THURSDAY, AUGUST 25, 1881

PAPIN

Dr. Ernst Gerland's Life and Letters of Papin. (*Leibnizens' und Huygens' Briefwechsel mit Papin, nebst der Biographie Papin's*, von Dr. Ernst Gerland. Berlin, 1881.)

AT a time when Britons have just been celebrating the centenary of Stephenson a glance at the life and works of one who in a much more accurate sense was *the inventor* of the steam-engine may not be amiss. Denis Papin, whose life and letters now appear from the editorial pen of Dr. Ernst Gerland, was born at Blois in 1647. In 1661 or 1662 he proceeded to the study of medicine at the University of Angers, and, receiving his degree in 1669, he appears to have intended to settle down to a physician's life in that city. Why this course was not fulfilled is not known; but we find him in 1674 at Paris, where also he had made the acquaintance of Huygens, with whom he was engaged in experimenting with the newly-discovered air-pump, an account of which was published in that year at Paris under the title of "Expériences du Vuide." The *Philosophical Transactions* of our Royal Society for the following year (1675) were enriched by no fewer than five papers on the same subject by Huygens and Papin jointly. In Paris also Papin met Leibniz, who sojourned there from 1672 to 1676. His acquaintance with Leibniz was however interrupted, for, very shortly after the publication of his "Expériences," he crossed the Channel to England, led, as Boyle tells us, by some hope that here he might procure a situation accordant to his genius. In London he assisted Boyle in his laboratory and with his writings, and shortly afterwards introduced into the air-pump the further improvement of making it with double barrels, and replacing the turncock hitherto used by the two valves. A little later he produced another instrument, the condensing pump, and in 1680, on Boyle's nomination, he was admitted to the distinction of Fellowship in the Royal Society. That honour he repaid in the following year by communicating to the Society his famous invention of "A new Digester or Engine for Softening Bones," in which instrument—now so universally known—he applied as it seems for the first time the now common device of a safety-valve. For that year and the next he devoted himself to experiments with the digester and its various applications. Then he received an invitation to proceed to Venice to take part in the work of the Accademia di Scienze Filosofiche e Matematice, then newly founded, in imitation of the learned societies of Rome, Florence, Paris, and London. Here he appears to have remained nearly two years, and early in 1684 we find him back again in London, where, on April 2, he was elected by the Royal Society as their "temporary curator of experiments," at a salary of 30*l.* per annum. This was in the palmiest days of the young Society, when Newton, Boyle, Hooke, Hawkesbee, and many other famous spirits took the most active part in its proceedings, and Papin shared in the work of bringing their experiments, embracing a most miscellaneous range of subjects, before the Society. Amongst the discoveries of his own, of which Dr. Gerland gives a summary, in this way brought before the Royal Society, was the so-called Würtemberg siphon. He also partially anticipated Franklin in his discovery of the ebullition of water under reduced pressure at lowered temperatures, concerning which point he observes: "This shows that liquors being freed from an external pressure will make bubbles upon the score of the elastic particles lurking in their pores, as has been observed long ago by the Hon. Mr. R. Boyle. I do therefore believe that the vapours raised by heat in an exhausted glass will make a pressure, which is quickly taken off when we condense these vapours by putting the glass into cold water or ice." On another occasion Papin brought forward a model of a machine for raising water by pumps to a height, the pumps being worked by a water-wheel driven by the flow of a river. In November, 1687, this occupation came to an end by Papin resigning his post of curator on being appointed by the Landgrave of Hesse to be Professor of Mathematics at Marburg. Here the most active portion of his life began, to be continued when, in 1696, his place of residence was exchanged for Cassel. Here too began the correspondence with Huygens and with Leibniz, which forms the major part of the volume before us. By both of these great men he was highly esteemed. Huygens explained to him in 1690 his new theory of double refraction in a letter of considerable length. Leibniz wrote to Luca about Papin, referring to his ingenious inventions in the most enthusiastic terms. The correspondence with Leibniz went on almost unbroken until Papin's final return to England heart-broken and worn out. That with Huygens ended much earlier, being terminated by Huygens' premature decease in 1695. To Huygens in 1691 Papin writes from Cassel about a project he is undertaking for the Landgrave, to construct a ship on the plan suggested previously by Drebbel to move under water. In the same letter he mentions the production of fog or mist in the receiver of the air-pump, for which phenomenon Huygens, in replying, propounds an explanation. At the same date we find Papin busy with another invention, a *rotilis suctor*, which was nothing else than a centrifugal fan for supplying a blast for furnaces and for ventilating mines, which instrument he had the satisfaction of applying to a mine in Germany in 1699, and six years later he made the further improvements related in the *Philosophical Transactions* of that year. But his greater work was drawing on. In 1698 he writes to Leibniz that he is constructing a machine for raising water to a great height by the force of fire, and that by the success of his experiments he is persuaded that this force can be applied to many other more important ends. Leibniz replied forthwith with the inquiry whether his invention was based upon the principle of rarefaction; adding that he also had some ideas on the point. After two months Papin replies that he relies upon the principle of rarefaction produced by the condensation of steam, but that he proposes also to use the pressure exerted by the steam in expanding, "the power of which is not limited as is that of the suction" (of rarefaction). He also says that he has made a little model of a carriage which is propelled by this force, but that he fears difficulties for such carriages from the inequalities and sharp turns of ordinary high roads—difficulties which for water carriages do not exist. Leibniz's reply is only known from a summary of it scribbled in his own handwriting upon the back of Papin's letter. He first congratulates Papin on having set himself to this work; expresses his fears lest the direct pressure of expanding steam should produce explosions; and then suggests, "pour faciliter le chariotage," an idea of his own (derived, he says, from the air-pump), that the steam which is to exert pressure should be introduced into a cylinder into which is fitted a second one, after the manner in which our modern gasometers fit into their cylindrical pits, the whole being rendered steam-tight by mercury. Papin, replying in August, announces that his machine has raised water to the height of 70 feet. He only half approves Leibniz's suggestion on account of the probable friction between the internal cylinders. To this Leibniz retorts that while the friction increases with the diameter of the piston of a pump, the pressure increases as the square of the diameter. The matter seems to have dropped at this stage for three years. In 1702 Papin, still at Cassel, announces to Leibniz that he has invented a steam *ballista*, "an invention to facilitate the capture of the strongest places," which "will reduce France to make most promptly a durable peace!" This invention was a cylinder 5 inches in diameter filled with a piston connected to pivoted lever, which on the descent of the piston on the condensing of the steam below it would project a stone weighing 2 lbs. to a distance of 40 feet. (A similar ballista, unknown to Papin, had been suggested by von Guéricke, in 1672, in his *Experimenta Nova.*)

Early in January, 1705, Leibniz sent to Papin a sketch of Savery's engine for raising water. This set Papin with renewed vigour to work, besides stimulating to emulation the breast of his patron, the Landgrave of Cassel. After some consideration he pronounced in another letter to Leibniz that he would surpass Savery's invention. He related how he had thought it best not to let the steam act directly against the surface of the water (as in Savery's machine), but that the pressure of the steam should be imparted to the water through the intermediation of a *piston* whose surface, becoming hot, would not produce condensation: and he added that experiment had proved the conjecture to be sound. His great difficulty now was, not to make pistons fit accurately, but to construct tubes sufficiently strong to bear the pressure of such columns of water as he wished to raise. Leibniz congratulated him when replying in August, and advised him not to try to force water high, but rather to lift it by a series of pumps, each drawing 30 feet, a suggestion which Papin on his part rebutted by observing that one force-pump driving water 500 feet high was more economical than ten pumps, each raising the water 50 feet. He further lets Leibniz know that he hopes to do away with the delay of letting the cylinder of his engine cool between each stroke (the very first of the improvements subsequently made by Watt), and that he has some ideas about the transmission of power to a distance, with which problem however he thinks it useless to concern himself, "because by means of the heat-engine one can produce, everywhere where one will, so much power, and so cheaply, that it would be a superfluous expense to carry it elsewhere." Strange commentary, indeed, on the present eager strife of inventors to supersede steam by the electric transmission of power! On October 19 he writes again that he is almost satisfied with his engine, which, though having but one cylinder and two valves, yet furnishes a continuous jet, surpassing Savery's machine, which had two pressure-vessels and four valves: he is only waiting the Landgrave's orders as to how he shall apply his engine to drive a mill. On the last day of the year 1705 he declares to Leibniz his intention of propelling vessels by steam, as he is persuaded that by this means one could have vessels which would follow their course correctly in spite of tempests and adverse winds. At this idea he laboured diligently for the next two years—in fact, so long as he continued to remain at Cassel—his devotion to the object in hand being remitted only for the sake of his correspondence and for the work of publishing his treatise, the "Ars Nova," in which his high-pressure boiler and its applications are described. It was towards the close of this time that (on February 4, 1707) he communicated to Leibniz the first suggestion of a hot-air engine, afterwards realised by Stirling and Ericssen. He was now preparing to leave Cassel, where the patronage of the Landgrave had grown on the one hand slack, on the other irksome, in order to regain the more congenial atmosphere of London and of the Royal Society. He strained every nerve and spent all his little resources to accomplish the building of the steam-propelled boat by which his return to England was to be made famous. He was certain that by this means two men on board his boat might do more than a hundred rowers could. In July and August of that year he made diligent efforts to obtain permission to descend the river *viâ* Münden and Bremen into the Veser, permission which was finally granted by the Elector of Hanover, in spite of the monopoly possessed by the guild of boatmen of Münden to pass boats from the Fulde into the Veser. With a boatman of Münden as captain, he sailed from Cassel on September 24, 1707, with his family. At Münden however the guild of boatmen asserted their privileges, the magistrates pronounced the boat confiscated, and a handsome offer of ransom was rejected. Papin pushed forward despairingly for England; only to find himself almost unknown and friendless. The old generation was fast passing away. For two or three years he continued his mechanical inventions, and several times applied through Sloane for a grant of money from the Royal Society to aid him in his work, but in vain. Misunderstanding and misery followed apace. The inventions on which he relied for fame and position were passed by unnoticed. In the loss of his ship he had made shipwreck of his life's hopes. He died in London, probably, in the early half of the year 1712, but in such obscurity that neither place nor date is with any certainty known.

Dr. Gerland appears to have spared no pains in collecting the scattered facts of Papin's life and work from which to build the volume whose contents we have endeavoured to make known to English readers. We congratulate him on his success, and trust that his efforts will be further rewarded by the discovery of the facts still required to fill the *lacunæ* in the career of this remarkable man. The light which the publication of the correspondence between Leibniz and Papin throws upon the relation between two prominent figures amongst men of science at that time is by no means the least interesting feature of the work; and we must henceforth place

Leibniz amongst the worthies to whom the credit of improvements in the steam-engine is given. The one common feature that runs through the many different types of steam-engine is the piston working within a cylinder. No engine before Papin's time was adapted for any useful purpose except for raising water, and none had a piston in a cylinder. No engine since Papin's time of the thousand varied types has been devoid of this feature. But the very feature which Papin introduced, and on the introduction of which his claim to be called *the* inventor of the steam-engine has been founded, was, as we now know, the suggestion of another mind. We owe the application of the piston-principle in the steam-engine, not to Papin, but to Leibniz.

CHEMISTRY OF THE FARM

The Chemistry of the Farm. By R. Warington, F.C.S. Pp. xiii. and 128. (London: Bradbury, Agnew, and Co., 1881.)

THE chapters of this little handbook appeared originally in detached portions in the *Agricultural Gazette*. They have been revised, and are now issued in a convenient and compact form. A well-ordered manual of agricultural chemistry, clearly written and perfectly abreast of recent advances in the sciences underlying the farming art, has long been wanted. So far as the limits of its size and scope allow, Mr. Warington's volume fulfils our expectations. It is a satisfaction which is seldom afforded us to read a book on agricultural chemistry written by a true chemist trained in laboratory work and versed in the progress made through English and foreign researches. The applications of chemistry to agriculture are manifold, and cannot be grasped by chemists who do not combine with their chemistry a competent knowledge of vegetable and animal physiology and of mineralogy. Yet to learn or to teach the Chemistry of the Farm without a knowledge even of the foundations of chemical science is commonly attempted, though it can never succeed in any true sense. And we quite agree with Mr. Warington that a wider range of scientific knowledge than this is needed even for the student of agricultural chemistry—much more then for the teacher. To talk about this applied science to persons without previous scientific knowledge, and to look for satisfactory results, is to expect a plant unfortunately destitute of roots to blossom and bear fruit.

