1}$, and that the last remainder is 4, we can obtain the period; for $4 \times 1 = 4$ so that the last figure but one must be 4, the last two figures must therefore be 41, multiply this by 4 we have 164, so that the previous digit must be 6, and so on. This process amounts to multiplying the 1 by 4, multiplying the 4 by 4, giving 6 and 1 over, multiplying the 6 by 4 and adding the 1, giving 25, *i.e.* 5 with 2 over, and so on, until the period is completed.

In general, in converting $\frac{1}{q}$ into a circulating decimal, if k be the last digit of the period, and r the last remainder $10r - 1 = kq$, so that the last remainder $= \frac{1}{10}(kq + 1)$ and $k = 9, 3, 7$ or 1 according as q ends in 1, 3, 7, or 9. This is, in fact, the property mentioned by Mr. Chartres and Mr. Toy; the class of relations to which it belongs, and the reason for their existence, is evident from what has been said above.

The most direct manner in which the foregoing principles can be applied to abbreviate the labour of division does not consist in multiplying the digits by the remainder from the end but from the beginning. For example, in finding the decimal equivalent to $\frac{1}{17}$ the first four digits are ·0588 and the remainder is 4; therefore $\frac{1}{17} = {}$·0588$\frac{4}{17}$, multiplying by 4, we have $\frac{4}{17} = {}$·2352$\frac{16}{17}$ whence $\frac{1}{17} = {}$·05882352$\frac{16}{17}$; we could then find the next four digits by multiplying the four digits last found by 4 and reducing the fraction $\frac{64}{17} = 3\frac{13}{17}$, so that the next multiplication would be a multiplication of the whole period already found by 13; but as in this case the remainder does not recur after eight digits (if it did recur after eight digits the remainder would be $\frac{1}{17}$ not $\frac{16}{17}$), it must consist of sixteen digits, and the next eight are the complements of the first eight, and are therefore 94117647.

The principle of the method is to continue the division till a relatively small remainder occurs and then to multiply the figures already found by this remainder, and so on continually till all the figures are obtained. This is the method that has been generally employed in finding the reciprocals of large numbers when the whole period was required. There are several points to be attended to in order that the process may be simplified as much as possible, but these I pass over. The greatest saving of labour afforded by the principle is when a 5 or 2 occurs as remainder early in the division, as then we obtain all the remaining digits as fast as the hand can write them by division by 2 or 5 in the respective cases, without the occurrence of any fractions. Thus, for example,

$$\tfrac{1}{61} = \cdot\dot{0}163934426229508196721311475409836065573770491803278688524\dot{5}9;$$

if we perform the division till we come to the quotient digit 5 we then have a 5 remainder, and all the other digits are obtained by halving the figures from the commencement, viz., 1639. . . . The quotient can also be completed rapidly by division whenever a remainder occurs that is a submultiple of one that has previously occurred. Thus in the case of $\frac{1}{61}$, the remainder after the first 6 is 24 and after the first 8 is 12, so that the figures that follow the 8, viz., 1967 . . ., are obtained at once by halving those that follow the 6, viz., 3934. . . .

In the *Messenger of Mathematics* for April, 1878, I published the following note:—

"Write down a 5, divide it by 2 giving 2 with 1 over, divide 12 by 2 giving 6, divide 6 by 2 giving 3, divide 3 by 2 giving 1 with 1 over, divide 11 by 2 giving 5 with 1 over, divide 15 by 2 giving 7 with 1 over, and so on till the figures repeat. We thus obtain the figures 52631578947368421, and these with a cipher prefixed are the period of $\frac{1}{19}$, viz.—

$$\tfrac{1}{19} = \cdot\dot{0}5263157894736842\dot{1}.$$

"If we start with 50 and halve in the same manner, prefixing two ciphers, we obtain the period of $\frac{1}{199}$, viz.—

$$\tfrac{1}{199} = \cdot\dot{0}0502512562814070351758793969849246231155778894\\472361809045226130653266331658291457286432160804020\dot{1}.$$

"Similarly, if we start with 500 and halve as before, we obtain, after prefixing three ciphers,—

$$\tfrac{1}{1999} = \cdot\dot{0}0050025012506253126563281640820410205 10\ldots,$$

and, generally, the process gives the reciprocal of 1 followed by any number of 9's.

