THURSDAY, APRIL 2, 1874

MARY SOMERVILLE

Personal Recollections, from Early Life to Old Age, of Mary Somerville, with Selections from her Correspondence. By her daughter, Martha Somerville. (London : John Murray, Albemarle Street, 1873.)

IT would have been a lasting blot upon the biographers of our time if such an illustrious woman as Mary Somerville—a woman unique, or almost unique from one point of view, though so beautifully womanly from others—had been allowed to pass from among us without a satisfactory memorial of her characteristic thoughts, conversation, and domestic life.

The "Personal Recollections of Mary Somerville" will not satisfy those readers who may have hoped to find in the autobiography of the author of the "Mechanism of the Heavens" and the "Connection of the Physical Sciences" any special expositions of Science or practical hints for a successful method of scientific training. The studied care with which Mrs. Somerville avoided bringing scientific questions prominently forward in conversation has been rigidly preserved in the story of her life, where little or nothing is said of the processes by which she attained so exceptional and distinguished a place in the world of Science, and only passing references are made to the extraordinary success that attended her self-acquired knowledge.

As the record of a life in which the fulfilment of all the natural and conventional claims upon a woman's time was combined with practical and theoretical pre-eminence in the most abstruse departments of physical inquiry, no book can, however, be more interesting and suggestive than this volume, in which the personal recollections of Mary Somerville are noted down for us by her own hand and that of her daughter. The story of her life has, moreover, the special interest that it may, with perhaps equal justice, be made to yield arguments for and against the claims advanced for women's equality to men in intellectual capacity. The champions of such pretensions may well point with triumph to her achievements in the higher branches of analytical geometry. Where, indeed, could another instance be found of a person who, after having had to ask, at the age of 16, the meaning of "the *x's* and *y's* mixed with strange lines," which first excited her notice in the pages of a magazine of fashion, should unaided—for she was in all essentials a self-taught mathematician—have been able to begin her career as an author by producing a work like the "Mechanism of the Heavens," which still ranks as the best exposition that we possess of Laplace's "Mécanique celeste"?

The approval which this work won from the first mathematicians and physicists of the day seems to have surprised no one more thoroughly than the writer herself, who had carried on her studies with such unostentatious industry within her own home, that she was scarcely conscious how exceptional were her attainments. And it may be fairly said of her that by the publication of the "Mechanism of the Heavens," in 1831, she suddenly awoke, at the advanced age of 51, to find herself famous, the one woman of her time, and perhaps of all times, for whatever may be the advantages which are now happily being placed within the reach of women for benefiting by high scientific training, we can scarcely expect to meet with many Mary Somervilles. Her genius was unique of its kind, and wholly exceptional, and this fact seems to have been frankly and generously admitted by all who came in contact with her, who were capable of measuring the depths of her knowledge. But so successfully did she conceal her learning under a delicate feminine exterior, a shy manner, and the practical qualities of an efficient mistress of a household, coupled with the graceful, artistic accomplishments of an elegant woman of the world, that ordinary visitors, who had sought her as a prodigy, came away disappointed that she looked and behaved like any other materfamilias, and talked just like other people. No one, therefore, could possibly have afforded a stronger refutation of the axiom, almost universally upheld half a century ago, that scientific acquirements of a high order are wholly incompatible with the proper exercise of the natural and ascribed functions of a woman's destiny. And accordingly the name of Mary Somerville has always been a tower of strength to the promoters of woman's emancipation from the enactments established by man for her exclusion from the enjoyment of the various social, legal, intellectual, and other privileges, of which he has so long had the virtual monopoly.

Her fame did not rest only on her first book—in which she had verified Laplace's own testimony, that she was the only woman who had ever read his works, which, moreover, were not understood by twenty men in France as well as she understood them—for the list of her writings includes, in addition to those more generally known from their semi-popular form as the "Connection of the Physical Sciences," "Physical Geography," &c.; monographs on the Analytical Attraction of Spheroids, the Form and Rotation of the Earth, the Tides of the Ocean and Atmosphere, and, besides many others of equally abstruse nature, a treatise of 246 pages on Curves and Surfaces of the Second and Higher Orders, which she herself tells us she wrote *con amore*, to fill up her morning-hours while spending her winter in Southern Italy. A truly marvellous *catalogue raisonné* of the results of a woman's knowledge and industry!

It is impossible to speak too highly of the sympathy and hearty recognition of the value of her labours that Mrs. Somerville received from all the most eminent of her contemporaries. In France, Laplace, La Croix, Biot Poisson, Arago, Ampère, and many others welcomed her as one of themselves; in England she enjoyed the intimate friendship of the Herschels, Lord Brougham, Professors Whewell, Peacock, Babbage, Sedgwick, and Brewster, and others pre-eminent in science; and surely no greater tribute could have been paid to the exceptional intellectual superiority of Mary Somerville than that rendered by the University of Cambridge when, at the earnest recommendation of Profs. Whewell and Peacock, her "Mechanism of the Heavens" was introduced into the University studies as "essential to those students who aspire to the highest places in the examinations."

It would not be easy to over-estimate the extent and degree in which Mrs. Somerville's acquirements differed from those of women generally at that period; but then it must be admitted that it is precisely through this exceptional character of her attainments that her case may be adduced in proof of the rule that women are not by nature adapted for studies which involve the higher processes of induction and analysis. If such powers as hers had been more generally granted to women, why is she the only woman on record amongst us who has exhibited them?

There was nothing exceptional in her bringing up, or her opportunities. In fact, no woman of her time and station could have had a more typical experience of life than she had. She was born nearly a century ago, in 1780, and spent her childhood and youth in Scotland, within an ordinary circle of the upper middle-class society of her age and country, and therefore very closely circumscribed by lines of defence against innovations and social changes of any kind. Her father, Captain Fairfax (a brave officer who commanded the *Repulse* during the war), received the news of her having taught herself the first six books of Euclid with the remark—"We must put a stop to this, or we shall have Mary in a strait-jacket one of these days. There was 'X.,' who went raving mad about the longitude!" This gallant captain was, moreover, a genuine good Tory, who took decided views in regard to all questions involving a departure from established precedents, and when his young daughter ventured to express her admiration for the short-cut hair, which was then the badge of a Liberal in politics, he exclaimed, "By G—, when a man cuts off his queue the head should go with it." Her mother, who found all her intellectual cravings amply satisfied with the reading of her Bible, a volume of sermons, and a stray copy of a newspaper, fully concurred in her husband's views of the education suited to young women, and was at great pains to thwart her daughter's unladylike taste for pursuits regarded at the time as the exclusive privileges of men, and to keep her mind and hands closely fettered by the bonds of a household possessed of very limited pecuniary means. The parents of the future authoress of the "Connection of the Physical Sciences" did not, therefore, afford her special facilities for mastering any of those higher branches of knowledge for which she seems to have had an instinctive yearning almost before she knew their names. Indeed, at the age of 10, Mary Fairfax was still a little ignorant savage, running wild over the hills and braes of Burntisland, and scarcely knowing her letters; yet before she was 13 she had surreptitiously possessed herself of some of her brother's books and taught herself Latin enough to construe "Cæsar's Commentaries." At that time she scarcely knew the simplest processes of arithmetic, but at the age of 17 the possession of a copy of "Bonnycastle's Algebra," procured for her by her uncle and future father-in-law, Dr. Sutherland—the only one of her relations who did not absolutely oppose her efforts to acquire knowledge—enabled her to solve the mystery of the X's and Y's; and from that hour till the day of her death, mathematics, in one shape or other, may be said to have formed part of her daily existence. For more than half a century they were the staple occupation of her morning hours when the duties of her house and family had been disposed of; at a very advanced age she began and mastered the study of Quaternions, and other forms of modern mathematics, and at 89 she "still retained facility in the calculus."

The restless activity of her intellect had indeed never slumbered. When she received her first lessons in painting and music, she had begun at once to try and trace out the scientific principles on which these arts are based, and never rested till she had gained some knowledge of the laws of perspective and of the theory of colour, and had learnt to tune her own instruments. In later years she may be said to have been always in the van of discovery—not indeed as an originator but as the readiest and aptest of students—and from the time when Young showed her how he conducted the experiments by which he claimed to have discovered the undulatory theory of light, and Wollaston made her one of the very first witnesses of the seven dark lines crossing the solar spectrum, whose detection laid the basis of some of the most won derful cosmical discoveries of this or any age, Mary Somerville, to the last day of her long life of nearly 92 years, followed with quick and appreciative understanding every step in the advance of modern research. Age could not quench the fire of her intellect, and even in her 92nd year, when the Blue Peter, as she quaintly remarks, had long been flying at her foremast, and she had soon to expect the signal for sailing, she could interest herself in the phenomena of volcanic eruption, speculate on their effects, and follow with lively sympathy the progress of scientific inquiry, and the issues of passing events.

In reading the personal recollections of this wonderful woman nothing strikes one more than the ordinary and even commonplace conditions under which her great intellect advanced to maturity. In her case the only exceptional features were her natural gifts and her perseverance in cultivating them; and this is precisely the point that should not be lost sight of. Mary Somerville will always present a noble instance of what a woman has been capable of achieving, but it would be straining the argument too far to say that we are justified from her special case to draw general conclusions in regard to women's aptitude for the study of the higher forms of physical science.

EXTINCT VERTEBRATE FAUNA OF THE UNITED STATES

Contributions to the Extinct Vertebrate Fauna of the Western Territories of the United States. By Prof. Joseph Leidy. (Government Printing Office, Washington.)

THIS important volume is the first of five which are to form the "Report of the United States Geologica Survey," and it will be supplemented by a memoir, embracing the same subjects, by Prof. Cope.

The large field for palæontological work recently opened up in the Western Territory of the United States has been as fruitful in the introduction of new and unexpected forms of extinct vertebrate life, as that so ably worked out by Cuvier, the Paris basin. By the establishment of a military station at Fort Bridger, opportunities have been afforded to geologists, which the offensive attitude of the Indian tribes had previously deferred, rendering inaccessible a district, the richness of whose past fauna must have been as remarkable as is its present desolation.

Fort Bridger is a military post about 100 miles E.N.E. of Great Salt Lake City, in the south-west corner of the Wyoming Territory. The valley in which it is situated stands nearly 7,000 ft. above the level of the sea at the base of the Uintah Mountains, which form its southern boundary; the Wind River Range defining it on the north-east, and the Wahsatch Mountains on the west separating it from the Great Salt Lake. The enclosed plain is evidently the remains of an extensive fresh-water lake, which in the Eocene period must have abounded with animal life, and whose borders must have been the haunts of animals, both huge and small, which lived and died by its marshy banks. Green River now runs through the plain, and it, with its smaller tributaries, by cutting up the easily eroded deposit, produces a scenery of a most peculiar character, consisting of flat-topped hills and cliffs, with perpendicular sides, and often most grotesque proportions. Those of the water-courses which do not dry up during the summer months are fringed with vegetation, such as cotton-wood, willow and aspen trees, but most of the country is treeless and barren, reminding the spectator more of the ruins of a colossal city, than of any other existing scenery.

The flat-topped hills, table-lands, and scarp-rocks are termed "buttes," and the fossils are generally found at their bases, having fallen there from the gradual atmospheric disintegration of their sides, along with the *débris* of the deposits. The fossils consist mostly of the bones and teeth of vertebrata, together with lacustrine shells. The bones are generally black or brownish, sometimes yellowish; they are generally distorted and much broken, except the small ones, such as those of the carpus and tarsus. They do not withstand the action of the air at all well.

The remains of mammals, which are very abundant, are mostly of genera which are not found elsewhere. Several, however, approach those of the Paris tertiary basin. The odd-toed Ungulata, or Perissodactyla, are particularly numerous, whilst even-toed Ungulate or Artiodactyla are as remarkably few. True Proboscidians are not found, but if Prof. Marsh is correct in placing Dinoceras in an order by itself, animals equally huge, of an independent type, were far from uncommon. Most of the other mammalian orders are most probably represented, though much has yet to be done in the identification of specimens.

Prof. Leidy has not yet seen any remains of bird, but we, some time ago, called attention to Prof. Marsh's discovery of *Odontornis*, a bird with well-developed teeth in both jaws; quite different from *Odontopteryx* of Owen, which has not true teeth, but teeth-like processes of the jaws.

The remains of turtles are most numerous; many of them were aquatic, and some belong to genera which cannot be distinguished from those now existing. What is also particularly interesting to note is that the remains of Crocodilia, which are not very abundant, are all derived from species of true *Crocodilus*, the old-world form, with the lower so-called "canines" fitting into a notch in the upper jaw, and not from *Alligator*, the genus which is now found in the Mississippi and its neighbourhood, with the lower "canine" fitting into a maxillary socket.

From the large amount of material which has passed through his hands, most of which is deposited in the Museum of the Academy of Natural Science of Philadelphia, there are some types of animals which Prof. Leidy has been able to work out in sufficient detail to make his results of general interest. Perhaps the most complete of these is *Palæosyops*, a perissodactylate Ungulate, of about the size of the Tapir, portions of the bones and nearly complete sets of the teeth of which have been several times discovered. The dental formula was complete, the typical forty-four teeth being present, all close together in the usual numbers, namely i. $\frac{3-3}{3-3}$, c. $\frac{1-1}{1-1}$, p.m. $\frac{4-4}{4-4}$, m. $\frac{3-3}{3-3} = 44$. The canines were peculiarly large, having much the same proportions as in an average carnivorous animal, like the bear. The molars have a resemblance to *Palæotherium*, the inner lobes of the crowns of the upper molars being, however, more completely isolated. There was a third trochanter to the femur, and three toes, as in the Tapir, were present on the hind feet. *Palæosyops paludosus* is the most common species. It is not known whether the neck was long and curved, as that of *Palæotherium* is now found to have been, or whether it was short and straight, as in the Tapirs. *Limnohyus* is a closely allied genus, named by Prof. Marsh.

Another perissodactylate, *Hyrachyus*, closely resembles *Lophiodon* of France, but has an extra premolar in the lower jaw, and a lobe less in the last lower true molar.

Perhaps *Trogosus* is one of the most interesting of the extinct mammals from the "Mauvaises Terres." It is also perissodactylate, and slightly smaller than the common pig. Its dentition would almost lead to the idea that the long-missing form which may be supposed to connect the Ungulata with the Rodentia, has at last been discovered; for with the usual complement of molar teeth there are no canines, and a huge pair of rodent-like incisors, which, in the lower jaw at least, had an intermediate pair of very small teeth. The large incisors had *persistent pulps*, and were formed in part of a circle; they wore down obliquely, in the same way as in the Cavies; were grooved longitudinally, somewhat as in *Aulacodus*, and were covered with enamel on the anterior surfaces only.

It is not to be wondered at, when small fragments of the skull of an animal so unknown and aberrant as *Uintatherium* (or *Dinoceras* of Marsh) were obtained, that each piece should have been referred to a separate genus and species, and Prof. Leidy, in the latter part of his memoir, puts together, as parts of *Uintatherium*, the tusks, horn-cores, &c., as parts of one and the same animal, which he had considered to be portions of different animals in the earlier part of his work, and which he had no reason for associating until Prof. Marsh had described the complete skull of *Dinoceras mirabilis*, which we figured some time ago. As we also mentioned at the time, Prof. Cope has also named this genus *Eobasileus* and *Loxolophodon*.

Besides the above mentioned, most characteristic forms, some from other territory strata west of the Mississippi River, are described—ungulate, rodent, and carnivorous—many of which are intimately related to those of the Paris basin, and throw further light on them. Prof. Leidy also figures and describes several of the Chelonia and other reptiles which come from the same locality.

The above notice of the results arrived at by American men of Science show that they deserve the careful study of English palæontolologists and geologists, as they have already thrown great light on the fauna of the Tertiary period, and give promise of adding much more to our knowledge of that epoch, so important to the student of the anatomy and classification of the higher vertebrata.

