THURSDAY, MARCH 9, 1871

THE TEMPLE MEMORIAL AT RUGBY

WE hear with peculiar satisfaction that one of the memorials of the Bishop of Exeter at Rugby is to be an Observatory. It is a very appropriate memorial to a man who, above most others, recognised the true relations of science and literature, and did so much to give science its rightful place. And it is a valuable addition to the resources of a school; at present perhaps it will not find its full use; but when it does it will be well associated with the name of one who has foreseen the future position of our great public schools.

Our great public schools fulfil two functions, they prepare for the universities, but they are themselves the universities for the great majority of the boys who go to them; those who go into business, into the army, and to many other occupations, do not in general go to Oxford or Cambridge. Their school education ends at eighteen. Now this large class is scarcely sufficiently contemplated at schools. For them it is necessary that a school should offer, not the first part of an education which requires many years to complete it, but the best education that can be given where this limit of age is imposed. For such boys as these it is highly desirable that their school course should have more elevation about it, and more of practical application. It would take us far from our present subject if we were fully to develop our meaning; but it seems a step, and an important one, in the right direction, to establish an astronomical and meteorological observatory at one of the public schools; for this is to assert that such knowledge of these subjects as is attainable ought to be within reach of boys who will not have the opportunity of studying them after complete mathematical training at the universities. It is one step further towards establishing the ideal education, the co-existence of religious influence, literature, science, art, and handicraft in the same institution.

Some amount of astronomical teaching is necessary in a school; it is very much neglected at present, because it does not pay in examinations. Yet we know by experience that few subjects are so interesting as astronomy. And in schools, as at Rugby, where geometry and mechanics are taught, the noblest illustrations may be taken from the mechanism of the heavens. But astronomy cannot be taught by a book only. The most useful, indispensable instrument to an astronomical teacher is an orrery. No descriptions, no diagrams, give boys a conception of the solar system so clearly as a few minutes with an orrery. The next most useful instrument is a telescope. And certainly Rugby seems to be fortunate in the instrument that is to be given to it.

The Temple Telescope is a noble one. The object-glass, of 8¼in., was made with especial care and pains by Alvan Clarke for Mr. Dawes. It is 8¼ inches aperture, and 108⅝ focal length. It is mounted equatorially, has an excellent driving clock, of an unique kind; and the eye-pieces range from 92 to 1,000.

An instrument like this is, of course, a luxury, but its beauty is, in itself, a great inducement to its use. There are few things so wondrously beautiful as the moon, or Jupiter, or a star cluster, seen with a low power in such a telescope. And any one who has tried to show the moon to several people with an ordinary telescope will appreciate the advantage of having it equatorially mounted, and of its being provided with a driving clock.

Other instruments for astronomy, surveying, and meteorology, will be added to the observatory; some of the masters having provided a fund adequate to give so fine an instrument all the surroundings that are required to make it practically available. And ere long we hope to see a really useful observatory established.

It is to be under the joint management of Mr. Wilson, who munificently gives the telescope, and Mr. Seabroke, an old Rugbeian, who is already favourably known as a worker. He still has his spurs to win, however, and will soon be very favourably situated for winning them.

The telescope is now the property of the Rev. H. E. Lowe, of Atherstone, but will become the property of Mr. Wilson in March. We greatly regret to hear that circumstances which have lately happened at Rugby affecting the tenure of masterships will prevent for the present the gift of the observatory to the school, and can only hope that it will not be long before the present difficulties are overcome. Meantime it will be established on private ground, and will be accessible to the school. Verily there be head-masters and head-masters, and masters and masters!

L.

THE EXPERIMENTAL AND NATURAL SCIENCES IN TRINITY COLLEGE, DUBLIN

THE condition of the Experimental and Natural Sciences in the various Universities is at the present time a subject of such general interest that we give the following sketch of what is now done for them in Trinity College, Dublin.

Until a student passes the Michaelmas term examination of his second (Senior Freshman) year he is supposed to confine his attention to classics, mathematics, and logic; but in his third and fourth years he must devote himself, to a certain extent, to the study of Experimental Physics, including heat, electricity, magnetism, and chemistry, and pass examinations on these subjects even at the ordinary term examinations. In the fourth year of his studies the student can go in for honours in Natural and Experimental Sciences, the course for which includes Jamin's "Cours de Physique," Lloyd's "Wave Theory of Light," Naquet's "Principles of Chemistry," Cotta's "Classification of Rocks," and Haughton's "Manual of Geology."

At the conclusion of his collegiate studies the student can graduate in either Experimental or in Natural Science, and the Court of Examiners can recommend the most distinguished of the candidates to the Board for gold and silver medals; candidates thus recommended receiving their B.A. degrees as Senior and Junior Moderators respectively.

The subjects for examination for the Moderatorships in Experimental Science are (1) Experimental Physics viz., Heat, Light, Sound, Electricity, and Magnetism; (2) Chemistry, Inorganic and Organic; (3) Mineralogy, including Crystallography; and the hundred marks allotted to the examination are as follows:—Light and

Sound, 20; Heat, Electricity, and Magnetism, 30; Inorganic and Organic Chemistry, 15 each; Mineralogy and Crystallography, 10 each. It need only be added that lectures on all these subjects are delivered every term by the respective Professors.

The subjects of examination for the Moderatorship in Natural Science are the following, each of which has equal weight:—(1) Physiological and Comparative Anatomy; (2) Zoology and Botany; (3) Geology (including Physical Geography and Palæontology).

It may be objected that the distinction between Zoology and Comparative Anatomy will not hold good, and the books ordered to be read under both sections seem to demonstrate this. But there can be little doubt that the establishment of this Moderatorship is a step in the right direction, and the course is such that every medical student ought to take it up and do his utmost to attain a place among the golds.

It is of course not to be denied that there are no scholarships, no studentships nor fellowships to be attained by a knowledge of these sciences, but perhaps even these may in time come. It is not so very long ago when a student could aspire to but few distinctions if he were not a first-rate mathematician; now this is completely altered, and as the world rolls on changes come with it.

The chemical and physical laboratories of the College leave nothing to be desired. The distinguished Medical Registrar takes care that there shall be every facility given to students to work out the Comparative Anatomy of the Vertebrates, and places at the disposal of the College the animals that from time to time die in the Zoological Gardens. The Professor of Zoology demonstrates the Anatomy of the Invertebrates to his class during two out of the three terms. The Botanical Gardens and the Herbarium are as extensive as any University can require, and there are two courses, one of forty and another of twenty lectures, delivered each year in Botany, besides garden demonstrations.

In conclusion we venture to suggest that if the M.D. degree should only be taken by the reading and publishing of a Thesis, as in some of the German Universities, it would help materially to assist the cause of the Sciences in Trinity College, Dublin; for, though some of the candidates might select practical subjects, others, doubtless, would turn their attention to the wide fields of Zoology and Botany. W.

SIR JOHN LUBBOCK ON THE ORIGIN OF CIVILISATION

The Origin of Civilisation and the Primitive Condition of Man: Mental and Social Condition of Savages. By Sir John Lubbock, Bart., M.P., F.R.S., &c. (Longmans, 1870.)

NOW that Sir John Lubbock's work on the "Origin of Civilisation" has reached a second edition, it is perhaps only natural that those who make it their business to warn the public against the encroachments of Science should raise an alarm against the first. In a recent number of the *Christian Advocate and Review* appears, accordingly, an article specially devoted to the demolition of Sir John's theories, and the vindication of human degeneracy. With the felicitous instinct of clerical antagonism, the Advocate and Reviewer makes his fiercest onslaught precisely where his opponent happens to be least vulnerable, and lays about him with all the fine, fervid imbecility distinctive of his particular clique. Such an attack, however, were ignorance its only characteristic, would hardly call for remark. We notice it, not for its absurdity, but because, in combining with its absurdity a certain unctuous disingenuousness, it is really a typical example of a kind of criticism unhappily influential, if obscure, and widely accepted, if not popular. It would perhaps be too much to expect that reviewers of this class should read through the books they review, but at least they have no right to misquote what they do read. On p. 256 (first edition), Sir John Lubbock, speaking of errors into which, in the absence of education, not even Christianity prevented mankind from falling, writes thus: "We know that a belief in witchcraft was all but universal until recently even in our own country. This dark superstition has indeed flourished for centuries in Christian countries, and has only been expelled at length by the light of science." He then proceeds to observe: "The immense service which science has *thus* rendered to the cause of religion and humanity, has not hitherto received the recognition which it deserved." His reviewer, omitting any reference to witchcraft, quotes Sir John as asserting that "the immense service which Science has rendered to the cause of religion and humanity, has not hitherto received the recognition it deserves"—a proposition which may or may not be accurate, but is certainly not the one laid down by Sir John Lubbock. But he is not content with merely misrepresenting the book under review. Sir John Lubbock, he correctly remarks, at Liverpool, "frankly avowed 'there was no opposition between science and religion,' an admission," he adds, "of no slight importance by so great an authority in the scientific world, *as it is such a quiet rebuff to the boast of Bishop Colenso, that the differences between these two are such as to render it hopeless to attempt their reconciliation.*" If, however, we are bewildered at the brisk audacity which could venture on such a statement without even a hint at its wholly fabulous character, what are we to say to a critic who gravely asserts that "the Drift age had not been invented at the time" when Sir Charles Lyell wrote his "Geological Evidence of the Antiquity of Man"? and who assigns to the "prehistoric period in Sweden a minimum antiquity of 20,000, or it may be of 20,000,000 years."

But enough of the *Christian Advocate.* Turn we now to other and nobler opponents. The conclusions maintained by Sir J. Lubbock in this work are, in his own words—

"That existing savages are not the descendants of civilised ancestors.

"That the primitive condition of man was one of utter barbarism.

"That from this condition, several races have independently raised themselves."

On the other hand, we have the opinion of the late Archbishop Whately, that "We have no reason to suppose that any community ever did or ever can emerge, unassisted by external helps, from a state of utter barbarism into anything that can be called civilisation;" and that of the Duke of Argyll, who holds that the primitive condition of man was one of civilisation; that "there is no necessary connection between a state of mere childhood in respect to knowledge and a state of utter barbarism," and that man "even in his most civilised condition, is capable of degradation; that his knowledge may decay, and that his religion may be lost."

That the general propositions laid down by Archbishop Whately and the Duke of Argyll contain a certain limited amount of substantial truth, will probably be admitted by the staunchest adherents of the opposite theory. That "external helps" of some kind or other have played a most important part in the case of all civilisations the history of which is accessible, is as little open to question as the fact that under certain conditions civilisation among certain races may be arrested or may even retrograde. At the very threshold, however, of any discussion in terms less general, we are met by the question "What is civilisation?" The baffling complexity, indeed, of the idea conveyed in the word "civilisation" is the fountain-head of most of the confusion which exists among writers on the subject. That development is the vital principle, so to speak, of civilisation is universally admitted, but there would probably be a very general disagreement of opinion as to the particular kinds and directions of development which constitute the essential elements of civilisation. As generally understood, civilisation appears to involve a development more or less advanced of commerce and the means of communication, of natural advantages, products, and wealth, of navigation and warfare, of the arts, mechanical and ornamental; of science, theoretical and practical; of legislation and the administration of the law; of customs and language; of morals and religion; of all the faculties of the individual and the race. It includes also a consideration of the diffusion of personal liberty, and of the proportion of those who participate in the general welfare and possess the necessary appliances both for physical comfort and intellectual culture.

This, of course, is an inadequate definition of civilisation; and it is further manifest not only that development in many of the directions indicated is not absolutely necessary to civilisation, but that no civilisation on record has been equally developed in every direction. What is still wanting is some standard by which to measure civilisation in any particular case. Mr. Wallace, following Montaigne, appears to consider civilisation compatible with a very low development in nearly every direction. Archbishop Whately would consider as civilised the Germans described by Tacitus. The Duke of Argyll goes further still, for he seems to consider that Adam and Eve when expelled from Paradise were, nevertheless, distinctly civilised beings. The diversity of opinion is, indeed, owing to the absence of a recognised standard, almost universal. Civilisation is nearly always measured by the recorded achievements of men of genius. Yet, if this were the true test, no nation of modern Europe is so highly civilised as was Greece in the age of Pericles, and English civilisation has been retrograding from the days of Elizabeth, nay, from those which gave us the Canterbury Tales and Lincoln Minster, if not from those of Anselm and the Norman Bastard. Another fruitful cause of error is the natural but illogical assumption of the superiority of our modern Western European civilisation over all other civilisation. That in certain respects, principally material, it is actually superior, we do not of course deny, but when we contemplate the condition of our criminal, our pauper, our agricultural population, the troglodytes of the city, and the nomads of the country, it is difficult to avoid the conclusion that other civilisations, less advanced in certain respects, were more advanced in others, perhaps in some cases to an extent which turns the balance in their favour. On the whole, therefore, we apprehend that the relative civilisation of any country must be estimated rather by the sum of its general development than by its development, however advanced, in any particular direction. And this leads us naturally to a standard which, wherever it can be applied, is an infallible indication of the general civilisation of any race. General civilisation involves the multiplication of ideas, and the multiplication of ideas involves the multiplication of the symbols which express them. The language, therefore, or more strictly speaking, the vocabulary of any race, becomes a crucial test of its development. The total disappearance of numberless languages and our necessarily limited acquaintance with those which survive, obviously diminish, not only the number of cases in which this test can be applied, but the certainty of its application in particular instances. In spite of these drawbacks, however, language still supplies material to the student of comparative civilisation, not less invaluable than the material supplied by geology to the student of comparative anatomy. The imperfection of the record in both cases is extreme, but in both cases, so far as it extends, it is authentic and decisive.

We now return to the controversy between Sir J. Lubbock and his opponents. Independent of the cogent arguments adduced by Sir J. Lubbock against the conclusions arrived at by Archbishop Whately, there is one which seems to have been altogether overlooked. The Archbishop's theory traces the history of mankind up to a single primeval pair, and assumes the impossibility of their survival after their expulsion from the Garden of Eden unassisted by some supernatural revelation. Some supernatural revelation of the same kind he also holds necessary in order to raise any race to that stage of culture at which it is enabled to make progress of itself. The perfectly gratuitous character of this hypothesis seems to us its sufficient refutation. Surely it is sufficient to believe that causes analogous to those which, in later ages, gave to the world the exceptional intellects of an Aristotle or a Newton, possessed potency enough at an earlier epoch to account for the appearance of men endowed with genius to make the successive inventions recorded, without resorting to a superfluous hypothesis of supernatural intervention.

Even the Duke of Argyll virtually abandons Whately's position, although, perhaps, his own is even less logically tenable. When he tells us that there is no necessary connection between a state of mere childhood in respect to knowledge and a state of utter barbarism, we are forcibly reminded of Mr. Phœbus's eulogy on the aristocracy of this country, whose strongest points he declares to be that they live in the open air and speak only one language. It is manifest that the Duke of Argyll when he penned this passage had in his mind's eye the ideal "noble savage," who has figured so picturesquely in works of historic fiction from the days of Anacharsis downwards—a being who, although represented as in "a state of mere childhood in respect to knowledge," meets the greatest of civilised monarchs as his inferior, and convicts of folly the acutest of civilised philosophers. Unhappily this charming ideal person eludes the search of authentic travellers. They tell us of savages whose presence and bearing stamp them as gentlemen, they record many keen and pregnant sayings of barbaric wisdom ; but the possession of the capacity for civilisation thus manifested is a very different thing from the possession of civilisation. Manly courtesy, strong commons ense, many of the moral virtues, are as compatible with a state of barbarism, as the absence of all these qualities is compatible with a state of highly-advanced civilisation. On the other hand, civilisation necessarily implies a familiarity with certain ideas to which "a state of mere childhood in respect to knowledge" is equally of necessity an utter stranger. In fact, both Archbishop Whately and the Duke of Argyll seem to have been the victims of a wholly imaginary necessity. They appear to have forgotten the syllogism implied in the old rhyme,

> When Adam delved and Eve span,
> Who was then the gentleman?

and have felt themselves under an obligation of crediting our first parents with a degree of civilisation utterly at variance with any accepted record of their condition. As Sir J. Lubbock observes, "Adam is represented to us in Genesis not only as naked and subsequently as clothed with leaves, but as unable to resist the most trivial temptation, and as entertaining very gross and anthropomorphic conceptions of the Deity. In fact, in all three characteristics—in his mode of life, in his moral condition, and in his intellectual conceptions—Adam was a typical savage" (p. 409, note). It may be added, too, that Adam's naming the beasts and birds is by no means incompatible with his otherwise barbaric condition. "It is remarkable," says Sir J. Lubbock, "that, supporting such a view, the Duke should regard himself as a champion of orthodoxy."