"If we start with 20, 200, 2000, &c., and divide continually by 5 instead of by 2, prefixing one, two, three, &c., ciphers, we obtain the periods of the reciprocals of 49, 499, 4999, . . . For example,—

$$\tfrac{1}{49} = \cdot\dot{0}2040816326530612244897959183673469387755\dot{1}$$
$$\tfrac{1}{499} = \cdot\dot{0}02004008016032064128256513026052104208 4\ldots.$$

"The process is very expeditious, the figures of the periods being obtained as fast as the hand can write them."

The results stated in this note were obtained as follows: the object was to find the divisors for which the first remainder was 5, so that the halving should begin from the first significant figure; these numbers are seen at once to be 19, 199, 1999 . . . Similarly the first remainder is 2 for the divisors 49, 499, 4999 . . . It should be mentioned that these are particular cases of Mr. Suffield's method of synthetic division.

If q be prime and there be only one period corresponding to q (as is the case for $q = 7, 17, 19, 23, 29, 47, 49, 59, 61, 97$, &c.), the $q - 1$ fractions $\frac{1}{q}, \frac{2}{q}, \ldots \frac{q-1}{q}$ have all the same period, viz., the $q - 1$ digits that form the period of $\frac{1}{q}$ are such, that if we multiply them by $2, 3, 4, 5 \ldots q - 1$, we always reproduce these same digits in the same cyclical order, but beginning at a different place. The case of the period of 7, viz., 142857, which is such that, multiplying it by 2, we have 285714, by 3 we have 428571, &c., is well known, and is often given as a puzzle; but the general result is a very remarkable one, *e.g.*, it is remarkable that it should be possible to write down 96 digits, such that their first 96 multiples should consist of the same digits in the same cyclical order. In the foregoing remarks I have confined myself entirely to the statement of the principles connected with the results referred to in NATURE, and to those which arise directly from them.

[J. W. L. GLAISHER

SCIENTIFIC SERIALS

American Journal of Science and Arts, December, 1878.—In the opening paper Gen. Warren considers that the Minnesota Valley and the Mississippi Valley above the Ohio have been, as a rule, formed since the deposition of the glacial drift, which exists in the banks of the river, and that the Winnipeg basin drained out southward along it; also, that the loess deposits extending up to the neighbourhood of Savannah are later than the last glacial drift, &c. The hypothesis of southern elevation and northern depression (probably reversed sometimes and repeated) is relied on to explain the effects.—Prof. Dana, continuing his valuable paper on some points in lithology, contends for basing distinction in *kinds* of rocks on difference in chemical and mineral constitution as regards chief constituents, and offers a classification in eight divisions.—The principle that when the entropy of any isolated material system has reached a maximum the system is in a state of equilibrium, is developed by Mr. Gibbs as a foundation for the general theory of thermodynamic equilibrium.—Mr. McGee distinguishes crania of the mound-builders of the Mississippi Valley from those of modern Indians by a greater development of the posterior molars.—An interesting paper by the Rev. C. Hovey, on discoveries in western caves, describes, *inter alia*, the remarkable acoustic properties of Echo River passage-way (in the Mammoth Cave), where a strong vocal impulse is prolonged with sustained vigour for fifteen seconds or more; also a locality discovered last April in the Wyandot Cave, in which "pits, miry banks, huge rocks, are overhung by galleries of creamy stalactites, vermicular tubes intertwined, frozen cataracts, and all, in short, that nature could do in her wildest and most fantastic mood." There is a row of musical stalactites, very broad and thin, on which a chord can be struck, or a melody played by a skilful hand.—Prof. Harrington analyses the Chinese official almanac, issued annually in December, and consisting of two parts, an astronomical and an astrological.