We think then that Mr. Warington's handbook is valuable on account not only of the knowledge with which its subject is handled, but also on account of the spirit with which that subject is approached. That a new work on agricultural chemistry was sorely needed does not admit of question. In France and in Germany the educational literature of this subject includes many excellent works which have no English counterparts. Johnston's treatise, full as it is of valuable observations, is too thoroughly out of date in method as well as in matter to admit of satisfactory revision; much the same judgment must be passed on Anderson's "Agricultural Chemistry," now twenty years old and out of print. Even if Georges Ville's work on Manures included (which it is far from doing) anything like the whole domain of the Chemistry of the Farm, it is about as unsafe and misleading a book as could be put into the hands of a student. Mr. Warington has given us, in fact, not all we want, but a good bit of it. He has used, and that judiciously, both German and English text-books, researches, and memoirs, and has put the main facts they enounce into a neat form, so as to be "understanded of the people." The two capital text-books of Emil Wolff have been laid under contribution by Mr. Warington; while the chief results of some of the matchless Rothamsted Memoirs by Lawes and Gilbert have been skilfully introduced into his pages. Of the contents of these it is now perhaps time to say a few words. In five chapters the growth, the food, the nutrition, and the products of farm crops are discussed; in another five, animal growth, food, nutrition, and products. Of the diverse origins and properties of soils but little is said; as to the utilisation of urban sewage, nothing. And we should have been glad to have found fuller accounts of many subjects which are but lightly touched upon in these pages. But the difficulty of treating so vast and complex a subject intelligibly in so few pages makes us surprised, not that some things are omitted from, but that such a large number of things are included in, this little book. Some facts and figures which the author would doubtless have liked to introduce have been kept out of his pages by the absolute necessity of finding room for numbers and arguments of primary importance. For instance, it would have been unwise to have curtailed the space bestowed upon the "Digestibility of Foods" and the "Albuminoid Ratio."

In reading through this handbook carefully we have been unable to discover more than a very few statements which we cannot completely endorse; in fact the majority of such alterations as we would suggest would be of form rather than of substance. We cannot, however, refrain from expressing our regret that the percentages of albuminoids in potatoes and roots as given in the table on p. 72 should be the old erroneous figures condemned on the very next page as greatly in excess of the truth. Mr. Warington has however so thoroughly recognised the importance of the discrimination between albuminoid and non-albuminoid nitrogen that we must attribute the inclusion of the incorrect figures in his tables to the difficulty of constructing a complete series of analyses comparable with one another in this particular of the percentage of true albuminoids.

A. H. CHURCH

OUR BOOK SHELF

A Handbook of the Vertebrate Fauna of Yorkshire. Being a Catalogue of British Mammals, Birds, Reptiles, Amphibians, and Fishes, showing what species are or have within historical periods been found in the County. By Wm. Eagle Clarke and Wm. Denison Roebuck. (London: Reeve and Co., 1881.)

THIS little volume is dedicated to the President-Elect of the British Association, and most seasonably makes its appearance on the eve of the meeting of that Association in the city and county of its origin, when it will celebrate the completion of the first fifty years of its existence. Its object is the enumeration of those animals with a vertebral column which either are or have been found in Yorkshire, and the careful definition of their faunistic position and geographical distribution within the county. It would appear that there has never been a list of the mammals, birds, or fishes of the county of York published, and in this respect it presents a striking contrast with its neighbouring counties of Norfolk, Northumberland, and

Durham; but by the energetic labours of Messrs. Clarke and Roebuck this reproach no longer exists, and this very useful handbook to the vertebrate fauna of the shire will, let us hope, be soon followed by a second volume, dealing with the larger and perhaps more difficult portion of its, to use a handy term, invertebrate animals. The number of British vertebrata not occurring in Yorkshire being comparatively small, it seemed desirable to the compilers to make this work not only a county handbook, but a complete nominal catalogue of the British species. In this we think they have done well, for such a catalogue undoubtedly furnishes a ready means of comparison with the faunas of other districts. The classification and nomenclature has in all cases been based upon the most recent or the most reliable authorities as to the extinct British mammalia. It having been considered advisable to include notices of these, or at least of such of these as had ceased to exist in Yorkshire within historical times, the species are inserted in their correct zoological sequence, but their names are printed in Old English characters, and they are left un-numbered, as not being now entitled to rank as true members of the fauna. The same has been done in the case of the Great Auk among the birds. To the catalogue is prefixed an interesting chapter on the physical aspect of Yorkshire, the largest county of the British Islands, containing an area of 3,936,242 statute acres—one which, while most compact in form, is perhaps the most varied in geological structure, soil, climate, and physical aspect. The introductory remarks also on the mammals, birds, reptiles, amphibians, and fishes are well worth perusal. From the general summary, the richness of the Yorkshire fauna can be at once seen, it including 513 out of the 717 known British vertebrates. We gladly recommend this volume to our readers, as in every way an excellent and scientific handbook to the vertebrate fauna of Yorkshire.

LETTERS TO THE EDITOR

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

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

Schaeberle's Comet

THIS comet, C 1881, was well seen here on the night of Sunday last, the 21st instant. At 9.30, the night being clear, it was at once detected with the naked eye at a point in the north-west, where lines drawn downward through α and β of Ursa Major (the pointers) and γ and δ of the same constellation would intersect, and just above ψ of Ursa Major, a star of the 3rd magnitude. Owing to the comet's close proximity to the horizon I could not use the 6″ equatorial, but the position must have been very close upon R.A. 11h. and D.N. 47°. The general appearance to the eye was that of a comet with two nuclei, the one in advance of the other. With a 2¼-inch binocular the comet was beautifully sharp and well defined, more so, I thought, than the great southern one when in the same position. The nucleus and star appeared of about the same intensity, but the yellow tint of the latter was strongly contrasted with the almost intense gas blue tint of the former. The tail was well defined, only slightly spreading, and nearly straight, stretching in a line a little to the left of β of Ursa Major, nearly as far as a small triangular group of stars just under β, marked in Maltby's atlas as 44·37 and 246·7. This would give a length of from seven to eight degrees. The tail did not, with the small instrumental power I was using, appear to have any central deficiency of light. The sharpness and brightness of the comet's appearance, as contrasted with the more diffused aspect of the one which has just disappeared, has been remarked upon by several observers.

Guildown, August 22 J. RAND CAPRON

The Descent of Birds

THERE is one passage in the report of Prof. Mivart's lecture on chamæleons (NATURE, vol. xxiv. p. 338) that I cannot allow to pass without demurring to, and that is the suggested probability of a "double origin" for the class Aves. I do not wish at present to raise the issue as to how far the division of all living birds into two groups—"Ratitæ" and "Carinatæ"—is, or is not, a natural one; for at present we have not, I think, sufficient information or evidence on the subject to allow of any very definite reply. But any one who is acquainted with the structure of a Tinamu will, I think, be unable to conceive of the many resemblances that group of birds presents to some of the "Ratitæ" as having been developed independently of any genetic connection between the two—and that is what Prof. Mivart's suggestion practically amounts to. That structures so peculiar as feathers—which, as far as we know, are absolutely confined to birds, though universal amongst them—should have been twice over developed, is to me in the highest degree improbable—as improbable, almost, as that the resemblances of the Tunicates and *Amphioxus* to the rest of the Chordata should also be accidental. W. A. FORBES

West Wickham, Kent

Mr. Wallace and the Organs of Speech

IN his article in NATURE, vol. xxiv. p. 244, Mr. Alfred Wallace has given one of the keys to the formation of speech-language. He says, "When we name the *mouth* or *lips* we use labials; for *tooth* and *tongue*, dentals; for the *nose*, and things relating to it, *nasal* sounds; and this peculiarity is remarkably constant in most languages, civilised and savage." Of this he gives examples from Australasia.

Perhaps it may be said there is not much novelty in Mr. Wallace's observations, as many of us have said the same. I have gone over some of his ground in my small "Comparative Philology" in 1852, but I did not hit the point. Indeed what Mr. Wallace gives us is very little, but when it comes to be applied it acquires the highest importance. We have all known that *nose* is often a nasal, but Mr. Wallace distinctly puts it that *mouth* is a labial, *tooth* a dental, and *nose* a nasal. This however gives us by these words and their connections, as stated by Mr. Wallace, a very poor vocabulary, and leaves most of the phenomena of speech-language unaccounted for, and it gives no explanation apparently of the derivation of speech-language from sign- or gesture-language, and the connection of character with both.

Setting Mr. Wallace's illustrations aside—for though they are true, and taken from his own domain, they are not the most apt —we will search farther afield. Chinese will be convenient. In Chinese, for a reason that need not be explained, *mouth* is not now a labial, but in the series connected with it there are many labials. The series is best illustrated by the characters. The old characters are round; the new characters, as in other classes, are now square, conventionally representing the round. Now *mouth* is a round or circle, ○ (or □). *Ring* is a round or circle ○ (or □). The character for *mouth* is in fact a *ring*, or round, or circle. On looking for other corresponding characters we have *eye* with ○ differentiated. Here we get a labial *mu*. *Face* is another round character, and that is *mien*. *Ear*, *head*, *blood*, *pot* (*ming*), *sun*, *moon*, *woman*, *mother* (*mu*), *white* (a labial), *field* or *garden*, *four* are all differentiated forms of each other and of mouth, as we know they ought to be. In cuneiform these characters are round, square, or triangular.

Of many of these psychological relations of words a list or dictionary will be found in the table of equivalents in my "Prehistoric and Protohistoric Comparative Philology." I observed and collected the facts, but did not know the full meaning of them for a long period; and in a paper as yet unpublished by the Biblical Archæological Society I carried the subject still further, particularly as regards cuneiform and Chinese. Indeed, when Mr. Wallace published his article, I had the facts just cited ready for reference in my hand. The reason I did not grasp the solution was this: I have known for years that words forming what I now call ring characters were related to *eye*, and that *eye* is almost a constant in these investigations, equivalent to a molar in various departments of biological research. Indeed it was by the use of *eye* as a constant that I was able to make those numerous and rapid philological analyses which have excited so much distrust among those unacquainted with the process I used.

I found that if I could classify *eye* in a language under examination, it gave me *sun* and many other words, and it led me to much valuable work, but I was often thrown out for reasons I did not then know. Empirically I found *eye* was a constant, and I knew it was a round, because in many languages east and west *sun* is the day eye or day's eye; *moon* is the night eye, and *eye* the head eye. In the North American languages and in the Malay, for instance, there was the evidence of a common law of psychological philology, which led me to greater results. My knowledge became modified to the extent that *sun* was not day eye, but day round. Until Mr. Wallace's article appeared, I still regarded *eye* as the pivot on which the "round" words and characters turned, although I knew that *mouth* was the prototype of *moon*, *mother*, *woman*, *egg*, &c., and of objects and ideas having a periodicity or a month. Having a false pivot, I was never able to bring the facts into a right connection, although coming very near. The Chinese modifications of the ancient character show that *mouth* and *ring* constitute the primary character, and thereby indicate the primary word.

The researches of Col. Garrick Mallery, U.S.A., and my own, in the paper unpublished, show the connection of sign language and characters, and I have determined a relation between sign language, character, and words, as in the sign or character || for son, offspring, &c. The characters in many cases appear as ancient as the signs, and may have preceded speech language. How words were connected with ideas and their representatives by signs was the problem. The new explanations of Mr. Wallace in your paper, or the old observations of others, in giving explanations from natural cries and sounds, &c., are not always exact, and do not account for the fact that the sounds are in relation with the sign language. Thus the words for *eye* and 2 are the same, and the words for *ear* and 3, and so forth.

In the brief remarks now made I endeavour to steer clear of many things which would require a long explanation, and to bring my observations to bear on Mr. Wallace's article. On speech language being constituted, the application of a labial to *mouth* gave a large series, and so of the dentals, &c. As the numerals are in relation to each object of the universe in primitive symbology, so they were supplied. Indeed nouns, adjectives, pronouns, verbs, numerals, particles, were supplied from a common fount. There are languages constituted of a few differentiated words, which can be traced throughout.

In connection with Mr. Wallace's remarks is to be taken what he says afterwards of the action of the lips. In the sign languages and the characters the lower organs supply a large number of ideas regarded as phallic. Such are ||, ·|·, O, &c. These ideas are not capable of direct connection with sounds; they came however into connection by the acknowledged correspondence of the parts in symbology and mythology. Thus the labial sounds became the representatives of actions or ideas illustrated by the corresponding lower organs, as in *go* and *come*.

Taking Mr. Wallace's terms and applying them, we therefore get the connection established between the sign languages and the speech languages, and we can see the psychological grounds on which they continued in working together, and why the speech languages have not everywhere *always* exterminated their ancestors. For this, and for the whole state of affairs, Mr. Wallace furnishes me with an explanation.

His naked statement is the best, that for *mouth* a labial was used. In the sign languages, and we find this in the prehistoric languages and their equivalents, several signs are used for one idea, and several ideas for one sign. When a labial was applied for the *mouth*, it was indifferent what labial. If one used a *b*, another would use *m*. This is one cause of the variety we find in the prehistoric primary languages, for there never was what philologists are fond of, one primitive language.