With regard to the question of degradation, however, the Duke has a slightly stronger case, though he has hardly made the most of it. That decline as well as progress in civilisation does really go on in the world is a historic fact beyond dispute. Egypt and Assyria, Greece and Rome, Mexico and Peru, groan with the monuments of ruined civilisation, and all history bears witness to periods of stagnation and decadence following on periods of progress and development. Nor is evidence wanting of an analogous sequence of events among the lower races. Degeneration is known in some instances to have taken place as the result of crossing ill-matched breeds ; in others as the result of conquest, when the conquering tribe is in any respect less civilised than the conquered ; in others, owing to the oppression of other tribes ; in others, by the expulsion of a tribe into less favoured territories ; in others, by a change in the external conditions of life,—in short, the whole of our present knowledge and experience tends to show that in every stage of civilisation from the lowest to the highest, development may be and frequently is succeeded by decline of greater or less duration and degree. So far we quite agree with the Duke of Argyll ; and although Sir J. Lubbock admits the fact of occasional degradation, he hardly seems to us to recognise its real frequency and extent. In fact, however, it is only less universal than progress. For just as the present population of the world represents the difference between all preceding births and deaths, so the existing civilisations of the world represent the difference between all foregoing developments and declensions in civilisation. In other words, the present civilisation of the world bears witness to a vast mortality among previous civilisations, much in the same way as the present population bears witness to a vast mortality among previous populations. This consideration, however, so far from being favourable to the gloomy views of the Duke of Argyll, tells, on the whole, manifestly in favour of Sir J. Lubbock's more hopeful theory, for the tendency of civilisation like that of population, is always to increase and multiply. In both cases certain conditions may and do counteract the tendency in certain times and places, but the tendency remains the same, and sooner or later always predominates on the whole. It thus happens that all the great civilisations of the world have been in some material respect in advance of any which preceded them, and at the same time have manifested development in a greater number of directions. Thus the civilisation of Greece is more complex and more advanced than that of Egypt, that of Rome than that of Greece, that of Elizabethan England than that of Rome, that of modern England than that of Elizabethan England. It is to be observed, however, that every civilisation has some special and distinctive glory of its own unsurpassed by any of the subsequent ones. Grecian art, for instance, of certain kinds has never since been equalled, but the student of the Roman rule and law will certainly not be disposed to rank Grecian civilisation as a whole so high as the Latin. In fact, loss of some kind accompanies every gain of civilisation. One savage possesses the eye of the vulture, a second the scent of the deer-hound, a third the fleetness of the stag—civilise these men, and you destroy their special characteristics of excellence. Nor is this true only of physical qualities. Civilised man knows nothing of that barbaric power of perception and memory which enables the savage to detect at once the loss of one sheep out of three hundred, though he cannot even calculate the number of his fingers. Such losses, however, are in the long run more than compensated by gains in other directions, just as the losses incurred by the decay of one civilisation are eventually more than compensated by the benefits conferred by another. We fully agree, therefore, with Sir J. Lubbock in his remark at the conclusion of his answer to the Duke, "that the past history of man, has, on the whole, been one of progress, and that in looking forward to the future, we are justified in doing so with confidence and with hope."

But we have hitherto said nothing about Sir J. Lubbock's book itself. When we remember that it is one of the first attempts to treat the Origin of Civilisation on a rational and philosophic basis, we are not disposed to quarrel greatly with its somewhat lax arrangement. Its necessarily miscellaneous character lends it no small part of its value, and renders it exceedingly readable, but a more rigorous method and proportion are required to render it easy of complete digestion. In his laudable anxiety, too, to collect and co-ordinate facts as the only trustworthy foundation of his hypothesis, Sir J. Lubbock himself has a provoking way of latitating for a whole chapter together behind a heap of quotations, just when we want him to tell us their precise significance in that particular connection. This, indeed, and a very general absence of dates, deprives many of his facts of some portion at least of their intrinsic value. Occasionally, too, we come across a statement which we want verified, as, for instance, at p. 283, where he tells us "it is said that among the Ancient Britons money was habitually lent on what may strictly be called 'post-obits,' promises, to pay in another world." We own that we should like to see the authority for the prevalence of so singular a commercial transaction among our grandfathers. So far as we know, the Bonzes of Japan, and not the early Britons are the real culprits. A letter from a Jesuit father in Japan, dated March 1565, printed in Maffei's collection (B. iv. 2), tells how certain Bonzes were in the habit of borrowing money to be repaid with interest in another life, and giving their creditors I. O. U.'s (syngraphas) for *post mortem* presentation.

After all, however, Sir J. Lubbock's work is the completest summary of barbaric life that we possess. It does not profess to be exhaustive. It is designed rather as a breaking of the ground for further research in a direction precisely opposite to the "high *priori* road," on which theorists about the origin of civilisation have walked so long towards nowhere, discoursing prettily about the family being the first of human institutions, language being the perfect instrument of primeval thought, and so forth. In this respect, as well as in being a handbook of facts nowhere else collected together, it is undoubtedly a most useful contribution to contemporary literature. Its main value, however, consists in what we have only lately found a word for,—its "suggestiveness." Very few, even of the "cultured classes," at all realise the profound and abject barbarism of primeval antiquity. We can only guess darkly at the life of those wild ancestors of our race who fashioned and wielded the flint tools of the drift while yet the Thames was tributary to the Rhine,—how they skurried to their caves or burrows from the wolf, the bear, and the tiger, kept watch on the rhinoceros snorting in the shallows, or trembled as the mammoth herds crashed through the jungle. What were the relations among them of man to man, of man to woman? Was the one generally either the slayer or the victim,—the other either the temporary slave of an animal lust or the material of a fireless feast? Had they language beyond the scream of terror or pain, the shriek of triumph, the chattering of menace, the muttered mumbling of gratified gluttony? Who can tell? We know only that between the lowest savages of to-day and their earliest ancestors lies an interval of years far beyond the limit of historic chronology; and carefully weighing the facts of the case, we find it on the whole one degree less inconceivable and incredible that they should have risen to their present level of utter barbarism from one still lower, than that they should be the degraded progeny of any known or unknown civilisation.

SEBASTIAN EVANS

OUR BOOK SHELF

A Sketch of the Life and Writings of Robert Knox, the Anatomist. By his pupil and colleague, Henry Lonsdale. (London: Macmillan and Co., 1870).

DR. KNOX was in many ways a remarkable man, and if his life had been written with greater clearness of statement and less redundance of language, it might have been made both interesting and instructive. But those who have read Dr. Lonsdale's life of Professor Goodsir will not be disappointed by the present volume.

Robert Knox was born in 1791, and his misfortunes began by an attack of small-pox which destroyed his left eye. He was educated at the High School of Edinburgh, and became a student of medicine in the University when nineteen years old. Five years later he was appointed assistant-surgeon in the army, and spent more than three years on duty in the Cape Colony, where he seems to have made his first studies in ethnology and natural history. He next studied at Paris, and after his return to Edinburgh in 1822, became curator of the Museum of the College of Surgeons. In 1825 he joined Dr. Barclay in his extra-academical lectures on anatomy, and at once took the highest position in Edinburgh as a lecturer. He had many distinguished pupils—Goodsir, Reid, Edward Forbes, Owen, and Falconer were among them—and he appears to have been as popular with the students as he was disliked by most of his colleagues. In the winter of 1828-9 the terrible discovery was made of the system of murder carried on by Burke and Hare; and one of their victims was traced to Knox's dissecting room. This exposed him to much opprobrium, and even to personal danger from the Edinburgh mob; but his reputation was fully cleared from any suspicion of complicity in these crimes by the report of a committee of inquiry on the subject, which Dr. Lonsdale prints at length, together with a moderate and manly letter written by Knox himself to the *Caledonian Mercury*. That this affair was not the real cause of his leaving Edinburgh is admitted by his biographer, and amid the chronological confusion of the whole book the reader is left to guess the causes which reduced a class of five hundred in 1832 to nothing in 1842. Knox had been a candidate for the University Chair of Pathology in 1837, and for that of Physiology four years later. He failed each time, and the letters in which he submitted his claims give abundant reason for the enmities with which he surrounded himself. The latter of these productions, full of personal abuse of eminent men, some of whom are still living, was discreditable to Knox at the time, and its publication in this volume is still more so. We are told that "regardless of both legal and moral obligations, he commenced lecturing on anatomy in Edinburgh in 1842, but got no class." He tried lecturing at Glasgow, failed again, and after various wanderings settled in London. Here he maintained himself as a popular lecturer and a literary hack. Among his other occupations during the last melancholy twenty years of his life, he was pathologist to the Cancer Hospital, he contributed to the Proceedings of the Anthropological Society, and he practised midwifery. Hard working to the last, he died in London at the age of seventy-one.

The moral of this life seems to be a very trite one; Knox was his own enemy. In spite of a biographer's adulation, we are told that "on matters of business involving a *bonâ fide* principle, Knox was prone to be evasive, whilst on matters of fact he was not always considered trustworthy."* Yet he must have had redeeming qualities, and made warm friends as well as bitter enemies. As an anatomist, he belonged to the older British school, and possessed many of its merits. He did good work on the *Cetacea* and *Sirenia*, and appears to have been the first to maintain the true nature of the ciliary muscle, as well as to describe the *fovea centralis* in the retina of reptiles, and the tracheal pouch of the emeu. He was also a leader in the study of ethnology as

* This statement is introduced by the remark that "a portrait of the anatomist, without its shadows and sinuous lines, would be no portrait at all." No doubt there were many such portraits of Knox on the blank walls of Edinburgh, when he was abused as the accomplice of Burke and Hare; but Dr. Lonsdale has done wisely in giving his readers the characteristic sketch of the anatomist lecturing, which was taken by his brilliant pupil, Edward Forbes. This sketch is the best thing in the book.

a branch of anatomy. But his great distinction was as a lecturer, and here his success was for a time unequalled. Like other teachers of the time, he had the advantage of introducing a great deal of comparative anatomy, physiology, and histology (so far as it was then known) into his lectures on human anatomy. He must have been gifted with great natural eloquence, and disdained none of the tricks of oratory. His biographer gives a wonderful account of the clothes he wore when lecturing, in what seems to have passed for "full dress" in Edinburgh about 1830, but also informs us that "the richest costume would hardly have availed had not the silvery tongue been brought into play." Knox never drew diagrams, and equally rejected the help of ready-made drawings for his class, nor did he ever use notes. Above all, he thoroughly enjoyed his work. He left little written that is likely to preserve his fame. His translation of Milne-Edwards' "Zoology," by which he is perhaps most widely known at present, is a bad specimen of his inferior work. The history of his life, besides its interest as the record of an able man's remarkable success and as remarkable failure, is of importance from the light it throws on the study of anatomy in this country, on Scottish university politics, and on methods of scientific teaching; so that the intention of the present work is more praiseworthy than its execution. P. S.

Strange Dwellings: being a Description of the Habitations of Animals. By the Rev. J. G. Wood, M.A., F.L.S. (Longmans & Co., 1871.)

MR. WOOD'S works are well and favourably known as presenting the study of Natural History in its most attractive form to the young. The present work is not new, being abridged from his "Homes without Hands," which obtained, some years ago, a deservedly large amount of popular favour; and even in its original form it consisted of course mainly of selections from accounts given by travellers and naturalists. It is, however, extremely well suited to place in the hands of any boy or girl who is already fond of reading about strange animals, or whom it is desired to interest in the study of nature. We find in it accounts of the curious trap-door spider of Jamaica, the bower-bird of Australia, with its remarkable edifice of twigs and shells, the mud wasps of Guiana, the repulsive-looking aard vark of South Africa, the weaver bird and tailor bird, and many other animals of singular habits, and illustrated with woodcuts, which combine with the lively style of the text to make the volume a very attractive one.

The Duke of Edinburgh in Ceylon: a book of Elephant and Elk Sport. By John Capper, *Times* correspondent. Illustrated with chromo-lithographs. (London: Provost & Co., 1871.)

THIS book is sufficiently described by its title, being a record of the visit of the Duke of Edinburgh to Ceylon last year, and of his success in the colonial sports of elephant hunting and elk hunting. It appeals to two sections of the public, those who eagerly seize upon every incident connected with the mode of life of any member of our Royal family, and those who are equally eager after any description from life of sport in those countries where wild beasts worthy of a hunter's rifle abound. We may quote the following as an instance of the perils encountered by our Prince in navigating the Cingalese rivers. "The stream was teeming with life. Fish of all varieties and sizes sprang into the boats as they paddled along, one of them finding its way into the Prince's coat pocket" (loyal fish!); "on all sides could be heard the snapping of alligators' jaws as tiny fish were caught in the monsters' mouths. The party had proceeded about a mile down the stream, when one of them, leaning down and resting his head on the gunwale of the boat, was startled from his quiet rest by the apparition of an alligator's gaping jaws, which made a direct snap at his head, fortunately missing it, but seizing, in place of it, the barrel of the rifle held in the hands of the Prince's English attendant, who was seated next to him, and which the monster nearly wrenched out of his hand, splashing the water about, and drenching every one in the canoe." Is the *Times* correspondent quite certain that alligators are found in Ceylon?

LETTERS TO THE EDITOR

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

Lunar Halo seen from Two Stations

I HAVE presented to the French Institute at its last sitting a copy of NATURE, January 26, with the sketches of a halo seen from Liverpool by two different observers from two stations at a little distance. It is the first time, as far as my knowledge goes, that we have had two different sketches of the same phenomenon. The difference is very considerable indeed, as a *paraselene* was seen by one of the observers, and not by the other. It would be very important to ascertain what was the exact situation of each of them, and I should be glad if you can take the trouble to ascertain it by an inquiry through your paper. When I return to England I will ascertain the circumstances of the observation, which may lead to some definite conclusions on the form and altitude of floating snow during the night of the 4th January.

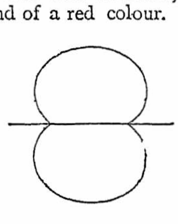
In my note to the French Institute, I say that, unquestionably, reflexion or refraction took place on several faces of crystalline snow, and each observer saw the refraction or reflexion on a particular face of crystal, placed conveniently for observation. So that double observation is very valuable, as affording a direct proof of the correctness of the explanations given by Mariotte, and others after him, of these magnificent appearances.

According to Mariotte's theory, the presence of a paraselene in one of the observations with the halo of 22° shows the presence of prismatic crystals, the faces of which must be hexagonal. The second appearance should, according to my opinion, show that these crystals were terminated by small pyramids, and the other observer perceived the halo reflected on the oblique face of these crystals. As he saw two or three halos almost concentrical, it must be supposed that one of them was due to the upper pyramid and the other to the lower.

Very probably the air was not disturbed by any wind, and elongated crystals were falling very slowly, or rather floating, owing to the smallness of their dimensions, which possibly may have been a small fraction of $\frac{1}{100}$ inch.

I send you these suggestions only to direct further inquiries, and I make no pretension to exhaust the subject, which is very interesting.