The Journal of the Russian Chemical and Physical Societies of St. Petersburg (vol. x. No. 8) contains the following papers:—On the chlorides of benzol, by Th. Beilstein and A. Kourbatoff.—On the preparation of glycol, by S. Stempnevsky.—On allyldipropylcarbinol, by P. and A. Saytzeff.—On pseudopropylacetylene, by F. Flavitzky and P. Kriloff.—Remarks by F. Flavitzky on M. Eltekoff's paper on the action of water upon the chlorides of ethylenes and similar compounds in the presence of oxide of lead.—Observations on nitrophenols, by M. Goldstein.—On the nature and the derivates of cholesterine, by M. Valitzky.—On the neutral products of the oxidation of cholesterine, by P. Latschinoff.—On the polarisation of electrolytes, by R. Colly.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December.—"Note on the Influence exercised by Light on Organic Infusions," by John Tyndall, D.C.L., F.R.S., Professor of Natural Philosophy in the Royal Institution.

Early last June I took with me to the Alps fifty small hermetically sealed flasks containing infusion of cucumber, and fifty containing turnip infusion. Before sealing they had been boiled for five minutes in the laboratory of the Royal Institution. They were carefully packed in sawdust, but when unpacked the fragile sealed ends of about twenty of them were found broken off. Some of these injured flasks were empty, while others still retained their liquids. The eighty unbroken flasks were found pellucid, and they continued so throughout the summer. All the broken ones, on the other hand, which had retained their liquids, were turbid with organisms.

Shaking up the sawdust, which I knew must contain a considerable quantity of germinal matter, I snipped off the ends of a number of flasks in the air above the sawdust. Exposed to a temperature of 70° or 80° F., the contents of all these flasks became turbid in two or three days.

The experiment was repeated; and after the contaminated air had entered them, I exposed the flasks to strong sunshine for a whole summer's day; one batch, indeed, was thus exposed for several successive days. Placed in a room with a temperature of from 70° to 80° F., they all, without exception, became turbid with organisms.

Another batch of flasks, after having their sealed ends broken off, was infected by the water of a cascade derived from the melting of the mountain snows. They were afterwards exposed to a day's strong sunshine, and subsequently removed to the warm room. In three days they were thickly charged with organisms.

On the same day a number of flasks had their ends snipped off in the open air beside the cascade. They remained for weeks transparent, and doubtless continue so to the present hour.

I do not wish to offer these results as antagonistic to those so clearly described by Dr. Arthur Downes and Mr. Thomas Blunt, in the *Proceedings* of the Royal Society for December 6, 1877. Their observations are so definite that it is hardly possible to doubt their accuracy. But they noticed anomalies which it is desirable to clear up. On July 10, for example, they found 9 hours' exposure to daylight, 3½ hours of which only were hours of sunshine, sufficient to effect sterilisation; while, on July 29, "a very hot day, with much sunshine," 11 hours' exposure "9 of which were true insolation," failed to produce the same effect. Such irregularities, coupled with the results above recorded, will, I trust, induce them to repeat their experiments, with the view of determining the true limits of the important action which those experiments reveal.

Chemical Society, December 19.—Dr. Gladstone, president, in the chair.—The following papers were read:—Researches on the action of the copper zinc couple on organic bodies, part ix.—Preparation of zinc methyl, by Dr. Gladstone and Mr. Tribe. (During the reading of this paper Dr. Frankland took the chair.) Methyl iodide in contact with the copper zinc couple is converted at the ordinary temperature, in from three to thirty days, into a crystalline mass of zinc methiodide. By distillation zinc methyl is obtained; the yield in one case was 99·27.—Dr. Debus made some remarks on the formula of glyoxylic acid. The author considers the formula of this acid to be $C_2H_2O_3$, in opposition to Perkin, who from quantitative experiments came to the conclusion that the true formula was $C_2H_4O_4$.—Mr. Wills gave a short communication on the production of oxides of nitrogen by the electric arc in air. The author finds that nitric acid was formed in four experiments equivalent to ·54, ·55, ·6, and ·7 gramme per hour, and points out the importance of this observation with reference to the proposed use of the electric light in dwellings.—On the action of alkaline hypobromite on oxamide, urea, and potassium ferrocyanide, part ii., by W. Foster.—On two new hydrocarbons obtained by the action of sodium on turpentine hydrochloride, by Dr. Letts. The principal point in this paper is the fact that the author has obtained a solid hydrocarbon having the formula $C_{10}H_{17}$, which he designates solid turpenyl.—On the formation of baric periodate, by S. Sugiura and C. F. Cross.—On erbium and yttrium, by T. S. Humpidge and W. Burney. The authors wished to determine the specific heats of these metals, but failed to obtain them in coherent masses. They determined the atomic weight of pure erbium to be 171·61.