Many will object to Mr. Wallace, that *mouth* is not always represented by a labial, and in the common course hold that the negative evidence overcomes the affirmative. In many instances *mouth* is a dental, because the idea includes the *teeth*, which are dental. Again *tongue* is not always a dental, but a sibilant, so far as it is connected with *snake*. It is the whole knowledge of the facts which will better enable us to complete our progress and to overcome difficulties. For myself I have derived particular advantage from Mr. Wallace, in being enabled to understand my own work. HYDE CLARKE

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Comets and Balloons

THE notion that the tails of comets are produced by an emission of the nucleus prevails at present among astronomers. I have just stated in a small pamphlet, 8vo, 32 pages, the reason why I presume to entertain another opinion on this subject. The details of my last aërial trip of July 2 show that by using an electric light night ascents at a reasonable distance from the sea may be considered as relatively without danger. The appearance of Schäberle's comet seems to me to afford a proper occasion for testing the emission theory, and I will try to explain my idea as shortly as possible.

It is pretty certain that any comet will lose something of its brilliancy in consequence of passage to the perihelion, consequently, *cæteris paribus*, it must be found with a diminished luminous power in the second part of its track. The consequence is that to test this theory the same comet should be observed in a similar position, as close as possible, in the first and in the second parts of its track.

By ascending with a balloon in the northern hemisphere to inspect Schäberle's comet on a moonless night, and estimating its luminous power in a clear sky at several determined heights, a great step will be made in reaching this desirable end.

It would be for the astronomers of the southern half of the world to ascend under similar conditions, and to make corresponding observations. If no visible diminution is proved to have taken place, much will have been accomplished in the determination of the true nature of this mysterious object.

The same observations could, it is true, be prosecuted without the help of aërostation, but not with the same amount of certainty, as much doubt remains as to the true luminosity of a celestial body when it is not inspected in a really perfectly clear sky, which can always be procured with a balloon—it is true not without incurring some personal risk, certainly not out of proportion, at all events, to the results to be expected.

W. DE FONVIELLE

Animal Instinct

I AM exposed to some annoyance from a clever old donkey, who, being turned out on to the green in front of my house, constantly lets himself into my garden to graze on my lawn. This he effects by pushing his nose between the rails of an iron gate, and then pressing down the latch of the gate. Expulsion, with ever so striking an appeal to his feelings, avails only a short time for his exclusion, unless the gate is locked.

Little Park, Enfield, August 19 W. B. KESTEVEN

ITALIAN DEEP-SEA EXPLORATION IN THE MEDITERRANEAN

AFTER my communication of the 4th inst. from Asinara I feel sure that many readers of NATURE will be interested to know something more of our doings; so I take the opportunity of our short stay here to send a very brief account of our doings since leaving Asinara.

The presence of a deep-sea fauna in the Mediterranean which I announced in my last is fully confirmed, and even though most of the species dredged are as yet undetermined, I can venture to say that the character of this fauna is "Atlantic," and, I may add, "Oceanic." My first bit of news was the capture of a *Willemœsia* identical, or very nearly allied, to *W. leptodactyla*; since then some ten or twelve specimens of that most interesting and characteristic Crustacean have been secured off the west, south, and east coasts of Sardinia, in depths varying from 950 to 2145 metres. All our deep hauls have brought up some living animals, usually Annelids and deep-red shrimps of at least three species; the greatest depth we have trawled in is 3115 metres; the greatest we have found sounding is 3630 metres in the eastern basin between Sardinia and Naples.

On the 10th inst., off the west coast of Sardinia we dredged two specimens of a Macrurid fish, which I take to be a *Malacocephalus*, from depths of 2805 and 2908 metres. South of the Gulf of Cagliari we got a new—to me—and exceedingly remarkable Macrurid, with what look like the so-called supplementary eyes along the belly and tail, the rare *Macrurus sclerorhyncus, Hoplostethus Mediterraneus*, and *Haloporphyrus lepidion*, from depths of 508, 656, 860, and 1125 metres. We have at least two species of *Terebratula* from depths varying from 600 to 1200 metres. Several most interesting Crustaceans besides those mentioned, and even a non-swimming Brachyurous Decapod, from 1125 metres! But what is still more interesting is the capture of several specimens of a *Hyalonema*, very probably *H. Lusitanica*, but without any spiral twist in its long spiculæ; we got them off the south and east coasts of Sardinia, in depths from 1600 to 623 metres; we have, besides, several other forms of Sponges, all siliceous, and several of a curious agaric-like form. We have a *Brisinga* from depths of 2145 to 2300 metres, but very few other Echinoderms; I do not yet give up the hope of seeing a *Pentacrinus* before our cruise is ended. We have, as I mentioned, various Annelids and Gephyreans, and some fine species of Madreporia of the deep-sea forms.

We have an interesting set of serial thermometric observations, which show that there is a slight difference in the bottom temperature between the basins east and west of Sardinia, the latter being slightly colder. Negretti and Zambra's new deep-sea thermometers have answered admirably, suspended in the peculiar frame devised by Capt. Magnaghi. HENRY HILLYER GIGLIOLI

Naples, August 20

ON THE VELOCITY OF LIGHT

THE result announced by Young and Forbes (Roy. Soc. *Proc.*, May 17, 1881) that blue light travels *in vacuo* about 1·8 per cent. faster than red light, raises an interesting question as to what it is that is really determined by observations of this character. If the crest of an ordinary water wave were observed to travel at the rate of a foot per second, we should feel no hesitation in asserting that this was the velocity of the wave; and I suppose that in the ordinary language of undulationists the velocity of light means in the same way the velocity with which an individual wave travels. It is evident however that in the case of light, or even of sound, we have no means of identifying a particular wave so as to determine its rate of progress. What we really do in most cases is to impress some peculiarity, it may be of intensity, or of wave-length, or of polarisation, upon a part of an otherwise continuous train of waves, and determine the velocity at which this *peculiarity* travels. Thus in the experiments of Fizeau and Cornu, as well as in those of Young and Forbes, the light is rendered intermittent by the action of a toothed wheel; and the result is the velocity of the group of waves, and not necessarily the velocity of an individual wave. In a paper on Progressive Waves (*Proc.* Math. Soc. vol. ix.), reprinted as an appendix to vol. ii. of my book on the "Theory of Sound," I have investigated the general relation between the group-velocity U and the wave-velocity V. It appears that if k be inversely proportional to the wave-length,

$$U = \frac{d(kV)}{dk},$$

and is identical with V only when V is independent of k, as has hitherto been supposed to be the case for light in vacuum. If however, as Young and Forbes believe, V varies with k, then U and V are different. The truth is however that these experiments tell us nothing in the first instance about the value of V. They relate to U; and if V is to be deduced from them it must be by the aid of the above-given relation.

When we come to examine more closely the form of this relation, we see that a complete knowledge of V (as a function k) leads to a complete knowledge of U, but that a complete knowledge of U—all that experiments of this kind can ever give us—does not determine V, without the aid of some auxiliary assumption. The usual assumption is that V is independent of k, in which case U is also independent of k. If we have reason to conclude from observation that U is not independent of k, this assumption is disproved; but we can make no progress in determining V until we have introduced some other.

It is not easy to see how the missing link is to be supplied; but in order to have an idea of the probable magnitude of the difference in question I have assumed the ordinary dispersion formula $V = A + Bk^2$ to be applicable. Taking the ratio of wave-lengths of the orange-red and green-blue lights employed as 6 : 5, I find that for red light $V = U(1 - \cdot 0273)$, so that the velocity of the wave would be nearly 3 per cent. less than that given by Young and Forbes as the result of the experiment.

Under these circumstances it becomes a matter of interest to examine the bearing of other evidence on the question of the velocity of light. Independently of the method of the toothed wheel, the velocity of light has been determined by Foucault and Michelson using the revolving mirror. It is not very obvious at first sight whether the value thus arrived at is the group-velocity or the wave-velocity, but on examination it will be found o be the former. The successive wave-fronts of the light after the first reflection are not parallel, with the consequence that (unless V be constant) an individual wave-front rotates in the air between the two reflections.

The evidence of the terrestrial methods relating exclusively to U, we turn to consider the astronomical methods. Of these there are two, depending respectively upon aberration and upon the eclipses of Jupiter's satellites. The latter evidently gives U. The former does not depend upon observing the propagation of a peculiarity impressed upon a train of waves, and therefore has no relation to U. If we accept the usual theory of aberration as satisfactory, the result of a comparison between the coefficient found by observation and the solar parallax is V – the wave-velocity.

The question now arises whether the velocity found from aberration agrees with the results of the other methods. A comparison of the two astronomical determinations should give the ratio $U:V$, independently of the solar parallax. The following data are taken from Mr. Gill's "Determination of the Solar Parallax from observations of Mars made at the Island of Ascension in 1877."

The time k, required by light to travel a mean radius of the earth's orbit, has been determined by two astronomers from the eclipses of Jupiter's satellites. Delambre found, from observations made in the last century, $k = 493 \cdot 2$s., but recently Glasenapp has obtained from modern observations the considerably higher value, $k = 500 \cdot 8\text{s.} \pm 1 \cdot 02$. With regard to the constant of aberration, Bradley's value is $20'' \cdot 25$, and Struve's value is $20'' \cdot 445$. Mr. Gill calculates as the mean of the best modern determinations (nine in number), $20'' \cdot 496$.

If we combine Glasenapp's value of k with Michelson's value of the velocity of light, we get for the solar parallax $8'' \cdot 76$. Struve's constant of aberration in conjunction with the same value of the velocity of light gives $8'' \cdot 81$. From these statements it follows that if we regard the solar parallax as known, we get almost the same velocity of light from the eclipses of Jupiter's satellites as from aberration, although the first result relates to the group velocity, and the second to the wave velocity. If instead of Struve's value of the constant of aberration we take the mean above spoken of, we get for the solar parallax $8'' \cdot 78$, allowing still less room for a difference between U and V.

Again, we may obtain a comparison without the aid of the eclipses of Jupiter's satellites by introducing, as otherwise known, the value of the solar parallax. Mr. Gill's value from observations of Mars is 8″·78, agreeing exactly with Michelson's light velocity and the mean constant of aberration. Some other astronomers favour a higher value of the solar parallax, such as 8″·86; but whichever value we adopt, and whether we prefer Cornu's or Michelson's determination of the light velocity, the conclusion is that there can be no such difference between the group velocity and the wave velocity as 2 or 3 per cent., unless indeed the usual theory of aberration requires serious modification. These considerations appear to me to increase the already serious difficulties, which cause hesitation in accepting the views of Young and Forbes. The advent of further evidence will doubtless be watched with great interest by scientific men.

One other point I may refer to in conclusion. Speculations as to harmonic relations between various spectral rays emitted by a glowing gas proceed upon the assumption that the frequency of vibration is inversely proportional to the wave-length, or, in other words, that the velocity of propagation V is independent of the wave-length, the question now at issue. If the views of Young and Forbes are correct, calculations of this kind must be overhauled. On the other hand, the establishment of well-defined simple ratios between wave-lengths would tend to show that V does not vary. RAYLEIGH

August 15

ELECTRIC LIGHT IN COLLIERIES

AUGUST 9, 1881, witnessed the first practical application in the United Kingdom of the electric light to the illuminating of coal-mines. The Earnock Colliery, near Hamilton, Lanarkshire, belonging to Mr. J. Watson, has been fitted with Swan's incandescent lamps specially arranged with outer lanterns of stout glass, air-tight, and provided with steel guards. The workings in which the lamps were fixed are 118 fathoms, or 708 feet below the surface. Twenty-one brilliant little lights placed at the pit-bottom, in the roads, and at the actual face of the seam where active operations were in process, supply an illumination of a very different character from the dismal glimmer of an occasional Davy. The electricity was generated by a dynamo-electric machine at the surface worked by a special 12 horse-power engine, and conveyed by two cables, first along telegraph poles to the pit mouth, then down the shaft to the workings, in one section to a distance of half a mile. The overhead wires are naked copper wires of 3/8 inch diameter, while those below ground are carefully insulated, and in the shaft are protected with an outer tube of galvanised iron. At suitable points of the circuit safety air-tight switches, the invention of Messrs. Graham of Glasgow, are inserted to afford control over individual lamps. The mine was visited two days after the installation of the light by members of the Mining Institute of Scotland, with whom was Mr. W. Galloway, whose remarkable experiments on the explosive effects of coal-dust will be remembered in connection with the more recent report of Prof. Abel. The party were photographed in the workings. An experiment was made with a lamp to test whether in the event of its being broken by accident a surrounding atmosphere of explosive gas would or would not be kindled by the strip of red-hot carbon before it had had time to cool. Into a box containing about three cubic feet of explosive gas a single lamp, removed from its outer protecting case of stout glass, was placed, and the current was turned on. The fragile bulb inclosing the incandescent carbon thread was then purposely broken, when the gas inclosed in the box immediately exploded. No such occurrence could possibly happen if the protecting case of stout glass is properly constructed. The risk of accident must be considered as immensely less than that of the ordinary Davy lamp, especially when it is remembered that with the brilliant light of the electric lamps they need no longer be carried in the hand or set down upon the floor near the actual spot where the coal is being got, but will be fixed overhead at a safe distance against the wall of the mine. The ease with which the light can be turned out during the firing of a blast is another point in their favour. The proprietor of the Earnock Colliery is greatly to be congratulated on the step he has taken. In 1880 the death-roll of the slain by explosions of fire-damp in Great Britain reached the figure of 499 persons. We venture to predict that the universal adoption of electric lighting in fiery mines would reduce this figure to one-tenth of its terrible proportions. How many years will it be, we wonder, before the adoption of electric lighting will be made compulsory by Act of Parliament? And how many colliery owners will discover, we would ask, when driven to this course by compulsion, that in the long run they effect an economy by discarding the clumsy and unsafe "safety"-lamp, which will so soon be numbered with the "flint mill" amongst the relics of the past?