I myself observed on Thursday, February 12, what is not quite unworthy to be noticed, as showing how inexhaustible is the field opened by Nature to her inquirers. The sun was setting when I arrived at St. Pierre le Calais with my friend Alexandre Lille. I noticed the disc was distorted, the two partial suns being almost alike, and of a red colour. This appearance was



certainly owing to the air not being of a uniform density. Two different streams were separated by a horizontal surface. The truth of this supposition was very easily ascertained, as two different kinds of clouds were flying in two different directions. Neither of them was heavy, and the distinction was very admirably made by the sun itself, which soon disappeared, leaving a rosy tint behind him. The lower clouds first presented a rosy colour, but soon became dark, and the upper clouds in their turn took the beautiful colour which the others below were just losing. The horizontability of the surface separating the two streams of air was a proof of great quietness in the atmosphere, and the night was magnificent, as well as the following day.

W. DE FONVIELLE

Quinary Music

MR. JEVONS, in his interesting paper on the Limits of Numerical Discrimination (NATURE, Feb. 9), asserts in support of his views (Rees Cyc. "Rhythm") that "no musicians have yet been found capable of performing" quinary music.

I have never found the slightest difficulty, nor can I conceive any, either in performing or inventing music subdivided into five isochronous measures, *i.e.*, with an accent recurring on every fifth unit of measure; nor do I see anything to justify his doubt that the ear can grasp divisions of 6, 8, 9, without regrouping them into smaller periods; as amongst skilled musicians there exists no doubt whatever about the continuous reading of those larger groups, without even a suggestion of such subdivision; and it would be easy to multiply quotations from the best writers of passages whose only correct performance *and reception by the listeners* would be entirely destroyed by such a sub-grouping as Mr. Jevons seems to think necessary.

Apropos :—I greatly doubt whether the question of musical time is at all pertinent to the subject of numerical discrimination, as an instantaneous conception; the latter being a synchronous mental act, while the former is altogether consecutive in its operation, in which every group, however small, is only a sequence of units. JOSEPH MULLEN

38, Synge Street, Dublin, Feb. 14

The Power of Numerical Discrimination

IN an article with this title in a recent number of NATURE, Mr. W. S. Jevons offered the results of some ingenious experiments he had been making to determine how many objects the human mind could count by an instantaneous and apparently single act of attention. He comes to the conclusion that the power of his mind was limited to something less than five.

If it were Prof. Jevons' purpose to ascertain the number of objects he could count within an interval too short for more than a single conscious act of attention, his experiments were, doubtless, conclusive, at least to him; but if he sought through them to prove that he or any other person could fix his attention upon more than one object at a time, I fancy he commits the mistake attributed to the Royal Society in puzzling over the question put them by Charles I. about the effect of throwing a shrimp into a pail full of water. He is trying to account for a phenomenon that never occurred and which can never occur.

Prof. Dugald Stewart, in his work on the "Philosophy of the Human Mind"* has proved very clearly, it seems to me, that the attention is never fixed upon two points or objects at the same time, but that it passes from one to another in certain cases, as in playing upon musical instruments, in feats of jugglery, &c., so rapidly as to seem to be instantaneous.

In addition to what Prof. Stewart has said upon this subject, permit me to ask how it is any more possible for the attention to be fixed on two beans at one time than for two beans to be in the same place at the same time? The argument that could demonstrate the absurdity of the last of these propositions would demonstrate the absurdity of the first.

If Mr. Jevons will try to look at both eyes at once of the first person he talks with, he will find that one of the eyes seems more distinct to him than the other, and every effort he may make to equilibriate his attention will only result in changing it from one to the other.

If he is talking with great earnestness, or in a way to make his interlocutor very anxious to divine his meaning and penetrate or anticipate the expression of his inner thoughts, he will notice that his interlocutor's eyes seem to be running from one of his own eyes to the other, as if in hopes of getting from one some disclosures not made by the other. This would not be done if both eyes could be seen simultaneously.

Mr. Jevons seemed to see five beans, because he was able to run over and count five in the minimum of conscious time.

I think there is no authority for saying that there is any period of conscious time necessary for any purely mental operation. To the mind itself, or the spirit of a man, there is neither space nor time. There are incidents of our material organisation which limit our capacity to notice and remember mental operations, but not the operations themselves. Therefore the rapidity with which the attention is transferred from one word to another in reading, or from one key to another of a piano when played by a master, authorises no presumption whatever that his attention is ever fixed upon more than one key at a time, while all the presumptions are against the possibility of any person's attention ever being in two places at the same moment. J. B.

Berlin, March 4

* Works of Dugald Stewart. Edited by Sir William Hamilton. Art. "Attention."

Eozoön Canadense

ON a careful consideration of Dr. Dawson's reply to the objections urged by me against the supposed organic nature of Eozoön Canadense, I confess my inability to see that one single fact is brought forward calculated to shake the position of those who regard it as a purely mineral production.

In opposition to all previously received opinion, Dr. Dawson would now confine Eozoön to the Laurentian period. I am glad to accept this as evidence that its Canadian discoverers begin to feel the force of the "difficulties" I have stated, and instead of ignoring them, make a genuine and *direct* attempt to meet them.

Though Dr. Hunt now doubts the accuracy of the observations which refer the Skye ophite to the Lias, he has elsewhere as good as admitted that it is not confined to the Laurentian period.* Gümbel has also determined it for Cambrian metamorphic rocks in the Fichtelgebirge, Bavaria, and Rupert Jones and Sandford for rocks of the Lower Silurian period in Connemara, as pointed out by Mr. Kinahan in his letter in NATURE (No. 66). The Tudor specimen, it is also considered, may belong to the Cambrian or Potsdam group. This, to say the least, is somewhat contradictory.

The determination of the age of the Skye ophite I am willing to leave Dr. Hunt to settle with Professors King and Rowney, merely remarking that both McCulloch and Geikie, as independent observers not looking for evidence in support of a theory, declare the rock to be of the Liassic age.

The lengthy disputes as to what is to be considered Eozoön and what is not, are most amusing. When each disputant takes up a different position and shifts it as occasion requires, how is he to be met? And is this not of itself sufficient *primâ facie* evidence of weakness such as to warrant a suspension of judgment on the part of those—and their number is great—who have accepted the "fossil" only on the strength of eminent names and reiterated assurances?

As far as I can make out, the whole positive evidence is now narrowed down to the determination of what is and what is not the true "nummuline layer." A reference to the published figures and descriptions gives no information by which we may detect any difference between the "nummuline layer" of the Skye ophite and that of the typical Canadian specimens. Thus, then, until Dr. Dawson points out the difference, this objection cannot be said to be "wholly irrelevant." To aver, without proof, that when the characteristic structure occurs in an unlooked-for position, that it is an *initiative form*, or, on the contrary, to assume it to be a *fossil* when discovered elsewhere, is easy, but does not tend to carry conviction to the unbiassed mind. To do this, we require distinct and ample evidence. The Eozoön before referred to as discovered by Mr. Sandford in the Connemara ophite, and "verified" by Rupert Jones,† belongs, according to Murchison, to the Lower Silurian age.‡ The discovery, it appears, had at the time induced Sir Roderick to class this rock as Laurentian, but shortly afterwards, purely from stratigraphical considerations, he pronounced it to be Lower Silurian.§ Here, again, we have a discordance with the views of Dr. Dawson; are we, then, to throw away such independent testimony, and say that the unfortunate Eozoön "Hibernicum" is an *imitative* form, or, are we to consider the veteran geologist wrong, and the Connemara marble Laurentian?

It is now seen that all the theories which attempt to meet the objections I have stated are in conflict—which then is right?

As regards the Tudor specimen, which, it is thought, I have too summarily "disposed" of, I would observe that it was brought forward with great *éclat* as a conclusive answer to all objections founded on the comparison of the structures of Eozoön with the forms of fibrous, dendritic, or concretionary minerals. The reasons why I dissent from this view are: 1st. The "chambers" are admittedly "more continuous and wider in proportion to the septa" than specimens found elsewhere. They are, in fact, little more than an aggregation of concentric plates or perhaps only bands, and according to the figure do not show the true segments. 2nd. The microscope reveals "for the most part merely traces

* Silliman's *American Journal*, July 1870
† Geological Magazine, vol. ii. p. 7.
‡ Ibid. p. 147. § Ibid. p. 97.

of structure consisting of small parts of canals filled with the dark colouring matter of the limestone," and in only "a few rare instances" are detected "with a higher power in the margin of some of the septa traces of the fine tubulation characteristic of the chamber wall of Eozoön." 3rd. It is almost an isolated example, and the measure of the metamorphism of its matrix together with its character—organic or otherwise—(from its generally doubtful nature as shown by Dr. Dawson's own description) could only be ascertained and settled by independent inquiry.

I may also here observe that other cases of "chambers and canals stated to be injected with calcite appear to me to be of a no more reliable character. If, indeed, we accept the Tudor specimen and Madoe fragments as Eozoön, why refuse this term to the Skye specimens which apparently possess the true features (chamber casts and nummuline layer) in a much more marked manner? As regards imitative forms, Dr. Dawson and myself are in agreement, excepting that I must contend, from all the facts we are acquainted with, that *all* Eozoönal forms are imitative, and not merely those that the exigencies of the discussion demand looked at from the organic stand-point.

Respect for your space prevents me going into further details, but I may be permitted to suggest that the truth of the matter in no way hinges upon the possibility of comprehending the constructive pseudomorphic theories advanced by King and Rowney. In what way though, excepting by pseudomorphism, I would ask, does Dr. Dawson account for the presence of the *imitative forms* which he thinks have confused other observers? And if we believe pseudomorphism to have originated them, why is it so trying to our faith to consider Eozoön Canadense to have been formed in the same manner? The fact is, chemical geology and the replacement and alteration of minerals—occurring as they do in the forms of other minerals—have been little studied by palæontologists, or probably Eozoön might have remained "unconstructed" to this day. It is well known that not only do minerals assume by replacement the crystalline forms proper to the mineral replaced, but also amorphous shapes filled with one mineral may be replaced by another without in any way destroying the original form.

There can be no doubt that a little knowledge of this kind would have infused the necessary caution, and have prevented anyone accepting as a fossil that which required the invention of a method of chemical deposition (excepting in these pages) hitherto unknown in nature. Serpentinous marble, as we may ascertain from all sources, is the typical Eozoönal rock, and, though the minerals filling the so-called chambers and canals may be Loganite or pyroxene, in addition to the serpentine, they are intimately related in a pseudomorphic point of view. Loganite and serpentine, as I have before stated, are both products of alteration. The organic hypothesis demands that we should consider the infilling to be, as contended for by Dr. T. S. Hunt, the *same mineral originally supposed to be deposited therein.* If this be so, then what becomes of the meaning of metamorphism? This to me is a trial of faith greater even than the acceptance of King and Rowney's "constructive criticisms."

I await with interest the publication of the papers sent to the Royal Irish Academy by Dr. Dawson and his colleague, Dr. Hunt, and trust they may contain new matter for consideration, as my only object has been to elicit further evidence, if there be any, in favour of the organic hypothesis.

Feb. 17 T. MELLARD READE

Ocean Currents

IN reference to Mr. Laughton's letter in NATURE of the 23rd of February, I must admit that the question of the movement of barometric depressions was not introduced into my first letter, for the reason that I did not anticipate the objection of a state of equilibrium which he raised, since the average differences of pressure only were dealt with; but I do not see that it necessitates a change of ground to show how this difficulty is met by the variations which occur in the region of lower average pressure, and how these changes themselves, taking place in a certain line of movement, might affect the surface currents of the ocean; and I am not disposed to accept the sweeping rejection of the whole power of differences in the atmospheric pressure, permanent or moving, which is contained in the last paragraph of the letter referred to, until more extended observations shall show what directions the great movements of changing pressure take in passing over the parts of the ocean which lie outside of the trade wind regions.

The action of a barometric depression in moving over the sea differs entirely from that of the winds in this, that by the former the level of a large area of the ocean surface may be raised and carried along with the depression round which the winds blow, whilst by the force of the latter the waters can only be drifted at the same level before the wind.

I have been particularly careful to suggest difference of atmospheric pressure only as a supplementary power in the production of ocean streams, not as a chief one, and it was indeed the partiality of the various theories of the causes of currents which led me to open the subject at all.

That the trade winds have a very large share in originating the Gulf Stream is undoubted, but that they can account for the whole of the phenomena of ocean currents, as Mr. Laughton maintains, appears more than doubtful.

The existence of the under outflowing current of the Mediterranean, corresponding to the inflowing surface stream, has been abundantly proved by the recent Government expedition under Dr. Carpenter. If this current be due to the action of the winds alone we should expect to find the direction of the upper and under streams reversed with a change of wind to opposite points east or west in the Strait. But the observations on the direction of the winds for six years at Gibraltar show that westerly winds (from N.W., W. and S.W.) prevail there for 198 days in the year, and easterly winds (N.E., E., and S.E.) for 144 days; further it happens that in the months of July, August, and September, in one of which the observations on the outflowing undercurrent were made, east winds prevail in the ratio of two days to one day of west wind. Experiments similar to that used to determine the presence of an under-current in the Strait of Gibraltar, have been made in the open ocean, and Maury (at p. 206 of the 9th edition of his work) quotes an instance of an apparatus constructed of a block of wood, sunk by weights to 500 fathoms, and attached by a line to a small floating barrel, having moved off "against wind and sea, and surface current." The members of the late German Arctic expeditions have observed that where the warmer blue waters moving from south-westward meet the impure waters of the Spitzbergen and east Greenland current, there is a definite line of demarcation which would indicate that the Atlantic water here dips down beneath the specifically lighter water of the ice-bearing current, a conclusion which is supported by the increase of temperature with the depth beyond this point.

Such under-currents can in no way be primarily caused by the action of the winds, and if difference of temperature and density must be called in to account for them, it must be admitted that these causes have to do with the upper streams also.

I would take this opportunity to correct a statement made in my second letter, of a probable movement of a barometric depression across the British Isles at the rate of ninety miles an hour. The depression there referred to appears to have had an oblong form, the longer axis moving nearly parallel to the length of the British Isles from N. to S., so that the record of its passage took place at Valentia and at Aberdeen within a short interval of time, thus giving an apparently great rate of travelling. But I have the authority of the Secretary of the Meteorological Office for the facts given beneath, which prove that a rate of depression movement of upwards of seventy English miles an hour may take place. On the 16th of December, 1870, a minimum reading of the barometer was registered at Valentia at $2^{h}\ 45^{m}$ P.M.; at Kew, at $9^{h}\ 30^{m}$ P.M.; at Yarmouth, at 10^{h} P.M., where the mercury remained at the same level for four hours. The interval of time between the registrations at Valentia and Yarmouth is $7^{h}\ 15^{m}$; the distance between these places is 520 miles. It seems probable also that the centre of the depression moved directly from Cork to Yarmouth, for the wind records prove that it passed north of Falmouth, and south of Holyhead and of Valentia.

KEITH JOHNSTON, JUN.

Perpetual Motion

PROBABLY your sense of justice will induce you to insert some very brief remarks on your review of my article in the *Quarterly Journal of Science*. The tone of the review is a penalty which all who venture to impugn commonly accepted theories must be prepared to submit to. Heresy in science meets with as little mercy as heresy in theology. I confess that in one sense of the word I am consciously a perpetual-motionist, but not in the sense of believing that any merely mechanical contrivance can produce perpetual motion. That there are forces in nature which can and do produce it, is a matter of daily, yearly, and secular experience. If I am a perpetual-motionist in this sense, I am in good company. You will find that Sir W. Thomson, in the *Philosophical Magazine* for February 1854, described a machine by which a steam-engine or water-wheel could produce thirty-five times the heat commonly considered as equivalent to the force used; or the corresponding amount of cold. At that time, then, two years after his paper read to the British Association (to which you refer me), he certainly did not hold such an opinion with regard to the mechanical equivalent of heat as to exclude the possibility of such an engine.