Meteorological Society, December 18.—Mr. C. Greaves, president, in the chair.—P. Doyle, F.S.S., J. M. Gray, Lord Hampton, G.C.B., M. Jackson, A. Proctor, G. Simpson, and E. C. Tisdall were elected Fellows of the Society.—The following papers were read:—Abstract of the meteorology of the Bombay Presidency, by C. Chambers, F.R.S., communicated by Sir G. B. Airy, K.C.B., F.R.S., Astronomer-Royal.—Experiments with Lowne's anemometer, by Capt. William Watson, F.M.S.—Meteorology of Bangkok, Siam, by J. Campbell, Staff Surgeon, R.N.—Results of meteorological observations taken at Calvinia, South Africa, by Kaufmann I. Marks, F.M.S.

Royal Microscopical Society, December 11.—Dr. C. J. Hudson, vice-president, in the chair.—Mr. John Harrison and Dr. Alabone were elected Fellows of the Society.—Dr. Hudson described a new species of Rotifer, *Oecistes sphagni*, coloured drawings of which were exhibited. He also exhibited a number of beautiful transparent diagrams of rare species of Infusoria which he described seriatim.—Mr. F. H. Ward read a paper on a new microspectroscope without a slit, and described this and other accessory apparatus to the instrument.—Mr. F. Crisp read a paper on Hoffmann's new camera lucida, in which he described this and other recent forms of the apparatus, figures of some being drawn upon the board. Another form of camera lucida, by Dr. Russell, of Lancaster, was described and figured by Dr. Millar, and a description of a new one by Swift was also given by Mr. Ingpen.—Mr. C. Stewart read a short communication from Mr. A. D. Michael announcing the discovery of the male of *Cheyletus venustissimus*. Attention was called to a new glycerine immersion lens received from America, by Mr. Ingpen.—Mr. Beck, in reference to a suggestion for a universal unit of microscopical measurement, gave his decision in favour of divisions of the millimetre, and presented to the Society a micrometer ruled with this, and also in $\frac{1}{1000}$ inch for ready comparison.