SINGULAR STONE HATCHETS [1]

MONSIEUR PITRE DE LISLE has lately called attention to a singular class of stone celts or hatchets which have for the most part, if not indeed only, been found in Brittany and North-Western France.

These hatchets, instead of tapering away to a more or less conical point at what has been termed the butt-end, suddenly expand close at that part, so as to present a somewhat button-like termination. In one instance, at least, the hatchet ends in a spheroidal ball not unlike that which one occasionally sees on the horns of cows which are inclined to make too free use of their natural arms of offence. In the case of the hatchets, however, the button is at the opposite end to that which was in use for cutting. These blades vary in length from about three inches to as much as fifteen inches, and are all made of rocks belonging to the family of Diorites, principally of Aphanite.

M. de Lisle has given to these instruments the name of "*haches à tête*" or "*haches à bouton*," and has pointed out the similarity which in some respects they bear to hatchets of Carib origin and to the *merés* of New Zealand. In these instances the object in view in forming a projecting rib round the end of the blade was no doubt to afford the means of preventing it from slipping out of the handle or hand which held it. He thinks that the same object led the makers of these French blades to adopt the same form, and that the hatchets, after passing through a transverse hole in their hafts, were secured by cords wound around them, which abutted against the projecting rims at their small end. In his opinion there is a representation of this method of hafting to be seen among the sculptures on the dolmen of Mané-Lud.

It is a remarkable circumstance that the hatchets of this particular form appear to be restricted to so small a district of France, and not to occur elsewhere. M. de Lisle is in consequence inclined to assign the development of this type to a late period in the Neolithic age, and has offered some reasons for inferring that in Brittany the use of bronze hardly found a home, and that stone was the principal material employed for cutting tools when first that part of Gaul was brought in contact with Roman civilisation. It seems probable enough that in that as in other countries there were districts which lay far away from the principal highways of progress and civilisation, and where old-world usages prevailed long after material advances had been made in more fortunate but not very distant regions.

We may however be allowed to doubt whether the country of the Veneti, the most enterprising maritime tribe of Gaul, whose ships in the days of Julius Cæsar were already provided with chain-cables of iron, were

[1] "Les Haches à Tête de la Bretagne, etc." (Nantes, 1880.)

among those in the rear of civilisation. However this may be, M. Pitre de Lisle has done good service to archæology in publishing his monograph upon this peculiar form of stone implement or weapon.

INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES

UNDER the authority of the Comité International, representing several countries of Europe, the United States, and South America, there has been recently published, by Gauthier-Villars of Paris, an important volume of Memoirs by Dr. Broch (Directeur du Bureau), and Drs. Pernet, René-Benoît, and Marek (Adjoints du Bureau), on the following subjects relating to the determination of units of measure and weight.

As the intensity of weight varies with geographical position and height above level of the sea, the Comité give in their first memoir tables of the ratio of the acceleration of weight at the level of the sea, for different latitudes, to its acceleration at latitude 45° (Paris), to which latitude the Comité recommend that all weighings might be referred. The tables are based on the formula of Laplace, the coefficients of which are corrected by Broch in accordance with recent deductions as to the figure of the earth. In the second memoir, which relates to the tension of aqueous vapour, certain corrections of hitherto accepted results are also indicated, particularly the errors of calculation in Regnault's tables as shown by Moritz, and new tables are given for tensions at all absolute barometer heights for normal degrees from − 30° to + 101° C.

With reference to the fixed points of mercurial thermometers, the Comité adopted the proposition that the point 0° of the Centigrade thermometer should be fixed at the pressure of 760 mm., when determined in 45° latitude, and at the mean level of the sea. Also at the Congress of Meteorologists at Rome in 1879 there was adopted the proposition of Dr. Pernet, to fix the boiling point of water, 100° C., under the above pressure, so as to render strictly comparable the temperatures observed at different places. Degrees of temperature between these points are termed normal-degrees.

Tables are also given, by which may be calculated the weight of a litre of pure air in different latitudes and at different altitudes. In London (lat. = 51° 30′, alt. = 6·7 metres) the weight is 1·2938 grammes. The Comité have adopted the term litre for expressing the volume of a kilogram of pure water, instead of the term cubic-decimetre.

In a report by M. Herr on the Austrian unit of weight (Vienna, 1870), the volume of pure water at various temperatures is stated from the means of observations by Muncke, Stampfer, Kopp, and Pierre, the maximum density of water being taken at 3°·92796 C. By this formula there have been calculated, under the directions of the Comité, tables of the volume and specific weight of water from 0° to 30° C.

One of the principal works executed during 1878-9 was the comparisons of the standard kilograms at Vienna, Paris, and London. An elaborate report on these comparisons is given by M. Marek, who, by improved methods and instruments, has obtained great accuracy. The probable error of his weighings is about 0·002 mgr., or 1-500,000,000th part of the whole weight. The results also show that the material of which the standards are made, 90 per cent. of platinum and 10 per cent. of iridium, is of all known bodies the least affected by time or atmospheric changes.

In a paper on Fizeau's apparatus for determining the rates of expansions of bodies by heat, by means of an optical method founded on the phenomena of interference, Dr. Benoit gives the results of his own experiences with a similar apparatus. The results show the wonderful delicacy of Fizeau's dilatometer, as the expansions by heat of small specimens of platinum are shown in a manner incontestable to millionths of a millimetre (0·00000004 inch).

An interesting account of the establishment and objects of the Bureau is given in a preface to this volume by the Secretary to the Comité, Dr. Ad. Hirsch; and it is hoped that the efforts made by the Comité to bring about international agreement on the scientific points above referred to will commend themselves to all engaged in accurate work. H. J. CHANEY

A MODEL PUBLIC LIBRARY

ENGLISHMEN are fond of descanting upon the evils of too much centralisation, which they see displayed in some foreign systems of government, urging the amount of red tape rendered almost necessary, its inflexibility, and lack of adaptation to the infinitely-varying circumstances of different communities. But, on the other hand, the extravagant cost of working every undertaking by a separate organisation, especially in a community not large enough to make such undertakings great matters, must come forcibly home to many of those who are naturally selected to work upon several.

There has lately come under our notice an admirable case of a public library avoiding this waste, securing all the energy of private zeal, and at the same time increasing the working power of it by becoming, as a public library should become, the centre of all secondary education and the parent stem of many and various branches. If any of the smaller towns of England feel that a free library would not in their case stand by itself on account of small income, we commend this to their notice as a specimen of the advantages of co-operation.

Watford has a population of about 10,000, and the penny rate on last year's gross rental of 34,589*l.* brought in 144*l.* 2*s.* Yet this small amount has developed round it an expenditure of 700*l.* a year, equal to five times the largest rate collected, besides a large outlay on buildings at the beginning, costing some 3000*l.* subscribed, in addition to the gift of the land. Ten distinct sections are worked in connection with it. The accounts of each are shown in a separate balance-sheet each year, and the agenda paper, with notice of committee-meetings, shows how methodically the work of each section is carried out and overhauled.

Section A, the Library proper, contains about 7000 volumes; a payment of three shillings a year, or fourpence a month, is required for taking books home to read; the yearly issues accordingly amount to about 12,000. The only *free* part of the library, the reading-room, shows a something similar use of books; it is patronised chiefly by young men in the winter time, under the arrangements of Section D. The small subscription enables the book committee to spend about 50*l.* a year in new books; magazines and periodicals being supplied by a separate club, connected, of course, with the institution. We should be glad to see the troublesome and irksome system of guarantors dropped. Towns which have freed themselves from the labour and annoyance they entail, though containing far larger proportions of the "great unknown" than can a place of the size of Watford, have found no evil result. The subscription also, though small, seems to render it less necessary here.

Section B is the School of Science and Art, the latter division showing clearly that the public library at Watford by no means attends to the wants of the industrial classes only, for non-Government pupils may pay six guineas a year for drawing only. For the benefit of the evening classes, at which non-Government pupils pay a guinea and a half for the year's instruction, and Government students (whose income, that is, or parents' income, does not exceed 200*l.* a year) half that, "the subjects are arranged to meet the requirements of artisans and those engaged in mechanical trades, and include mechanical drawing, building, construction, modelling, model drawing, outline drawing, and shading."

This section includes science teaching also, and classes were formed last year in inorganic chemistry, botany, principles of agriculture, fully illustrated by experiments; shorthand also, a useful help to all such studies.

Section C is Entertainments, and is set going or stopped as occasion offers; of course it is expected to be a help to the general fund.

Section D is the Youths' Institute, supported also by special honorary subscribers and by one penny a week paid by its members for admission to the reading-room, *because without that the room was too full.* Let us hope that such a very unsatisfactory statute of limitations may soon cease to be necessary. An extra rate would hardly be grudged in such a case, and it is a strong argument for Parliament authorising one.

Section E is a Private Subscription-room, supported by about a hundred members, who subscribe ten shillings a year each, spent in newspapers and periodicals, made available to the public after their use by the club.

Section F is a School of Music, where some of the best masters obtainable in London are engaged, two of whom take the combined instruction, and four take separate instruction. Nine different classes of lessons are arranged for, again not free, but all made available to those who wish for them, at very little expense or labour to themselves. It is now supported by 160 students, and by subscriptions from the vice-presidents.

Section G is the nucleus of a Museum; and how treasures of local interest are lost to a town for want of such a nucleus in trustworthy hands, the writer knows well! Central museums also can easily supply duplicate treasures to such institutions in almost perpetual succession.

Section H, the English Literature Club, meets weekly through the winter, at the library rooms, adding greatly to the care with which books are read, and, consequently, to the pleasure and information drawn from them. A very small subscription pays its, no doubt very small, expenses.

Even needlework in elementary schools, though spread about half the County of Herts, and patronised by a goodly company of influential ladies throughout the district, has its "head centre" in a committee of three gentlemen of the Watford Public Library. Fifty-four schools compete, and 1500 specimens are shown, in six classes of work, all having undergone a strict and very systematic examination.

The same association supports also a School of Cookery.

Other offshoots of the Library are the Herts Natural History Society, the Foresters' Club, Junior Foresters' Club, and the Shepherds' Club, each having its meeting-place at the library rooms.

Now among this variety of work there is probably none which is not carried on in nearly all the larger towns of England by *some* means. What we wish to set forth is the reasonableness of its all forming together the work of a single "committee of education," not necessarily elementary only by any means; and that a rate-supported public library should be the central institution, whose committee should set in co-ordinate motion all the parts of this local educational machine. Such a committee need not attempt to take into its hands the entire control of each separate branch, but should work all together with as little friction and loss of labour as possible, and especially should this be the case, as we have said, in our smaller towns. Very great is the economy of one institution working all together, in the matter of rooms, advertising, and printing; in one man receiving, as the librarian does at Watford, all the subscriptions and fees paid to these various societies, the 5 per cent. allowed him upon all giving him a tangible interest in increasing each, as such a central worker must have the means of doing, and in stirring all up to a friendly rivalry in well-doing. And the advantage can hardly be over-stated of the power of such an organisation to bring together earnest workers, who might otherwise have followed either a secluded path or one crossing that of other workers; in the one case, occurring most frequently in small communities, doing little for the advance of intelligence and information; and in the other case, to which large cities are most liable, wasting time and efforts which are often thwarted by mere local jealousies.

NOTES

IN June of the present year the freshwater jelly-fish (*Limnocodium Sowerbii*) reappeared in the Royal Botanical Society's Gardens, Regent's Park, though in no great numbers. At the suggestion of Mr. George Busk, F.R.S., and with the courteous assistance of Mr. W. Sowerby, a small number were captured and transferred to the Victoria tank in Number 10 House at the Royal Gardens, Kew. Nothing was known of their fate till about a week ago, when it was observed that the whole tank was swarming with the progeny of the small colony brought from London.