The final judgment of the question I confidently leave to time and facts. When any of the "grand founders of a rapidly progressive science" can spare time from their investigations to refute my fallacies, I shall gladly retract them. H. HIGHTON

The Spectrum of the Aurora

IN the sketch appended to my letter on this subject in last week's NATURE, I notice that the engraver has made the line at 4·1 much too sharp and definite in both spectra. It really shades off rapidly to the more refrangible side, at least in the spectrum of the vacuum tube, and possibly also in that of the aurora. Though much the brightest line in the auroral spectrum, it is not the most conspicuous in that of the tube, but the relative brightness of lines frequently varies much at different temperatures. The band at 8 in the auroral spectrum is also represented too narrow. Those who have practical experience with the spectroscope will appreciate the great difficulty of representing faint spectra correctly in a woodcut.

HENRY R. PROCTER

Science Teaching for the People

THE subject of Science Teaching in our elementary schools having been ably brought forward by Mr. Henry Ullyett in a recent number of your Journal, and the scientific instruction under the Science and Art Department, South Kensington, having been at various times the subject of consideration in it, I venture to ask for a short space in your columns in order to submit the following proposition for the consideration of your readers, many of whom, have, probably, special opportunities of coming to a correct judgment on the point. The question I wish solved is this: Is the spread of scientific education, under the auspices of the Science and Art Department, likely to be best promoted by the whole of the Department's assistance to any one town being dispensed by a single committee, by whom a central school shall be provided, of which all other schools established, or that may be established, in various districts of the town, shall be considered only as branches, and be subject to the control of the central committee, on whose books the names of all students would be borne, and through the one secretary of which all the returns and other communications to and from the Department would have to pass?

Is it not better that the schools established in various districts of a town, say in connection with each elementary school, should each have their separate organisation of committee and secretary, at least in so far as the teachers connected with each are different? In this town, in common with a great many others, the latter plan has been the rule, but a suggestion has now been made that all these committees should be amalgamated, together with those of the art-classes also, the plea being that it might lead to the erection of a central building for the purposes of an art and science school.

Now, however necessary for art it is that there should be a central building for the provision of higher instruction than can be given in the night classes, I cannot see that there is anything in the study of science that demands greater facilities than can easily be provided by any district school, and the possession of which is indeed required by the Science Directory before any science school receives the approval of the Department.

Why, then, the science committees should be asked to unite in this town alone, by which a most dangerous precedent would be established, I cannot understand, for I do not believe that such a course would be beneficial to the town at large, while it would be very prejudicial to the interests of the existing district schools, and of any persons wishing to commence teaching in future.

Perhaps some of your readers will favour us with their views on the question I have stated, which I venture to think involves a principle of very general interest to the science schools of the kingdom. A MEMBER OF A SCIENCE COMMITTEE

A Rare Moth

IT may be interesting to know that the rare and beautiful moth, *Deilephila Galii*, appeared somewhat plentifully in the neighbourhood of Derby during the past summer. I have in my possession a fine male and female which were captured at Long Eaton, some few miles from Nottingham, whilst flying in company over a bed of geraniums. Is not this the first recorded instance of their appearance in this locality? *Zeuzera Æsculi* also appeared in unusual abundance in the same garden, upwards of fifty specimens being taken by one person. W. H. G.

Measurement of Mass

WILL you allow me a few words in explanation of a sentence in my last letter, which has strangely been misunderstood by Prof. Everett. In defending the system which makes the standard pound a unit of force on the ground that although not the most philosophical, it simplifies the conception of mass which is always difficult for beginners, I said, "The assumption of a hypothetical force of gravity not dependent on latitude, seems to stand on the same footing as the employment of a mean solar day," meaning, of course, that just as we assume (for convenience) that a solar day is the same length at whatever period of the year we take it, so we may assume a mean force of gravity (the actual force of gravity in latitude 45°) which is the same all over the earth. Such an assumption will enable us to explain the unfamiliar notion of mass by the familiar one of weight, and when it does become necessary to take into account the variation in the force of gravity at different points of the earth's surface, the correction is easily made.

Prof. Everett seems to think that I suppose that the average length of the apparent solar day is not the same at all places on the earth. Will you allow me to quote the following passage from the original in support of my first assertion, that Prof. Everett's tacit assumption that everybody knows what mass is, is less likely to lead to clear ideas than the explanation given by Deschanel. He says—"Un corps a une masse plus grande qu'un autre lorsque la même force lui imprime une vitesse plus petite, et reciproquement, ... si nous considérons en particulier le poids d'un corps, on aura, entre ce poids, la masse et l'accélération de la pesanteur, la relation fondamentale,

$$P = Mg$$

Cette formule nous montre que dans le même lieu le poids est proportionnel à la masse, parceque g a la même valeur pour tous les corps. Il n'en est pas de même quand on passe d'un lieu à un autre; mais comme après tout les variations sont extrêmement petites, en réalité la masse et le poids sont deux quantités toujours sensiblement dans le même rapport. Toutefois il faut se rappeler que ces deux expressions correspondent à des notions distinctes, et, abstraction faite de toute évaluation numérique, la masse d'un corps est quelque chose qui lui est propre et qui est indépendant du poids. La pesanteur n'existerait pas qu'il n'en serait pas moins vrai qu'une sphère de plomb a une masse plus grande qu'une sphère de liége de même diamètre. Nous reconnaissons ce fait ordinairement à ce que la poids de la première sphère est plus grande que celui de la seconde; mais à défaut de la pesanteur, l'emploi de toute autre force pourrait nous conduire au même resultat." W. M. W.

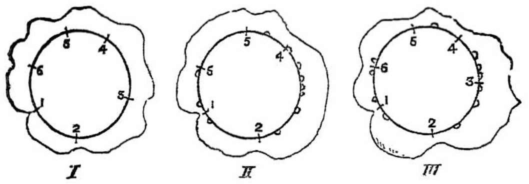
PHOTOGRAPHS OF THE ECLIPSE

PERMIT me to call your attention to the position of the woodcut illustrating my remarks on the Eclipse Photographs. The south point is where the north should be. As what I have now to say refers to the picture I shall feel obliged if you will permit its reinsertion in its true position.* With reference to the power of the light of the Corona, I used the word *actinic*, not *active* as printed.

The readers of NATURE may perhaps be glad of the opportunity to compare for themselves tracings of the American and of my own photographs, which I now give in outline in illustration of remarks in your second article

* This vexing mistake was due to a blunder of the printer in reversing the block after it had been placed on the machine. Its re-insertion this week will rectify the mistake.—ED.

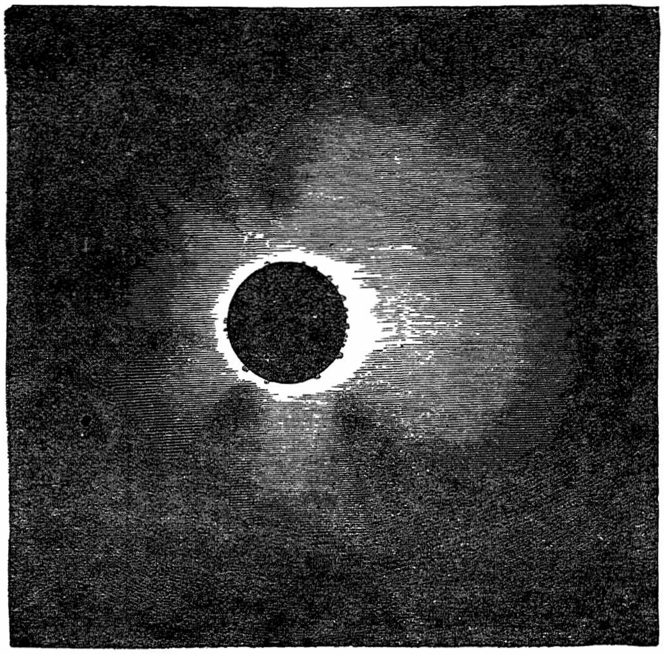
on the Eclipse Expedition. In Mr. Lockyer's article it is stated:—"Now at Syracuse Mr. Brothers also photographed rifts, three rifts, but the sketches did not record a single one;" forgetting, evidently, that at Syracuse no



From Prof. Watson's Drawing. From American Photo. taken at Cadiz. From Photograph taken at Syracuse.

attempt was made to sketch the Corona either by our own party or the Americans. At Agosta Mr. Brett was stationed, but as the Eclipse was only visible there for about *five seconds*, of course in that time no artist could pretend to make a drawing. It happens, however, that Prof. Watson was at Carlentini, and being favoured with a clear sky he succeeded in making a very careful drawing, which I had the good fortune to see and compare with my photograph No. 5 a few days after the Eclipse. An outline of this drawing I now give, so that it may be compared with the photographs made in Spain and at Syracuse.

There are two or three points which must be considered in comparing drawings and photographs. The photographs will differ according as they are made with a camera or telescope, and the drawings will differ according as they are made with the aid of a telescope or without. With the telescope the field of view is limited, and the eye is naturally attracted chiefly by the intense light of the red prominences and the corona near the moon's limit. Naked-eye drawings ought to be as valuable as photographs, but I doubt if any two artists will ever be found to make sketches agreeing in every particular. On photography must we depend for settling doubtful points of this nature, and it seems to me in this case to be absolutely settled that three rifts are identical. The outline sketches speak



THE LATE ECLIPSE, AS PHOTOGRAPHED AT SYRACUSE

for themselves. A pair of compasses applied to the points formed by lines drawn from the moon's centre to the centres of the depressions (or rifts) in the corona, will show whether or not the places of the three gaps are the same.

It may be said that Lord Lindsay's photographs taken five miles from the station occupied by the American observers in Spain, do not show the rifts. This, I think, must be accounted for by the presence of cloud. The cloud may have been so thin as to be quite invisible in the feeble light of the Eclipse, but yet sufficient to prevent the photographic delineation of the rifts. Three of my photographs were taken through cloud, and they show us traces of rifts. The fifth plate shows three distinctly, and less plainly five or six others.

Professor Watson's drawing shows two gaps corresponding with 1 and 6 in both photographs, and depression in the corona agreeing very closely indeed with my picture.

This evidence seems to me to be absolutely irresistible as to the identity of the great rifts in the corona.

In explanation of the way the outline drawings have been made, I may say that the points marked from 1 to 6 have been pricked through the photographs, Professor Watson's drawing having been reduced to the same scale as the photographs, and pricked off in the same manner.

A. BROTHERS

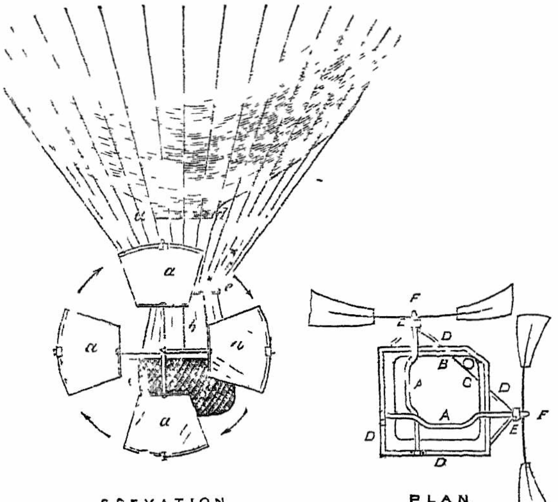
EXPEDITION OF THE "DUQUESNE"

M. RICHARD, master in the Royal Navy, directed the Expedition, and is now attached to the Lille aëronautic station for the Department of the North. I have interrogated him and elicited from him the following details, which can without inconvenience be placed before the eyes of the general public. The French Republican Government having in view the promotion of general knowledge, as well as the defence of the national integrity, did not object to any communication which is not directly connected with warfare.

The aërostat, "Le Duquesne," was despatched from Paris on January 9, at three o'clock in the morning, before a large attendance, among them some members of the French Institute. The reigning current was a strong S.W. wind, which was unfavourable for escaping the Prussian lines, as the intended directing power was only a motion of three feet per second. The experiment should have been postponed for a fair trial. Another drawback to the Expedition was the despatch of the balloon in the night-time, although the moon, being almost full at the time, afforded some light to the aerial travellers, being very low on the horizon when "Le Duquesne" left the Orleans railway station. There were in the car M. Richard and three sailors of the national navy, so *that two could be kept pulling without interruption*; three sacks of despatches, four pigeons, eight sacks of ballast, thirty kilogrammes each, were also in the car. The provisions were bread, wine, and chocolate. The weight of the machinery was 300 pounds. As will be seen by the accompanying diagram, the two screws were worked by a very simple contrivance, rotating only at a rate of twenty-five rotations per minute, but the diameter four yards, so that the motion in feet per second of the screws was about sixteen, five or six times more than the rapidity intended to be given to the balloon, which was three miles an hour.

Before leaving ground the car had been placed with its diagonal due north. But that precaution proved useless, as the balloon rotated when ascending before the screws could be put into operation.

We will give the explanation under M. Richard's own authority, with some remarks. We are certain of his perfect truthfulness, but it is very hard to say if he saw everything correctly for the whole length of the eventful journey.



ELEVATION PLAN

THE DUQUESNE DIRECTING BALLOON, DESPATCHED FROM PARIS JANUARY 9, 1871

a a a a segments of the screw revolving in the direction of the arrow or the opposite direction, with variable rapidity; *b* valve line; *c* car; *d* appendage *e* hoop.
A A handle for moving screw, two men pulling at once; C place of the captain, B place of one man; D D board in iron tubes fixed to the car; E E extremities of the axes; F F fixing screw, which can be removed so that wheels may be thrown overboard when landing.

The observers on the ground had placed themselves in a right position to ascertain the effect of the screws, and the *compte-rendu* published in Paris gave the summary of their impressions. They suppose M. Richard succeeded in giving to his balloon a deviation of 15° from the due course of the wind during the earlier part of his journey. If experiments had been made during daylight, matters should have been more easy to ascertain. If I can procure authorisation, I will get an experiment tried anew at the Crystal Palace before an English audience. The fact is that two of the three sailors pulled with all their strength during a few minutes, after having exhibited some hesitation in the first instance. The scenery was so magnificent that it was necessary to call them twice before they began to pull.

M. Richard soon perceived that he was unable to ascertain the effect of the propeller. He resolved upon letting the balloon follow its way undisturbed, and he noted carefully the barometric altitude and the direction. When passing over Prussian lines at an altitude of $0^{mm}·69$, some shots were directed at the balloon without any result.

At 3^h 10^m altitude 0·68° was reached; temperature 20° F. The Valley of the Marne was below.

M. Richard turned the screws and tried to pull towards the north, in order to go N.N.E; but the rotation was difficult to stop. It was necessary to work only one screw during a long time in order *to rotate the aërostat in the right direction*. It was only when some real torsion was established between aërostat and car that the required revolution took place. But when movement was given to the balloon it was difficult to prevent it, and the aerostat executed one entire revolution against the will of its captain. Then the two screws were worked together; the same effects were produced, but it was only with great difficulty that the car was placed in the right direction. When the two screws were worked together, the balloon was rotating sometimes in one way and sometimes in another. These observations, moreover, says M. Richard, were made at night by a man who had never before ascended in a balloon. As already said they cannot be considered as wholly reliable, as rotation depends on the changing of the fuel as well as on the friction in the forward half or on inequality in the pulling of the screws. But it is very easy to understand that the fact of the screws being able to rotate the balloon in a given direction is unquestionable, although men and captain were equally unable to move it in the right direction for escaping Prussian lines.

The difficulties experienced were so great that M. Richard stopped the experiment, and tried it again only after day-break, but with no other success than previously.