OUR BOOK SHELF

The Laboratory Guide, a Manual of Practical Chemistry for Colleges and Schools, specially arranged for Agricultural Students. By Arthur Herbert Church, M.A. (London: Van Voorst, 1874).

TEACHERS of chemistry will be glad to welcome the third edition of Prof. Church's "Guide," to which much new matter has been added. Being specially adapted for students of agricultural chemistry, the book is necessarily somewhat limited in its scope, but the amount of information conveyed within the small compass of 215 pages is very great, and is moreover lucid and accurate. The book is divided into three portions, the first treating of a chemical manipulation, the second of qualitative analysis, and the third of quantitative analysis. The author's preliminary remarks upon manipulation are excellent, and should be graven upon the mind of every chemical student. In the "Introduction" we are told that the student "must never forget that the experiment is the means, not the end. . . . . Merely to make a coloured precipitate or a flash of bright flame is not the end of experimenting."

These remarks are much to the purpose, and we commend them to the notice of chemists of older growth, as well as to beginners. The sudden introduction of equations on p. 8 without any previous explanation of the meaning of symbolic formulæ appears somewhat unsystematic, but the student is recommended by Prof. Church to attend some course of lectures on inorganic chemistry, and to study the corresponding chapters in Roscoe's Chemistry, at the same time that he is working through the "Guide." As the "Guide" is at present arranged, the student will find this absolutely necessary. The classification of the metals adopted by the author calls for remark—iron and manganese are classed as dyads and aluminium as a triad. Further on it is explained that this last metal is only a pseudo-triad, being in reality a tetrad. Why not class it with the tetrads at once? Hexad metals and pentad metals are ignored altogether, although manganese forms a hexafluoride, arsenic, and antimony, penta-haloid compounds, &c. We must protest also against the use of the words "vinculant," "vinculance," "univinculant," &c. No advantage is likely to accrue to the science from this new phraseology, and the terms "atomicity," "monatomic," "diatomic," &c., which are in general use, express the idea perfectly. The tables for qualitative analysis differ but little from those generally adopted. The quantitative processes for the analysis of natural products, soils, foods, &c., will be found very useful. In addition to the direct benefit arising from the issue of books like the present, there is an indirect benefit for which we ought to be also indebted to Prof. Church—we refer to the expulsion from the market of hastily compiled and inaccurate works by so-called "Science Teachers," such as it has been our duty to condemn on former occasions.

LETTERS TO THE EDITOR

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

Prof. Tait and Mr. Spencer

AS is shown by the passage from his *Thermodynamics* which he re-quotes, Prof. Tait holds that "Natural philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth."

I hold, on the contrary, that as there are *à priori* mathematical truths, the consciousness of which results, not from our individual experiences, but from the organized and inherited effects of ancestral experiences, received throughout an immeasurable past; so are there *à priori* physical truths, our consciousness of which has a like origin.

I have endeavoured to show that Prof. Tait himself, by saying of physical axioms that the appropriately-cultivated intelligence sees "*at once*" their "*necessary* truth," tacitly classes them with mathematical axioms, of which this self-evidence is also the recognised character. Further, I have contended that the laws of motion are *à priori* truths of this kind; are enunciated by Newton as such; are adopted from him by Prof. Tait; and are not furnished by Prof. Tait with any such experimental proofs as he asserts are needful for the establishment of physical truths. And I have gone on to show that no experimental proofs of them are possible—that every supposed proof, whether derived from terrestrial phenomena or from celestial phenomena, involves a *petitio principii.*

In the course of the discussion I have examined the reason Prof. Tait gives for asserting that the laws of motion are not to be accepted as valid *à priori.* The reason is that "as the properties of matter might have been such as to render a totally different set of laws axiomatic, *these laws must be considered as resting on convictions drawn from observation and experiment, and not on intuitive perception.*"

The worth of this reason I have tested by asking the origin of Prof. Tait's professed knowledge that "the properties of matter might have been" other than they are. Here is the passage:—

"It will suffice if I examine the nature of this proposition that 'the properties of matter *might have been*' other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments. Is it an intuition? If so, then along with doubt of an intuitive belief concerning things *as they are*, there goes confidence in an intuitive belief concerning things *as they are not*. Is it an hypothesis? If so, the implication is that a cognition of which the negation is inconceivable (for an axiom is such) may be discredited by inference from that which is not a cognition at all, but simply a supposition. Does the reviewer [a critic whose attack I was answering] admit that no conclusion can have a validity greater than is possessed by its premises? or will he say that the trustworthiness of cognitions increases in proportion as they are the more inferential? Be his answer what it may, I shall take it as unquestionable that nothing concluded can have a warrant higher than that from which it is concluded, though it may have a lower. Now the elements of the proposition before us are these:—*As* 'the properties of matter might have been such as to render a totally different set of laws axiomatic' [*therefore*] 'these laws [now in force] must be considered as resting . . . not on intuitive perception:' that is, the intuitions in which these laws are recognised, must not be held authoritative. Here the cognition posited as premiss, is that the properties of matter might have been other than they are; and the conclusion is that our intuitions relative to existing properties are uncertain. Hence, if this conclusion is valid, it is valid because the cognition or intuition respecting what might have been, is more trustworthy than the cognition or intuition respecting what is! Scepticism respecting the deliverances of consciousness about things as they are is based upon faith in a deliverance of consciousness about things as they are not!"

From this passage Prof. Tait has quoted a small part which, standing by itself, appears somewhat strange; but which ceases to appear strange when read along with the rest. In seeking the authority which Prof. Tait has for asserting that "the properties of matter might have been" other than they are, I have tried all possible suppositions; and as he professes to have faith only in experimentally-ascertained truths, I have asked whether this is one; by way of showing, unmistakeably, that in the absence of experimental warrant he must admit it to be, if not a mere hypothesis, then an intuition. Whence results the incongruity I have pointed out.

Prof. Tait says this argument of mine reminds him of a student whose conceptions of algebraic processes were shown by asking—"But what if x should turn out after all *not* to be the unknown quantity?" His imagination suggests to Prof. Tait an analogy too remote for me to perceive; and one which I think few will follow him in perceiving. It seems to me that in this case "the unknown quantity" is the application of his story.

I have to add that Prof. Tait's letter gives the erroneous impression that I have made a gratuitous assault upon his views. Contrariwise, I have said respecting them no more than is needful in self-defence. A critic who thought me greatly in need of instruction respecting the nature of proof and the warrants we have for our ultimate scientific beliefs, quoted, for my benefit, the foregoing passage from Prof. Tait; and he did this in a manner implying that when he had told me what Prof. Tait said, there remained for me no alternative but to abandon my position. As I did not coincide in his general estimate of Prof. Tait's *dicta*, and as this particular question is one of some philosophical interest, I thought it worth while to justify my own belief, and, in so doing, was obliged to assail that of Prof. Tait.

In Prof. Tait's desire to avoid controversy I quite sympathise. Though sometimes scarcely avoidable, it entails, as I know too well, a grievous loss of time. But as Prof. Tait decided not to answer, I think it would have been better to keep silence absolutely, rather than to try and dispose of the matter by tearing a sentence from its context, and telling, *à propos* of it, a story not to the point.

Athenæum Club, March 30 HERBERT SPENCER

Herbert Spencer versus Sir I. Newton

PROF. TAIT is not the only one who has to complain of hard treatment in the pamphlet by Mr. Herbert Spencer, referred to in the Professor's letter of last week. As the unlucky author of the obnoxious criticism that gave rise to the pamphlet in question, I of course come in for a lion's share of the abuse; but neither Prof. Tait nor myself are, after all, treated so cruelly as is Newton, who, though his life was spent in maintaining the experimental character of all physical science, is cited as an authority for the *à priori* character of the most important of all physical truths—the well-known Three Laws of Motion.

Mr. Spencer had asserted that these Laws of Motion are *à priori* truths, and had supported this statement by alleging that Newton gave no proof of them, and therefore intended them to be so regarded. After sheltering myself under the authority of Professors Tait and Thomson, I answered that "the whole of the *Principia* was the proof," whereon Mr. Spencer replies as follows:—

"I have first to point out that here, as before, the reviewer escapes by raising a new issue. I did not ask what he thinks about the *Principia* and the proof of the laws of motion by it; nor did I ask whether others, at this day, hold the assertion of these laws to be justified mainly by the evidence the solar system affords. I asked what Newton thought. The reviewer had represented the belief that the second law of motion is knowable *à priori* as too absurd even for me openly to enunciate. I pointed out that since Newton enunciates it openly under the title of an axiom, and offers no proof whatever of it, he did explicitly what I am blamed for doing implicitly. And thereupon I invited the reviewer to say what he thought of Newton. Instead of answering, he gives me his opinion to the effect that the laws of motion are proved true by the truth of the *Principia* deduced from them. Of this hereafter. My present purpose is to show that Newton did not say this, and gave every indication of thinking the contrary. He does not call the laws of motion 'hypotheses;' he calls them 'axioms.' He does not say that he assumes them to be true *provisionally*, and that the warrant for accepting them as actually true will be found in the astronomically-proved truth of the deductions. He lays them down just as mathematical axioms are laid down—posits them as truths to be accepted *à priori*, from which follow consequences which must therefore be accepted. And though the reviewer thinks this an untenable position, I am quite content to range myself with Newton in thinking it a tenable one—if, indeed, I may say so without undervaluing the reviewer's judgment."

To the sneer in the last sentence, and the remark that follows to the effect that the reviewer had evaded an issue "which it was inconvenient for him to meet," I shall reply by recommending Mr. Spencer to dogmatise either less elaborately or less rashly about the views of a philosopher like Sir I. Newton, whose works are so accessible and whose style is so clear, and at once pass on to call his attention to two passages in Newton's letters to Roger Cotes, who was at the time superintending the printing of the *Principia*.

In speaking of the special sense in which he used the word "hypothesis"—a sense which quite justified him in saying of himself "hypotheses non fingo"—Newton says:—

"In experimental philosophy it is not to be taken in so large a sense as to include the first *Principles or Axiomes which I call the laws of Motion. These Principles are deduced from phenomena and made general by Induction, which is the highest evidence that a Proposition can have in this Philosophy.*" (Letter lxxxi., edited by Edleston.)

And in the next letter he says:—

"On Saturday last I wrote to you representing that Experimental philosophy proceeds only upon Phenomena and deduces general Propositions from them only by Induction. And such is the proof of mutual attraction. And the arguments for y^{e} impenetrability, mobility, and force of all bodies, and *for the laws of motion are no better.*"

I must confess to feeling a difficulty in reconciling the above extracts with the view that Newton posits the laws of motion "as truths to be accepted *à priori.*"

THE AUTHOR OF THE ARTICLE IN THE BRITISH QUARTERLY REVIEW.

An Experiment on the Destructive Effect of Heat upon the Life of Bacteria and their Germs

I RECENTLY carried out an experiment which I shall not soon have the opportunity of repeating, and which I am consequently anxious to put on record. It is probably now familiar to those interested in the matter, that the experiments of Dr. Sanderson have established the fact that in an infusion of turnips and cheese prepared as directed by Dr. Bastian, heating to a temperature of 102 C. is sufficient to prevent the subsequent development of life (Bacteria) in the infusion even when the exposure to that temperature is only maintained for a few minutes. Boiling for five or ten minutes, according to Dr. Sanderson, is not sufficient to prevent the subsequent development of Bacteria, but according to the experiments of Dr. Pode and myself, boiling for ten minutes or a quarter of an hour *is* sufficient, provided that care has been taken to exclude visible lumps of cheese, and when the infusion is enclosed in a tube which tube is submerged in boiling water. Further, Dr. Sanderson has stated the following most important result, namely, that exposure to the boiling temperature (100° C.) was in all cases sufficient to prevent the subsequent development of Bacteria if it was carried on for so long as *one hour*.

This being the case, it occurred to me that since in all probability Bacteria and their germs, or spores, are killed by *through-heating* to a temperature a little below 70° C. (as established by various experiments in regard to Bacteria, but not in regard to possible germs, and admitted by both sides in the controversy as to their biogenetic or abiogenetic origin), it is desirable to recognise in our experiments the two distinct factors of this through-heating to any given temperature—namely, (1) the temperature to which the infusion to be heated is to be exposed; and (2) the length of time during which it is exposed to that temperature. If one of these variables—the time—be taken as a horizontal, and this line be divided into equal spaces representing periods of five minutes—whilst the perpendicular represents the range of temperature divided into degrees from 65° C. to 120° C—and if the results of observations with a given infusion indicating the time of exposure to a particular degree of temperature required in order to prevent the subsequent development of Bacteria be marked off on such a scheme, we should expect to obtain a series of points defining an asymptotic curve, the time required at the highest temperature being infinitely small, and at the lowest temperature infinitely great. This curve would vary in its character according to the properties of the infusion made use of. It was my intention to determine the principal points in this curve for Dr. Bastian's turnip and cheese infusion, but at present I have only made a tentative experiment at a low temperature. Using tubes of quarter-inch bore and three inches in length half filled with Dr. Bastian's infusion, and then submerged in water maintained at the desired temperature, I found that exposure for *six hours* to *a temperature of* 75° C. *was sufficient* to prevent the subsequent development of Bacteria. The same infusion enclosed in a similar tube and not heated at all, teemed with living Bacteria after four days; the same infusion boiled for ten minutes in an open tube remained barren. I submit this plan for a series of experiments to the readers of NATURE, without attaching much importance to the single but definite result which is above recorded.

I have not seen any reference in the pages of NATURE to the experiments which have been carried on in German laboratories in consequence of Prof. Huizinga's advocacy of Abiogenesis. Dr. Paul Samuelson, experimenting with Huizinga's infusions under the direction of Prof. Pflüger of Bonn, has obtained results which negative the inferences of Prof. Huizinga. Dr. Samuelson's paper appeared in Pflüger's *Archiv*, during the past year, and another experimenter (to whom I am unable to refer explicitly) has obtained equally definite results opposed to the speculations of Bastian and Huizinga.

Paris, Feb. 8 E. RAY LANKESTER

Animal Locomotion

I AM surprised to find that the Duke of Argyll prefers a charge of plagiarism against me in 1874 (NATURE, vol. ix. p. 381), said to have been committed by me in a lecture delivered at the Royal Institution of Great Britain in 1867. As his Grace was present at the lecture in question, and lodged no complaint in writing or otherwise, it appears to me that the charge, if not unfounded and out of place, is at least out of time. As I am not conscious of having perpetrated the plagiarism attributed to me, I wish to apprise your readers that the lecture referred to is published *in extenso* in the Proceedings of the Royal Institution of Great Britain, under date March 22, 1867, and may be consulted by all interested in the present discussion. That I had no wish to appropriate from his Grace, but was, on the contrary, desirous of giving him due credit for what he had done, will, I hope, be evident from the following quotation :—"In order to utilise the air as a means of transit, the body in motion, whether it moves in virtue of the life it possesses or because of a force superadded, must be heavier than it. If it were otherwise, if it were rescued from the operation of gravity on the one hand, and bereft of independent movement on the other, it must float about uncontrolled and uncontrollable, as happens in the ordinary gas balloon. The difference here insisted upon was, I have learned since writing the above, likewise pointed out by his Grace the Duke of Argyll, in his very able and eloquent article in *Good Words*, entitled the 'Reign of Law.' . . . This article, I am glad to find, has been reprinted in a separate form with numerous illustrations, and should be read by all interested in the subject of aëronautics." ("On the various Modes of Flight in Relation to Aëronautics ;" Proceedings Royal Institution of Great Britain, March 22, 1867.)