THE Local Committee at York have been making laudable exertions for the accommodation of those who intend to be present at the meeting of the British Association next week. They have prepared a long list of hotels and lodgings, with prices, at the same time stating that the prices of the lodgings are higher than will be eventually charged, "as there is abundance of good accommodation at reasonable rates." They have also issued a time-table of the arrivals and departures of trains at York station from the principal towns in the kingdom, with special tables for the local lines. A map of the city has besides been prepared, showing the situation of the principal buildings, the meeting-places of the various sections, and the principal hotels, of which there are fourteen.

THE *Times* Geneva correspondent gives some further particulars concerning Prof. Raoul Pictet's model steamer now in course of construction, with which he expects to reach a speed of forty miles an hour, and which will make a trial trip on the lake in November next. Her dimensions are—16 metres long and 3·50 metres wide. When lying at anchor she will draw 33 centimetres fore and 44 centimetres aft; at full speed 1 centimetre forward and 16 centimetres aft. The engine will be placed amidships, from which point to the stern the screw-shaft and the keel form an inclined plane; the bows are long, tapering, and wedge-shaped. Prof. Pictet reckons that his invention will lead to a great saving of fuel, inasmuch as a steamer built on his plan, after being started with say 100 horse-power, may be kept up full speed with an expenditure of force equal to thirty horses. The form of the hull, on which the maintenance of the ship's equilibrium will depend, cannot be explained without a diagram. Prof. Pictet is quite confident in the success of his invention, and his previous scientific achievements have been so remarkable that many people who cannot follow his reasoning have no hesitation in accepting his conclusions.

THE inhabitants of Havre are collecting money for raising a statue to Sauvage, who is considered in France as having applied the screw to the propelling of steamers.

A TELEGRAPHIC experiment of a singular description was tried last week at the Trocadéro. It consists merely in the reading of large silvered zinc letters, a square metre in size, fixed on a blackened board, by refracting telescopes. This method has succeeded very well from the Trocadéro to the Panthéon, a distance of about three miles. The inventor, an officer in the French service, thinks he will succeed in reading messages at a distance of sixty miles under favourable circumstances.

M. EUGÈNE GODARD, the celebrated French aëronaut, who has been making ascents at Gotha, has been made by the Grand Duke a knight of his order. M. Darmentiers, another French aëronaut, having ascended from Montpelier, has been less successful. He was driven by a strong wind out to the Mediterranean Sea, where he perished according to all probability, no news having been received from him up to the latest date.

AN advertising vehicle is circulating in the Paris streets lighted at night by voltaic electricity obtained by bichromate elements. It circulates all round the Boulevards.

THE triple granite concentric vaulting of the St. Gothard Tunnel, in the quicksand formation under Andermatt, is now completed, and as the rings previously constructed remain intact, it is confidently hoped that a difficulty at one time thought to be insuperable has been conquered, and that the great tunnel will be finished by the end of October.

FROM the Report in the last Technological Examination of the City and Guilds of London Institute we see that the number of candidates and centres for examination have largely increased. The results are generally better than in the previous year, though from the examiner's reports there is, in most subjects, much to be desired. No doubt in the course of a few years, when the Institute has been fairly at work, the improvement in technical knowledge in the country will be very marked.

AN official investigation shows that the phylloxera infests an area of over 8000 square metres of the vineyards at Heimersheim, near Remagen, on the Rhine. The diseased vines were imported from Austria. Energetic steps are being taken for the annihilation of the disease.

THE death is announced of Capt. Popelin, one of the Belgian African explorers, who had established the station at Karema, on the south-east shore of Lake Tanganyika, where, apparently, he has succumbed to fever. Capt. Popelin was only thirty-four years of age.

THE British Archæological Association is holding its meetings at Malvern, which forms an admirable centre for excursions to antiquities of all kinds.

THE "Polytechnic" expires this week, many of our readers will be sorry to hear. The age seems to have outgrown its toy science, though doubtless the institution did good in its day in paving the way for the popularisation of real science. In the memories of our older readers it will doubtless be associated with many a happy day.

MR. WILLIAM ARCHER, F.R.S., Librarian of the National Library of Ireland, has just issued a pamphlet which we commend to his fellow-librarians. The pamphlet consists of "suggestions as to public library buildings, their internal plan and construction, best adapted to effect economy of space (and, hence, saving of cost), and at same time most conducive to public, as well as administrative, convenience, with more especial reference to the National Library of Ireland." We cannot enter into the details discussed by Mr. Archer, but his leading principles as to arrangement may be thus summed up in his own words:—"Central reading-rooms and offices connected, by short and sufficiently numerous radii, with a continuous circuit of book-rooms around and beneath the same, the books in the book-rooms on a greater or less number of tiers of standing presses, these not more than eight feet high—thus securing the immense boon of the abolition of ladders and galleries, and saving at once space (and cost) as well as the time of the public." Mr. Archer draws attention to the fact that the question of space is gradually becoming more and more urgent as regards this, the only large public library in Ireland, and that if the providing of enlarged accommodation is to be delayed until a science and art museum and a metropolitan school of art should be built, so as to hand over the buildings at present occupied by these for library purposes, it looks very probable that the library must come to a "stand still." He states further that the number of readers was never before so great as during the past winter, the evening readers having especially increased. The National Library of Ireland, therefore, even in its present cramped and inadequate quarters, is fulfilling a mission of usefulness.

AN important discovery has been made in the vicinity of Kenneh, Upper Egypt. No less than thirty-six well-preserved sarcophagi have been brought to light. They almost exclusively belong to the kings and queens of the older Thebes Dynasty. They contained mummies, papyrus scrolls, Osiris statuettes (some thousands), ornaments, and talismans. The royal names of Raskenes, Amenophis I., Ahmes, Nofretari, Aahhotep, Totmes II. and III., Seti I., Ramses XII., Pinotem, and other Pharaohs are mentioned in the texts, and show the importance of the discovery. The sarcophagi were all found in one sepulchral chamber.

THE "Smithsonian Report" for 1879 affords ample evidence that the Smithsonian Institute continues to carry on its great work with increasing efficiency. Its grants of money have been devoted to the carrying out of anthropological researches and geographical explorations, and to the publication of a large number of works bearing on the progress of science. Large additions have been made to the various departments of the free museum of the Institute. The Appendix to the Report, occupying the larger half of the volume, contains a number of papers bearing on the anthropology of the North American Indians, one paper of considerable length being "A Study of the Savage Weapons at the Philadelphia Exhibition," by Mr. E. H. Knight. Other papers in the Appendix are: "On the Present Fundamental Principles of Physics," by Prof. Franz Joseph Pesko; "A Universal Meteorograph, designed for Detached Observatories," by Prof. E. S. Holden.

THE *Transactions* of the Norfolk and Norwich Naturalists Society for 1880–81 contains several useful papers. One of the most interesting is that of Mr. Southwell, "On the Extinction of Species by the Indirect Acts of Man." Mr. Southwell adduces a number of instances in point, showing that experimental acclimatisation sometimes leads to disastrous results. Mr. H. W. Feilden contributes some remarks on the Natural History of Franz-Josef Land. From the presidential address we are glad to see that the Society continues to prosper.

A SHORT time ago a colossal whale was captured and killed on a sandbank near Westerland-Lylt (Schleswig). It measured 52 feet in length, 26 feet in circumference, and its tail-fin spanned 7 feet across. The animal must have entered the Watten Sea during high tide and lost itself in shallow water, when the receding tide left it upon the sand.

THE Medical and Sanitary Exhibition, organised by the Committee of the Parkes Museum, was open for the last time on Saturday, August 13, when the number of visitors, exclusive of season-ticket holders, was 1,221, making a total of 24,333 visitors for the four weeks during which the Exhibition has been open, allowing only for one visit by each season-ticket holder. During the day the secretary, Mr. Mark Judge, visited the different exhibitors for the purpose of ascertaining their opinion as to the success of the Exhibition. The exhibitors generally expressed themselves as well satisfied with the result, some going so far as to say that they had done an exceptional amount of business owing to the fact that a very large proportion of the visitors had been either medical men, architects, or engineers. The representatives of the exhibitors, who have been in daily attendance during the Exhibition, marked their appreciation of the arrangements made for their convenience by presenting on Saturday a small purse of gold to the superintendent, Mr. Smithson. The closing of the Exhibition was taken advantage of by the St. John Ambulance Association to give a demonstration of ambulance practice, and during the afternoon a large number of the visitors assembled in the conservatory to witness the practice, which was conducted by Major Duncan, Mr. Cantlie of Charing Cross Hospital, Mr. Furley, Dr. Crookshank, and Surgeon-major Baker. Prizes were competed for by squads of the Grenadier Guards, the Finsbury Rifles, and the Metropolitan Police. Mr. John Eric Erichsen (the chairman), Dr. Poore, Dr. Steele, Mr. George Godwin, Mr. Rogers Field, and other members of the Exhibition Committee were present during the day. It is expected that the prizes which have been awarded will be distributed at the Annual Meeting of the Parkes Museum in the autumn.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Lemur (*Lemur catta*) from Madagascar, presented by Mr. E. O. Brookfield; a White-collared Mangabey (*Cercocebus collaris*) from West Africa, presented by Mr. James Jameson; a Diana Monkey (*Cercopithecus diana*) from West Africa, presented by Mr. Louis Wyatt; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. R. Edge; two Vulpine Phalangers (*Phalangista vulpina*) from Australia, presented by Mr. George White; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from the Moluccas, presented by Mrs. Beard; two Wonga-wonga Pigeons (*Leucosarcia picata*) from New South Wales, presented by Mr. J. Burnham; a Royal Python (*Python regius*) from West Africa, presented by Mr. G. H. Garrett; a Grey Parrot (*Psittacus erithacus*) from West Africa, deposited; a Lesser White-nosed Monkey (*Cercopithecus petaurista*), two Pluto Monkeys (*Cercopithecus pluto*) from West Africa, purchased; a Zebu (*Bos indicus*), a Pampas Deer (*Cariacus campestris*), born in the Gardens. Amongst the additions to the Insectarium during the past week are pupæ of *Attacus aurota* (one of which has since emerged) and *Ceratocampa ixion*, from Brazil; larvæ of the Madder Hawk-Moth (*Deilephila galii*), the Death's-head Hawk-Moth (*Acherontia atropos*), the Poplar Hawk-Moth (*Smerinthus populi*), and Fox Moth (*Bombyx rubi*), and perfect insects of the Water Stick-insect (*Ranatra linearis*).

PHYSICAL NOTES

M. SAMUEL, of Ghent, has brought before the Belgium Academy a method of registering telegraphic signals received through the mirror galvanometer (*Bull.*, No. 5). On the screen receiving the light are fixed two selenium elements, one to the right, the other to the left. When either is illuminated its conductivity of course increases, and it acts as a relay on an electro-magnet, which causes a Morse dot or dash to be marked on paper. There are two local batteries, one having two circuits, which pass through the selenium pieces and the electro-magnets, while the other is for the electro-chemical writing. In this latter, a band of paper saturated with iodide of potassium passes continuously over a small copper cylinder which is connected with one pole of the second battery. Above the paper are the ends of the armatures of the electro-magnets; to one is attached a vertical platinum rod, to the other a small triangle with platinum base (horizontal). The rod and triangle are connected, through the armatures, with the other pole of the second battery, and they press the paper band on the cylinder each time the armatures are attracted, giving a dot or a dash as the case may be. The dashes, instead of being longitudinal, are at right angles to the length. If the ordinary lamp of the galvanometer be replaced by sunlight or lime-light, the electro-magnets (M. Samuel points out) may be actuated directly without use of a galvanometer relay; Bell's selenium elements having an average resistance of only 150 ohms in sunlight, and 300 ohms in darkness.

AN evaporimeter with constant level has been recently described by Prof. Fornioni (*Reale Ist. Lomb.*, vol. xiv. fasc. x.-xi). It consists of an oblong wooden case with a brass spiral descending into it from a micrometric screw. The spiral carries at its lower end a small glass vessel which acts as feeder. A glass siphon extends outwards horizontally from the feeder, and has at its outer end a small cup, in which the evaporation takes place. As the water evaporates in the cup the feeder is lightened, and rises by action of the spiral, thus keeping the level constant. A fine layer of oil in the feeder prevents evaporation from its water-surface. There are guides to control the vertical movements of the feeder, which, moreover, are indicated by means of a weighted thread, affecting an external index on a disk. The graduation of the instrument is expressed in millimetres of the height of water in the evaporating vessel.