At 7 o'clock in the morning, altitude 65 centimetres, temperature 11° Fahr., cirrus was visible at a great height, cumulus down below, and stratus covering the earth.

At 11 o'clock only one sack of ballast was left. The balloon stopped its descent; voices of peasantry were heard from above the stratus. They cry "Versy; commune of Chigny." The balloon emerging in stratus, fell rapidly by condensation. All the ballast was thrown away, and the balloon ran horizontally for two miles. The guide rope only had been thrown; then the grapnel was thrown out. *The wind was so strong that the grapnel rope was broken.* The screws and wheels had been thrown overboard, but the car incliners and the screw axes project outside. One of these comes into contact with the ground, the car is upset, and the crew are dragged under it for a length of time, 600 yards. The balloon is stopped by the loss of gas and the peasantry. The three sailors are slightly injured, the captain is left for dead. The sailors, helped by the peasantry, make their escape, and conceal balloon, car, and despatches in the woods. The captain is brought like a corpse into a neighbouring town, but is found to be alive. He is cured, and sent secretly into Lille, where the balloon arrived two days later.

W. DE FONVIELLE

NOTES

AT a recent meeting of the American Philosophical Society it was resolved "That a committee, consisting of the President of the Society and five members, be appointed, whose duties it shall be to consider and report whether it is desirable, and if desirable, whether it be practicable, to establish in the City of Philadelphia, under the auspices of the Society, an Observatory, astronomical and physical, either or both; and if so at what cost, on what site, and what instruments are requisite for such purposes, and at what cost such instruments can be procured." We suppose such a proposal for England would be looked upon as a joke, and yet already we cannot compare with America in our observing power.

WE are informed that the Royal Commission on Scientific Instruction and the Advancement of Science have their First Report nearly ready.

DR. J. CLERK MAXWELL, F.R.S., was elected yesterday to the Professorship of Experimental Physics in the University of Cambridge.

It will have been observed that Sir F. Goldsmid carried his motion in the House of Commons last week: "That, in the opinion of this House, young men qualified by character and attainments for admission into the service of the Government of India as civil engineers, ought not to be excluded from such service by reason of their not having been educated at a Government College." If the facilities afforded by existing institutions are not found sufficient for the training of practical engineers, it is quite right that the Government should step in and supply the deficiency; our recent article on the subject shows whether the Institute of Civil Engineers is alive to the wants of the time in this respect. And if the Government establishes such a College, it is quite right that it should examine all candidates who present themselves, and give diplomas to those who pass them with credit; otherwise, it shows but little faith in its own system of education. Mr. Lowe has well pointed out that the Government will place itself altogether in a false position if it abandons its intention with reference to the proposed College, and yet takes no notice of the want of adequate instruction in the existing institutions. A leading article in the *Engineer* of last week on the subject points out that the idea of a competition between the Royal Engineers and the proposed College is a bugbear.

THE number of candidates for admission to the Royal Society this year is fifty.

THE members of the French Institute have learned with deep regret the death of one of its most celebrated members. M. Lartet died in the department of the Gers during the investment of Paris. It is in that very department that he discovered an immense quantity of fossils at Saint Salut. M. Lartet was professor of Palæontology at the Museum of Natural History, filling the place of the lamented d'Archiac. He was ill from the time of his nomination, and was unable to deliver a single lecture except his inaugural address. M. Lartet is known to the whole scientific world by an immense quantity of academic and scientific memoirs. His only work of consequence was published in connection with Mr. Henry Christy, an English merchant well known for his love of geology. That publication, which marks a starting point in palæontological inquiries, is called *Reliquiæ Aquitanicæ*, and cost the late Mr. Christy an immense sum of money.

WE are very glad to observe that a fund is being raised in this country for the relief of French horticulturists and gardeners who have suffered by the recent war. Many of them have been reduced to a deplorable plight, their gardens, greenhouses, and orchards having been completely destroyed. Their condition presents a strong claim on their more fortunate *confrères* in this country. Contributions may be made in money, plants, cuttings, grafts, stocks, seeds, tools, mats, &c., and it is urgently requested that subscriptions in money be forwarded at once to the treasurer, Mr. G. F. Wilson, F.R.S., Heatherbank, Weybridge Heath, or to the honorary secretary, Rev. H. H. Dombrain, Westwell Vicarage, Ashford, Kent.

THE impulse given to the study of Natural Science at Rugby is already bearing fruit. Mr. R. J. Williamson, who was elected to a Natural Science Studentship at Christ Church, Oxford, on the 4th, and Mr. C. J. Taylor, who obtained Miss Burdett Coutts's Geological Scholarship on the third of this month, were both Rugbeians. Including these, Rugby has had six Natural Science Honours during the last twelve months, viz., two Oxford "Firsts," one Cambridge "First," two Christ Church Studentships, and the Geological Scholarship. All this is encouraging for the future.

THE *Bookseller* records the death at Dartford, at the age of eighty-two, of Mr. Augustus Applegarth, the eminent mechanist and inventor, to whom the world at large and the printing trade in particular are largely indebted for his improvements in the printing-press. Until substituted by Hoe's American machine, the *Times* was printed from an Applegarth, which printed from 8,000 to 10,000 copies per hour. Mr. Applegarth took out no fewer than eighteen patents for various purposes, including three for the printing of bank-notes, and for printing silk; but, like many inventors, although he enriched others, he failed to secure a fortune for himself.

M. SORREL, one of the youngest members of the Observatory, has been a victim of the privations endured during the investment of Paris. He was engaged in the artillery of the National Guard. He had made one ascent on board the "Géant" and another on board the "Pole nord."

THE cattle plague is raging amongst the herds which were congregated for the revictualling of Paris, and the carcases of dead animals are every day brought out in open carts, and conducted to Monfauçon to be buried. It appears that the same plague followed the invasion of 1814. M. Bouley, a member of the French Institute, contended in its last sitting that the meat of those animals could be eaten with impunity, and he said in support of his argument that cases of plague were discovered during the investment of Paris, and that the animals were not thrown away as useless. But it must not be forgotten that the mortality during the siege was very high, and that a part of the deaths may possibly be attributed to the bad quality of the meat taken from infected animals.

THE weather at Paris is now very mild, and some fears are expressed of an outbreak of epidemic, in consequence of the burial of so many dead bodies of men and horses round Paris. It is supposed that 200,000 men and more than 100,000 horses were killed, besides 60,000 persons who died from different affections.

ONE of the most interesting scientific novelties of the day is the discovery of a true bone cave near Phœnixville, Pennsylvania, by Mr. C. M. Wheatley, reported in *Harper's Weekly*. The remains of animals, all extinct, are quite abundant and varied, and include bones of mastodon, horse, mylodon, and other forms, and are in great part entirely new to science. A preliminary report upon them, by Prof. Cope, was to appear in the March number of the *American Journal of Science*.

THE same journal refers to the explorations made during the past summer in the western territories of the United States by the parties of Prof. Hayden and Prof. Marsh, the latter of whom has just published an interesting *résumé* of his geological observations. The principal field of his labours, as already stated, was the neighbourhood of Fort Bridger, among certain freshwater deposits from an ancient lake, the strata of which formed a thickness in places of at least 1,500 feet. Vertebrate remains of great variety were found entombed in these deposits, differing in marked features from those belonging to the Miocene basin east of the Rocky Mountains. In the latter bones of ruminating animals were especially abundant, while fishes and reptiles, with the exception of a single species of tortoise, were entirely wanting. In the Fort Bridger basin, on the other hand, reptilian life was in great development, and was represented by crocodiles, tortoises, lizards, and serpents, together with numerous fish, while many mammals, allied to the tapirs, as well as other small quadrupeds, occupied its borders. A detailed report of this exploration will be found in the March number of the *American Journal of Science*.

LIEUTENANT PAYER, well known for his geological investigations in the Alps, has lately communicated some facts in regard to discoveries in Greenland by the late German expedition, of which he was a member; and in this he calls attention especially to the probability of the hypothesis that Greenland is essentially a congeries of islands similar to that west of it, and not a huge continental mass, as has been supposed by most authors. One strong evidence of this he considers to be furnished by the deep inlet discovered by the expedition, previously unrecorded on any chart, and which received the name of Emperor Francis Joseph's Fiord. This was found to extend deep into the interior of the land, continually opening into new arms, and widening in places until it was traced out for over one-third of the estimated breadth of Greenland, and without any indication of coming to an end. Indeed, in a south-westerly direction it opened out into what looked like a great basin into which the fiord itself emptied. The circumstance also that the saltness of the fiords is generally greatly diminished by the fresh-water streams pouring into them when they are simply *cul-de-sacs*, and the fact that the great Greenland fiord, notwithstanding the enormous addition of fresh water, retained all its saltness, pointed to a maritime communication with the opposite side of the country. Time was not allowed to the party to prosecute the exploration of this supposed strait; but it is believed, as stated, that it finds its opposite opening in Baffin's Bay. Another still more potent argument in favour of the assumption that Greenland is a congeries of islands, and not a continent, was found in the apparent absence of great longitudinal valleys, such as usually characterise continents, these being entirely wanting in the north-eastern part of Greenland.

WE understand that the collection of books belonging to the late A. H. Haliday, A.M., has been left by his will, dated in 1847, to the Royal Irish Academy. This collection is very rich in rare entomological tracts, and it is expected that a catalogue of it will be formed under the directions of the librarian of the Academy. Any duplicates of works at present in the library of the Academy are to go to the library of the Natural History Society of Belfast. The entomological collection of Mr. Haliday was bequeathed to the Museum of Trinity College, Dublin. This collection chiefly consists of British Diptera and Hymenoptera, and contains the type specimens of the many classical monographs and papers of Mr. Haliday. It also contains the results of some years' collecting in Italy, and especially the insects, many of them of great interest, taken during a tour in Sicily in 1868 by Mr. Haliday and Professor E. P. Wright. Unfortunately the collection is still in store or collecting boxes, and is for the most part unnamed, the localities of the European insects, which, however, form far the smallest portion of the collection, being indicated only by dates. We hope soon to be able to report what steps will be taken to have this collection named and arranged, knowing well that the authorities at Trinity College, Dublin, will not be slothful in this matter.

A COMMITTEE has been raised for the purpose of collecting subscriptions to present Mr. John Banting Rogers with a testimonial, as a mark of the high appreciation in which the shipping community of this country hold his untiring labours in the introduction of his valuable invention for life-saving purpose.

A CORRESPONDENT calls our attention to a state of things in the village of Woking which appears to require the notice of the Government inspector. He states that the drainage from the convict prison at Woking and from the gas-works is allowed to flow into an open meadow, which has become perfectly saturated with it. Fever has been prevalent in the neighbourhood for the last two years, and small-pox is now added. At the neighbouring village of Horsell a school of 200 children has been closed in consequence. As usual, it seems impossible to fix on anyone the responsibility for abating the nuisance.

CAPTAIN C. F. HALL is busily engaged in fitting out his vessel for his cruise next summer, and will be amply provided with everything necessary for his comfort and the success of his enterprise. It is understood that Dr. David Walker, the well-known companion of Sir Leopold M'Clintock during his expedition in the *Fox* in search of Sir John Franklin, will accompany Captain Hall in charge of the scientific department.

DR. A. W. EICHLER, the Editor of Martius's "Flora Brasiliensis," has been appointed Professor of Botany and Director of the Botanic Garden at Graz in Austria. No change will be necessary in the editing of the magnificent work under Prof. Eichler's charge.

THE region bordering upon the Black Sea has long been known to be full of antiquarian treasures of the highest interest, as evinced by the superb reports published from time to time at the expense of the Russian Government. A late exploration of the peninsula of Toman, situated between the Black Sea and the Sea of Azov, in continuation of previous researches, has brought to light many striking objects, particularly of those belonging to a past period of Greek art, and consisting of gold ornaments, sarcophagi, terra cotta statuettes, &c.

IN the Intercolonial Exhibition, held last year in Sydney, New South Wales, much interest and curiosity were felt in the specimens of wine exhibited. The report of the jury on the Victorian produce was highly satisfactory, more especially with regard to those wines retailed in Melbourne at *2d.* and *4d.* per tumbler. These wines are said to compare more than favourably with the cheap wines in general consumption in the wine countries of Europe, and promise ere long to supplant the use of beer and spirits in the classes most accustomed to use and abuse those beverages. For experience has long proved that men will drink wine in hot and dry climates in preference to all other beverages if they can get it abundantly, cheap, and good in its kind. The jury say that the cheap and wholesome wines sold in Melbourne at the above prices all over the city, and even a better class of wine retailed at *1s.* per quart bottle, are the outward evidences of the hold which pure wine is taking on the masses in Victoria.

IT would appear from a communication in *Le Journal de Quebec* that the sea is steadily swallowing up the land at St. Thomas, and possibly other points on the lower St. Lawrence, a correspondent of the *Engineer* asserting that at low tide, some thirty years ago, he saw the ruins of the third church built in that locality within the space of two hundred years, and that since the battues, or banks covered by the tide, have advanced from one to two leagues into the interior.

MEAT-PRESERVING appears to be getting a profitable branch of trade in all countries where stock abounds or pasture exists. A new company with a capital of 50,000*l.*, under the title of the "Victoria Two-fold Bay, and London Meat-preserving Company" has been started in Melbourne. The meat is to be preserved by a process of cooking *in vacuo* by which air and moisture is drawn from the inside of the tins during the cooking.

THE Monthly Notices of Papers and Proceedings of the Royal Society of Tasmania for March, April, and June, 1870, contain several valuable articles, including Contributions to the Phytography of Tasmania, by Dr. Ferd. von Müller; and some additional Observations on more recent changes which have taken place in the star η Argus and its surrounding nebula (with diagrams), by Mr. F. Abbott.

THE great booksellers like Hachette and Co. have been surprised to learn that their branch offices in occupied districts had transacted an immense amount of business. The reason is singular enough, and worthy to be noticed. The Prussian soldiers are learning the French language, and are purchasing an immense number of books. There is a new market opened for French authors, an unforeseen consequence of bloody defeats in the field.

AT one of the sittings of the French Academy, Dr. Felix Rochard proposed to establish on the Seine floating ambulances for the wounded as being probably more free than common hospitals from miasmatic influence. The proposition was warmly supported by Baron Larey, the great practising surgeon.

PAPERS ON IRON AND STEEL

II.

THE BESSEMER PROCESS (CONTINUED)

IN the first part of this paper* I described the facts of the Bessemer process, and now proceed to a theoretical examination of these. In order to do this at all satisfactorily, it is necessary to have, at the outset, a clear idea of the composition of the raw materials,—the pig-iron and the spiegeleisen. I insist the more urgently upon this, because the descriptions or definitions of cast-iron or pig-iron usually given in our chemical text-books are by no means satisfactory, and are frequently erroneous.

The following are the results of my own analyses of fourteen brands of pig-iron and five brands of spiegeleisen, all of which are rather extensively used in the manufacture of Bessemer steel. In addition to the substances there determined, most of the pigs contain a small quantity of calcium, but this and the small traces of the metals of the other alkaline earths, and of the alkalis, were not determined, as the analyses were made for commercial purposes, and I have not been able to detect any practical modifications in quality of the finished iron or steel, which is due to the presence of these metals in the pig-iron. For this reason the statement of "iron by difference" is but an approximation, and somewhat in excess.