The only passage in the lecture bearing upon the point at issue *is opposed* to his Grace's explanation of the direction of the down stroke of the wing and in accordance with that originally given by me and defended by Mr. Wallace in NATURE, vol. ix. p. 301. *It cannot consequently be regarded as a plagiarism.* The Duke, it will be remembered, contends that the wing of the bird strikes *vertically downwards* during the down stroke. I, on the other hand, believe that the wing, during the down stroke, invariably strikes *downwards and forwards*. In this Mr. Wallace agrees with me. The passage in question runs as follows :—

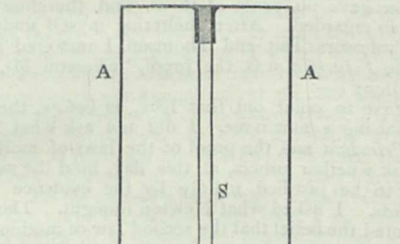
"All wings obtain this leverage by presenting oblique surfaces to the air, the degree of obliquity gradually increasing in a direction from behind *forwards and downwards* during extension, when the sudden or effective stroke is being given, and gradually decreasing in an opposite direction during fluxion or when the wing is being more slowly recovered preparatory to making a second stroke. The effective stroke in insects, and this holds true also of birds, is therefore delivered *downwards and forwards*, and not as the majority of writers believe, vertically, or even slightly backwards. This arises from the curious circumstance that birds, when flying, actually fall through the medium which elevates them, their course being indicated by the resultant of two forces, viz., that of gravity pulling vertically downwards, and that of the wing *acting at a given angle in an upward direction*. The wing of the bird acts after the manner of a boy's kite, the only difference being that the kite is *pulled forwards* upon the wind by the string and the hand, whereas in the bird the wing is *pushed forwards* on the wind by the weight of the body and the life residing in the pinion itself." (*Op. cit.*, March 22, 1867.) The Duke, it is true, compares the expanded motionless wings of a bird when sailing to a kite, while I, as stated, attribute a kite-action to the wings both when they rise and fall. The kite-action in the one instance is, however, not to be confounded with the kite-action in the other. That the wings invariably strike *downwards and forwards* during the down stroke, and *upwards and forwards* during the up stroke, and act as kites in either case, is a matter of observation, but still more of experiment. I have again and again witnessed the movement in the crow, cormorant, wild duck, and other birds, and repeated experiments with natural and artificial wings serve more and more to convince me that what I state is correct. But for the downward and forward and upward and forward curves made by the wings during the down and up strokes, progressive flight would be impossible. The curves in question, when the bird is advancing, unite to form *waved tracks* on either side of the body, thus representing the paths pursued by the vibrating wings in every form and variety of flight.

With regard to the poetical quotation introduced by me in my lecture and alluded to by his Grace, I venture to think that few will regard this as a case of plagiarism.

Edinburgh, March 23 J. BELL PETTIGREW

Electric Experiment

THE following striking experiment to show the rapidity of the influence of sulphuric acid in removing the invisible film of moisture that in ordinary circumstances adheres to the surface of glass and deprives it of its quality as an electric insulator, was recently shown to the Natural Philosophy class in the University of Glasgow by Sir William Thomson, and as it may be interesting to some of your readers, I send you an account of it. The apparatus used were a gold-leaf electroscope, and one of the ordinary table insulators long used in this University, of which the following is a description. A A is a hollow cylinder of brass, the lower part of which can be readily detached, replaced, and fixed in position by a bayonet-joint. The cylinder is supported at the top by the glass rod S, which passes through a circular opening in the bottom of the cylinder and is fixed to the sole plate C. In the lower part is placed a circular canal of lead containing a number of pieces of pumice stone *p p*, which for insulating purposes are moistened with a few drops of strong sulphuric acid. On the previous evening the pumice *pp* was moistened with a few drops of water, the cylinder closed and



left till morning. The experiment was then performed thus The gold leaves of the electroscope were connected with A A by a fine wire and a charge communicated ; the gold leaves at first repelled each other, but almost immediately collapsed. This was repeated once or twice, to show distinctly that there was no insulation.

The pumice containing water was then removed, and was replaced by other pieces moistened with sulphuric acid (in both cases the moistening was so little that the pumice had the appearance of being quite dry) and the vessel was closed. As the experiment was made towards the end of the lecture and time was pressing, a warm hand was placed on the side of the insulator to accelerate the drying process by creating connective currents in the air. Whether this hastened the effect or not sensibly it is impossible to say, but the insulation at once began to improve, and in less than five minutes it was shown to be perfect by the gold leaves remaining diverged to their full extent.

The University, Glasgow, March 21 D. M'FARLANE

Fertilisation of the Fumariaceæ

THE accompanying note has been given me by my friend Mr. J. Traherne Moggridge, and I should feel obliged if you would insert it in NATURE with the view of eliciting the communication both of other similar phenomena, and of some explanation of them. ST. GEORGE MIVART

Mentone, March 18

Note on apparently useless Colouring in the Flowers of a Fumitory (Fumaria capreolata var. pallidiflora, F. pallidiflora Jord.)

I observe that in this plant at Mentone the flowers attain their brightest colouring after the ovaries are set, and when fertilisation is no longer necessary, or indeed possible. During the period previous to impregnation, the flowers are pale and nearly white, and the pedicels erect or horizontal; afterwards they become pink, or even crimson, and the pedicels are recurved, and the colour of the petals, which retain their form and position ntil the ovary has nearly attained its full size, intensifies with the lapse of time.

If the reverse had been the case there is little doubt that we should have regarded the bright colouring as specially adapted to attract insects, and as existing for that purpose, insects being, according to Prof. F. Hildebrand,* important agents in the fertilisation of fumitories; but here, as the brighter flowers are those which no longer need or are capable of profiting by the interference of insects, this explanation ceases to be possible.

This little fact, therefore, would seem to be one which might be classed with those which teach us that, side by side with the developments and modifications which are plainly beneficial to the organism of which they form a part, there are others, which, as far as we can see, are neither useful nor harmful to their possessor, though they may, and frequently do, supply features which especially attract our attention and admiration.

J. TRAHERNE MOGGRIDGE

OCEAN CURRENTS

TWO papers which Mr. Croll has recently published "On the Physical Cause of Ocean Currents" (*Philosophical Magazine* for Feb. and Mar. 1874), bring the main question at issue between him and myself into very distinct view; and as the results of the *Challenger* Temperature-survey of the Atlantic, lately made public by the Admiralty, afford (as it seems to me) important data towards the settlement of this question I shall be glad to be allowed to point out what seem to me their chief bearings upon it.

The position taken by Mr. Croll is, that all the great movements of ocean-water, deep as well as superficial, depend on the action of winds upon its surface. And whilst freely admitting that Polar water finds its way along the floor of the great ocean-basins into the equatorial area, he affirms that this is merely the reflux of the current which has been driven into the Polar basins by the agency of winds.

On the other hand, it is fully recognised by myself, that the *current* movements of *surface*-water are, for the most part, produced by the agency of winds; but these movements, I contend, all belong to a *horizontal circulation, which tends to complete itself*,—a surface indraught being produced wherever a surface outflow is kept up, as we see in the horizontal circulations of the North and South Atlantic, the North and South Pacific, and the Indian Ocean, depicted in Mr. Croll's own map. But I maintain that the *deep* movements of ocean-water are the result of a *vertical circulation*, which is maintained by the continuance of a disturbed equilibrium between the Polar and equatorial columns, occasioned by the surface-action of Polar cold and equatorial heat.

As Mr. Croll is unable to understand why I should speak of Polar cold, rather than equatorial heat, as the *primum mobile* of this vertical circulation, and accuses me of an ignorance of the fundamental principles of physics in so regarding it, I may be allowed first briefly to explain myself; since others may experience the same difficulty, from some want of precision on my part in stating my case. The eminent physicists, however, with whom I have had the advantage of discussing this point, do *not* share Mr. Croll's objection, but hold my statement to be perfectly correct.

Heat applied to the *surface* of any body of *fresh* water, whether by solar radiation, or by the experimental application of a heated plate, will raise the temperature of the *surface-film*, without producing any downward convection. Limited downward convection, however, is occasioned in *salt* water by the sinking of the surface-films which are concentrated by evaporation; but this convection I found in my Mediterranean observations, which have been fully confirmed by those of the *Challenger* in the equatorial area, to be practically limited to the first fifty fathoms. Water in a long trough may thus be superficially heated (as I have experimentally ascertained), by the application of surface-heat to one-sixth of its length, until the temperature of its whole surface-film is raised to 100° or more; but the further application of surface-heat expends itself in vaporisation, and does not communicate itself in any sensible degree to the mass of water beneath, which, therefore, *can not be put in motion* by such application. On the other hand, the moment that *surface-cold* is applied, a downward convection is produced, as Mr. Croll may easily ascertain for himself if he will only try the experiment; and the continued application of such surface-cold to any one portion of the surface will maintain a constant movement through the entire mass of the liquid, until thermal equilibrium is restored by the cooling-down of the whole. But if the restoration of this thermal equilibrium be prevented by the application of heat to another part of the surface, the disturbance of equilibrium will be kept up, and a *vertica circulation* maintained, as long as these two opposing agencies are in operation. If Mr. Croll cannot see that this *must be* the case, I am not responsible for his failure to apprehend that which theory and experiment alike sanction.

I re-affirm, then, that *cold* applied to the *surface* has exactly the same motor power as *heat* applied at the *bottom*; and that its motor agency is more potent than that of heat applied at the surface, simply because the former is diffused by convection through the entire mass of the water, which it keeps on *cooling* and *moving*, whilst the latter is limited to the surface-film, and expends itself in producing evaporation.

Mr. Croll objects to this, that, if it were true, nearly the whole mass of oceanic water must have an almost Polar temperature. I accept this issue; and refer to the *Challenger* temperature-soundings, as justifying it. If he will look at the section taken across the equator, he will find that—*as I had predicted*—Polar water there lies within a very short distance from the surface. At less than 100 fathoms' depth, the temperature falls from 78° at the surface to 55°, and the isotherm of 40° is reached at about 320 fathoms. Below this lies a *stratum of more than* 2,000 *fathoms' thickness*, whose temperature, ranging downwards from 40° to 32°·4, shows it to consist mainly of Polar water. And as, from the data supplied by the Mediterranean and Gulf of Suez temperature-soundings, a body of equatorial water secluded from all connection with the oceanic circulation might be expected to have the uniform (or isocheimal) temperature of 75° from 50 fathoms downwards, it is clear that the influence of Polar cold here extends itself upwards within 100 fathoms of the surface.

Again, Mr. Croll says that I have made no allowance for the *excess* of salinity in equatorial water, which, according to him, must counterbalance the increase of specific gravity produced in Polar water by the reduction of its temperature. Here, again, he is unfortunate

* "Ueber die Bestäubungsvorrichtungen bei den Fumariaceen," in Pringsheim's "Jahrbücher," vol. vii. part 4, p. 423 (1870). Reviewed in "Bull. Soc. Bot. de France," xix. (1872), p. 145.

as regards his facts. He appears to have overlooked the observations proving the *lower* salinity of inter-tropical water, which I had cited as furnishing an additional indication that Polar water is constantly rising from the bottom towards the surface in the equatorial area. These observations have been most remarkably confirmed by those taken by the physicists of the *Challenger*. For, whilst in the extra-tropical area the sp. gr. of surface-water was in excess of that of bottom-water, in the equatorial area it was reduced to an almost precise correspondence with it, due allowance for temperature being of course made.

According to Mr. Croll's doctrine, the whole of that vast mass of water in the North Atlantic, averaging, say, 1,500 fathoms in thickness, and 3,600 miles in breadth, the temperature of which (from 40° downwards) as ascertained by the *Challenger* soundings, clearly shows it to be mainly derived from a Polar source, is nothing else than *the reflux of the Gulf Stream*. Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic Basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf Stream to be about 1-900th that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf Stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east; the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison such a mere rivulet—the north-east motion of which, as a distinct current, has not been traced eastward of 30° W. long.—seems still more obvious.

Lastly, the *Challenger* observations in the South Atlantic have proved exactly what I had anticipated, viz., that the bottom-temperature is lower, and that the Polar underflow lies much nearer the surface in this ocean than in the North Atlantic. Now this case appears to me to afford the *experimentum crucis* between Mr. Croll's doctrine and my own. For my prediction of this result was based on the fact, that, as there is here a perfectly open communication between the Polar and equatorial areas, the vertical circulation would take place more freely. On the other hand, according to Mr. Croll's doctrine, it would have been expected that there should be a far smaller reflux, or no reflux at all. For, though a portion of the equatorial current passes southwards when it meets the coast of South America, there is no ground whatever for believing that it ever goes near the Antarctic circle; and if it did find its way thither, there is no "closed basin" from which it can drive back a return current.

As it is usually considered in scientific inquiry that the verification of a prediction affords cogent evidence of the validity of the hypothesis on which it is based, I venture to submit that so far my case has been made good.

WILLIAM B. CARPENTER

THE DEATH OF DR. LIVINGSTONE

THE daily papers have obtained from the London office of the *New York Herald* the following telegram, containing details of the death of Dr. Livingstone, dated Suez, Sunday, March 29 :—

"The *Malwa* arrived off Suez at eleven on Saturday night, having Mr. Arthur Laing and Jacob Wainwright on board, with the body of Livingstone. He had been ill with chronic dysentery for several months past. Although well supplied with stores and medicine, he seems to have had a presentiment that the attack would prove fatal. He rode a donkey, but was subsequently carried, and thus arrived at Muilala, beyond Lake Bemba, in Bisa country, when he said, 'Build me a hut to die in.' The hut was built by his followers, who first made him a bed. He suffered greatly, groaning night and day. On the third day he said, 'I am very cold; put more grass over the hut.' His followers did not speak or go near him. Kitumbo, Chief of Bisa, sent flour and beans, and behaved well to the party. On the fourth day Livingstone became insensible, and died about midnight. Majuahra, his servant, was present. His last entry in diary was on April 27. He spoke much and sadly of home and family. When first seized he told his followers he intended to change everything for ivory, to give to them, and to push on to Ujiji and Zanzibar, and try to reach England. On the day of his death the followers consulted what to do. The Nassick boys determined to preserve the remains. They were afraid to inform the Chief of Livingstone's death. The secretary removed the body to another hut, around which he built a high fence to ensure privacy. They opened the body and removed the internals, which were placed in a tin box and burned inside the fence under a large tree. Jacob Wainwright cut an inscription on the tree as follows:—'Dr. Livingstone died on May 4, 1873,' and superscribed the name of the head man Susa. The body was preserved in salt, and dried in the sun for twelve days. Kitumbo was then informed of the death, and beat drums and fired guns as a token of respect, and allowed the followers to remove the body, which was placed in a coffin formed of bark, then journeyed to Unyanyembe about six months, sending an advanced party with information addressed to Livingstone's son, which met Cameron. The latter sent back a bale of cloth and powder. The body arrived at Unyanyembe ten days after advanced party, and rested there a fortnight. Cameron, Murphy, and Dillon together there. Latter very ill, blind, and mind affected. Committed suicide at Kasakera; buried there. Here Livingstone's remains were put in another bark case, smaller, done up as a bale to deceive natives who objected to the passage of the corpse, which was thus carried to Zanzibar. Livingstone's clothing, papers, and instruments accompany the body. When ill Livingstone prayed much. At Muilala he said, 'I am going home.' Chumah remains at Zanzibar. Webb, American consul at Zanzibar, is on his way home, and has letters handed to him by Murphy from Livingstone for Stanley, which he will deliver personally only. Geographical news follows. After Stanley's departure the Doctor left Unyanyembe, rounded the south end of Lake Tanganyika, and travelled south of Lake Bemba, or Bangneoleo, crossed it south to north, then along east side, returning north through marshes to Muilala. All papers sealed. Address Secretary of State, in charge of Arthur Laing, a British merchant from Zanzibar. Murphy and Cameron remain behind."

These details are few but intensely touching. We believe that the Peninsular and Oriental Company's Bombay steamer *Malwa*, with Dr. Livingstone's body on board, is due on April 13 at Southampton. The body will be landed at that port and conveyed to London, by railway, for interment in Westminster Abbey; it is to be regretted that the faithful Chumah does not accompany his master's remains. It is impossible that Government will fail in doing what the whole civilised world takes for granted it will do—pay all possible honour to the remains of H. M. Consul, and of probably the greatest traveller that this or any other country ever produced.