Geological Society, December 18, 1878.—Henry Clifton Sorby, F.R.S., president, in the chair.—Rev. Frederick Charles Lambert, Robert Plant, and Ernest Swain were elected Fellows of the Society.—The following communications were read:—On remains of *Mastodon* and other vertebrata of the miocene beds of the Maltese Islands, by Prof. A. Leith Adams, F.R.S. The author recognised the following Maltese formations:—Upper Limestone.—Maximum thickness over 250 feet, passing into a sandy rock, and that into a hard red limestone. Fossiliferous, containing four Brachiopoda, several Lamellibranchs and Gasteropods, and twenty-five Echinodermata (ten being peculiar). Sand Bed.—Maximum thickness about 60 feet, variable in character, characterised by vast abundance of *Heterostegina depressa*; fifteen vertebrata. The Marl Bed.—Maximum thickness over 100 feet, but sometimes almost wholly thinned out. Organic remains rarer than in the sand bed. The Calcareous Sandstone.—Maximum thickness rather over 200 feet. Contains bands of nodules, of which the second is rich in organic remains. Hence come the noted teeth of Squalidæ. Among its invertebrate fauna are many Pecteus, with other Lamellibranchs, Gasteropods, and Brachiopods. Also twenty-two species of Echinodermata. The Lower Limestone.—Maximum thickness over 400 feet. *Scutella subrotunda* and *Orbitoides despansus* are abundant in the upper part, and it is generally fossiliferous. In a nodule-seam in the calcareous sandstone in the Island of Gozo two rather imperfect teeth of a *Mastodon* have been found. Both are penultimate molars. They agree most nearly with the teeth of *Mastodon angustidens*, but the characters are not sufficiently well preserved to differentiate the species with certainty. The same formation has furnished teeth of a *Phoca*, to which the specific name *rugosidens* has been given by Prof. Owen. Large teeth referable to the Phocidæ are found in the nodule seams of the calcareous sandstone and in the sand bed; the marl bed has also furnished a portion of a jaw. The Woodwardian Museum contains a part of a jaw of *Squalodon*, evidently from a nodule-seam of the calcareous sandstone (found by Scilla circ. 1670). The sand bed and calcareous sandstone have furnished remains of more than one species of *Delphinus*, and large-sized Cetacean vertebræ are found in nearly all the beds, especially the sand bed. *Halitherium* has been obtained from the sand bed, marl bed, calcareous sandstone, lower limestone, and (?) upper limestone. One specimen of *Ichthyosaurus gaudensis*, Hulke, has been furnished by the calcareous sandstone; the same has also furnished *Melitosaurus champsoides*, *Crocodilus gaudensis*, and *Sterrodus melitensis*. *Myliobates toliapicus* and allied species have come from all the deposits except the upper limestone. *Otobates subconversus* from the sand bed and marl. The squalidæ are abundant from all the deposits except the first. There are ten species belonging to the following genera:—*Carcharodon*, *Carcharias*, *Oxyrhina*, *Hemipristis*, *Corax*, *Odontaspis*, *Lamna*. Remains of *Notidanus*, *Platax*, and *Diodon* have also been found.—Dinosauria of the Cambridge greensand, Parts I.-VII., by Prof. H. G. Seeley, F.L.S. F.G.S. The author stated that this paper was founded upon the collection of more than 500 dinosaurian bones preserved in the Woodwardian Museum, for the opportunity of studying which he was indebted to the kindness of Prof. T. McKenny Hughes. He described the conditions under which the specimens occur, and accounted for the apparently worn state of the bones as the results of exposure to the air, and subsequent maceration.—I. Note on the axis of a dinosaur from the Cambridge greensand.—II. On the vertebral characters of *Acanthopholis horridus*, Huxley, from the base of the chalk-marl near Folkestone.—III. On the skeleton of *Anoplosaurus curtonotus*, Seeley.—IV. On the axial skeleton of *Eucercosaurus tanyspondylus*, Seeley.—V. On the skeleton of *Syngonosaurus macrocercus*, Seeley.—VI. On the dorsal and caudal vertebræ of *Acanthopholis stereocercus*, Seeley.—VII. On a small series of caudal vertebræ of a dinosaur, *Acanthopholus eucercus*, Seeley.