Composition of Bessemer Pig-Irons

No.	Combined Carbon.	Graphite.	Silicon.	Phosphorus.	Sulphur.	Manganese.	Iron by Difference.
1	0·60	4·12	1·92	0·10	0·06	0·32	92·88
2	trace	3·52	3·10	0·07	0·06	0·30	92·95
3	0·70	2·68	3·60	0·09	0·12	1·16	91·65
4	0·55	1·85	3·60	0·13	0·18	1·22	92·47
5	0·55	1·92	3·65	0·09	0·13	none	93·66
6	0·75	2·00	3·36	0·10	0·23	trace	93·56
7	0·54	2·20	3·15	0·10	0·26	2·00	91·75
8	0·72	2·10	2·90	0·06	0·26	1·00	93·06
9	0·62	4·00	1·40	0·02	0·06	0·38	93·52
10	0·50	3·00	2·00	0·03	0·10	trace	94·37
11	0·20	2·80	2·33	0·02	0·10	1·92	92·63
12	trace	3·20	1·58	0·11	0·12	2·22	92·77
13	0·37	1·95	4·08	0·15	0·23	1·15	92·06
14	1·25	1·65	2·15	0·24	0·21	1·10	93·40

For commercial reasons, which will be readily understood, I abstain from publishing the names of the above brands. No. 12 is a Swedish pig iron; all the rest are English hæmatite pig-irons, made expressly for Bessemer purposes. Nos. 9, 10, and 11 produced exceptionally good steel; Nos. 1, 2, and 3, good average qualities; No. 4, inferior; No. 13, *very* inferior; No. 14 produced such bad steel that the whole parcel was returned, though it came from a well-known firm, it was of the same brand as No. 4.

Composition of Spiegeleisens

No.	Combined Carbon.	Graphite.	Silicon.	Phosphorus.	Sulphur.	Manganese.	Iron by Difference.
1	4·10	0·45	1·23	0·12	0·16	4·60	89·34
2	4·10	0·40	0·96	0·03	0·26	5·86	88·39
3	4·50	0·40	0·88	0·04	0·10	9·61	84·47
4	4·10	0·42	0·65	0·05	0·15	8·64	85·99
5	3·00	0·70	0·14	0·04	0·07	6·44	89·61

No. 3 is the best of these; No. 4, the next in quality, rather better than average; No. 2, rather below average; No. 1, inferior quality; and No. 5 so poor that it was rejected.

* NATURE, Vol. iii. No. 63, p. 211

Excluding the rejected samples, the average of the above is as follows:—

Average Composition of 13 *Brands of Bessemer Pigs*

Combined Carbon	0·47
Graphite	2·72
Silicon	2·84
Phosphorus	0·08
Sulphur	0·14
Manganese	0·90
Iron by difference, about	92·85
	100·00

Average Composition of four Brands of Spiegeleisen

Combined Carbon	4·20
Graphite	0·42
Silicon	0·93
Phosphorus	0·06
Sulphur	0·17
Manganese	6·63
Iron by difference, about	87·59
	100·00

The sulphur of this average of the spiegeleisens is excessive, being raised unduly by the very unusual quantity contained in No. 2: 0·12 per cent. would state the general average more correctly. In like manner the phosphorus average is raised by the excessive quantity in No. 1. Excluding this the average is reduced to 0·04.

I will pass over the small amount of chemical change which results from the mere melting of the pig and speigeleisen in the cupola, and regard the above as the composition of the material which enters the converter. When a mixture such as these Bessemer pig-irons is fused and exposed to the action of atmospheric air, the silicon is the most readily oxidised, silicates of iron and manganese are formed, which separate and float on the surface, forming the "cinder." The carbon oxidises simultaneously with the silicon, but in a much smaller degree, until the silicon is nearly all burnt out. When the silicon is reduced below one per cent., the combustion of the carbon takes the lead, and the small remainder of silicon is but slowly oxidised, the last traces resisting oxidisation with considerable stubbornness.

I have made some special investigations of this subject, and shall show in the course of another paper that manganese is remarkably efficient in removing these last traces of silicon. I should also mention that the above-stated generalisations respecting the prior combustion of silicon and the suppression of carbon combustion by the presence of unburnt silicon, are based chiefly on examinations I have made of the actions which take place in the "refinery;" the difference between the Bessemer converter and the refinery being, that in the one, air is blown upon or a little below the surface of the melted pig-iron, while in the other, it is blown through it from below, and thereby acts with far greater efficiency and rapidity; the *kind* of action is, however, the same in both cases, the difference is only in *degree.*

In order to test the accuracy of the above conclusions, I have requested my late assistant at the Atlas Works, Mr. G. C. Barker, to make analyses of the Bessemer material during different stages of the same blow. This he has kindly done, and the following are his results. The carbon, silicon, sulphur, phosphorus, and manganese only are determined.

The first column shows the percentage of these constituents in the pig-iron after being melted just before being poured into the converter.

Second, the same after six minutes blowing.

Third, the same after twelve minutes blowing.

Fourth, the same when the blowing was finished, but before the spiegeleisen was added.

Fifth, the finished steel when poured into the ingot moulds.

	1	2	3	4	5
Combined carbon	1·000	3·040	1·640	0·190	0·370
Graphite . . .	2·570	trace	trace	trace	trace
Silicon	2·260	0·955	0·470	trace	trace
Sulphur	0·107	0·091	0·098	0·093	0·090
Phosphorus . .	0·073	0·070	0·070	0·070	0·059
Manganese . . .	0·410	trace	trace	trace	0·540

We shall now be able to understand the changes I have described as occurring in the flame. Before the full combustion of the carbon can commence, there is about 2½ per cent. of silicon to be converted into silicic acid. In a charge of 6 tons this amounts to 3 cwt. For the complete combustion of this, nearly 3½ cwt. of oxygen, or about 14 cwt. of atmospheric air, is necessary. My explanation of the smaller and less brilliant flame that at first roars from the mouth of the converter is that it is mainly a silicon flame, mingled, however, with a small proportion of carbon flame; that the amount of silicon combustion goes on diminishing, and in a proportionate degree the carbon combustion increases, as the demand of the silicon upon the oxygen of the blast diminishes in consequence of its less abundant diffusion among the melted iron.

I shall not be surprised if this explanation is controverted, as in offering it I fly in the face of the spectroscope, which has made such glorious conquests that modern philosophers are disposed to trust it as implicitly as successful soldiers rely upon the general who has led them continually to victory; but without failing in due deference to those who are more skilful than I am in the use of this instrument, I am satisfied that in this and other cases where the question has been to determine the presence or absence of the *metalloids*, the *negative* replies of the spectroscope have been too hastily accepted. Prof. Roscoe, who devoted a considerable amount of time and labour to the spectroscopic examination of the Bessemer flame, says, "Those who are practically engaged in working this process would like spectrum analysis to do a great deal more; they would like to be told whether there is any sulphur, phosphorus, or silicon in their steel; questions which, unfortunately, at present spectrum analysis cannot answer, for this very good reason, that these substances do not appear at all as gases in the flame, but that they either remain unvolatilised in the molten metal, or swim on its surface in the slag of the ore; and, consequently, the lines of these bodies are not seen in the spectrum of the flame." Dr. Watts's observations and conclusions accord with those of Prof. Roscoe.

If by the above Prof. Roscoe is to be understood as asserting that no portion of the Bessemer flame at any period of its existence is due to the combustion of silicon, or that silicon is not present in the Bessemer flame, I must very decidedly affirm that such conclusion is erroneous. I do not for a moment question the accuracy of the observations of both Prof. Roscoe and Dr. Watts. I merely maintain that the absence of "the lines" of silicon in the spectrum of the flame does not prove its absence as a constituent in producing such flame, and for the following reasons:—

We know that that the silicon existed in the pig-iron in the proportion already stated, and that although a very small quantity of that which ordinary analysis detects may have existed as entangled silicate in the pig, and another small portion is of course oxidised in the cupola, the bulk of it enters the converter as unoxidised silicon, and that it is oxidised and converted into silicic acid during the blow. We also know that silicon when heated in air or oxygen burns brilliantly, and that the product of such combustion when heated with a blast such as that which supplies its oxygen in the Bessemer converter, is sufficiently volatile to form concretions in the throats of furnaces, which have been compared to natural chalcedony. Besides this a large quantity of solid matter is mechanically forced into the flame, and is seen above as a red smoke, which, without the slightest indication of unburnt carbon, is often sufficiently dense to hide the mid-day sun. The greater the quantity of silicon in the pig the more dense is this red smoke, which appears to consist of silicate and peroxide of iron. I maintain, therefore, that silicon is there, and that it must contribute to the luminosity of the flame, though it shows no characteristic "lines" in the spectrum.

Under such circumstances, we have no good *a priori* reasons for looking for the silicon *lines*; a *continuous spectrum* being that which we are theoretically justified in anticipating as the result of such combustion of silicon, and this is exactly what the spectroscope reveals. The spectrum of the Bessemer flame at the commencement and early stages of the blow is of a most uncommunicative continuous character: occasional flickerings and vanishing ghosts of lines and bands come and go with perplexing irregularity; and even the brilliant and ever obtrusive sodium lines do not appear at this stage, but commence with spasmodic flashings across the spectrum at about the period when the elongation and brightening of the flames which I have described is most decidedly taking place. When the flame has reached its maximum of extension and brilliancy, the sodium lines cease their intermittent flashings, and become a steady stream of light, the lithium band appears (though not in every blow), and the whole spectrum becomes striped, but the continuous spectrum still remains as the permanent background.

My general reason for questioning the negative conclusions of the spectroscope in reference to silicon and the other metalloids is, that these bodies usually give a continuous spectrum when, as elements, they combine freely with oxygen, as in direct unrestrained combustion under ordinary pressure in the open air. It appears to me that there is thus presented a broad distinction between the spectra of the metals and of the non-metallic elements, which is of great practical importance, and which has not been sufficiently considered, when conclusions have been based on negative spectroscopic results. I have already, in chapter 13 of "The Fuel of the Sun," referred to the worthlessness of the negative evidence of the spectroscope in reference to the non-existence of the metalloids in the sun, and maintain that "they may all be there though the spectroscope should not detect one of them." The mere fact that nothing but metals (I include hydrogen with these) should have been discovered in the sun is very suggestive. W. MATTIEU WILLIAMS

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 23.—"On the Thermo-electric action of Metals and Liquids," by George Gore, F.R.S. It is well known that the degree of rapidity with which a metal immersed in an acid, alkaline, or saline liquid is corroded varies considerably with the temperature, and that the speed of corrosion usually increases with the heat; also a few experiments have been published (Gmelin's "Handbook of Chemistry," vol. i. p. 375) showing that changes of electrical state occur in metals under such circumstances; but a further examination of the relations of the temperature and chemical change to the electrical state has not, that I am aware, yet been made.

In an investigation on the development of electric currents by unequally heated metals in liquids (*Phil. Mag.* 1857, vol. xiii. p. 1), I found that hot platinum was electro-negative to cold platinum in liquids of acid reaction, and positive to it in alkaline ones, provided in all cases chemical action was completely or sufficiently excluded. In the present experiments I have endeavoured to ascertain what electrical changes are produced in cases where chemical action more freely occurs, and I have therefore employed not platinum plate, but plates composed of a metal (copper) which is more easily corroded.

The following are the general results arrived at.

The chief fact brought out conspicuously by the experiments with copper dishes is, that in many cases an increase of chemical action produced by heat instead of making the hot metal electro-positive makes it considerably negative.

The results show that hot copper was positive to cold copper in the following liquids:—hydrochloric, hydrocyanic, boracic, and tribasic ortho-phosphoric acids; chloride of copper (weak solution); chloride of cobalt; chloride of manganese; chromic acid; chloride of chromium; sulphate of zinc (weak solution); sulphate of magnesia; chloride of calcium; nitrate and chloride of strontium; chloride of barium; nitrate of sodium (strong solution); chloride, iodide, carbonate, and biborate of sodium; sulphate of sodium (strong solution); tribasic phosphate of sodium; nitrate, chloride, and chlorate of potassium; bromide of potassium (strong solution); iodide of potassium (strong solution); carbonate, acid carbonate, and bichromate of potassium; aqueous ammonia; chloride of ammonium; cyanide and ferrocyanide of potassium; acetate of zinc; and acetate of sodium. And negative in the following ones:—nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids; ferrous sulphate; chloride of copper (strong solution); sulphate of copper; sulphate of zinc (strong solution); nitrate and iodide of sodium (weak solutions); bromide and iodide of potassium (weak solutions); iodate of potassium; chrome alum; nitrate of ammonium; oxalic, acetic, tartaric, and citric acids. The number of liquids in which hot copper was positive was thirty-six, and those in which it was negative was twenty.

In several instances where the hot metal was negative with a weak solution, it became positive with a strong one; for instance, with sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide, and iodide of potassium; but with chloride of copper the reverse occurred. These results may be connected with the fact that in weak neutral solutions the chemical action is generally the most feeble, and therefore interferes the least with the direct influence of the heat in producing electric currents.

The influence of free hydrochloric, hydrocyanic, boracic, ortho-phosphoric, and chromic acids, was to make the hot copper positive; whilst that of nitric, chloric, hydrobromic, hydrofluosilicic, sulphuric, and some of the organic acids was to make it negative.

In consequence probably of the small amount of interference by chemical action in solutions of oxalic, acetic, tartaric, and citric acids, the direct influence of the heat made the copper negative, similar to its influence on platinum, in all acid liquids which do not attack that metal.

The nature of the acid in a salt appears to exert much more influence than that of the base on the direction of the current; for instance, in nearly all chlorides, including those of a considerable variety of bases, hot copper was positive, probably because copper is more readily attacked by acids than by bases.

In all decidedly alkaline liquids the hot copper was positive; this is similar to the behaviour of platinum in such solutions, and is probably due to the same course, viz. the direct influence of the heat, as well as to chemical action.

The results also show that the quantity of the current obtained with any given liquid generally increases with the number of molecules of the substance contained in the solution; in some cases, however, as with sulphuric acid, carbonate of potossium, chloride of ammonium, and acetate of zinc, there was a limit to this increase; and beyond that limit the quantity of the current decreased up to the point of saturation of the liquid.

In the great majority of cases the value of the deflection increased much more rapidly than the strength of the solution, particularly with solutions of sulphate of magnesia, and also of hydrochloric acid and of chloride of sodium, probably because two causes operated, viz. increased strength of solution and diminished resistance; in a very few cases, however, the opposite result took place, as with solutions of chloride and nitrate of strontium.

Inversions of the direction of the deflection by difference of strength of the liquid occurred with solutions of chloride of copper, sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide, and iodide of potassium.

Irregularities of the amount of deflection were very apt to take place with liquids which gave strong deflections, or which acted much upon the copper plates (for instance, nitric acid), especially if bubbles of air remained under the plates, or the dishes were wetted on their side above the liquid by the solution.

In certain acid liquids, viz., nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids, the hot copper was strongly negative (notwithstanding the chemical action upon it was distinct, and in some cases even strong); this is similar to the electrical behaviour of platinum in such liquids, and may be attributed either to the more direct influence of the heat alone (such as occurs with platinum plates), or to a different influence of the chemical action produced by the heat. Both these causes probably operate in such cases.

It is probable that in all cases where the hot copper was positive in liquids of strongly acid reaction, the positive condition was due to chemical action alone.

With some liquids, especially with solutions of hydrocyanic, boracic, acetic, tartaric, and citric acids, the deflections were very feeble, and the chemical action on the plates not perceptible; whilst with others, such as nitric and chloric acid, solutions of the chlorides of strontium, sodium, potassium, and ammonium, and of carbonate, acid carbonate, and cyanide of potassium, the deflections were considerable, and the chemical action distinct, and in some cases strong. In none of the liquids (except hydrobromic and chromic acids) did the hot plates appear to be *less* stained or corroded than the cold one; probably, in all cases, it was the most corroded, although in some cases the corrosion was not perceptible.

The amount of deflection was not always proportionate to the amount of chemical action; for instance, with solutions of chloride of copper and iodate of potassium there was considerable corrosion, but only feeble currents, probably because the plates became covered with a badly conducting film, whilst with hydrochloric acid, chloride of cobalt, chloride of manganese, and nitrate of potassium, the reverse occurred.