REPORT OF PROF. PARKER'S HUNTERIAN LECTURES "ON THE STRUCTURE AND DEVELOPMENT OF THE VERTEBRATE SKULL"

THE new Hunterian Professor, Mr. W. Kitchen Parker, has just completed his course of eighteen lectures at the College of Surgeons, embodying in them the results of his researches on that most difficult problem, the development of the vertebrate skull. The plan pursued by Mr. Parker has been to describe first in their adult state, and afterwards in the various stages of their development, the skulls of certain prominent vertebrates which should serve as types for the other members of the group, and to deduce from the facts thus established the principles on which the cranium is constructed in the whole sub-kingdom. The types selected were the shark, skate, salmon, axolotl, frog, snake, fowl, and pig.

I.—*Morphological Elements of the Skull.* Nothing can be more hopeless than the attempt to unravel and explain the vertebrate skull by the study of adult forms only. The modification of face and brain-case, in the long line of creatures which begins with the lamprey and ends with man, are so endless that, until the study of embryology put the matter on a new and firm foundation, the best observers failed signally to produce a true "theory of the skull," the most elaborate attempt of the kind—the "vertebral theory" started by Goethe and Oken in Germany, and perfected by Prof. Owen in England—having resulted only in a convenient working hypothesis.

When, however, instead of starting with the highly differentiated skulls of adult animals, the embryos of these animals from their earliest conditions are made the subject of investigation, a new light is shed on the whole question. It is found that the skulls of all the vertebrata which have yet been thoroughly worked out, originate in, practically, a precisely similar manner; and even in some of the more advanced stages it would be hard to point out very essential differences between the skulls of a fish, a bird, or a mammal. Before entering upon the description of the skull of the shark, the first type to be gone into,* it will be advisable to consider the distinct elements of which the *primordial cranium* of any vertebrate animal is made up.

a. On either side of the anterior termination of the notochord, or primitive axis of the body (Fig. 1, Ch.), is developed a cartilaginous plate (I.M), which as a rule unites both above and below the notochord with its fellow of the opposite side. These plates taken together were termed by Rathke the *investing mass*: in the language of Prof. Huxley, they constitute the *parachordal elements* of the primordial skull or *chondro-cranium.*

b. In front of and below the investing mass, cartilaginous thickenings with intervening spaces are developed in the side walls of the body, enclosing, rib-like, the

FIG. I.

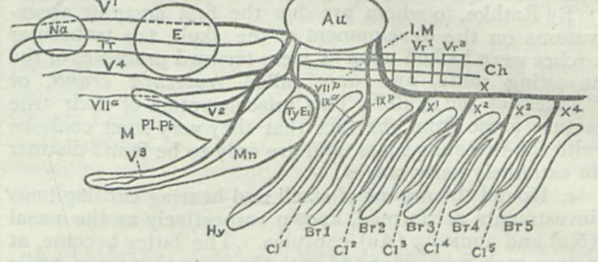


FIG. 1.—DIAGRAM OF VERTEBRATE SKULL. Ch, Notochord; Vr^1, Vr^2, first and second vertebræ; I.M, investing mass; Au, auditory capsule; E, eye; Na, nasal capsule; Tr, trabecular; Mn, mandibular; Hy, hyoid; Br.1-5, branchial arches; Pl.Pt, pterygo-palatine process; M, mouth; Ty.Eu, tympano-Eustachian passage: Cl.1-5, branchial clefts; V^1, orbito-nasal; V^2, maxillary; V^3, mandibular; V^4, palatine division of the trigeminal nerve; VII^a, vidian; VII^p, hyomandibular divisions of portiodura; IX, glossopharyngeal; X, vagus.

pharyngeal cavity. These are the *visceral arches* (pleural elements, Huxley), the spaces between them the visceral clefts. The usual number of these bars is eight, although in certain exceptional cases they may be increased to nine (Hexanchus) or ten (Heptanchus). Taken from before backwards, the pleural arches are named as follows:—1. trabecular (Tr), 2. mandibular (Mn), 3. hyoid (Hy), 4-8. branchial (Br. 1—5), the clefts separating them being in like manner, the mouth (M), the tympano-Eustachian passage (Ty Eu) and the branchial clefts (Cl 1—5). At a very early period, the mandibular arch gives off a forward process, the palato-pterygoid arcade (Pl Pt) which in certain cases takes on the form of a distinct pleural element. In the branchiate vertebrata (Fishes and

FIG. 2

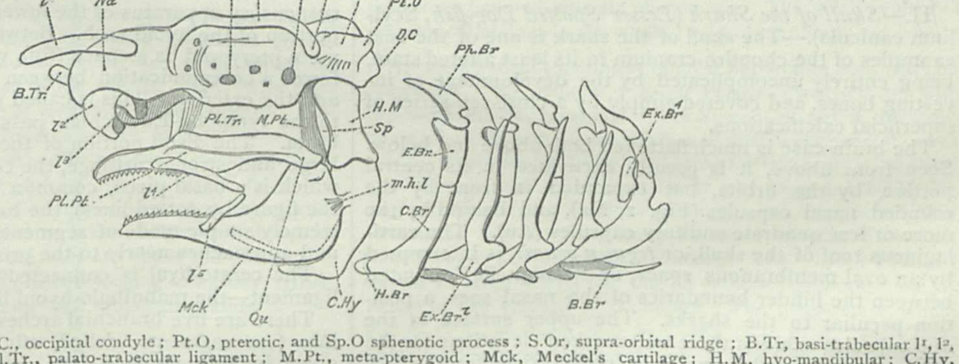


FIG. 2.—SKULL OF SHARK. O.C., occipital condyle; Pt.O, pterotic, and Sp.O sphenotic process; S.Or, supra-orbital ridge; B.Tr, basi-trabecular l^1, l^2, l^3, l^4, l^5, labial cartilages; Pl.Tr., palato-trabecular ligament; M.Pt., meta-pterygoid; Mck, Meckel's cartilage; H.M, hyo-mandibular; C.Hy, cerato-hyal: Ph.Br, E.Br, C.Br, H.Br, and B.Br, pharyngo-, epi-, cerato-, hypo-, and basi-branchial; Ex.Br, extra-branchials; Sp, spiracle; II, Optic foramen.

Amphibia) all these arches with the single exception of the trabecular may bear functional gills, the presence of these organs being the chief physiological test of their serial homology.

Far more important, however, in the determination of these elements of the skull, are the morphological landmarks afforded by the cranial nerves, especially by the 5th, 7th, 9th, and 10th pairs. It is the constant habit of these nerves to fork above a visceral cleft, one of the branches thus formed supplying the posterior face of the arch in front of the cleft, the other the anterior face of the arch behind and (see Fig. 1). The orbito-nasal or ophthalmic (V^1) and the palatine (V^4) divisions of the trigeminal are the special trabecular nerves, the former passing over the optic nerve, the latter below it. The posterior primary subdivision of this nerve passes behind the mouth-cleft, and divides into two branches, both of which are distributed to the anterior (or in the altered position of the three foremost arches, outer) side of the mandibular arch; the mandibular, or inferior maxillary nerve (V^3) passing along the original direction of the arch, the superior maxillary (V^2) following the pterygo-palatine process. The seventh nerve, or portiodura, divides above the tympano-Eustachian passage, its anterior

* The development of the skull in the Marsipobranchii (Lamprey and Hag), the lowest group of craniate Vertebrata, has not yet been studied with sufficient accuracy to allow of the determination of its parts with any degree of certainty.

branch (VII[a]) going to the inner (posterior) side of the mandibular arch, and its posterior (VII[p]) division to the outer side of the hyoid. The glossopharyngeal (IX.), in like manner, is distributed to the inner side of the hyoid and the front face of the first branchial, the hinder face of which, as well as all the remaining branchial arches, is supplied by the great tenth nerve, or vagus (X).

By Rathke, to whom are due the first accurate observations on the development of the skull, the trabecular arches were looked upon as mere forward processes of the investing mass, and were called *trabeculæ cranii*, or "rafters of the skull." This misconception of their true nature arose from the fact that they very soon coalesce with the investing mass, and are only to be found distinct in extremely early stages.

c. Round the organs of smell and hearing cartilaginous investments are formed, known respectively as the nasal (Na) and auditory (Au) capsules. The latter become, at a very early period, united with the investing mass, while the nasal capsules come into close relations with the anterior or distal ends of the trabeculæ.* These are the *paraneural elements* of the primordial skull.

d. Certain cartilages may be developed in relation with and external to the visceral arches, called from this circumstance "extra-viscerals." Of this nature are the labial cartilages, which take so large a share in the formation of the skull of many cartilaginous fishes.

e. Lastly, the general membranous roof and walls of the brain-case may chondrify to a greater or less extent, but this chondrification is in nearly every case continuous with the trabeculæ and nasal capsules in front and below, and with the investing mass and auditory capsules behind.

Not only is the originally membranous cranium thus strengthened by deposits of cartilage, but osseous deposits may take place either in the primordial skull itself, or in the subcutaneous tissue surrounding it. The latter are called "investing-bones," or *parostoses;* the former may be of two kinds; when occurring as mere calcifications of the substance of the cartilage, they are known as *endostoses*, when having the structure of true bone, as *ectostoses*.

II.—*Skull of the Shark* (*Lesser Spotted Dog-fish*, Scyllium canicula).—The skull of the shark is one of the best examples of the chondro-cranium in its least altered state, being entirely uncomplicated by the development of investing bones, and covered simply by a close-set series of superficial calcifications.

The brain-case is much flattened both above and below. Seen from above, it is greatly excavated in its central portion by the orbits, but expanded in front by the rounded nasal capsules (Fig. 2, Na), and behind by the more or less quadrate auditory capsules (Au). The cartilaginous roof of the skull, or *tegmen cranii*, is interrupted by an oval membranous space, or "fontanelle," situated between the hinder boundaries of the nasal sacs, a position peculiar to the sharks. The upper surface of the otic capsules exhibits three well-marked elevations for the semi-circular canals, and just within that for the anterior canal, a small rounded aperture, the remains of the primitive involution of the integument from which the organ of hearing arose. An elevation on the hinder end of the posterior canal marks the position of the epiotic ossification so well developed in the osseous fish; the pterotic is also indicated by a large outstanding process (Pt O) which forms the postero-external angle of the skull, and the sphenotic (post-frontal of Cuvier) by the posterior portion of the supra-orbital ridge (S Or) when it coalesces with the auditory capsule (Sp O). The anterior extremity of this ridge forms in like manner the pre-frontal process. One very noteworthy point, observable both in an upper, under, or side view, is the presence between the nasal capsules of a short rod of cartilage (B Tr) representing the median basal portion, or keystone of the trabecular arch, and hence called the basi-trabecular.

Viewed from behind, the skull presents a large foramen magnum, bounded below and at the sides by the well-developed occipital condyles, between which is a slight elevation, showing the point where the notochord originally entered the investing mass. External to the occipital foramen, and marking the original boundary between the parachordal and otic elements of the skull is the foramen for the exit of the 9th and 10th nerves. The trigeminal foramen, which always points to the anterior limit of the otic region, forms a large aperture in the side wall of the brain-case (V), as also does the optic foramen (II).

The jaws are very loosely united to the other parts of the skull, and consist of an upper and a lower dentigerous arch, the former of which is connected with the skull by two bands of ligamentous fibres. The lower arch (Mck), which articulates with the posterior end of the upper, is the homologue of Meckel's cartilage, the rod which forms the foundation of the lower jaw in all vertebrata, but which as a rule, owing to the great development of investing bases, is reduced to a more slender style, or is even suppressed altogether. The posterior portion of the upper dentigerous arch (Qu) answers to the quadrate, a bone which in all Teleostei, as well as in Amphibia and Sauropsida, gives attachment to the mandible. The remainder of this "upper-jaw" represents the series of bones (pterygoid, meso-pterygoid, and palatine) which in the osseous fish connect the quadrate with the fore-part of the skull, the meta-pterygoid or proximal end of the mandibular arch being represented by the band of fibrous tissue (M Pt) which connects the quadrate with the auditory capsule.

Close behind the attachment of the meta-pterygoid ligament, a large phalangiform cartilage (H M) is articulated to the auditory capsule; this represents the hyomandibular of the osseous fish, the largest bone in the suspensory apparatus of the lower jaw, and the uppermost portion of the hyoid arch. Between this cartilage and the meta-pterygoid is a space (Sp), which in the recent state forms a communication between the cavity of the mouth and the exterior. This is called the spiracle, and answers to the tympano-Eustachian passage of the higher vertebrata. The distal portion of the hyoid arch consists of a large and strong cartilage, the cerato-hyal (C Hy), below which is a basal piece, common to both sides (shown in the figure by dotted lines), the basi-hyal. This is an extremely simple mode of segmentation of the hyoid arch, and approaches nearly to the primitive condition.

The cerato-hyal is connected with the mandible by a ligament—the mandibulo-hyoid ligament (mhl).

There are five branchial arches, all of which are split up into four segments, called, after the names originally given by Prof. Owen to the corresponding parts in the Teleostei, pharyngo- epi-, cerato-, and hypo-branchial. The inferior median piece, or basi-branchial (B Br), occurs only in the hinder part of the series.

The extra-viscerals are represented by the labial cartilages (l^1—l^5) and by the extra-branchials (Ex Br 1—5), between which and the branchial arches extend cartilaginous rods, acting as supports to the septa between the gill-pouches. The last arch, however, bears no gill and has no extra-branchial corresponding to it. The hyoid also is devoid of an extra-visceral, although it bears a series of greatly divided cartilages, which support the anterior wall of the first gill-sac; this arch, consequently, carries a half-gill. The branchia of the mandibular arch is represented by a vascular plexus (pseudo-branchia) on the anterior side of the spiracular opening.

* The Sclerotic, the fibrous (mammalia), or cartilaginous (Sauropsida and Ichthyopsida) capsule developed around the organ of sight (E) never really forms part of the skull, although in the sharks and rays, and some osseous fish, it is articulated with the side walls of the brain case by a cartilaginous pedicle. The form of the skull is, however, greatly governed by the presence of these optic capsules.

NOTES

AN International Horticultural Show is to be held at Florence in May, from the 11th to the 25th. The *Société Royale Toscane d'Horticulture* offers 100 medals of gold, 221 of silver, and 131 of bronze, and five grand *prix d'honneur* are offered respectively by the King of Italy, the Minister of Agriculture and Commerce, the province of Florence, the town of Florence, and the lady patronesses. Prince Demidoff and Prof. Parlatore have also placed gold medals for special classes, at the disposal of the committee. Coincidently with the Show the International Botanical Congress will be held at Florence under the presidency of Prof. Parlatore. The programme of subjects for discussion includes questions on the nature and functions of hairs on plants, on cell circulation, on the latex, on the automic movement of the leaves of plants, on the causes which determine the direction of the root in the germination of a seed, on the causes which influence the direction of the growth of branches, especially of weeping trees, on the analysis of the organs of reproduction between cryptogams and phanerogams, as well as many other subjects more widely known, and subjects of debate such as the origin of *Bacteria*, the determination of fossil plants by their leaves, the distinction between *species*, *race*, and *variety*, and the origin of insular and alpine floras. The President and Secretaries of the *Société Royale Toscane d'Horticulture* announce their readiness to communicate with any botanists who wish for further information with a view to attending the Congress. The official language of the Congress will be Italian, but papers may be communicated and discussions conducted in any language. Representatives to the Congress have been appointed from the various countries of Europe, and from Egypt, Australia, Mexico, Brazil, &c. Among the names of those who are expected are announced the following English botanists :—Messrs. Hooker, Trimen, Ball, Hiern, Hogg, Maw, Murray, Allmann, and Binney. As a measure of precaution against the introduction of the *Phylloxera*, the importation of vines and of other fruit-trees into Italy has been rigorously prohibited since October 31 last.