SIGNOR MAURI (*Riv. Sci. Inst.* No. 11) obtains an economical and very compact battery carbon, intimately united with the electrode, as follows:—Finely-powdered graphite is mixed with an equal weight of sulphur (which should be free from carbonates), and the mixture is heated in an iron vessel until complete fusion of the sulphur. The temperature should not be raised beyond 200°. When the mass is fluid it is poured into a suitable metallic mould, and a thick copper wire, bent zigzag, is quickly inserted, a part being left projecting. The mass is let cool slowly: then it is easily drawn from the mould and is ready for use. These carbons have a conductivity practically equal to that of retort carbon, and are more electro-negative, consequently better adapted for electromotive force. Coke-powder cannot be substituted for graphite, because it has too little conducting power. By increasing the proportion of sulphur, the resistance may be increased at will, and strong resistances may be thus easily prepared in place of resistance-coils of copper wire. (S. Mauri further indicates a way of utilising graphite in construction of a miner's fuse.)

REPEATING Mercadier's experiments in which an intermittent beam meets smoked a surface within a glass tube containing aqueous or ammoniacal vapour, and furnished with an ear-tube, Prof. Mugna lately (*Riv. Sci. Ind.* No. 11) made the effects much better heard by attaching a small microphone to an elastic membrane closing the tube; and it was possible to operate at such distance from the interrupting apparatus, that its noise was no longer disturbing. Prof. Mugna further experimented by suspending horizontally from a cocoon-fibre, within a glass case, a short fine glass rod, with terminal laminæ of card or glass, or very fine metallic foil. An intermittent beam sent against one of the laminæ when they were in a position parallel to the wheel drove the system round in direction of the beam, indicating (the author considers) a direct action of the latter. An air-current due to thermal action should produce the opposite motion. Besides, the phenomenon is still better when the air is rarefied.

IN a note to the Vienna Academy, Dr. Margules calls attention to the beautiful figures that are produced in glycerine, when the liquid is moved in a regular way, by rotation of a disk in contact with it. These figures afford an insight into the form of the surfaces and paths of the currents. They are due to the water contained in the glycerine.

THE method described by Herren Kirchhoff and Hahnemann last year for determining the heat-conductivity of metals, has been applied by them (*Wied. Ann.*, No. 7) to three varieties of iron, and to lead, tin, zinc, and copper; and the electric conductivity of these metals has also been measured. The conclusion is that the ratio of these conductivities is in general constant in these different metals, with exception of iron, and it is thought the exception may be connected with magnetic properties. Herr H. F. Weber's result disagrees with this, for he finds the ratio to be a linear function of the product of specific heat and density. The authors are unable to discover the cause of this discrepance.

AN initial attempt to elucidate the ratio of the specific heat of liquid organic compounds to their composition has been made by Herr von Reis (*Wied. Ann.*, No. 7). It appears from his researches that the difference of the molecular heat of homologous compounds at 20° boiling point, and from 20° to 100°, is very regular: in the former case it is 8·0 and in the latter 7·5.

Alcohols form an exception, having a comparatively high specific heat; they gave the differences 9·7 and 8·5 respectively. Isomeric substances of similar composition have the same molecular heat, while those of unlike composition have a different. In the tables which give carbon and hydrogen differences there are exceptions along with regularity. For a right development of the theory Herr von Reis feels that more extended observation is necessary.

THE idea of qualitative analysis of substances by microscopical examination of crystalline forms is worked out to some extent by Herr Lehmann (*Wied. Ann.*, No. 7). A shallow watch-glass is substituted for the cover glass, and serves for turning over in various ways the crystals which form in the inclosed solution. The domain of regular forms is avoided as unsuitable, and only irregular forms observed—the so-called growth-forms, crystal skeletons, trichites, &c., produced by acceleration of crystallisation, viscosity, and so on. For details of Herr Lehmann's method and apparatus we must refer to his paper.

HERR SCHULLER has lately described to the Hungarian Academy of Sciences (*Wied. Ann.*, No. 7) a mercury air-pump which works automatically, and in which all greased glass combinations are dispensed with, the hermetic closure being effected with only glass and mercury. The evacuating power of the apparatus was not exactly measured; there are proofs that it is high.

MR. J. MILNE has written a careful account of the vertica and horizontal motions accompanying the earthquake of March 8, 1881, in Japan. This is believed to be the first earthquake in which a complete continuous record of both components of the motion has been obtained for a period exceeding twenty-five seconds. The actual maximum displacement appeared to be about 1·33 millims., recurring at the rate of about seven vibrations in five seconds. From the phenomena of this shock, and from some experiments on artificial earthquakes produced by letting an iron ball weighing about one ton fall from a height of about thirty-five feet, Mr. Milne argues that the waves that are felt are *transverse* to the line of propagation of the shock.

BIOLOGICAL NOTES

RELATIONS BETWEEN THE CRANIUM AND THE REST OF THE SKELETON.—These relations form the subject of a paper by M. Manouvrier, read at the last meeting of the French Association. The following are the author's conclusions:—1. The weight of the cranium varies, in a general way, with the weight of the skeleton, but not proportionally, like the weight of the brain. 2. The weight of the skeleton, less the cranium, in a given race, varies nearly in proportion to the weight of the femur. 3. The weight of the cranium is greater relatively to that of the femur, the lighter the latter is. 4. The weight of the cranium is much more considerable relatively to that of the femur in woman than in man. 5. This sexual difference is so pronounced that it constitutes one of the best secondary sexual characters. About 82 women in 100 have the cranium heavier than the two femurs, while 82 men in 100 have it lighter. 6. The lower jaw is heavier relatively to the cranium in the anthropoids than in man, is inferior than in civilised races, in man than in woman, and in the adult than in the child. 7. The weight of the cranium is smaller relatively to that of the lower jaw, the heavier the latter is, &c.

THE COLOUR CHANGES OF AXOLOTL.—Prof. Semper has lately examined axolotl with regard to the influence of light on its colour (*Würzburg Phys. med. Ges.*). When young axolotl are reared in darkness they become quite dark; nearly as dark in red light; in yellow, on the other hand, pretty bright; and brightest in bright daylight. The difference is connected not only with the chromatic function found in various degrees in all amphibia, but on pronounced formation of a peculiar diffuse yellowish green colouring matter, increase of white, and diminution of dark chromatophores. Further, when axolotl are exposed to daylight in white dishes covered with white paper, much less dark pigment forms in them than when they are kept in white dishes without a paper cover (other things equal); though in the latter case they are apparently exposed to the most intense light; these darker axolotl are, however, still much brighter than those reared in red light or in darkness. Since (as experiment showed) the white covering paper let through much light, but very little of the chemical rays, it appears that chemical rays play no part in the formation of pigment. But the causes of the whitening in bright daylight and the darkening in absence of light remain unknown as before.

SIREDON LICHENOIDES.—Mr. W. E. Carlin publishes in the June number of the *Proceedings* of the United States National Museum some very interesting details about this remarkable form. Its chief habitat is a body of water some two and a half miles in circumference called Como Lake. This has no known outlet, but is fed by a perennial stream of pure spring water. The lake is shallow, and its water very strongly impregnated with an alkali; it is very disagreeable to the taste. The Siredon never enter the freshwater stream; they abound in the alkaline waters of the lake in immense numbers. When about one hundred and fifty were placed in fresh water they seemed to suffer no inconvenience, but it had a remarkable effect in hastening their metamorphosis into the Amblystoma form. Of an equal number kept in fresh water and in the lake water, quite a change occurred with the former after twenty-four hours, while the latter showed no change after several days of captivity. Those that were kept well fed in jars usually began to show a slight change in from two to three weeks, and all of them completed the change into the Amblystoma inside of six weeks, while in some kept, but not specially fed, there were but three changes in three months. Specimens kept in captivity became quite tame, soon learning to know that tapping the jar in which they were, meant a fly, and, rising to the surface, would snap at whatever they saw first, pencil or fly.

FISH MORTALITY IN THE GULF OF MEXICO.—We glean a few more particulars as to this strange mortality from the June *Proceedings* of the United States National Museum. The fishing interest of Key West is an important one, supplying thousands with the means of subsistence. The fishermen state that a volcanic spring exists, the waters from which are of a high temperature. The polluted waters are of a red brick colour; their influence is seen for a distance of 200 miles. A scant supply of sea water from the Gulf of Mexico sent to Washington was examined by Mr. F. M. Endlich of the Smithsonian Institution. That in which the fish died (A) contained a large quantity of algæ and infusoria, and the pure water (B) had none. They gave the following analysis:—

	A.		B.
Spec. grav.	1·024	...	1·022
Solids per cent.	4·0780	...	4·1095
Ferric compounds per cent. ...	0·1106	...	0·0724
Injurious organic matters ...	ratio = 3	...	= 2

Even on spectroscopic analysis Mr. Endlich could not find in A any mineral constituent which could noxiously affect the fish, and he thinks that death must be caused by parasitic algæ, while Surgeon Glazier agrees with the prevalent opinion that the catastrophe is due to the salt water being impregnated with gases discharged from volcanic or geyser-like springs. During November last the waters of Tampa, Sarasota, and Charlotte Harbour were covered with thousands of dead fish, and the stench was quite overpowering.

THE BLOOD OF INSECTS.—Operating with the larva of *Oryctes nasicornis*, M. Fredericq has observed (*Bull. Belg. Acad.*) that the blood of the animal, drawn off in a small glass cannula, is a colourless liquid, but on exposure to the air presently takes a decided brown colour, and coagulates. The coloration he regards as a purely cadaveric phenomenon. The substance which becomes brown is probably formed in the moment of coagulation, and does not serve in the body as a vehicle between the external air and the tissues, like *hæmoglobin* in Vertebrates and many Annelids, *hæmocyanin* in Crustaceans, &c. When the larva is kept a quarter of an hour in hot water (50° to 55°), the blood extracted does not coagulate or become brown. Once the substance which browns is produced, even a boiling temperature does not prevent its browning. The brown substance once formed is very stable, not being decomposed either by acids or alkalies, and not made colourless by being submitted to vacuum or kept in a closed vessel. The existence of an intermediary in insects corresponding to hæmoglobin M. Fredericq thinks very problematical in view of the anatomical system, letting air penetrate into the heart of the tissues.

NEW PYCNOGONIDA.—The result of the examination of the collection of Pycnogons made during the cruise of the U.S. steamer *Blake* by Edmund B. Wilson, has just been published as No. 12, vol. viii. of the Harvard College Museum *Bulletin*. This collection was found to possess features of considerable interest, and though the species in it were few, some of them were of remarkable size, quite colossal in comparison with shallow water or littoral forms. Of the three species of Colossendeis, two of which are described as new, the smallest has a span of 14 cm. between the tips of its outstretched legs, while the largest has an extent four times as great. A new genus (Scæorhynchus) has been established for a species with a span of 19 cm., a gigantic size as compared with the dimensions of its nearest allies. The most abundant species of Nymphon is the largest of that extensive genus, and one species of a new genus (Pallenopsis) is more than twice as large as any of the species of allied genera, such as Pallene, which are known only from the littoral zone. It is further interesting to note that in a number of forms the visual organs (ocelli) are either rudimentary and destitute of pigment, or are entirely absent. In Pallenopsis, however, the ocelli are relatively of unusually great size. The species of Scæorhynchus and Colossendeis show clearly from anatomical evidence the complete independence of the accessory legs and the first pair of ambulatory legs, as had been already proved by Dohrn from embryological data. In all cases the palpi and accessory legs are supplied with nerves from the same ganglion, and this latter shows in the adult no indication of being composed of two coalesced ganglia. But Dohrn states that there are in the larvæ of Achelia two ganglia. This question is of great interest, having a direct bearing on the affinities of these Pycnogons with the Arachnids. Mr. Wilson describes ten species, of which one half are given as new, and with figures.

New Zealand Desmids.—As a contribution to our knowledge of the pretty green unicellular algæ known as Desmids, which are to be met with in New Zealand, Mr. Maskell's paper in the recently-published volume of the *Transactions* of the New Zealand Institute is most welcome. It would seem to render more than probable the idea that these minute algæ are to a large extent cosmopolitan. The author is evidently under great disadvantage as to identifying the species he meets with, but this is to a great extent done away with by his fairly careful descriptions and accurate (as to outline) figures. He enumerates between sixty and sixty-five species, some of which are very noteworthy and fine additions to the list of Desmids; thus *Aptogonum undulatum* is a highly interesting new species. *Triploceras bidentatum* is not only a very distinct, but also a very noble, new species, and equally distinct as a species is his *Closterium selenæum*. Doubtless a more prolonged search in fresh localities will enable the author to add many old and new species to the list. He may feel sure that his further researches will be looked for with interest by those working at the freshwater algæ in Europe.

Protoplasm Stained whilst Living.—Mr. L. F. Henneguy publishes the result of some experiments made on living infusoria, in which he confirms the observations of Brandt, made in 1879, that an aqueous solution of aniline brown, known in commerce as Bismarck brown, will give an intense brownish-yellow colour to the protoplasm of the infusoria without in any way interfering with their enjoyment of life. The coloration first appears in the vacuoles of the protoplasm, then this latter is itself stained, the nucleus being most generally not at first coloured, and so being made more conspicuous. Experiments made on vegetable protoplasm seemed to exhibit the same result.