CAMBRIDGE

Philosophical Society, November 18.—Prof. Liveing, president, in the chair.—The following communication was made to the Society:—Some results of the two last total solar eclipses, by Dr. A. Schuster. Every scientific investigation passes through a preliminary stage, in which a general survey of the facts is taken, and by means of which the most hopeful line for future inquiry is determined. Eclipse observations may be said to have just passed through that preliminary stage. The present is therefore a fitting time for a general survey of what has been done, and a discussion of what remains to be done. Eclipse observations may be divided into three classes: spectroscopic observations, polariscopic observations, and general observations on the outline and shape of the corona, which can best be carried on by means of good photographs. 1. Spectroscopic observations.—The spectrum of the corona consists: of a continuous spectrum, in which the dark Fraunhofer lines are faintly seen; of the spectrum of hydrogen gas, and of an unknown line in the green. The pressure of a continuous spectrum indicates the presence of solid or liquid particles, and is most likely partly due to matter falling into the sun. During the last eclipse the first systematic attempt to determine the height to which the continuous spectrum extends was made by Prof. Eastman, assisted by Mr. Pritchett. The result was rather remarkable, for although the corona was not equal in intensity in the four directions, the spectrum disappeared nearly at the same distance all round the sun. The importance of obtaining photographs of the spectrum was pointed out. The various attempts that have been made were mentioned, and the result of the Siamese photographs was compared with that of a photograph of the spectrum obtained by Dr. Henry Draper during the late eclipse. The comparison proves that during the late eclipse, the line spectrum was much fainter. All observers agree on this fact, and Prof. Young's opinion, which is decisive on that point, was quoted. The idea of connecting this fact with the minimum of sun-spots through which we are at present passing is obvious. 2. Polariscopic observations.—Polariscopic observations tend to show that close to the sun the polarisation is small, that it increases up to a distance of a few minutes, and then rapidly diminishes. The author has made a calculation as to what the polarisation ought to be, and has come to the result that in whatever way the scattering matter is distributed, as long as it vanishes nowhere, the polarisation ought rapidly to increase with the distance from the sun. The only way to account for the discrepancy between this result and the actual fact is by assuming that as we move away from the sun, more light is reflected in the ordinary way and less light is scattered. Matter falling into the sun and being gradually broken up by the heat would account for all the facts. 3. General outline of the corona.—It has often been remarked that the corona shows an approximate symmetry round the sun's axis. The author supports the view that the greater extension in the direction of the sun's equator is due to meteor streams which approximately circulate in that plane. He quotes in support of this a fact noticed by him during several eclipses, which indicates that a certain departure from this symmetry takes place in such a way that the corona is wider and more extended on one side of the axis than on the other, and he gives evidence that this departure from symmetry takes place in a direction fixed in space. The statement made by several observers that there is a connection between sun-spots and the sun's corona has induced the author to look carefully over photographs and drawings of the corona made during the last eight eclipses. He has found that during this time the general outline has varied gradually and systematically in a cycle corresponding to that of the sun-spots. The following hypothesis, which seems to account for many facts, was brought forward by the author. A meteor stream is circulating round the sun in a very eccentric orbit. A number of meteors in their perihelion passage are falling into the sun, owing to the increased chances of collision amongst themselves, disintegration owing to rise of temperature and entry into the solar temperature. The local increase of temperature caused by the fall must give rise to currents on the surface of the sun, and may give rise to cyclones which we call sun-spots. If the meteors have a period, so that every eleven years an increased quantity passes the perihelion, a greater number of sunspots would form, and at the same time we should observe a difference in the shape of the corona, which may well be of such a nature as is actually observed. Dr. Schuster also exhibited to the Society Grant's small calculating machine, for the multiplication of eight figures by eight; he explained its construction, and compared it with that of Thomas of Colmar, which is in general use. Grant's machine is much smaller than Thomas's, but does not perform subtraction directly, as is the case with the latter.

BOSTON, U.S.A.

American Academy of Arts and Sciences, December 11, 1878.—Hon. Charles Francis Adams in the chair.—Prof. Alexander Graham Bell presented a paper upon the use of the telephone in tracing equi-potential lines and surfaces. The results of previous observers, especially those of Prof. Adams, were referred to, and Prof. Bell showed that these lines could be traced more readily with a telephone than with a galvanometer. He made use of a steel-band telephone, which could be clasped about the head, leaving the hands free to perform the experiments. In this way the lines were traced in solids and liquids. By the use of metal exploring rods the equi-potential lines could be traced in the earth about one's feet, or in the neighbourhood of metallic deposits, and might lead to the discovery of metallic deposits or peculiarities in the homogenity of the earth.—Prof. John Trowbridge read a paper upon the results of measurements conducted by himself and Prof. W. H. Hill, of the United States Torpedo Station, at Newport, R.I., upon the heat produced by the rapid magnetisation of iron, nickel, and cobalt. The nickel and cobalt contained from $\frac{2}{10}$ to $\frac{6}{10}$ of 1 per cent. of iron, which was inappreciable in the electro-dynamic experiments. The work done was measured in metre grammes, and gave the result that the molecular heating of equal volumes of iron, nickel, and cobalt can be expressed in metre grammes as follows:—Iron = 2381·43, cobalt = 1906·50, nickel = 1112·11.