THE Syndics of the Cambridge Botanic garden in their annual report state that the Curator has nearly completed the re-arrangement of the herbaceous plants, and it is hoped the laborious task will be finished in the ensuing year. The plant houses are in a good state of repair, but over-crowded. The Professor and Curator are unable to see in what manner the number of plants kept in them can be materially reduced without injuring the efficiency of the garden. Several of the finest and most valuable specimen plants now threaten to grow through the roof of the houses. The Syndics acknowledge some donations of foreign seeds and plants, but they are under the necessity of discouraging gifts of seeds of plants belonging to warmer regions, because of the want of room for their proper cultivation.

WE are very glad to hear that negotiations are pending for the transfer of the valuable Museum of Natural History, which was formerly in the possession of the East India Company, from the India House, where it has been for some time stored, to South Kensington, where it will at last be available for reference and study. This desirable transfer we strongly recommended in an article which appeared about a year ago (NATURE, vol. vii. p. 457).

A PURSE of 540 guineas has recently been presented by members of the British Association and other friends to Mr. W. Pengelly, F.R.S., F.G.S., as a testimony to the high value of his labours in conducting the exploration of Kent's Cavern, Torquay, and of his other services to science. After the presentation it appeared that many of Mr. Pengelly's friends and advisers had been left in ignorance of what was proposed. To enable all such persons to join in this mark of appreciation the hon. sec. to the testimonial fund, Mr. J. E. Lee, F.G.S., Villa Syracusa, Torquay, is prepared still to receive subscriptions up to the 17th of April.

A GERMAN Natural History and Anthropological Society for Eastern Asia has now existed for a twelvemonth, having been established on March 22, 1873. The headquarters is at Tokio, and the Society consists of fifty-two members, twenty-three being resident at Yokohama, twenty at Tokio, seven in Hiogo, and two at Singapore. Herr von Brandt, the Minister for the German Empire in Japan, is its president. The Society has already published a volume of "Proceedings," containing several interesting and important papers on the subjects for the cultivation of which the Society was founded, especially on the ancient customs and history of Japan.

A MONUMENT to Antonio Bertolini, author of "Flora Italica," has been inaugurated at Bologna.

DR. ASCHERSON, of Berlin, has gone to Egypt as a member of a commission of exploration. Prof. Planchon, of Montpellier, has been sent by the French Government to the United States to inquire into the new vine disease caused by the *Pemphigus vitifolia*. Other botanists at present occupied with foreign exploration are, Sig. Pichler in the east, and Messrs. Lorentz and Hieronymus.

THE chairs of Botany at the Universities of Genoa and of Modena were announced vacant at the end of January.

THE new edition of Pritzel's "Thesaurus" will be edited by Prof. Jessen.

PROF. STEFANO DE' ROSSI has just started in Rome a *Bulletino del Vulcani*. The learned geologist has undertaken to chronicle and to comment upon all the volcanic phenomena which are observed in Italy and the surrounding islands. Two parts of the periodical have been published, giving details of every commotion felt during 1873. The ground was in such activity that Prof. Stefano de' Rossi has been able to report more than three hundred separate phenomena. The mean number of seismic commotions in the whole Peninsula is almost *one daily*.

AN aëronautical experiment of great importance took place on March 22. The balloon "Etoile Polaire" was sent up with two aëronauts, M. Sivel and Croce-Spinelli, to test if the respiration of an air rich in oxygen would enable observers to reach a high level without being suffocated by the rarity of the surrounding medium. The experiment was suggested by M. W. de Fonvielle in his "Science en Ballon," and an apparatus was constructed by M. Paul Bert, Professor at the Sorbonne, and a Member of the National Assembly. The "Etoile Polaire" started at 11.40 A.M. from the La Villette gasworks, and at 12.4 P.M. had reached 5,000 metres. The temperature, which was + 13° C. on the ground, had sunk to − 10° C. M. Croce-Spinelli was almost suffocated, but by using the oxygenised-air respirator he recovered. His pulse, which was beating 86 on the ground, was beating 140, and with the respirator 120 only. These experiments were conducted from 12.4 to 1.30 P.M., when the balloon had reached 7,400 metres, where the thermometer sank to − 24°. No observation was taken during the descent, which took place at 2.12 P.M. at Bar-sur-Seine, 120 miles from Paris. On landing, the temperature was + 17°. M. Croce-Spinelli had with him a little hand spectroscope supplied by M. Janssen. He states that all the aqueous lines belonging to the vapour had disappeared, and that the solar rays *D* and *F* were growing very dark. When not using the respirator the sky seemed quite dark, but the blue colour was restored when respiring oxygenised air. The measurement of the balloon was 2,800 cubic metres. It was elevated with 1,650 metres of lighting gas, carrying with it 380 ballast, of which no more than 40 were left for descending from 7,400 metres. The altitude is higher than any French aëronaut has reached up to the present time, but Mr. Glaisher, without the help of any oxygenised air, navigated the atmosphere on several occasions to that distance from the sea-level—as on August 28, 1862, April 18, 1863, June 26, 1863, &c. He had no feeling of suffocation except when he reached 10,000 metres on September 5, 1862. It is to be supposed that with proper care and with persons properly trained and selected, the method, although efficacious, is not required except at higher level or for the purpose of increasing the comforts of aërial travellers. It is very probable that it is useless to keep several mixtures and that pure oxygen is better as being more efficacious and less bulky.

THIS year's International Exhibition at South Kensington opens next Monday.

AT the annual general meeting of the Royal College of Physicians of London, held on March 30, Sir George Burrows, F.R.S., was re-elected President of the College.

ASTRONOMERS will be interested to learn that among the numerous able men whom the President of the republic of Ecuador has gathered to that city in order to develop the University of Quito, there has appeared one, Father Menten, whose interest in astronomy has been such as promises to settle the long-mooted question as to an observatory in that city. Menten has now returned to Quito laden with a portion of the instrumental outfit that he was ordered to secure at Munich. Among the apparatus is a six-inch meridian circle. Father Menten was for some time a pupil of the eminent Argelander.

THE Royal Academy of Belgium has announced the following subject for its prize essay for 1875:—The relation of heat to the development of phanerogamic vegetation, especially with respect to periodic phenomena, and the value of the dynamic influence of solar heat on the evolution of plants.

IT has been proposed to hold a fungus show in Scotland; a preliminary meeting is to be held in Marischal College, Aberdeen, on April 14.

MR. J. C. MELLIS, late Commissioner of Crown Property at St. Helena, announces as in the press a work on the geology, fauna, flora, and meteorology of St. Helena. Separated so far as the island is from any mainland, and showing no trace of any former connection with a continent, there are many questions of interest as to the origin of its flora and fauna. This work is stated to be based on Mr. Mellis's own observations, and promises to be of great value.

MR. WATSON has brought to England a series of paintings by a Japanese artist illustrating some of the customs of the Ainos, who inhabit the island of Yezo, or Sesso as it is sometimes spelt, north of Niphon. Not much is known of the Ainos, but they are regarded as the aborigines of Yezo, who have been driven inland by the fringe of Japanese settlements all round the coast. The first of the series is said to represent the traditional origin of the race. A woman is in a cave weeping, and a dog is carrying to her a red flower, apparently a rose. It does not, however, appear to be known what this symbolises. A coloured drawing of a male Aino executed with great care and regard to detail, shows bows and arrows of native make and a sword of Japanese manufacture. A cord is worn round the head to help to form a support for a weight, as they never carry burdens on their backs. The portrait of a female shows the broad tattooing round the upper and lower lip and on the arms, as the sign of marriage. Both the male and female wear earrings, which is a contrast to the Japanese, who never wear personal ornaments. The woman is playing on a kind of guitar with five strings. In the views of the interior of the dwellings all cooking utensils have their Japanese names written against them by the artist, so that their use may be known though their shape is not familiar. Some of the series illustrate whale harpooning, which is done with a two-pronged harpoon, and many are devoted to the festival of the bear. After killing a bear they appear to sit round it in state solemnity, as if worshipping it and offering it food and drink. Dresses of many colours are worn, and the bear is decorated. Their commerce is represented as the collecting of seaweed, drying it, packing it in bundles and selling these by weight to Japanese. The drawings of the articles used for food, laminaria, crabs, holothuria, cuttle-fish, &c., are drawn with great detail and delicacy of colouring.

THE first volume of "Repertorium annuum Literaturæ Botanicæ Periodicæ," has appeared, published at Harlem under the editorship of Van Bemmalen. It professes to give an account, for the year, of all botanical papers read in full or in abstract before Societies, and also to notice memoirs and communications to Societies.

DR. L. JUST, Professor of Agricultural Chemistry in the Polytechnic School at Carlsruhe, has just published, with the co-operation of several distinguished men of science, an Annual of Botany, intended to form a complete record of all botanical works published during the year.

THE Bureau des Longitudes is no longer connected with the National Observatory of Paris. It is to have its own budget, library, and a public building will be arranged for its private use. The most influential member and president is M. Faye, who is making a public appeal to French astronomers asking them to devote themselves to the spectroscopic observations of the sun.

MR. W. S. CLARK, president of the Massachusetts Agricultural College, publishes a lecture On the Circulation of Sap in Plants, containing a great amount of information as to our knowledge of the subject, and a number of tables and diagrams to illustrate the maximum and minimum pressures of the sap in different trees. Mr. Clark maintains, in opposition to some recent statements, that there is a flow of crude sap upwards in the wood, and a flow of organisable material essential to the life of the plant proceeding from the leaf to the root through the bark and cambium layer, from which the growth of the season is formed.

THE annual *réunion* of the Sociétés Savantes des Départements of France, will take place at the Sorbonne from April 8 to 11.

ONCE every year French astronomers hold a general meeting at the office of the Minister for Public Instruction. It will take place this year in the beginning of April.

FRENCH officers belonging to the general staff are regularly attending the observatory to be trained in astronomy. A special building has been constructed for their use by M. Leverrier.

DR. HYRTL, the eminent Professor of Anatomy in the University of Vienna, delivered his final lecture on March 16, and took leave of his class. In the afternoon he met an assemblage of more than three thousand pupils and friends, who presented him with addresses and other expressions of esteem, which were feelingly acknowledged by him.

THE additions to the Zoological Society's Gardens during the past week include two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mr. J. Hayward; a White-necked Crow (*Corvus scapulatus*) from the Gold Coast, West Africa, presented by Capt. E. Whitehead, 42nd Highlanders; an Eland (*Oreas canna*), born in the Gardens; two Yellow-headed Conures (*Conurus jendaya*) from south-east Brazil, purchased.

CELESTIAL CHEMISTRY *

II.

WE have now gone through as briefly as I can manage to do it, the principal points in chemistry and the principal points in spectrum analysis with which we have to deal when we are dealing with the chemical substances of our own planet. You see the point of inquiry is the chemical and spectroscopic study of the differences between atoms and molecules. We have now to apply these two lines of thought to a consideration of what I have called Celestial Chemistry.

In the first place, how is the study of Celestial Chemistry carried on? To answer this question I will give you an idea of the way in which the spectroscope is added to the telescope. We, of course, require a spectroscope in order to bring into play refraction or diffraction, and we equally, of course, require a telescope in order to collect the light which comes to us from the various light sources which are to be found in the sky. You now see a photograph of what is technically called the eye-piece end of the most perfect and most powerful telescope in the world, which I am proud to say is in England. It is the telescope belonging to Mr. Newall of Gateshead. We see the principal telescope and the various finders, which enable the observer to find the most delicate object which shines in the sky.

You will get an idea of the enormous quantity of light collected by this instrument, when I tell you that the diameter of the pupil of the eye is less than a quarter of an inch, and that the diameter of the object-glass of this telescope is 25 inches, and that the quantity of light utilised depends upon the squares of the diameters. Of course, when we have to deal with feeble celestial objects lying at an immense distance from us, and which give but little light, it would be absolutely impossible to use the prism with any effect unless in the first instance we thus collected a large amount of light to work with.

Now then, having our spectroscope attached to our telescope in the way indicated, to what must we turn our attention? You will see in a moment that it is useless to consider such a body as the moon. I say useless, of course bearing in mind that we are here dealing with chemical considerations, because the light which we get from the moon is simply sunshine second hand, and the moon has no atmosphere. The same remark applies to a certain extent when we deal with the planets, because they also, as you know, are lit up like the moon by the sun, but they have atmospheres, and much is to be learned from them in this way. What we have to do in the main to get a just general outline, in order to study this Celestial Chemistry is, to confine our attention to those bodies which shine by their own light. And if you think the matter over for a minute you will see that there are two distinct classes of such bodies. In the first place there are the *Nebulæ*, and *Comets*, and *Meteors*, which shine by their own light. And again, there are the *sun* and the *fixed stars*, which shine also by their own light; but with this important difference, that while we get the initial light radiated by a nebula, a comet, or meteor, the light which we get from a star is not the initial light, but a difference light; a great deal of the light radiated having been stopped by the atmosphere of the star. So that in the case of a sun, by which of course I mean a star—our sun being merely the star which is nearest to us—although it shines by its own light, we get a difference light while we get what we may call the total light from the first class.

And here let me add that it is the chemical composition of the atmosphere of the star which thus stops the light which we can study. If the stars had no atmospheres there could be no star chemistry, because their spectra would be continuous, and in that case neither qualitative nor quantitative spectrum analysis would be possible.

We deal then with radiation in objects of the first class and with absorption in objects of the second class.

Let us commence our study of those objects which shine by their own light by the study of the comets and the meteors. It is unfortunate that since the more general use of the spectroscope, no large comet has made its appearance, but still some small ones have been observed.

There is a spectrum which in the main is common to a great many of the compounds of carbon; and here I may parenthetically remark that carbon is, of the sixty-three elements to which I have referred, the only element which with the electric power that we can employ in the arc presents us with the distinct appearance of a compound spectrum. Side by side with this generic spectrum of the compounds of carbon I will show you the spectrum of the head of a comet which was observed some years ago; and I may add that several other comets have been observed since, with the indication that their spectra are to a very large extent, if not absolutely, the same; so that we may

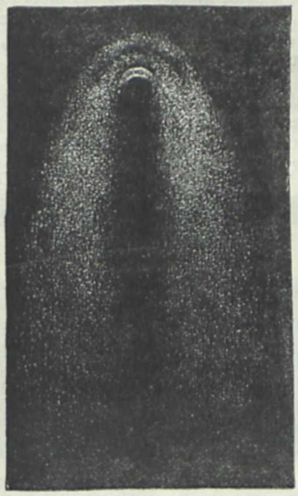
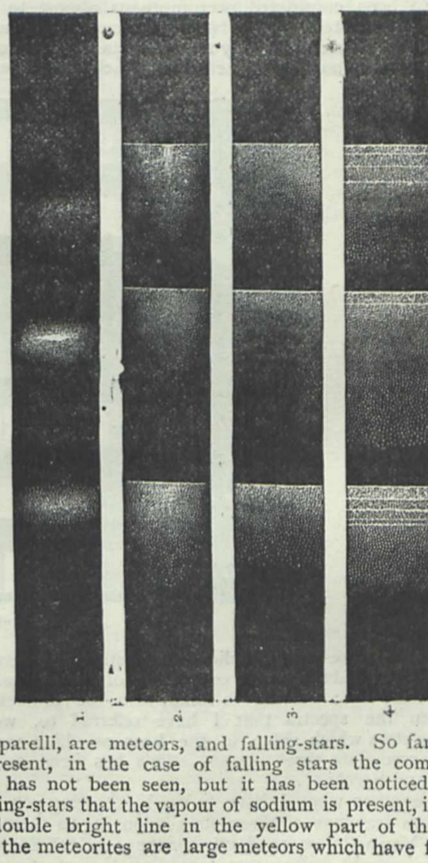


FIG. 1.—Head and Envelopes of a Comet.

say there is a probability that, in the case of some of the comets at all events, we are dealing with a class of bodies in which a compound of carbon is concerned, or if not that, that the molecules of the comet resemble somewhat those of the chemical substances which give us such spectra.