LARGE TELESCOPES[1]

The small amount of work accomplished with large telescopes has often been the subject of unfavourable comment. This criticism applies with especial force in America, where there are nearly a dozen telescopes having an aperture of a foot or over, besides two of the largest size now in course of construction, and two of twenty-six and twenty-four inches aperture which are unmounted and have been for several years perfectly useless. Among so many it seems as if one might be spared for a trial of the following plan, which, if successful, would produce at a small expense far more work than could be obtained with a mounting of the usual form.

Suppose that the telescope is placed horizontally at right angles to the meridian, and that a plane reflector inclined to its axis by 45° is placed in front of it. This reflector may revolve around an axis coinciding with that of the telescope. Such a mounting has been used in transit instruments, and gives much satisfaction in the meridian photometer of the Harvard College Observatory. The principal difficulty with a large instrument would lie in the flexure of the reflector. This difficulty has however been overcome in a great measure in reflecting telescopes by various ingenious devices. In the present case, since the reflector rotates only around one axis instead of two, the problem is much simplified. A slight motion at right angles of perhaps 5° would be a great convenience, as will be shown below, and would probably be insufficient to materially affect the flexure. It may be said that it is more difficult to make a plane surface than one that is curved. But the principal effect of a slight curvature would be to change the focus of the telescope, the aberration being much less than the effect of the varying flexure. Let us admit, however, that the best definition cannot be obtained, in considering the purposes to which such an instrument could be applied without disadvantage.

Many advantages will be apparent on comparing such a mounting with an equatorial. Great steadiness would be secured, since the mirror would be the only portion moved, and this would be placed directly upon a low pier. Instead of a large and expensive dome which is moved with difficulty, the mirror would be protected by a small shed, of which the roof could be easily removed. It would therefore be opened and ready for use in a very short time, and would quickly take the temperature of the surrounding air. The object-glass would be mounted directly upon a second pier, and, as it would not be moved, would be in very little danger of accident. The tube could be made of tin or other inexpensive material, as its flexure is of no importance. It could easily be protected from the changes of the temperature so troublesome in the tube of a large equatorial. If preferred it might even be exhausted of air, or filled with hydrogen, and the effect of the changes of temperature thus greatly reduced.

The eyepiece could be mounted on a third pier, and would be so far distant horizontally from the mirror and object-glass that there is no reason that it should not be inclosed in a room which may be warmed. The comfort in winter of working in a warm room will be appreciated by those who have used a large telescope in a cold climate. The result is sure to be an increased precision in the observations, and a possibility of prolonging them over longer intervals. A similar effect is produced by the constant direction of the line of sight. No especial observing chair is needed. There is no limit to the size of the attachments which may be made to the eyepiece, since they need not be moved. This is a great advantage in certain spectroscopic and photometric measurements. A strong wind interferes seriously with many observations, as it is impossible to make a telescope so stiff that it will not be shaken by sudden gusts. In the plan here proposed the mirror alone is exposed, and its surface is too small to give trouble.

By means of a long handle the position of the mirror may be regulated from the eye-end, and the declination of the object observed read by small telescopes. If the mirror can be moved at right angles to the meridian 5° from its central position, an object at the equator may be followed for forty minutes, and other objects for a longer period. Without this motion an object may be followed for three or four minutes by moving the eyepiece alone. Clockwork may be applied to the mirror, or less easily to the eyepiece. The focal length may be increased almost indefinitely if desired, and certain advantages will be thus attained in the diminution in the defects of the object-glass, although those of the reflector will not be affected. If the telescope is to be erected at a great elevation the advantages of the present plan are at once apparent. Many nights of observation would be secured which otherwise would be lost owing to the wind and cold. The simplicity in the construction of the building would be a great advantage, as a large dome in so exposed a situation would be kept free from snow with much difficulty, and might be a source of danger in winter storms. If found impracticable to observe during the winter, it would be possible to have a duplicate mounting below, and remove the lens and mirror from one to the other.

It is evident that the saving of cost would be very great, not only in the observatory building and dome, but in the tube, observing chair, clockwork, &c.

If a reflector could be constructed whose surface was the portion of a paraboloid whose abscissa equalled that of the focus, the instrument could be much simplified. No object-glass would then be required, the reflector taking the place both of mirror and lens. All the light intercepted by the objective would thus

[1] By Edward C. Pickering, communicated by the author.

be saved, and but a single surface need be adjusted and corrected. With the advance in mechanical methods this does not seem wholly impracticable, especially with a mirror of long focus. Since the final correction must always be made by hand, it makes but little difference what is the exact form of the surface.

In any case it would be a great advantage that the mirror could be reground, repolished, or resilvered without moving it from its place. It would only be necessary to place it horizontally, and the grinding machinery could be kept permanently near it. If plane, the perfection of its form could also be tested at any time by setting it on edge, and viewing the image it reflected by a collimating eyepiece attached to the large telescope. Another method would be to place a heliotrope a few hundred yards to the north or south of it, and the light from this would form an excellent artificial star, available whenever the sun shone.

The greatest advantage is the rapidity with which observations could be made. No more time would be lost in identification than with a transit instrument, so that a large number of objects could be examined in the course of a single hour. Any one who has worked with a large telescope knows how much time is lost in opening and closing the dome and in finding and identifying minute objects.

Let us now consider to what purposes a large telescope mounted as suggested might be applied.

1. Sweeping. For the discovery of new objects this mounting presents especial advantages. It might be used for the detection of new double stars, of nebulæ, of red stars, or of objects having singular spectra, as planetary nebulæ, banded stars, and variables of long period. Suppose that the field of view had a diameter of somewhat over one minute of time, and that a small motor was attached to the mirror which would move it uniformly over 5° in declination in this time, and then bring it quickly back to its first position. The observer would then have presented to him a series of zones 5° long and one minute wide. The sweeps should overlap by a small amount, so that the entire region could be covered in a single evening. The observer could have a few seconds rest between each zone, while the motion of the mirror was reversed. If an object of interest was suspected, it could be located by merely noting the time at which it was seen. The right ascension would be given directly, and the declination would be found by interpolation from the time of beginning and ending the sweep. An examination of the object and a determination of its exact location should be made on another evening.

2. Measures of position. For many purposes positions could be determined with this instrument as in a transit circle. It would generally be better however to make the measures differential, leaving the mirror at rest and observing the transits of the object to be determined and of two or more companion stars. The method of the ring micrometer might be employed, or some modification of that with inclined lines. In the latter case the zero of position could be found by the transit of preceding stars, by setting the reticule by a divided position circle, or perhaps better by keeping it in a fixed position, determining the direction of the lines once for all, and applying a correction for the declination of the object observed. Stars could be compared differing nearly a degree in declination, as the eyepiece could be moved without danger of disturbing the reticule. For the same reason the star could be followed for three or four minutes, and its transit over a great number of wires observed. It is here assumed that the distortion produced by the mirror is not very great. A slight distortion would do little harm, as it would be the same for all stars of equal brightness. If the stars differ greatly in brightness, the observer should determine his personal equation between them in any case, and the same operation would eliminate the effect of the distortion. The large aperture of the instrument would permit the observation of stars quite beyond the reach of any meridian circle. The faintest asteroids could thus be readily measured, and could probably be followed in many cases on successive evenings to their stationary points. Zones of stars could be observed very conveniently for the formation of charts or catalogues, for the discovery of asteroids, stars with large proper motion, &c.

Probably the definition could not be sufficiently good for the measurement of the closer double stars, but if clockwork was attached, faint companions could be measured, or approximate positions of the coarser pairs determined very rapidly. The positions of nebulæ could also be observed with a view to detecting their proper motion. Stars having a large proper motion might be observed, and the observations so arranged that any very large parallax would be detected. A similar search for a large parallax of variable stars, short-period binaries, minute planetary nebulæ, or stars having singular spectra, might lead to interesting results. The argument that no ordinary star is very near does not apply to such objects.

3. Spectroscopy. The increased dimensions which could be given to the spectroscope, and its steadiness, would compensate in a great measure for a defect in definition. By Zöllner's reversion spectroscope the slit might be dispensed with, and also the necessity of clockwork. So many stars could be observed in a single evening that systematic errors could be in a great measure eliminated, and as the spectroscope would not be moved, we should have a great assurance that the deviations were real. Of the 6000 nebulæ hitherto discovered we know nothing of the spectrum of more than 300 or 400, while the observation of all the others with a large horizontal telescope would not be a very formidable undertaking. It would also be interesting to observe the spectra of all the clusters. It is possible that some may consist of stars having singular spectra, or even of disconnected nebulous masses, in fact forming clusters of planetary nebulæ. The interesting discovery by Dr. Copeland that Burnham's double nebula in Cygnus is gaseous, shows the same tendency to aggregation in these bodies as in stars. Observations of the spectra of all the red stars and variables would also probably lead to interesting results.

4. Photometry. Should the instrument be devoted to photometry numerous problems suggest themselves. Variable stars could be observed near their minimum when too faint to be identified with an equatorial without great loss of time. Faint stars in zones or faint companions to bright stars could be measured very rapidly. The relative light of all the asteroids would be an interesting problem. Many coarse clusters appear to consist of stars of nearly equal brightness. Their light compared with their distances apart might aid our study of their formation. Another useful investigation would be to measure the brightness of all the nebulæ.

In the application of physics to astronomy doubtless many other problems will suggest themselves. Thus no satisfactory results have been obtained in the attempt to measure the heat of the stars with the tasimeter. The use of this instrument would be vastly simplified if it was placed on a solid pier near the ground, was not moved during the observation, and could be perfectly protected from other changes of temperature than those which it was intended to measure.

As either of the problems proposed above would occupy the time of a telescope for at least one year, it is obvious that there could be no difficulty in keeping such an instrument occupied indefinitely.

The horizontal monnting is especially adapted to an elevated position, and would permit the use of a telescope where an equatorial mounting would be quite impracticable. On the other hand, to an amateur, or for purposes of instruction, an instrument which could be set quickly from one object to another, and where the observers need not be exposed to the cold, would offer many advantages. The impossibility of observing far from the meridian would be less important with a large instrument, where the number of objects to select from is very great.

There are certain purposes to which this mounting could not be advantageously applied. The study of close double stars and other objects requiring long examination and very perfect definition could be better left to other instruments. The sun, moon, and planets can also generally be better observed off the meridian. If, however, the entire time of an instrument can be employed to advantage, and it can collect several times as much material as an instrument of the usual form, it is no evidence against its trial that there are certain problems to which it cannot be advantageously applied.

The working force required for such an instrument should consist of at least one observer, an assistant to record, and a number of copyists and computers to prepare the working lists, reduce the observations, prepare them for the press, and read and check the proof-sheets. A large volume of valuable observations could thus be produced every year, which would require at least double the time and money to produce by the same telescope mounted equatorially. The difference in the amount of work will be evident when we compare the number of objects observed with a transit instrument per night, with those observed with an equatorial. A hundred objects in various declinations might be examined in a single evening, while it is seldom that the same number could be identified and measured by an equatorial in a week.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

Tests afforded by the Stars

WE will now see how the views which have been put forward are borne out by the facts which are presented to us by the stars. There is no need to occupy much time, in fact reference need only be made to Dr. Huggins' paper which was communicated to the Royal Society in the course of last year, and with that paper we may compare some earlier writings. It was as early as 1864 that Dr. Huggins, who was then associated with the late Dr. Miller, called attention to the intensely strong lines of hydrogen visible in the hottest stars.[2] In this paper they pointed out at the same time that other metallic lines associated with those lines of hydrogen were thin and faint. It has been already mentioned that, as we have independent evidence that these stars are hotter than our sun, we had strong grounds for believing that here we were in presence of a result brought about by a higher temperature, associated as it was with a simpler spectrum, and, therefore, presumably with simpler constituents. We need not stop now to discuss the objection which has been put forward by an ingenious person ignorant of the facts, that the broadening of these lines may not be due to an increase of temperature at all, but really to a very rapid equatorial rotation of the star. This is a fair sample of one of the classes of objections one has to meet. Of course it is at once put out of court by the fact, also stated by Dr. Huggins, that, associated with the thick lines, are excessively thin lines. Any enormous equatorial velocity of the star should have made all the lines thick, but this is not the fact. Now we have only two lines in the solar spectrum at all comparable in thickness with these hydrogen lines in the hottest stars, taking Sirius and α Lyræ as types.

In a paper communicated to the Royal Society in 1876[1] it was remarked that laboratory work indicated the possibility that line-spectra might, after all, really not result from the vibration of similar molecules; and at that time the evidence seemed to be so clear in the case of calcium that it was pointed out that the time had arrived when evidence touching calcium itself ought, if possible, to be obtained from the stars by means, of course, of photography, because the part of the spectrum in question—the region of H and K—is exceedingly faint in the case of the stars.