PARIS

Academy of Sciences, December 16, 1878.—M. Fizeau in the chair.—The following papers were read:—Observations on M. Pasteur's note on alcoholic fermentation, by M. Berthelot. He describes an arrangement he made for effecting simultaneous hydrogenation and oxygenation of sugar (by electrical means); he notes that there was a slight production of alcohol.—Study of ordinary and compound engines, &c. (continued), by M. Ledieu.—Report on Mr. Wharton's marine compass, with needle of nickel. Its trial in the navy is recommended.—On the reptiles of primary times, by M. Gaudry. This relates to permian fossils found at Autun. In the vertebræ of Actinodon the parts of the centrum, already in great part formed, but not united, indicate the passage of the imperfect vertebrate to the perfect. M. Gaudry refers to two new reptiles, *Pleurononia pellati* and *Euchyrosaurus rochei;* the latter's name indicates the fact of its having been more adroit with its fore-limbs than reptiles of the present.—Reply to M. Sire's observations on a gyroscopic apparatus, by M. Gruey.—On a new phenomenon of static electricity, by M. Duter. He repeated his experiment, with vessels of the same volume, but with different thicknesses of glass. The variations of volume were nearly in inverse ratio of the squares of the thicknesses.—Artificial production of nepheline and amphigene, by the method of igneous fusion and reheating at a temperature near fusion, by MM. Fouqué and Levy. —Third note on vaccinal infection; elaborative *rôle* of the lymphatic ganglions, by M. Raynaud.—The memoir by Sadi Carnot, "Reflexions sur la Puissance motrice du Feu," published in 1824, and regarded as the origination of the new science of thermodynamics, had very little publicity. His brother, M. H. Carnot, has issued a new edition, with notes (hitherto unpublished), which show that S. Carnot foresaw, with much distinctness, the consequences that would result from his ideas. A copy of the work, with the MS., was presented to the Academy.—M. Mouchez presented drawings of heavenly bodies, by M. Trouvelot (United States).—On the solar spots and protuberances observed with the equatorial of the Roman College, by P. Ferrari. Little more than two tables relating to the second half of 1877.—On the summation of series, by M. André.—On elimination, by M. Mansion.—On the different properties of the mode of distribution of an electric charge on the surface of an ellipsoidal conductor, by M. Boussinesq.—On the spectrometric measurement of high temperatures, by M. Crova. Take, as term of comparison, the flame of a moderator lamp, and let it be 1,000 on the (arbitrary) optical scale of temperature. Then measure with the spectrophotometer the ratio of the intensities of two radiations λ and λ' in the source of unknown temperature and in the lamp-flame. The quotient of these two ratios will be above or below 1,000 according as the temperature of the source in question is above or below that of the lamp-flame. M. Crova gives several examples of his measurements, and thinks the method applicable to measuring the temperature of the sun and stars; also of various industrial hearths.—Specific heat and heat of fusion of palladium, by M. Violle.—Influence of temperature on rotatory magnetic power, by M. Joubert. This relates chiefly to flint (regarding which there has been some discrepancy). M. Joubert finds that the rotatory power increases with rise of temperature, and about $\frac{1}{10}$th of its value, in passing from the ordinary temperature to that of fusion. His methods are described. He succeeded also in measuring the rotation in a body under the sole influence of terrestrial magnetism alone.—On the densities and the coefficients of dilatation of liquid chloride of methyl, by MM. Vincent and Delachanal.—On the oxidation of some aromatic derivatives, by M. Etard.—On the nature of certain accessory crystallised products, in industrial treatment of petroleums of Pennsylvania, by MM. Prunier and David. These rank parallel (mostly) with those extracted from coal oils or derivatives by pyrogenation from benzine.—Researches on urea, by M. Picard.—On hæmocyanine, a new substance from the blood of the poulp, by M. Fredericq. This contains copper, and seems to play a similar *rôle* in respiration to the hæmoglobine in vertebrates.—Influence of different colours of the spectrum on development of animals, by M. Yung. The experiments were on eggs of frog, trout, and Lymnea. Violet is the most favourable light, next comes blue, then yellow and white; red and green seem hurtful. Darkness does not prevent development, but retards it.