I consider the currents in all these experiments of difference of temperature to be due either (1) to the direct influence of heat, the effect of which is to make the hot copper negative in acid liquids and positive in alkaline ones (see *Phil. Mag.* 1857, vol. xiii. p. 1); (2) to chemical action, which sometimes overpowers the direct influence of heat and reverses the effect; or (3) to both these influences combined. The more ultimate cause, however, of the phenomena in these causes must be sought for in the *molecular movements* produced by heat in the metals and liquids.

The current obtained with copper plates were no doubt influenced in their amounts (if not also in their direction) by the oxidising action of the air upon the liquid and metal at their line of mutual contact, for we know that metals in contact with liquids oxidise much more quickly if oxygen has access to their wet surfaces; and the currents were also influenced by the action of unequal temperature upon the air-contact line, for we know that wet metals oxidise still more rapidly if heat is applied.

General Conclusion.—The electric currents produced by the direct influence of unequal temperature or friction of platinum or copper electrodes, in conducting liquids which do not act chemically upon those metals, have their origin in temporary changes of cohesion of the layers of metal and liquid which are in immediate and mutual contact, and may be considered as a very delicate test of the kind and amount of temporary molecular movements produced by those causes.

"Further Experiments on the effect of Diet and Exercise on the Elimination of Nitrogen," by E. A. Parkes, M.D., F.R.S.

"Magnetic Observations made during a Voyage to the North of Europe and the Coasts of the Arctic Sea in the Summer of 1870," by Capt. Ivad Belavenetz, I.R.N., Director of the Imperial Magnetic Observatory, Cronstadt.

"On the Mutual Relations of the Apex Cardiograph and the Radial Sphygmograph Trace," by A. H. Garrod, of St. John's College, Cambridge.

Geological Society, February 22.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Mr. John Thornton Harrison, C.E., and Mr. M. Hawkins Johnson, were elected Fellows of the Society.—The following communications were read:—1. "On supposed Borings of Lithodomous Mollusca," by Sir W. C. Trevelyan, Bart., M.A., F.G.S. The author referred to Mr. Mackintosh's paper on this subject (*Q. J. G. S.* vol. xxv. p. 280), and stated his conviction, from examination of specimens, that the holes in question are the work of *Helices*, or other terrestrial Mollusca. He ascribed the same origin to the so-called "*Pholas*-borings" in the limestone at Orme's Head and elsewhere. He considered length of time to be a necessary element in the formation of these holes. The author also remarked that he had suggested a glacial origin for the terminal curvature of the laminæ of slate-rocks as early as 1849. Mr. Gwyn Jeffreys read extracts from a work published by the Rev. Mr. Hodgson in 1827, on the Natural History of Northumberland, in which these borings in limestone were referred to the action of snails. Mr. Jeffreys considered the foot to be the sole instrument employed by the boring Mollusca in excavating their burrows. He exhibited specimens of Lias from Lyme Regis perforated by *Pholas*, and of hard limestone from Malta perforated by *Lithodomus*, and remarked, in connection with the notion that asperities on the shell might be boring agents, that the shell of *Lithodomus* is perfectly smooth. Prof. Ramsay mentioned that he had seen *Helices* taken out of these holes at Tenby by Dr. Buckland, who believed that the snails effected the perforations by the agency of an acid. Mr. Charlesworth thought that if so much uncertainty could prevail upon such a subject, it threw great doubt upon some of the grandest generalisations of geology. He referred to the evidence connected with the glaciation of the Great Orme's Head, in which the origin of the perforations under discussion was of much importance, Mr. Darbyshire maintaining that they were the work of *Pholades*, while Mr. Bonney asserted that they were produced by snails. In the same way the origin of the celebrated borings in the Temple of Jupiter Serapis might be disputed, and the generalisation founded upon it rendered doubtful. Mr. Charlesworth noticed the necessarily small proportion of borers to the whole snail population of Britain, and remarked especially upon the absence of perforations in the chalk districts. He considered that repeated observations were necessary before this snail-engineering could be admitted, and suggested a systematic course of experiments. Mr. Boyd Dawkins suggested that the carbonic acid exhaled by snails in respiration might act upon limestones, and remarked that chalk weathers too rapidly to preserve the excavations.—2. "On the probable Cause, Date, and Duration of the Glacial Epoch of Geology," by Lieut.-Col. Drayson, R.A. In this paper the author started from the fact that the pole of the ecliptic could not be the centre of polar motion, as the pole varied in its distance from that centre. He indicated the curve which the pole did trace, and this curve was such as to give for the date 13,000 B.C. a climate very cold in winter, and very hot in summer for each hemisphere. The duration of the glacial epoch he fixed at about 16,000 years. The calculations resulting from this movement were stated to agree accurately with observation. Prof. Ramsay inquired whether the author's theory involved the recurrence of glacial epochs, and whether he considered the course of phenomena to be constant in early astronomical epochs. Rev. Osmond Fisher inquired whether the theory was founded on observed facts, or whether it was a purely physical theory. He also asked whether the line representing the change in the direction of the pole formed a re-entering curve, and whether the theory would account for the climate of Greenland in Miocene times. He suggested changes in the form of the earth which must have affected the direction of its axis. The President remarked upon the difficulty that arose from astronomical theories differing so much among themselves. He referred particularly to Adhémar's theory, and remarked that the difficulty connected with it is, that it invokes a recurrent cause, which must produce similar effects every 21,000 years, whilst there is very little evidence of glacial action during the whole long period of the Tertiary epoch. The author, in reply, stated that he could not go back beyond 30,000 years, but that he thought glacial conditions must recur. He had not astronomical data beyond 2,500 years, and these were very vague. The motion would be the same in kind, but uncertain in degree. His theory was based entirely upon observed facts. In laying down the curve, he considered it safe to go as far as the semicircle, as he had observations covering 40°; but he could not say whether the curve would be a re-entering one, although it showed a tendency that way, and would certainly be very nearly so. With regard to the change of climate of Greenland, as evidenced by its Miocene flora, he was not sufficiently versed in botany to pronounce an opinion. He remarked, in conclusion, that the distance of a planetary body from the sun did not seem to affect climate, and stated that Venus is at present suffering under a most severe glacial epoch.—3. "On Allophane and an allied Mineral found at Northampton," by Mr. W. D. Herman. In this paper the author gave analyses of an amorphous, translucent, reddish-yellow mineral, found incrusting sandstone in the ironstones of the Northampton sands, the comparison of which with Mr. Northcote's analysis of allophane from Charlton leads him to infer the identity of the two minerals. He also noticed a soft white substance found in certain joints in a section of the Northampton sand, and also referred to allophane by the late Dr. Berrell, who analysed it. This substance was said to occur not unfrequently in the inferior oolite of the Midland Counties. By analysis, it was shown to agree nearly with Samoite and Halloysite. Mr. David Forbes stated that he had found phosphoric acid in the first-mentioned mineral, which was perhaps the cause of its lustre. The mineral was probably not pure allophane. Prof. Morris suggested a chemical and microscopical examination of the strata above the places in which these minerals occur, which would probably reveal the conditions under which they have been formed. They were probably produced by the decomposition of silicates in the overlying rocks during the percolation of water. This applied also to the Charlton locality. Mr. Carruthers mentioned that allophane often fills the inflorescence of the Cycads of the Yorkshire Oolite, entirely destroying the vegetable structure, and that it also occurs in clay nodules from the coal-measures. Mr. Carruthers suggested that the decomposition of vegetable matter in clays might aid in the production of the mineral. —4. "Notes on the Peat and underlying Beds observed in the construction of the Albert Dock, Hull," by J. C. Hawkshaw, Esq., M.A., F.G.S. The Albert Dock is situated on the foreshore of the River Humber. The excavation for the dock extended over an area of about thirty acres, and they were carried down to a depth varying from 8 feet to 27 feet below low water of spring-tides. Beneath the more modern deposits of Humber silt a bed of peat, Hessle Clay, Hessle Sand, and purple clay, were successively met with. The peat was found at the west end of the Dock at the level of low water; at the east end the bed dipped so that the upper surface was found at eight feet below the level of low water. In the peat were found the remains of a fire, which the writer attributed to human agency. Oak-trees of large size were imbedded in the peat, some of which had grown where they were found, as was shown by the stools remaining with the roots penetrating the Boulder-clay beneath. In one oak-tree, five feet in diameter, a hole was found filled with acorns and nuts. Many of the nuts were broken open at the ends, and had evidently formed part of the store of a squirrel. Remains of Coleoptera were found, and one horn-core of a *Bos*. The excavation did not extend below the upper parts of the purple clay. Some of the borings, however, penetrated the chalk at a depth 85 feet below low water level, passing through a bed of sand 16 feet thick below the purple clay. Several thousand cubic yards of this sand were brought up into the foundations by springs of water which flowed up through old bore-holes. The abstraction of this sand from beneath the clay-bed caused it to subside many feet. The writer thinks that analogous subsidences may take place from natural causes; for instance, where large springs occur in tidal rivers. Two sections exhibited showed the beds above the chalk for a distance of rather more than a mile along the foreshore. The Hessle Sand was shown to thin out to the westward. It does not, in the writer's opinion, increase in thickness in that direction, as it was shown to do in a section already published in the Proceedings of the Society. The President remarked upon the singularity of the occurrence of a bed of ashes at such a depth in these deposits. Mr. Gwyn Jeffreys referred to the President's paper on the Kelsey Hill beds, and remarked on some of the mollusca obtained by Mr. Hawkshaw. Mr. Boyd Dawkins mentioned the occurrence of a submarine forest on the coast of Somersetshire, forming a layer of peat, beneath which was a land-surface, on which the forest had grown, and in which flint-flakes were found at Portlock and Watchet on digging through the peat. He remarked on the depression of the coast of Somersetshire within the human period, and suggested that the forest at Hull may have been contemporaneous with that of Somersetshire. Prof. Morris inquired whether any trees or roots were found as when growing. The shells obtained were estuarine. Prof. Morris remarked on a submerged forest near Whittlesey, with terrestrial plants and freshwater shells imbedded in the overlying clay. The author, in reply, stated that the trees had fallen where they grew. The general appearance of things led him to the belief that the fire which had destroyed part of the forest was of human production. The following specimens were submitted to the meeting:—Specimens of Allophane, from the Northampton Sands; exhibited by Mr. F. V. Runler, F.G.S. Specimens of Websterite, from the junction of the Tertiaries and the Chalk, near Bromley, Kent, exhibited by Mr. W. Whitaker, F.G.S. Specimens from the Peat-beds at the Hull Albert

Dock; exhibited by Mr. J. C. Hawkshaw, F.G.S., in illustration of his paper. Specimens of *Pholas*-borings from Lyme Regis, and of *Lithodomus*-borings from Malta; exhibited by Mr. J. Gwyn Jeffreys, F.R.S.

Chemical Society, March 2.—Prof. Williamson, F.R.S., president, in the chair. The following gentlemen were elected Fellows:—G. D. Harding, W. H. Hudleston, A. H. Mason, J. J. Nicolson. The following papers were read:—"On the distillation and boiling point of glycerin," by T. Bolas. It is known that when glycerin is heated under the ordinary atmospheric pressure so much as to cause ebullition it is more or less decomposed. This decomposition may be, however, prevented by a reduction of the pressure in the apparatus employed. The author has in that way found that pure glycerin boiled under a pressure of $12 \cdot 5^{mm}$ at $179 \cdot 5^{\circ}$ C and under a pressure of 50^{m} at about 210° C.—"On the action of Heat on Silver Nitrite," by Dr. E. Divers. The products of this action consist principally of silver nitrate, reguline silver, and oxides of nitrogen. But the relative proportions of the quantities of these substances to each other, and consequently the composition also of the gaseous matter, vary considerably in different experiments. When the nitrite is heated in an open vessel over a lamp or in an oven at any temperature between 85° and 140° C. the result of the operation may be represented by the equation $3\ NO_2\ Ag = N_2\ O_3 + Ag_2 + NO_3\ Ag$. When, instead of an open crucible, a closely covered one is used, so that the gaseous and fixed products of decomposition may be kept for a time in contact, the ultimate change effected in this way approaches, though not closely, to what is expressed by the equation $2\ NO_2\ Ag = NO + Ag + NO_3 Ag$. In a third series of experiments, where the nitrite was heated in a vessel only nearly closed, the facts observed show that there is a tendency to yield only metallic silver and nitrogen peroxide, thus: $NO_2\ Ag = Ag + NO_2$. From all his experiments Dr. Divers draws the conclusion that like other silver salts the nitrite splits up under the influence of heat into metallic silver and the acid radical or its components, and that silver nitrate, nitric oxide, and, perhaps, nitrous anhydride are formed only by secondary reactions. The fusion which occurs in the mass of heated nitrite so soon as it has undergone some oxidation causes the author to throw out the suggestion that the nitrate formed perhaps combines with the nitrite to a nitrite-nitrate or hyponitrate.—After the reading of the above papers Dr. Gladstone communicated some remarks on the "Relations of Chemical Reaction and Time." He had instituted most varied experiments bearing on this subject, and, in briefly mentioning some of them, he wished to call the attention of chemists to this wide field of inquiry. Hitherto experimentators seemed to have limited their observations to only the circumstances at, and the products with, which a chemical reaction begins and ends, all that happens between was left wholly unnoticed. How fruitful attention paid to the intermediate products of a reaction could be is seen in the beautiful results which Prof. Williamson had gained on his researches on Etherification. The President, Dr. Odling, Mr. Vernon Harcourt, and others concurred in Dr. Gladstone's view as to the importance of a closer study of this subject.

Anthropological Institute of Great Britain and Ireland, March 6th.—Dr. R. S. Charnock, Vice-president, in the chair.—The following new members were elected:—Messrs. C. P. L. Naidoo Garroo, Henry Cook, Joseph Sharpe, LL.D., Danby P. Fry, Charles Edward Moore, Jesse Tagg; and W. S. W. Vaux, F.R.S., an honorary member.—Colonel Lane Fox exhibited a flint implement from Honduras.—Mr. Edward Blyth exhibited some cloth from West Africa.—Mr. Josiah D. Harris read a letter from his son on some remains found in the Macabi Islands, Peru.—Mr. J. W. Jackson read a paper "On the Racial Aspects of the Franco-Prussian War." After some remarks on the Aryan and Semitic divisions of the so-called Caucasian race, the former being defined as the flower of a Turanian, and the latter of a Negroid root, the author said that in the present imperfect state of our knowledge, it was impossible to decide whether Europe or Asia should be regarded as the primal and appropriate habitat of the Aryan, although he inclined to the former hypothesis. Neither could we yet assign the date when, and the place where, the various sub-divisions of this great race originated, and so must be contented with the fact of finding Slavons, Iberians, Teutons, and Celts on their existing areas of occupation, when, like the flora and fauna that accompany them, they must be regarded as Telluric organs. From a rapid survey of the earlier periods of European history, it was shown that the Celtic area of Gaul and Britain must have been ethnically effected at the time of the Roman conquest, which civilised but did not physically regenerate the Provincials. This was effete at the Gothic conquest of the Empire, when the Gauls received a slight and imperfect, and the Britons an effectual, baptism of Teutonic bone and muscle. The result of this diversity of fortune is seen in the fact that France, which retained more of the refinement, and with this more of the corruptions, of classic culture than Britain, preceded the latter in the attainment of civilisation, and now, after some centuries of quasi-imperial leadership in literature, science, manners and taste, is once again sinking into national weakness as an inevitable result of racial exhaustion. Hence it is that she no longer produces masterminds in any department, not even in war. Where are the successors of Cuvier and La Place, of Corneille, Racine, and Voltaire? This ethnic collapse of France, however, does not necessarily imply a subsidence of the entire Celtic area of Western Europe, as Britain is still at her maximum of racial vigour, and, like Rome after the decadence of Greece, will probably inherit that portion of the mission of imperial leadership forfeited by her effete sister and former rival. The Germans cannot do this, having so recently attained to unification, and being consequently devoid of any great capital like London, which may serve as the future metropolis of cultivation. Their mental constitution is, moreover, not adequately synthetic for the mission of Imperial centrality, which must accordingly devolve on England, the geographical terminus of the great north-western march of empire from the Euprates to the Thames. Discussion having ensued, on the motion of Mr. Joseph Kaines, seconded by Capt. Pim, it was adjourned till the 20th instant.