Closely connected with comets by the recent hypothesis of Schiaparelli, are meteors, and falling-stars. So far as I know at present, in the case of falling stars the cometary spectrum has not been seen, but it has been noticed in several shooting-stars that the vapour of sodium is present, indicated by the double bright line in the yellow part of the spectrum. Now the meteorites are large meteors which have fallen to the



1. 2. 3. 4.

FIG. 2.—1, Spectrum of Brorsen's comet; 2, Spectrum of Winnecke's comet; 3, Spectrum of carbon in olefiant gas; 4, Spectrum of carbon in olive oil. (Huggins.)

* Revised from short-hand notes of a Lecture delivered at the Quebec Institute, on Tuesday, December 16, 1873. Continued from p. 414.

earth, and this being so, we can chemically examine them as easily as substances which have consolidated here. These bodies may be roughly divided into iron meteorites and stone meteorites, and roughly again into meteorites which contain elementary metallic molecules, and others which contain compound molecules, that is, mixtures of metals with the metalloids.

The observations of the spectra of nebulæ we owe mainly to Mr. Huggins, a distinguished English observer, who has given very much attention to this branch of inquiry. One thing we know for certain about the nebulæ from these observations is this: we are dealing either with gases alone or with solids banging about in gases, and one of the bright lines which we observe tells us that we are dealing with hydrogen gas; so that the same method of inquiry which, applied to comets, tells us that probably in some cases we are dealing with a compound of carbon, tells us that we are chiefly dealing, in the case of nebulæ, not with a compound of carbon or with a compound of anything, but with a true element—hydrogen.

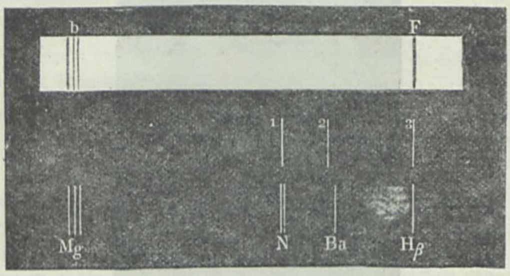


FIG. 3.—Spectrum of the nebulæ.—1, 2, 3, lines observed. Above, the solar spectrum is shown from *b* to F; below, the bright lines of magnesium, nitrogen, barium, and hydrogen, in the corresponding part of the spectrum.

That, you see, is a very great step; and a very few years ago it would have been considered presumptuous almost to think that man could ever tell what substances were building up the nebulæ which lie at such infinitely remote distances from us.

So much then for comets and meteors and nebulæ. These exhaust the bodies in space which shine by their own light, the light not being subsequently absorbed by an atmosphere through which it passes; we will now therefore pass to the class of Stars.

With regard to stars, I have a diagram to bring before you, which summarises in a convenient way a good deal of the work which has been very carefully done by Father Secchi of Rome. I shall have to refer to several other diagrams afterwards; but this, I think, is the best one to place before you in the first instance.

The spectra in the diagram are the spectra of various stars. You will at once see that there is a difference between those spectra, and you will see that there is a double difference between some of them. In the first place, you have an extreme simplicity in some cases and complication in the others. But

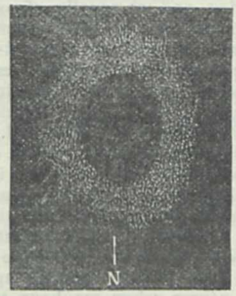


FIG. 4.—Ring nebula in Lyra, with its spectrum.

you see, also, that the question of simplicity and complication is not the only question, that is a question merely of degree; but there is a difference in kind. For instance, you will at once acknowledge a difference in kind between the spectrum of α Herculis and Sirius.

The diagram of the former, although it has been made in no laboratory, and although it deals with no metalloids or no compounds which have been got upon the earth, is as good a diagram as I can put before you to explain what I mean by the channelled structure of the spectrum of the metalloids as opposed to the line spectrum of the metals.

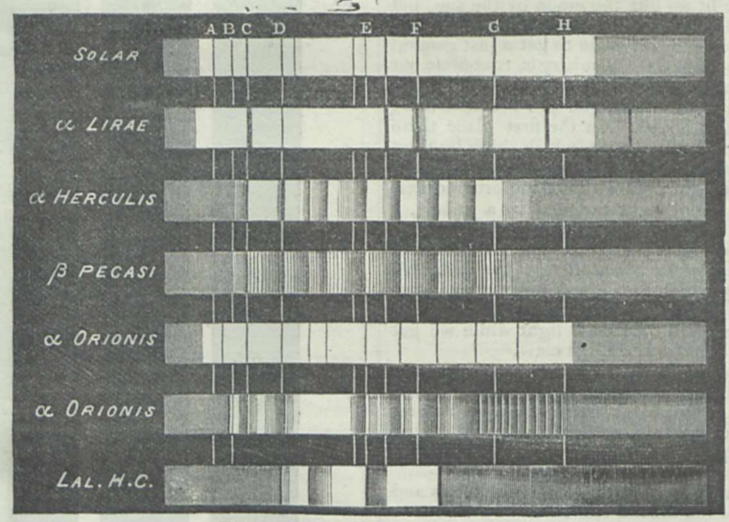


FIG. 5.—Various Stellar Spectra (Secchi).

Bearing in mind the great simplicity of the spectra of stars like α Lyræ and Sirius, and the greater complexity of the spectral lines in a star like the sun; bearing in mind, also, the difference in kind between the spectra that I have referred to, we can divide all the stars which shine in the heavens which we have already observed, into three classes. This has been done by Rutherfurd and Secchi.

Let me strengthen what I have to say by showing you rather more elaborate drawings of three stars belonging to these different classes. You will now see the importance of the considerations which I have brought before you regarding the spectra of the metals and the metalloids. There is the channelled space spectrum of the star α Herculis; there is the banded spectrum of the star α Orionis; and there is the equally banded spectrum of a star in the constellation of the Scorpion. In all these cases you will see we are dealing, not with the first class in which we have simple spectra, but with the second and third classes.

Now let me contrast on another diagram the spectra of two stars, one in the first class and the other in the second. Let us contrast the spectrum of Sirius with the spectrum of the star in Orion, to which I have already referred. In the spectrum of

Sirius observe—for this is a very important point—the extreme thickness of the lines, which are the lines due to hydrogen, and contrast the thickness of these lines and the simplicity of the spectrum with the thinness and great number of the lines in the star in Orion, and the complexity of its spectrum; and remembering that both these maps are on the same scale, let me point out that all the lines which are so thick and so obvious in the spectrum of Sirius, are altogether wanting in the spectrum of the star in Orion.

I hope I have convinced you, by the sight of these diagrams, that supposing the observations on which they are based to be true, we have in the stars which shine three perfectly different kinds of absorption of light going on in the atmospheres of those stars. We have an absorption which we may call a simple absorption, seeing that the lines are few in number; we have an absorption of the same kind, but different in degree, which we may call complicated, seeing that the lines are still lines, but that they are very much increased in number. And, again, in the third class we have an absorption of a different kind altogether; instead of having an absorption of lines we have an absorption of bands. This I shall venture to call a metalloidal absorption.

Of course if we were merely limited to the spectrum of these distant stars, in spite of the enormous skill and care which Mr. Huggins and Father Secchi have brought to bear upon this inquiry, we probably should never go very much further; but you know that the

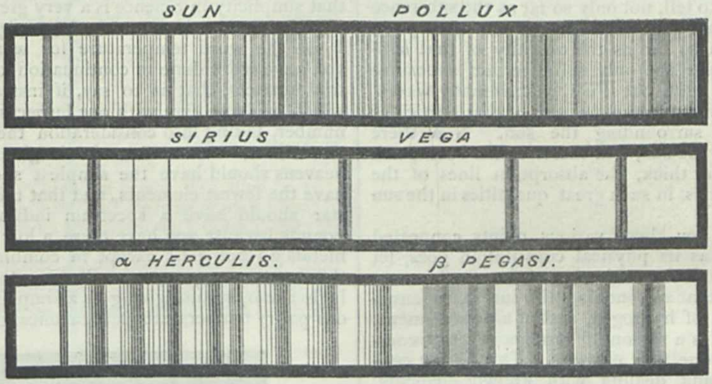


FIG. 6.—The Three Classes of Stars (Secchi).

sun is, after all, our sun, merely for the reason that it is the nearest star; and therefore it is clear to you that if we observe the sun with anything like the attention that it deserves, bearing in mind its comparative nearness to us, we ought to be able to get out of the sun a great number of facts which will help us the better to understand the various appearances in the different stars.

I need not say to you that a great deal of trouble has been taken to understand the sun, to study its physical and even its chemical constitution; and if you will allow me, I will put before you two or three considerations having reference to the sun, which have a bearing of considerable importance upon Celestial Chemistry.

In the first place, let me call your attention to the sun as we see it ordinarily. We see that on the sun there are spots, and that on the limb there is a dimming; both the dimming of the limb and of the spots being due to the absorption of the sun's atmosphere which is at work, as I have already told you in the case of the stars, and which separates the stars as a class from the comets, meteors, and the nebulæ.

Next consider the sun as it is seen in an eclipse. Some of you may be surprised to learn that the sun, as we see it every day, is not by any means the whole sun, but only, so to speak, the kernel of an enormous mass of vapour extending for thousands and tens of thousands of miles around the visible sun.

Now in an eclipse, when all the sun ordinarily visible is hidden,

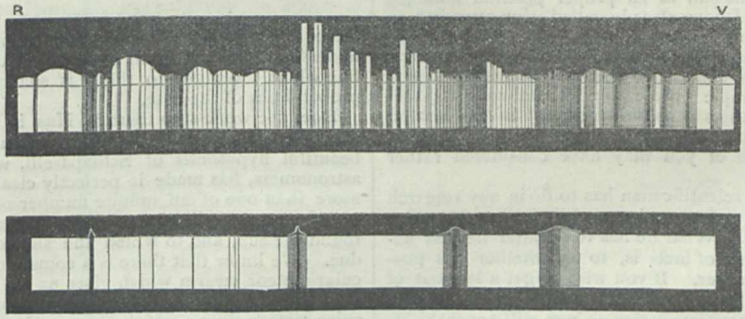


FIG. 7.—Spectra of α Orionis and Sirius (Secchi). In the spectrum of α Orionis the brightness of the spectrum is represented by the height.

we get indications of a very bright something extending to some little distance above the visible sun. On this point I may specially call attention to a photograph taken in the eclipse of 1870, at Syracuse, in which, outside the dark moon which is covering the sun, and therefore outside the sun, a mass of light which we know to be due to vapours surrounding the sun is indicated.

In a photograph taken in India in the year 1871, under somewhat different conditions, we were fortunate enough to record a great deal more of the sun's surroundings. In this, surrounding the dark moon, we have an immense mass of something not very bright extending to a very considerable distance indeed above the visible sun.

Now, by studying these phenomena in the case of the sun, of course we are studying similar things in the case of every star; and what do we find? We find, in the case of the sun, that surrounding the visible sun there extends to a very considerable distance an atmosphere of an element that we have not here, and which is probably lighter than hydrogen. Immersed in this, and therefore extending to a smaller distance from the sun, is another envelope, which has been called the chromosphere, consisting, in the main, of hydrogen. The brightest part of this lies pretty close to the sun. This region is excessively bright—so bright, that by a certain method it can be seen without any eclipse whatever. Immersed in this hydrogen and therefore still nearer the sun there is an enormous quantity of vapours of the different elements existing in the sun, in what we may term a reversing layer, and it is to the absorption of the elements in this layer that the absorption of the sunlight, and therefore, so to speak, the creation of the spectrum of the sun, is in the main due.

I will now direct your attention to two photographs of the solar spectrum, and reminding you that the complexity of a spectrum depends upon the number of elements, and upon the pressure at which the vapours of those elements exist in the atmosphere of any star, you will gather from these photographs a pretty good idea of the extreme complexity of the sun's reversing layer to which I have referred.

In a photograph of any part of the sun's spectrum each of the lines of course has its story to tell, not only so far as the substance which is at work doing that particular part of the absorption is concerned, but also even so far as the quantity of that substance is concerned; because not only will a certain substance absorb particular waves of light, but it will absorb many waves, or few, or none at all, according to the quantity of that particular substance in the envelope surrounding the sun. Now there is a great deal of calcium in the sun, and therefore the absorption lines of the calcium are very thick, the absorption lines of the other metals which do not exist in such great quantities in the sun being very much thinner.

Having brought before you these various points connected mainly with the sun, so far as its physical constitution goes, let us consider what is the chemical constitution of the sun.

I have already told you that surrounding the sun is an envelope composed in the main of hydrogen, and of a new element, and that nearer to the sun is a region of vapours of great complexity, containing at least one new element. This region contains, besides hydrogen, and dealing with known elements, magnesium, sodium, titanium, calcium, nickel, chromium, iron, manganese, aluminum, copper, zinc, barium, cobalt, and so on, and latterly we have had reason to suppose that some six or seven new elements must be added to the list—potassium, lead, cerium, uranium, strontium, and cadmium. Further, if instead of the new "atomic weights" of the elements we take the old "combining weight" we find that the arrangement of these layers round the sun follows the vapour densities of the various substances either absolutely or very closely.

This then is the verdict of the prism with regard to the chemical constitution of the sun, the nearest star that we can get at; and I think you will acknowledge that if the prism had done nothing else it would have done good work. But I think it has done very much more, because it has enabled us not only to chronicle those things as existing in the sun, but in connection with the other facts which I have already brought before you it has enabled us to place the sun in its proper position amongst the stars. For instance, I have already called your attention to the first, second, and third classes of stars. Is the sun in the first, the second, or the third class? Does its spectrum contain few or many lines? or are there channelled spaces or bands? Its spectrum is not excessively simple; there are no channelled spaces or bands; and therefore the sun is to be placed in the second class of stars. Can we then go beyond this chronicle of facts, which I am afraid some of you may have considered rather dry?

You know that what a scientific man has to do in any research is not merely to add fact to fact, and to go blindly looking after facts irrespective of order. What he has to do after he has accumulated a certain number of facts is, to try whether it is possible to arrange them in order. If you wish to get a law out of any accumulation of facts in physics, in chemistry, or astronomy, you must first get your facts into order or you will never do it. Is there any possible order into which we can group these various facts to which I have referred? I venture to think there is.

Call to mind the three classes of stars. Is there any other physical quality tacked on to those differences? Yes. The stars with the simplest spectra are on the whole the brightest stars in the heavens; and the channelled spaced stars are on the whole the dimmest stars in the heavens. Of about 500 stars which have been already observed, over 300 are of the complicated second order or type. There are a great many bright stars of the first order, but an extremely small number, only, I think, about 27 of the third order with the channelled space spectrum. Now, if this be true, and if it be fair to assume that the star which is the brightest is on the whole the hottest, and I think it is fair to say so, if you take all other things as equal, then you come to a generalisation of this kind, that the brightest and hottest stars in the heavens have the simplest spectra, and the dimmest, reddest, coldest stars have a spectrum entirely different. If this be so, can we connect these facts? I think so. Grant these facts (and the future alone will show whether they are facts), and the thing is clear. We may group them all together by supposing that in the stars of the first and second classes there are dissociating forces at work which, from considerations which I have not time to bring before you now, we can imagine to be infinitely higher, or at least considerably higher, than any dissociating force that we can get here even with the electric spark. If you imagine in these stars an atom-severing force greater than we can obtain here, you can at once group in a working hypothesis all the facts which I have brought before you, and in a simple way; and let me add that simplicity in Science is a very great evidence of truth.