December 23, 1878.—Explosion of fuze materials, by M. Dupuy de Lome. This relates to a recent accident to M. Zédé when experimenting with a mixture of gun-cotton and nitrate of ammonia. The mode of combustion suddenly changed under a very slight increase in the tension of the gas.—Formation of leaves and order of appearance of their first vessels in Gramineæ, by M. Trecul.—Craniology of the Papuan race, by M. de Quatrefages. A *résumé* of the seventh volume of his and M. Hamy's work, "Crania Ethnica."—Experiments on the movements of liquid molecules of current waves, considered in their mode of action on the progress of ships, by M. de Caligny.—Mr. Norman Lockyer communicated his paper recently read to the Royal Society.—M. Damon was elected free member, in place of the late M. Belgrand.—On a process for measuring with precision the variations of level of a liquid surface, by M. le Chatelier. A point immersed in the liquid is raised gradually till its extremity is tangent to the surface. The moment at which this is passed is indicated by deformations of the liquid surface, and these deformations are observed by means of light thrown on the surface, reflected, and observed with a lens, the focal plane of which passes through the end of the point. So long as the point is under water one sees a circle uniformly illuminated, but immediately the point emerges a black spot appears in the circle. The method gives very delicate measurements, and one application designed is a very sensitive manometer for detecting weak currents of air (as in mines). —On the determination of the imaginary roots of algebraic equations, by M. Farkas.—On the theory of perturbations of comets, by M. Mathieu.—Results of solar observations during the third quarter of 1878, by M. Tacchini. The calm was increased. Of 100 days of observation, 90 were without spots. He thinks the minimum will probably be passed in 1879. In the zones of maximum frequence of the protuberances there is a minimum of the faculæ, and *vice versâ*. There is a difference in distribution of the protuberances at the epochs of maximum and minimum of spots. There were no metallic eruptions or elementary spectra, &c.—On a new thermograph, and on a general method of integration of any numerical function, by MM. Pictet and Cellerier. Knowing the tension of a vapour, one may determine *à priori* the corresponding temperature.—Magnetic rotation of the plane of polarisation of light under the earth's influence, by M. Becquerel. M. Joubert's experiment was a repetition of M. Becquerel's.—On a new phenomenon of static electricity, by M. Duter. He repeated M. Govi's experiment with mercury (which had left doubts), and got contraction as in other cases.—On four singular epochs of the annual course of meteorological elements, by M. Ragona.—Preparation of cobaltocyanide of potassium and some derivatives, by M. Descamps.—Action of trimethylamine on sulphide of carbon, by M. Bleunard. He describes some of the properties of sulphocarbonate of trimethylamine, and its combinations with the mineral acids.—On the chromatic function in the poulp, by M. Fredericq. The changes of colour in the animal's skin are analogous to those produced by the vaso-motors in the human face; they express various emotions, especially anger or fear. The deep coloured phase represents the state of activity of the muscles of the chromatophores; the phase of decoloration, the passive state of retraction of these bodies.—On the excretory apparatus of *Solenophorus megalocephalus*, by M. Poirier. Previous accounts he finds erroneous.—New researches on suspension of the phenomena of life in the embryo of the hen, by M. Dareste. A continuation of his former experiments, but with use of different temperatures. The results were conformable to what he expected.—On the tertiary strata of Britanny.

CONTENTS