Linnean Society, March 2.—Mr. G. Bentham, president, in the chair. The following papers were read:—On the Tamil Mames of Plants, by Rev. S. Mateer; Contributions towards a Knowledge of the *Curculionidæ*, by Mr. H. P. Pascoe.

Royal Institution of Great Britain, March 6.—Sir Henry Holland, Bart., M.D., F.R.S., president, in the chair. Messrs. W. Blenkin, J. Browning, E. Maynard Denny, F. A. Eck, Sir Frederick Elliot. K.C.M.G., Col. A. Lane Fox, Mr. P. Graham, Col. J. A. Grant, C.B., Mr. E. W. Grubbe, Dr. G. Harcourt, Capt. F. Helbert, Mr. G. W. Henderson, Mr. G. Middleton Keill, Dr. J. Kennedy, Mr. J. Macauley, Mr. K. R. Murchison, Mrs. Sheffield Neave, Mr. G. W. Royston Pigott, Mr. Eustratrios Ralli, Mr. F. S. Reilly, Mr. W. C. Roberts, Mr. W. Dehague Routh, Mrs. W. C. Smith, Mr. T. Sowerby, were elected members.

MANCHESTER

Literary and Philosophical Society, February 7.—Mr. E. W. Binney, F.R.S., president, in the chair. "On the Organisation of an Undescribed Verticillate Strobilus from the Lower Coal Measures of Lancashire," by Professor W. C. Williamson, F.R.S., &c.—"The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena, part ii.," by Prof. Osborne Reynolds, M.A.—"Further Experiments on the Effects of Cold upon Cast Iron," by Peter Spence, F.C.S., &c.

February 21.—Mr. E. W. Binney, F.R.S., president, in the chair. "The Overthrow of the Science of Electro-Dynamics," by John Hopkinson, D.Sc. In science no theory should be considered unquestionable and no man's work held sacred from attack, and our scientific periodicals should afford the freest scope to discussions no matter how hostile to established notions. Still, it is evident that the journals ought not to publish everything that may come to hand; they should at least take care that a hostile critic understands the meaning of what he criticises. Two papers appeared last month in the *Quarterly Journal of Science* and the *Chemical News* respectively, in which the author (the Rev. Mr. Highton) somewhat summarily disposes of the science of Thermodynamics, fancying he has disproved the equivalence of heat and work. I will only trouble you with one or two quotations with a view to support my opinion that the papers in question ought never to have been permitted to appear in any journal pretending to scientific position. In the *Chemical News*, p. 42, we find, speaking of Joule and Scoresby's experiments on electro-dynamic engines—"They say that 'the quantities of zinc consumed' (that is, respectively, when the engine is at rest and doing work) 'being as a to b, $(a - b)$ represents the quantity of heat converted by the engine into useful mechanical effect.' Therefore, since on the supposition of a mechanical equivalent of heat a grain of zinc consumed equals 158 foot pounds, if x = pounds raised a foot high per consumption of a grain of zinc in the battery,—

$$x = \frac{(a - b)\ 158}{a}$$

Hence the authors draw the conclusion :—'Therefore when b vanishes, or becomes infinitely small, the economical duty is a maximum.' Certainly this is a most startling result; that the maximum of work should be done when no zinc at all is consumed." The last sentence is a mis-statement of the conclusions of Joule and Scoresby's paper, in which (*Philosophical Magazine*, vol. 28, p. 451) it is stated that "the economical duty will be a maximum when b vanishes or becomes infinitely small in comparison with a. In this case $x = 158$, while the power of the engine will become infinitely small with regard to work performed in a given time." Comparing the phrases "economical duty" and "maximum of work," as he uses them, he evidently confuses the duty of an engine with the whole work done by it. A little further on we have—"They calculate the maximum theoretical power of a grain of zinc to be 158 foot pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than $\frac{1}{4}$ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a - b)$ used to produce work W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electromagnets passed $\frac{1}{8}$ of an inch from them, doubtless W would be greater, but so also would $(a - b)$, and it does not follow that $\frac{W}{(a-b)}$ with which we are concerned would be at all changed. What becomes then of the dogmatic assertion that the duty of a grain of zinc could be increased tenfold? Now let us turn to the paper in the *Quarterly Journal*. Here we may find enough in one article for our present purpose, taking chap. ii. art. 2,—"Why are we forced to suppose . . . . . . that the same amount of fuel produces the same amount of energy, whether it is consumed in the steam-engine, the horse . . . . . . the gnat? At any rate, we may observe that the very phrase is certainly a misnomer, and a misnomer of such a kind as to have a fatal effect in producing a false conception of things. For mechanical energy just as often produces cold as heat; it may produce either heat or cold, or neither. In fact, as a general rule, though with notable exceptions, every pushing or compressing force produces heat, and every pulling or expanding force cold. Place a weight on a pillar, and the weight produces heat in the pillar; hang it on a wire and it cools the wire. In exactly the same way, in a fire-syringe use force to press down the piston, it produces heat—heat enough to kindle tinder; but use the same force to pull up the piston, and it produces cold." Surely this is enough to show that the author's notions of what he is attacking are, to say the least of it, shallow; for what he quotes as paradoxes are simple deductions from the two laws of Thermodynamics. That a wire is cooled by stretching follows from the fact that heat expands it. In the case of the fire-syringe the case is simpler. The working body is the air in the syringe; on pulling up the piston this air does work, and therefore uses up heat and is cooled. Mr. Highton seems to imagine that because the arm of the experimenter does work, it is done *on the air in the syringe*, whereas this column of air and the observer are really co-workers in raising the air *external* to the cylinder. To point out all the fallacies of these papers in detail would take too much of your time. My object was to show that if the *Quarterly Journal of Science* and the *Chemical News* are to represent scientific opinion with any degree of truth, they would do well to use a little discretion as to what they print.—"Remarks on Mr. Spence's Experiments on the Effects of Cold on the strength of Cast Iron," by Joseph Baxendell, F.R.A.S. This was in reply to an assertion made by Mr. Spence the previous week that "he had so much confidence in the experiments then detailed, that he had no hesitation in giving it as an ascertained law that a specimen of cast iron having at 70° F. a given power of resistance to transverse strain will, on its temperature being reduced to zero, have that power increased by 3 per cent." Taking all the experiments on the effect of cold on iron which have yet been brought before the Society, they can only be regarded as indicating that if any effect at all is produced, it is more apparent on iron of good quality than on inferior iron, but that its amount is so small as to be wholly inadequate to account for the railway and other accidents which have been attributed to it.—"Further Observations on the Strength of Garden Nails," by J. P. Joule, F.R.S., &c. Since communicating the paper on the Alleged Influence of Cold in giving brittleness to Iron, I have collated the results with cast-iron nails in order to show the range of strength in such specimens:

Height of Fall of Hammer.	Percentage of Fractures.
2 inches	0
$2\frac{1}{2}$,,	0
3 ,,	6 25
$3\frac{1}{2}$,,	23·5
4 ,,	30
$4\frac{1}{2}$,,	36·4
$5\frac{1}{2}$,,	37·5
$6\frac{1}{2}$,,	48
7 ,,	65·5
$7\frac{1}{2}$,,	62·3
$8\frac{1}{2}$,,	75
10 ,,	92·8

I chose the garden nails for experiment after some thought, as presenting a marked variety of metal in contrast with the iron and steel wire, tempered and untempered. I did not expect them to possess great strength, but having found them to require a heavier blow than I expected to fracture them, I have had the curiosity to make some experiments on them which may be interesting to the Society. I took pairs of the nails, placed them head to point parallel to each other, so that pressure applied in the middle by pincers sufficiently forcibly would fracture one of them. Paper slips were pasted on the edges of the nails, and their distances asunder measured by a microscope with micrometer eyepiece divided by lines corresponding to $\frac{1}{300}$ of an inch. Weights were gradually added to the lever of one arm of the pincers until fracture took place, which was always accompanied with a sharp report. The observed deflection or bending of the nails was taken continuously as the weights were laid on, and the calculation of what it would have been at the moment of rupture taken from the immediately preceding observations. The amount of deflection was almost exactly proportional to the weight laid on in each experiment.

No. of Experiment.	Length of Nail between Supports.	Breadth of Nail in Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. lbs.
1	1·05	0·13	0·127	·0062	145·5
2	1·1	0·114	0·125	·0067	141
3	1·1	0·120	0·115	·0090	171
4	1·08	0·111	0·106	·0073	142·5
5	1·12	0·122	0·145	·0098	189
6	1·06	0·138	0·120	·0087	184·5
7	1·08	0·150	0·118	·0095	201
Average	1·084	0·1264	0·1223	·0082	167·8

If we compare the above with Mr. Brockbank's experiments, we shall find, approximately, on reducing them to the dimensions he adopted, viz., three feet between supports and one inch section:—

	Breaking weight.	Deflection.
Mr. Brockbank's, with large bars...	860·7	·740
My own, with nails	2673·	1·106

The metal, in the form I used it, was therefore more than three times as strong as that of the large bars to resist a compressing and tensile force, while its extent of spring at the breaking weight was half as much again. Therefore, so far from being of inferior quality, it would sustain a very much heavier blow without fracture.—"On the Action of Sulphurous Acid on Phosphates," by Dr. B. W. Gerland.

Maidstone and Kent

Natural History and Philosophical Society, Feb. 20.—An aggregate meeting of the members of the Maidstone and Mid Kent Natural History Society was held at the Charles Museum, when Dr. Monckton, one of the vice-presidents, delivered a lecture on "The Metaphysics of Zoology."

Edinburgh

Royal Society, February 20.—Mr. W. F. Skene, V.P., in the chair. The following papers were communicated:—1. "On the Pentatonic character of Scottish Music," by the Hon. Lord Neaves. 2. "On the Motion of Solids in a Liquid," by Sir W. Thomson. 3. "Laboratory Notes on Thermo-electricity, and on Phyllotaxis," by Prof. Tait.

Paris

Academy of Sciences, January 27.—M. Janssen sent to the French Institute a letter to explain how his expedition failed, owing to the persistency of the clouds. He acknowledges fully the kind proposition of the Organising Committee to take steps to procure a *laissez-passer* from M. Bismarck on his behalf. In five hours M. Janssen proceeded from Paris to the mouth of the Loire, where he landed on the 2nd December at 11 o'clock in the forenoon. The journey was magnificent. M. Janssen travelled at a height of 1,100 metres at the beginning, but after sunrise his balloon elevated itself through the warmth of the sun, and he reached 2,000 metres without throwing out any ballast. M. Janssen has invented an instrument for helping aëronauts in the determination of their way along the earth. This contrivance, which was highly praised by M. Dumas. will be fully described.—M. Bazin presented a projectile gun which explodes in the air, where it is sent by a larger gun remaining on the ground. This projectile gun, when exploding at a certain point of the *trajectoire*, sends an explosive bullet with a new impulse. It is impossible to claim precision for such a projectile, which is called very properly *a double effect*, but the distance to which it reaches is increased, and the final bullet was sent to 7,919 metres, it appears with some effect. The experiment was tried against the Prussians during the last days of the siege.

February 21.—The sitting was very thinly attended by members. The public was more numerous. No scientific paper had yet resumed its publication. M. Faye presided over the sitting. M. Elie Beaumont and M. Dumas sat on the platform, reading over alternately the correspondence. An article on the "Duquesne" Expedition was sent by M. de Fonvielle. M. Elie de Beaumont, who had predicted that the "Duquesne" would go to Switzerland on account of its directing power, opposed the reading.—M. Stanislaus Meunier read a paper on the nature of meteorites, which, he thought, are evidently of astral nature. He is opposed to the theory of Schiaparelli, who accounted for them by supposing they are in some respects allied to comets.—M Chevreuil read over a letter from M. Vaillant, who hopes to be soon enabled to resume his seat.—M. Delaunay read the translation of a letter received from Prof. Piazzi Smyth, the director of the Observatory at Edinburgh. He noticed the admirable working of the International French Telegraphy. That service was transferred to Tours and from Tours to Bordeaux, where it is now working under M. Marié Davy's superintendence.—M. Ch. Saint Claire Deville reported that Dr. Berigny of Versailles had not interrupted for a single day his admirable series of observations kept regularly for 24 years. M. Renan, who is living at Vendôme, did not interrupt his observations, although he was several times arrested as a spy, but he was saved by his thermometer! The maximum of cold has reached $-16°$ at Montpellier, $-17°$ at Bordeaux, and $-3°$ at Perigueux. The winter was a sharp one. It was predicted by M. Renan, as belonging to a series of recurring sharp winters. This law of recurrence was published in *Comptes Rendus* eighteen months before war broke out.—News was circulated of other learned men amongst the members. M. Martillet remains at St. Germain as curator of the Prehistoric Museum. One new hall was opened during the Prussian occupation, the Prussian Emperor, Princes, and generals frequently visiting the galleries. M. de Verneuil has established himself at Duc de Broglie's castle in Normandy. He is a relative of the present French ambassador, who is very much interested in scientific matters. M. Janssen is at Bordeaux, and expected daily by his family.

DIARY

THURSDAY, March 9.

Royal Society, at 8 30.—Magnetic Observations made at Stonyhurst College Observatory, from 1863 to 1870: Rev. S. J. Perry.—Preliminary Notice on the Production of the Olefines from Paraffin by Distillation under Pressure: T. E. Thorpe and J. Young.—On the Action of Hydrobromic Acid on Codeia: Dr. C. R. A. Wright.
Society of Antiquaries, at 8.30.—On a probable allusion to the Chsritians in a passage of the Sixth Satire of Juvenal · Earl Stanhope, P.S.A.
London Mathematical Society, at 8.—Remarks on the Mathematical Classification of Physical Quantities: Dr. Clerk Maxwell, F.R.S.—On Skew Cubics: Prof. H. J. S. Smith, F.R.S.—Note on the History of Certain Formulæ in Spherical Trigonometry: I Todhunter, F.R.S
Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.
London Institution, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

FRIDAY, March 10.

Royal Astronomical Society, at 8.
Quekett Microscopical Club, at 8.
Royal Institution, at 9.—The latest Scientific Researches in the Mediterranean and Straits of Gibraltar: Dr. W. B. Carpenter, F.R.S.
Royal College of Surgeons, at 4—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, March 11.

Royal School of Mines, at 8.—Geology: Dr. Cobbold, F.R.S. (Swiney Course.)
Royal Institution, at 3.—Spirit of the Age: Mr. O'Neil.

SUNDAY, March 5.

Sunday Lecture Society, at 3.30.—On Ferns: Dr. Cobbold, F.R.S.

MONDAY, March 13.

Royal Geographical Society, at 8.30.
London Institution, at 4.—On Astronomy: R. A. Proctor, F.R A.S. (Educational Course)
Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, March 14.

Photographic Society, at 8.
Royal Institution, at 3.—Nutrition of Animals: Dr. Foster.

WEDNESDAY, March 15.

Society of Arts, at 8.—On the Different Methods of Extracting Sugar from Beet-root and Cane: Ferdinand Kohn.
Meteorological Society, at 7.—Evaporation, Rainfall, and Elastic Force of Vapour: J. R. Mann.
Royal Society of Literature, at 8.30.
Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, March 16.

Royal Society, at 8.30.
Society of Antiquaries, at 8.30.
Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.
Linnean Society, at 8.
Chemical Society, at 8.

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