If you assume that at the highest possible temperature—here I use the word temperature for want of a better—of a star you have work done in continuation of the work done in terrestrial furnaces, that is to say, if instead of having 63 elements which we have here with our furnaces, there is a much smaller number, taking into consideration the increase of temperature, you will see at once that the brightest and hottest star in the heavens should have the simplest spectrum, because there you have the fewest elements, and that the coldest, reddest, dimmest star should have a spectrum indicating metalloids and compounds because you have there a low temperature, at which the metals do not exist except in combination. And if you think this matter over you will see that this suggestion of a higher temperature giving us a simpler condition of what we with our paltry temperatures call chemical elements, instead of making

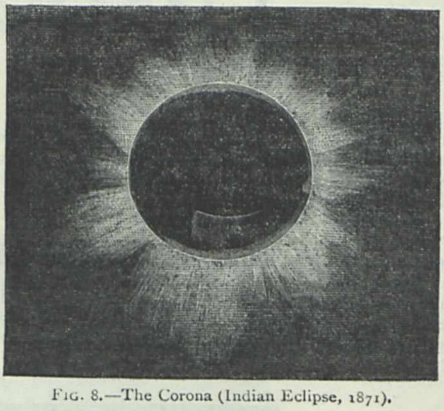


FIG. 8.—The Corona (Indian Eclipse, 1871).

these stellar spectra complicated, difficult to recollect and to understand, puts them all in one line easily to be grasped, and a line which I venture to think is somewhat coincident with the probabilities of the case.

Does the work stop there? Has it nothing to say to comets and meteorites? Here again it has a question to ask. The beautiful hypothesis of Schiaparelli, which is accepted by all astronomers, has made it perfectly clear that a comet is nothing more than one of an infinite number of meteors or meteorites, or whatever you like to call them, travelling in cometary orbits round the sun, and to which the showers of shooting stars are due. We know that there is a comet connected with that particular meteor swarm which gives us the November meteors, and we know that there is another comet connected with that particular meteor swarm which gives us the August meteors; and we assume that in all probability there are millions, or any enormous number that you like, of meteors, or meteorites, or shooting stars peopling a part or the whole length of that concrete orbit so to speak.

How is it then that there is only one comet amongst all that infinite number of potential meteors or meteorites? Here again I am sure that the future will enable the prism to throw an immense light, and we already have a glimmering. If, for instance, you assume that out of a star of the second class, in the reversing layer of which there are no metalloids, portions of the atmosphere are, by forces which we know to be at work, thrown bodily from the sphere of the star's attraction into space, those vapours on being cooled would give us very much the same kind of chemical composition that we get in the well-known masses called iron meteorites, composed principally of iron and nickel. While on the other hand, in the case of stars of the third class, in which it seems excessively probable that we have both metalloids and compounds, and very little pure metal, that is to say, metal not in combination, in the reversing layer, we have also the large class of silicate meteorites, the origin of which is possibly due to such stars in exactly the same way as the origin of the iron meteorites would be due to stars of the second class.

If this be so, then it would seem that a comet is simply a meteorite which contains something which is volatile at a very low temperature. Amongst the vapours known to chemists, which are volatile at the lowest temperature, are the hydrocarbons. I have already pointed out to you that as far as observations have gone on comets we have been able to detect nothing but the possibility of a spectrum of carbon, or of a compound of carbon.

Here again dimly and darkly the prism is pointing us to a possible connection between all the stars in heaven and all the comets and all the meteors which flit through the celestial spaces and fall upon our own earth.

I have already referred to the verdict of the prism in connection with the nebulæ, and there can be very little doubt, I think, that before the world is very much older the prism will also be perfectly competent to connect nebulæ with stars as it may possibly have already connected comets and meteors with them; but this point certainly is at present one of great difficulty, and it is a difficulty which no student of science will care to get out of, since in matters of this kind a difficulty is a matter of the highest importance, showing you as it does that part of the field of nature which requires most study.

I quite feel that this enormous subject, which modern science is opening up, is one the importance of which is so great and the interest in which is so general that I am sorry that the task of talking about it has not fallen upon the shoulders of one who is more competent to do it than I am. I hope, however, that feeble as my advocacy may have been, you are prepared to agree that the time will come when Celestial Chemistry, as investigated by means of the prism, will be acknowledged to be one of the most important branches of modern science.

J. NORMAN LOCKYER

SCIENTIFIC SERIALS

Journal of the Franklin Institute, February.—In this number Mr. Prindle has a paper (with numerous illustrations) On Recent Improvements in Construction of the Gunpowder Pile-driver.—A long and instructive paper by Mr. Loiseau, On Artificial Fuel, gives a *résumé* of what has hitherto been done in this direction; the author describing his own method, in which a mixture of 5 per cent. clay and 95 per cent. coal-dust, moistened with milk of lime, is moulded into oval lumps, which are then bathed in a waterproofing liquid (rosin dissolved in crude benzine) and dried. With 14 men only, a production of 150 tons per day can, it is said, be easily attained.—Prof. Houston announces the discovery of a new allotropic modification of phosphorus, obtained by boiling good phosphorus repeatedly in potassium hydrate (under certain conditions). This new modification retains for an indefinite time, apparently, the liquid state, even when exposed to temperatures considerably below the melting-point of ordinary phosphorus, from which it also differs in its non-oxidation on exposure to air, and, consequently, its not shining in the dark.—Mr. Chesebrough describes the construction of the Detroit River Tunnel; and Prof. Thurston has a note relative to the estimation of the chemical value of coals containing large quantities of ash.—Among the "Items and Novelties," it is stated that Prof. Thurston has gone very carefully into the subject of a scheme published by Mr. Chesebrough, for keeping canals open in winter by warming the water. The professor's calculations are given, and he finally arrives at an estimate of 5,412,500 dols. as the first cost of apparatus for a canal 350 miles long, 70 feet deep, in the latitude of Central New York; and 1,670,200 dols. for the maintenance per annum. He thinks the scheme deserving of investigation.

Astronomische Nachrichten, Nos. 1,976 and 1,977.—These numbers contain a paper by M. Lohse on the estimation of the depth of sun-spots, and at the same time to ascertain the influence of solar refraction. The principle of his method is as follows:—When a spot having its umbra concentric with penumbra, when seen from the vertical, is seen near the sun's limb, the umbra becomes excentric, and the depth of the umbra p' will be given by the formula $p' = \frac{\epsilon}{\cos \alpha}$ when ϵ is the excentricity and α the heliocentric position-angle of the spot from the axis, and assuming the spot steady, p' should remain constant as α and with it ϵ change. This, however, is found not to be the case, for p' gradually increases as the spot gets from the limb, showing it was raised by refraction at the limb, and therefore this change in the value of p' gives a measure of the sun's refraction. The same author also contributes a paper on the effect of the atmosphere of Venus in the transit over the sun, and he recommends the examination of its atmosphere with the spectroscope for absorption-bands.—J. Hortazzi gives the observations of transits for longitude of Nikolagew.—J. Palisa gives a large numb r of ring-micrometer observations on the minor planets and a few comets.—Dr. Holetschek gives the position of some seventy comparison-stars for planets and comets.—E. Stephan gives a list of ten new nebulæ discovered and observed at Marseilles; all seem excessively small. The elements of the new comet are given by Wilhelm as follows:—

$$T = \text{March } 9{\cdot}8125 \text{ Berlin time.}$$
$$☊ = 48^\circ\ 17'\ 37''{\cdot}6$$
$$\pi = 309^\circ\ 27'\ 46''$$
$$i = 52^\circ\ 29'\ 52''{\cdot}3$$
$$\log. q = 8{\cdot}591600$$

Memorie della Soc. degli Spectroscopisti Italiani, Oct. and Nov. 1873.—These numbers contain an interesting paper by P. Rosa, assistant at the Observatory at Rome, on the variability of the sun's diameter. He discusses the observations at Greenwich and other places from the year 1750 to 1870, during which time some 13,000 measurements of the solar diameter were made. From these observations he has constructed curves showing the variation of diameter, together with the variation in the number of spots. The agreement of these two curves is not very strong, but on his constructing secondary curves from every fourth year of these primary curves, beginning with any year, the resemblance becomes striking. There are also monthly curves given, showing the mean variation in diameter of the sun for each month, taking a mean of ten years for each curve. These show two maxima in March and September, and two minima in January and June. On examining the curve of variation in diameter, a marked minimum occurs about the year 1792. The author sets forth a theory to account for this variation, that since a comet, when at its perihelion, throws off large quantities of matter, so the sun when at its periastre may throw off matter and become reduced in size, and if such is the case its periastre happened in 1792.—G. Lorenzoni contributes papers on observations on the chromosphere, and on observing contacts in eclipses of the sun with the spectroscope.—G. de Lisa gives observations on solar spots in September, October, and November 1873, made at Palermo, giving a mean of about ten spots a day.—Prof. Young gives a note on the use of M. Rutherford's gratings in the place of prisms for the spectroscope.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 19.—On the Attractions of Magnets and Electric Conductors, by George Gore, F.R.S.

Being desirous of ascertaining whether in the case of two parallel wires conveying electric currents the attractions and repulsions were between the currents themselves or the substances conveying them, and believing this question had not been previously settled, I made the following experiment:—

I passed a powerful voltaic current through the thick copper wire of a large electromagnet, and then divided it equally between two vertical pieces of thin platinum wire of equal diameter and length (about six or seven centimetres), so as to make them equally white hot, the two wires being attached to two horizontal cross wires of copper.

On approaching the two vertical wires symmetrically towards the vertical face of the one pole of the horizontally placed magnet, and at equal distances from it, so that the two downward currents in them might be equally acted upon by the downward and upward portions respectively of the currents which circulated round the magnet-pole, the one was strongly bent towards and the other from the pole, as was, of course, expected; but not the least sign of alteration of relative temperature of the two wires could be perceived, thereby proving that not even a small proportion of the current was repulsed from the repelled wire, or drawn into the attracted one, as would have occurred had the attraction and repulsion taken place, even to a moderate degree, between the currents themselves; and I therefore conclude that *the attractions and repulsions of electric conductors are not exerted between the currents themselves, but between the substances conveying them.*

Some important consequences appear to flow from this conclusion, especially when it is considered in connection with Ampère's theory of magnetism, and with the molecular changes produced in bodies generally by electric currents and magnetism.

As every molecular disturbance produces an electric alteration in bodies so, conversely, the discoveries of numerous investigators have shown that every electric current passing near or through a substance produces a molecular change, which is rendered manifest in all vessels, liquid conductors, and even in the voltaic arc by the development of sounds, especially if the substances are under the influence of two currents at right angles to each other. In iron it is conspicuously shown also by electro-torsion, a phenomenon I have found and recently made known in a paper read before the Royal Society.

Numerous facts also support the conclusion that the molecular changes referred to last as long as the current. De la Rive has shown that a rod of iron, either transmitting or encircled by an electric current, emits, as long as the current lasts, a different sound when struck; and we know it also exhibits magnetism. The peculiar optical properties of glass and other bodies with regard to polarised light discovered by Faraday also continue as long as the current. A rod of iron also remains twisted as long as it transmits and is encircled by electric currents; and in steel and iron the molecular change (like magnetism) partly remains after the currents cease, and enables the bar to remain twisted.

That the peculiar molecular structure produced in bodies generally by the action of electric currents also possesses a definite direction with regard to that of the current, is shown by the rigidly definite direction of action of magnetised glass and many other transparent bodies upon polarised light; also by the difference of conductivity for heat and for electricity in a plate of iron parallel or transverse to electric currents; by the stratified character of electric discharges in rarefied gases, and the action of electric currents upon it; and especially by the phenomenon of electro-torsion. In the latter example an upward current produces a reverse direction of twist to a downward one, and a right-handed current developes an opposite torsion to a left-handed one; and the two latter are each internally different from the former. As each of these four torsions is an outward manifestation of the collective result of internal molecular disturbance, and possesses different properties, these four cases prove the existence of four distinct molecular movements and four corresponding directions of structure; and the phenomena altogether are of the most rigidly definite character.

As an electric current imparts a definite direction of molecular structure to bodies, and as the attractions and repulsions of electric wires are between the wires themselves and not between the currents, repulsion instead of attraction must be due to *difference of direction of structure* produced by difference of direction of the currents.

Although the Ampèrean theory has rendered immense service to magnetic science, and agrees admirably with all the phenomena of electro-magnetic attraction, repulsion, and motion, it is in some respects defective; it assumes that magnetism is due to innumerable little electric currents continually circulating in one uniform direction round the molecules of the iron; but there is no known instance of electric currents being maintained without the consumption of power, and in magnets there is no source of power; electric currents also generate heat, but a magnet is not a heated body.

If, however, we substitute the view that the phenomena of attraction and repulsion of magnets are due, not to continuously circulating electric currents, but (as in electric wires) to definite directions of molecular structure, such as is shown by the phenomena of electro-torsion to really exist in them, the theory becomes more perfect. It would also agree with the fact that iron and steel have the power of retaining both magnetism and the electro-torsional state after the currents or other causes producing them have ceased.

According to this view, a magnet, like a spring, is not a source of power, but only an arrangement for storing it up, the power being retained by some internal disposition of its particles acting like a "ratchet," and termed "coercive power." The fact that a magnet becomes warm when its variations of magnetism are great and rapidly repeated, does not contradict this view, because we know it has then, like any other conductor of electricity, electric currents induced in it, and these develop heat by conduction-resistance.

According also to this view any method which will produce the requisite direction of structure in a body will impart to it the capacity of being acted upon by a magnet; and any substance, ferruginous or not, which possesses that structure has that capacity; and in accordance with this we find that a crystal of cyanite (a silicate of alumina) possesses the property, whilst freely suspended, of pointing north and south by the directive influence of terrestrial magnetism, and one of stannite (oxide of tin) points east and west under the same conditions.

Geological Society, March 11.—John Evans, F.R.S., president, in the chair.—The following communications were read:—On the relationship existing between the *Echinothuridæ* Wyville Thomson and the *Perischoechinidæ* McCoy, by R. Etheridge, jun. In this paper the author referred in the first place to the peculiar characters of the genera *Calveria* and *Phormosoma* Wyville Thomson, and especially to those in which they approach the cretaceous genus *Echinothuria* S. P. Woodward, and which led Prof. Wyville Thomson to include these three forms in his group Echinothuridæ. He remarked that an overlapping of the interambulacral plates, more or less like that occurring in these three genera, is met with also in *Archæocidaris* McCoy, and *Lepidechinus* Hall, belonging to the group of palæozoic Echini which McCoy proposed to call Perischoechinidæ, and which is characterised by the presence of more than three rows of plates in the inter-ambulacral areas. As there is no overlapping of these plates in the other genera referred to this group, it includes two types of structure. The author then discussed the characters presented by the test in the genera of the Perischoechinidæ (namely, *Archæocidaris*, *Palæchinus Perischodomus*, *Lepidechinus*, *Eocidaris*, *Melonites*, and *Oligoporus*), and pointed out that although we have no conclusive evidence of the presence of membranous interspaces along with the overlapping plates in *Archæocidaris*, the fragmentary condition in which the remains of that form are usually found would lead us to infer their existence. No known palæozoic genus exhibits the want of distinction between the ambulacra and interambulacra on the ventral half of the test seen in the recent genus *Phormosoma*. In *Melonites* and *Oligoporus* the author described an increase in the number of rows of plates in the ambulacra, and he indicated that all the Perischoechinidæ differ from the later Echini by the increased number of perforations in the ocular and genital plates.—On the discovery of Foraminifera, &c., in the boulder-clays of Cheshire, by William Shone, jun. In this paper the author described the occurrence of Foraminifera, Entomostraca, and some other small organic bodies in the boulder-clay at Newton by Chester and at Dawpool. They were found partly in the interior of specimens of *Turritella terebra*, and partly free in the boulder-clay.—On the occurrence of a Tremadoc area near the Wrekin in South Shropshire, with description of a new fauna, by Charles Callaway. The author stated that in an exposure of light green, micaceous shales dipping south-east at 50° at Shineton near Cressage, which are represented as of Caradoc age in the Geological Survey Map, he found a series of trilobites and other fossils which induced him to regard these Shineton shales as belonging to the Lower Tremadoc series. He described as new species:—*Asaphus eos*, *Conocoryphe salteri*, *C. angulifrons*, *Platypeltis croftii*, *Conophrys salopiensis*, *Lichapyge cuspidata*, *Lingulella nicholsoni*, *Metoptoma sabrinæ*, and *Theca lineata*. The author regarded these shales as the equivalents of beds containing *Dictyonema*, found near Malvern and at Pedwardine.

Anthropological Institute, March 24.—Prof. George Busk, F.R.S., president, in the chair.—The President exhibited and described an Ashanti skull. The specimen, with other bones of the body, was taken by Surgeon-Major Gore from an outlying camp which had been deserted on the approach of the British troops. It presented the characteristics rather of a female than a male skull, but Mr. Gore affirmed that he had never heard of the Ashantis carrying about the bones of a woman. Women, in fact, held such an inferior position, that it could scarcely be believed that the Ashantis would take trouble in the preservation of their remains. If the skull exhibited belonged to a man, he could not have been a military leader, but he might have had such a rank in his tribe as entitled him to the honours that were evidently bestowed on his remains. The paper gave full descriptions and detailed measurements.—A paper was read by Rev. Dunbar I. Heath, On the Origin and Development of the Mental Function in Man. He thought that in the ordinary view the mind is considered as a central essence. Around it is the brain, and still further on the outside the world surrounds the brain. It would conduce towards explaining the facts of mental function if we supposed a material film to coincide with the outside surface of the brain, which might be specialised under the name of Psychoplasm. To that film he would confine mental, as distinguished from cerebral, function; so that the mind would be imaged, not as being the centre, but between brain and world. The paper explained mental growth on that hypothesis.—Mr. W. L. Distant read a paper On the Mental Differences between the Sexes. The question discussed in the paper was—Is there clearly proved to be mental difference between the sexes, and is that difference one of kind or only of degree? Authorities were quoted to show the undoubted physical differences, such as weight of brain, form of skull, &c., also the now moderately well-established fact that in primitive races the hair of women approximates more closely to that of man than obtains in a higher state of civilisation. But it having been clearly proved that the advance of man is shown by a higher form of skull and increase of the cranial capacity, an attempt was made to show some of the conditions that had retarded woman in the mental struggle. The result seemed to prove that the mental divergences might be greatly accounted for—firstly, by sexual selection, difference of education, and force of custom; secondly, by physiological conditions; and that as the race progresses, the cranial capacity of the sexes, though not becoming identical, which is a physical impossibility, will yet become much less distinct and divergent, which is a moral certainty if based on moral conclusions.

Physical Society, March 21.—Dr. J. H. Gladstone, F.R.S., in the chair.—J. H. Fleming, read a paper On the new contact-theory of the Galvanic Cell. After discussing the most recent views regarding the contact and chemical theories, Mr. Fleming exhibited the action of his new battery in which metallic contact of dissimilar metals is completely avoided. The battery consisted of thirty test-tubes of dilute nitric acid alternating with the same number of tubes of sodium pentasulphide, all well insulated. Bent strips of alternate lead and copper connected the neighbouring tubes. By this device the terminal poles are of the same metal. On connecting with a coarse galvanometer, the needle was violently and permanently deflected. Tested by the quadrant electrometer the potential was shown to increase regularly with the number of cells. The sixty cells on first immersion showed an electromotive force exceeding that of Daniell's cells. The principle upon which the action depends is that in the acid lead is positive to copper; in the sulphide it is negative. Mr. Fleming further showed how by using the single fluid nitric acid and the single metal iron, a similar battery could be constructed, provided one-half of each iron strip was rendered passive. In this form also no metallic contacts occurred.—Prof. F. Guthrie illustrated by experiment the distribution of a current of electricity in passing from one pole to another across a conducting medium. This was shown in the case of solids by the stratification of iron filings in the sheets of tin-foil and lead. A current of electricity was passed between two points in a horizontal line lying on the surface of metal placed vertically in the magnetic meridian, and the distribution explored by means of a freely suspended magnet needle. As the needle was gradually lowered its direction of deflection was observed to change at a certain point from east to west. This point was ascertained by experiment to be at a distance below the horizontal line, in which the current entered and left the plate, equal to one-third of the interval between the poles. A similar effect was shown in a liquid conductor.—Prof. G. C. Foster, Dr. Wright, and Dr. Gladstone took part in the discussion of the communications.

Royal Horticultural Society, March 18.—Scientific Committee. Dr. Hooker, P.R.S., in the chair.—The Rev. M. J. Berkeley brought for exhibition Montagne's original drawings of *Artotrogus*. He pointed out that this was only the $\frac{1}{1100}$ inch in diameter, while *Volutella*, with which Mr. W. G. Smith had supposed it might be identical, was from $\frac{1}{800}$ to $\frac{1}{400}$ inch in diameter. Montagne had also found a second species of *Artotrogus*, and of this he showed a drawing. He also remarked that a knowledge of the resting spore of *Peronospora infestans* was a great desideratum. It was to be hoped that, as 100*l*. had been presented to Prof. de Bary to investigate the whole subject, that would be a matter on which he would throw some light.—Mr. Smee communicated a paper on a disease at present very destructive to *Daphne indica*. Numerous diseased plants were exhibited, and the opinion of the Committee was requested upon them. Prof. Westwood said that as the young leaves of the Daphne were entirely free from acari or the young larvæ of *Coccidæ* or *Aphidæ*, although the adjoining full-grown leaves were much diseased, he was not inclined to regard the disease as originating from the attacks of any of these insects, although it might be due to punctures of some flying species of *Capsidæ*, such as *Phytoxoris campestris*, which attacks the buds and young foliage of the common Chrysanthemum, flying from plant to plant.—Prof. Westwood adverted to the Tea Bug of Assam, which he believed to be identical in Upper India, Java, and Ceylon, and not a new species. The insects of Java were often identical with those of Assam, but he supposed that in this case the insect might have been conveyed from one to the other. Dr. Hooker said that this was very probable. The Ceylon tea-plant was the so-called "hybrid variety" introduced from Assam, and was probably sent from Ceylon to Java.—Prof. Thiselton Dyer exhibited a specimen of an Acacia with a curious white balaniform exudation of insect origin, from the Botanic Garden at Cape Town. Prof. Westwood stated that the insect upon the Acacia was quite new to him, and was closely allied to the *Cionops cataphractus*, a rather rare British insect, allied to the *Coccidæ*; the specimens were females, which had emitted a mass of waxy matter, striated in ridges; the waxen mass was in many places covered with minute larvæ, differing in form from the ordinary larvæ of the *Coccidæ*.

General Meeting.—Mr. H. Little in the chair.—The Rev. M. J. Berkeley called attention to pods ripened in the gardens of Mr. W. Terry, of *Vanilla aromatica* (it has fruited this year abundantly in the Victoria house at Kew); a charming specimen of *Aloe plicatilis*—a miniature tree in form, with fine flowering spikes—came from Mr. J. T. Peacock's collection.

NEWCASTLE

Chemical Society, Feb. 26.—Dr. Lunge, president, in the chair.—A paper was read by Mr. J. Pattinson On the rate at which bleaching powder loses its available chlorine. The examination of a number of samples of bleaching powder, from time to time, during about twelve months, was undertaken with the view of making a contribution towards the solution of this question, "How much available chlorine does bleaching powder lose in a given time?" and also to the further one, "Does weak bleaching powder, say containing 32 per cent. of chlorine, retain its strength better than a stronger bleaching powder?" Three sets of samples were obtained from different manufactories on the Tyne, each set consisting of three samples. It was intended that the three samples of each set should be taken from the same portion of lime—one when it contained about 33 per cent. of available chlorine, one when it contained about 35 per cent., and the third when it contained about 37 per cent.—and with this object the lime was placed in a box in the chlorine chamber, so that it could be easily removed in order to take out the samples at each stage. On examining the tables given in the report it is seen that, with reference to the question as to the relative stability of weak and strong bleaching powder, there is practically no difference in the rate at which they lose available chlorine.

GLASGOW

Society of Field Naturalists, March 20.—Annual Meeting.—Mr. J. Allan, vice-president, in the chair.—Mr. P. Cameron, jun., exhibited two sawflies new to Britain: *Blennocampa aterrima* Klug, taken by Dr. Buchanan White at Braemar; *Hoplocampa pectoralis* Thomson, taken by the Rev. T. A. Marshall, F.L.S., at St. Albans. The only recorded locality is Gothland, where it was captured for the first time by Prof. Boheman.—Mr. Cameron also exhibited the two new sawflies described by him in the last number of the *Entomologists' Monthly Magazine*: *Taxonus glottianus*, of which a single specimen was taken at Kenmuir Bank near Glasgow in May; and *Nematus graminis*, a not uncommon species in the district, the larva of which feeds on grasses. The Annual Reports of the Secretary and Treasurer having been read and adopted the Officers and Council for the ensuing year were elected.

PHILADELPHIA

Academy of Natural Sciences, December 2, 1873.—Dr. Ruschenberger, president, in the chair.—Fertilisation of Yucac.

Mr. Thomas Meehan detailed at length the discoveries of Dr. Engelmann and Prof. Riley in regard to the fertilisation of the Yucca by the aid of a small night moth, *Pronuba yuccasella* of Riley, and observed that in this region the fertilisation was effected by this insect every year. In the Rocky Mountains of Colorado in 1871, he saw the *Yucca angustifolia* everywhere seeding in great abundance; but in his journey in 1873 he saw not a solitary seed-vessel in any of the plants, and he suggested that perhaps some periodical insect might take the place of the *Pronuba* in that country.—Note on a Fungoid Root Parasite. Mr. Thomas Meehan exhibited a small Norway spruce, in which the branches and leaves were all of a golden tint. He explained that when plants had little food, or lost their fibres in wet soil by which they could not make use of food, the yellow tint was generally exhibited in the leaves of plants. The similarity of the appearances suggesting, he examined and found the roots thickly enveloped by the mycelia of a fungus, which destroyed the young fibres as fast as they were developed. He had supposed it was one of the small microscopic forms of fungi; but in October of the present year the mycelia developed into a brown agaric with a pileus about two inches broad, but the exact species of which he could not positively determine.

Dec. 9.—Mr. Vaux, vice-president, in the chair.—On the Expansion of the Coma in *Asclepiadaceæ.* Mr. Thomas Meehan exhibited some seed-vessels of *Gonolobus obliquus*, and remarked that, though the hairy appendage to the seed known as the coma in asclepiadaceous plants was of course well understood, he knew of no one who had placed on record any observation in regard to the suddenness of the expansion after the seed left the capsule. It was indeed so very rapid, that the common expression of "like a stroke of lightning," was scarcely an exaggeration. It was only with difficulty that the eye could follow the motion. In the seed-vessel each set of long silky hair was drawn up into a close linear fascicle; but on the instant of the seed being relieved from its case, the coma expanded into a perfect hemisphere. Some of the hair formed a right angle, and others more or less acute ones, each seeming to have its fixed place to fall back to. It was generally supposed that these hairy appendages, and others of a similar character in seeds, were for the express purpose of aiding in seed distribution by wind; but he had failed in so many instances to see the advantages, that it often seemed as if it were the seed profiting by developed organs, rather than that these were especially formed for an express purpose. In the case of the *Gonolobus*, it did seem as if there were better grounds than perhaps in any other case for believing that the hairy appendage is designed expressly to facilitate distribution by wind or air currents. The seeds are heavy, and are borne on the plant but a few feet from the ground; they would fall there in a few seconds on the opening of the capsule, if the mass of hair remained long in its closely compact condition.—On Lingula in a Fish of the Susquehanna. Prof. Leidy.

PARIS

Academy of Sciences, March 23.—M. Bertrand in the chair.—The following communications were read:—Thermal study of the phenomena of solution; reaction of water upon nitric acid, by M. Berthelot. As the result of his investigations the author finds that the heat evolved by the addition of an equivalent of water to acids and bases generally decreases in accordance with a law analogous to a geometrical progression when the equivalents of water (n) increase in arithmetical progression. A formula is obained approaching $Q = \frac{A}{\rho^n}$ where the quantity of heat is Q and ρ, a number near unity. The author discussed the relationship between this formula and the analogous one obtained by M. Becquerel for the electromotive force of acid and alkaline solutions, viz., $= x \frac{a}{r^n}$.—On an operation of transfusion of blood performed by M. Béhier at the Hôtel Dieu: note by M. Bouley.—On the origin of the Muscado mace and of mace in general, by M. H. Baillon.—On the pathogenetic rôle of ferments in surgical maladies; new method of treatment for amputations: note by M. A. Guérin.—On the plane distribution of pressures in the interior of isotropic bodies in the state of limited equilibrium; mode of integration of the differential equations: note by M. J. Boussinesq.—On the law of astronomical attraction on the masses of the different bodies of the solar system, and particularly on the mass and duration of the sun, by M. E. Vicaire. The author seems to think it far from being demonstrated that the number called the *mass* of the sun is a real measure of the quantity of matter contained in it.—Programme of a system of geography founded on the exclusive use of decimal measures, of an international meridian 0°, and of stereoscopic and gnomonic projections, by M. B. de Chancourtois.—On the refraction of compressed water, note by M. Mascart.—Reply to the critical observations of M. H. Sainte-Claire Deville, on a method for the determination of vapour densities, by M. Croullebois. The author attempted to defend the apparatus, of which a description had previously been communicated to the Academy.—On the compounds of hydrogen with the alkaline metals, by MM. L. Troost and P. Hautefeuille. The authors have obtained compounds of potassium and sodium with hydrogen, having the formulæ K_2H and Na_2H, and have studied the tensions at every 10° of the hydrogen evolved on heating these compounds from 330° to 430°. K_2H dissolves a further quantity of hydrogen; Na_2H dissolves only a very small quantity of this gas. The authors find that lithium heated to 500° in hydrogen gas at 760 mm. pressure absorbs seventeen times its volume of the gas, while thallium under the same conditions absorbs only three times its volume.—On some bronzes from China and Japan, by M. H. Morin.—On the exotic terrestrial lombricians of the genera Urocheta and Pericheta, by M. E. Perrier.—On some general facts which arise from comparative androgenesis, by M. A. Chatin.—Atmospheric dusts, by M. G. Tissandier. The author has determined the suspended matter in the air of Paris and made analyses of atmospheric dust.—Researches on the formation of superphosphate of lime, by M. J. Kolb.—On the systems of curve-planes, algebraical or transcendental, defined by two characteristics, by M. Fouret.—Explicit condition that a conic may have a fifth-order contact with a given curve, by M. Painvin.—Two new theorems on the wave surface, by M. A. Mannheim.—On a Greek sundial found by M. O. Rayet at Heracleum of Latmos.—On the magnetisation of steel, by M. E. Bouty.—Calorific effects of magnetism in an electro-magnet with several poles, by M. A. Cazin.—Researches on trichloracetates and their derivatives, by M. A. Clermont. The author has obtained trichloracetyl-urea, by acting upon trichloracetateof ureawith phosphoric anhydride, and also by the action of trichloracetyl chloride upon urea. The same substance was obtained by this last reaction by Tommasi and Meldola in this country in January.—On some endosmotic properties of the membrane of the shell of birds' eggs, by M. U. Gayon.—On the red colouring-matter of the blood, by M. Béchamp.—On the employment of potassium bisulphate for the distinction of native sulphides, by M. E. Jannettaz.—Observations on the spermatophores of decapod crustacea, by M. Brocchi.—Differentiation of induced and spontaneous movements.—Study of the action of some reputed anæsthetic agents on the functional irritability of the stamens in *Mahonia*, by M. E. Haeckel.—Experimental study upon "ammoniémie," by MM. V. Feltz and E. Ritter.

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