Why, it may be asked, was it important to get this evidence from the stars? I will read an extract from a book,[2] published some years ago, which puts this view forth:—"It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms, those that give us line-spectra, are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and, therefore, no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other similar atoms at all lower temperatures. Hence the association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end."

That was one point.

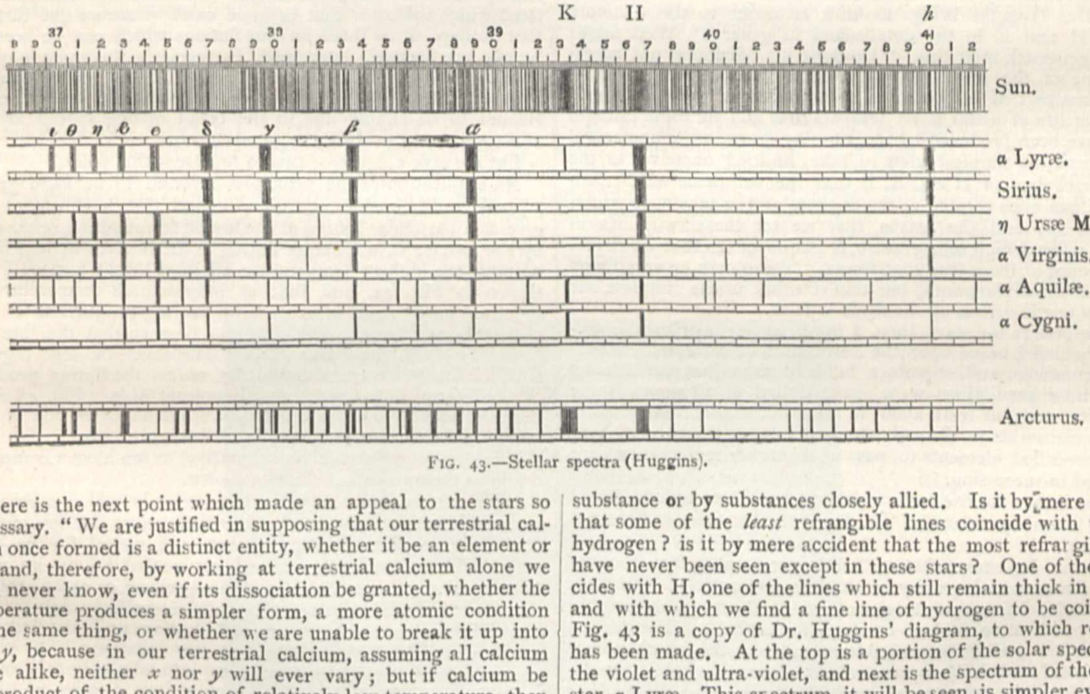


FIG. 43.—Stellar spectra (Huggins).

Here is the next point which made an appeal to the stars so necessary. "We are justified in supposing that our terrestrial calcium once formed is a distinct entity, whether it be an element or not, and, therefore, by working at terrestrial calcium alone we shall never know, even if its dissociation be granted, whether the temperature produces a simpler form, a more atomic condition of the same thing, or whether we are unable to break it up into $x + y$, because in our terrestrial calcium, assuming all calcium to be alike, neither x nor y will ever vary; but if calcium be the product of the condition of relatively low temperature, then in stars hot enough to enable its constituents to exist uncompounded we may expect these constituents to vary in quantity; there may be more x in one star, and more y in another. If this be so, then the H and K lines would vary in thickness, and the extremest limit of variation will be that we shall have only H represented, or x in one star, or only K represented or y in another, and intermediately between those extreme conditions we have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others."

What, then, are the results of this appeal to the stars which Dr. Huggins has made with such splendid success? We have in the hottest stars a spectrum so regular, so rhythmic, that it seems impossible not to consider it as produced either by the same substance or by substances closely allied. Is it by mere accident that some of the *least* refrangible lines coincide with those of hydrogen? is it by mere accident that the most refrangible lines have never been seen except in these stars? One of them coincides with H, one of the lines which still remain thick in the sun, and with which we find a fine line of hydrogen to be coincident. Fig. 43 is a copy of Dr. Huggins' diagram, to which reference has been made. At the top is a portion of the solar spectrum in the violet and ultra-violet, and next is the spectrum of the hottest star, α Lyræ. This spectrum, it will be seen, is simpler even than the spectrum of the solar prominences, and not only is there this wonderful simplicity, but note the exquisite rhythm by which the distance between the lines gradually increases as we go from one end of the spectrum to the other. Note also that the least refrangible line shown on the diagram is coincident with *h* in the violet part of the solar spectrum, and that the next line is coincident with the line H, to which reference has been made in the notes I have read. Note also the relative intensities of the lines H and K in the sun, in which their intensities are about equal, and in η Ursæ Majoris, in which K is altogether absent. These are the first points in this diagram to which attention must be drawn. There will be other points as we proceed further.

But in descending from the general to the particular Dr.

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 370.

[2] "On the Spectra of some of the Fixed Stars" (*Proc.* Roy. Soc., 1864, p. 242).

[1] "Preliminary Note on the Compound Nature of the Line Spectra of Elementary Bodies" (*Proc.* Roy. Soc., No. 168, 1876).

[2] "Studies in Spectrum Analysis," p. 196.

Huggins writes:[1]—"The spectrum of Vega may be taken conveniently as typical of the whole class of white stars, so that the distinctive features of the other stars of this class may be regarded as modifications or departures from this common typical form." He then adds: "There are principally three directions in which changes take place"; one of these consists "in the presence or absence of K, and if present, in its breadth and intensity relative to H." He goes on, "One of these modifications, which possess great suggestiveness, consists of the absence, or difference of character presented by the line K. In all the stars of this class this line is either absent or is very thin as compared with its appearance in the solar spectrum, at the same time that H remains very broad and intense. In the spectrum of Arcturus, a star which belongs to another class which includes our sun, this line K has passed beyond the condition in which it occurs in the solar spectrum, and even exceeds the solar K in breadth and intensity." Arcturus is given in the lower part of the diagram, and it will be seen that there K is relatively thicker than H; and also that with this relative increase in the thickness of K we get a considerable complexity of spectrum, very much more approaching the solar spectrum in the number of lines that we have to contend with. But at the same time I should point out that the positions of these lines vary from the positions of lines in the solar spectrum. "The spectra of these stars," Dr. Huggins continues, "may therefore be arranged in a continuous series, in which first we find this line to be absent. Then it appears as an exceedingly thin line. We then pass to another stage in which it is distinct and defined at the edges; in the solar spectrum it becomes broad and winged, and lastly in Arcturus there is further progress in the same direction, and the line, now a broad band, exceeds in intensity H." Absolute continuity we see in the story which Dr. Huggins brings us with reference to this concrete case of H and K in the details and in general. Well might Dr. Huggins ask after this: "Do these modifications not represent some of the stages through which our sun has passed?" In another part of his paper he uses the term "life changes." Now the life of a star is its temperature, and all these changes must have been produced by the running down of temperature, and I think the simplest view to take, limiting ourselves to the concrete change of H and K, is that the substance which produces K has been formed at the expense of the substance which produced H, and the reason that we see these two lines in calcium when a high temperature is employed is that we reveal the presence of these true root-forms. There may be very many more difficult explanations, but that I think is the simplest one to which one is driven by the logic of facts.

The appeal to the stars, then, I think, amply justifies the prediction which I based upon the comparison of solar with terrestrial phenomena, and, therefore, helps to show that the basis on which those predictions were founded had at all events some little glimmering of truth about it. I think also that it increases the dissociation stages through which we must assume the vapours of our so-called elements to pass when higher temperatures are employed in succession.

So much then for the tests which we have been able to apply to these views by means of Dr. Huggins' remarkably beautiful researches.

The wide departure of stars hotter presumably than the sun (taking the centre of gravity of the absorption, so to speak, as the indication of temperature) from the solar type shows that there is much more work to be done in this field. The success of my former prediction emboldens me to make another one. *It will in all probability be found that the remaining thick lines in stars of the Sirius type are represented in many cases by the lines brightened in solar prominences.*

Tests afforded by the Phenomena of Fluted Spectra

So far we have dealt with line spectra, but we must not limit ourselves to a consideration of this class of spectra if we wish to test this view to the very bottom, as it is our bounden duty to do. We have therefore to ask ourselves the question with reference to other regions of spectrum analysis beyond that particular part which we have been discussing: Is the evidence to be got from those other regions the same? Does it tend in the same direction as the evidence which has been supplied from the consideration of the highest possible temperature in our coils and in the sun? I have no hesitation in saying that, so far as I know, the evidence is absolutely strengthened by a consideration of the low temperature phenomena observed spectroscopically.

[1] *Phil. Trans.*, 1880.

In fact the view was started very many years ago by observations at much lower temperatures than those we have been considering. Plücker and Hittorf, who worked at spectrum analysis before Kirchhoff and Bunsen, were bound to acknowledge that some of the substances with which they dealt had really two distinct spectra, which they called spectra of the first order, and spectra of the second order, which spectra changed as the temperature they employed changed; and although they came to the conclusion that these simply represented allotropic conditions, not molecular dissociation, I think when one comes to inquire into the subject thoroughly, one will find there cannot be any very great difference between those two considerations. In fact the question of double spectra, which has been fought for many years, but which I think is now nearly at rest, was started by the observation of Plücker and Hittorf. Of course the view they put forward was objected to very strongly, and was met by the assertion that they were misled by impurities in the substances which they experimented on. For instance, they found a second spectrum for hydrogen; this second spectrum, which had a very special character of its own, was referred to acetylene. Soon after, a part of the carbon spectrum which was entirely different from the second spectrum of hydrogen, was referred to acetylene. So that those gentlemen who saw in these phenomena nothing but impurities were perfectly content to give an explanation which would be quite right, provided hydrogen and carbon could only be supposed to have one spectrum; the *impurity* acetylene having two. Later work has shown that it is too *coarse* a view to think that the fluted spectra which represent the spectra of the first order of Plücker really represent the vibration of one molecule in the same way that the line-spectra were supposed to do. Evidence has been accumulated which indicates that in some cases where we get three or four flutings, those three or four flutings which can be seen one by one are inversely intensified, in precisely the same way that various lines can be seen one by one, or almost one by one, and inversely intensified. It seems as if even flutings cannot be considered to be simply due to the result of one special kind of allotropism, but probably represent several.

Let me give a figure or two to represent this point.

An application of the principles referred to in Figs. 32 and 33 will readily enable us to understand that a substance may give us a particular fluting at the lowest temperature, represented by the furnace C, a different fluting in the furnace B, and finally a line at the highest temperature afforded by the furnace A, as shown in Fig. 44, and that at intermediate temperatures its spectrum may consist of mixtures in varying proportions of each of these constituents, and it will also be seen that the line produced by the highest temperature can never be seen together with the lowest temperature fluting, unless the fluting produced by the intermediate temperature is present also. Fig. 45 shows the facts actually observed when the spectrum of carbon is photographed under various conditions of temperature.

The results are strikingly suggestive, as we have a compound origin to the two sets of flutings shown.

But there is a lower region yet, a region in which much work has been done which seems to show that before the substance is fit to give us flutings, that it can still record for us—in a very feeble sort of way—it can yet record for us its vibrations by absorption at one end of the spectrum or at the other, so that the story of simplification is really intensified when we leave the high temperature spectrum, and it seems as if the first effect produced by the action of heat on any substance is to give us general absorption which breaks up into absorption in the red and absorption in the blue, and then we get a series of flutings more or less complex according to the temperature of the body; and then when we have passed from this stage we get the series of line spectra to which I have drawn attention. Again, in passing from a low temperaure to a high temperature, so far as I can see there is absolutely no break, nor is there any difference of kind that we are acquainted with in the passage from a compound body and the passage from a known form of, let us say, gold or silver, at a low temperature, to that same substance at a higher temperature.

If we assume that these various spectra are really due to different molecular aggregations, we shall have the following series, going from the more simple to the more complex:—

First stages of complexity of molecule	Line-spectra.
Second stages...	Channelled space or fluted spectra.

Third stage	Continuous absorption at the blue end not reaching to the less refrangible end. (This absorption may break up into channelled spaces.)
Fourth stage	Continuous absorption at the red end not reaching to the more refrangible end. (This absorption may break up into channelled spaces.)
Fifth stage	Unique continuous absorption.

So that the story is one of absolute conformity, absolute continuity from one end of the series to the other; but on this subject I need not say more, because my friend Capt. Abney will have a great deal to say about the red molecules and blue molecules when he comes to deal with the red end of the spectrum, and I may safely leave this part of the subject in the hands of one who has so brilliantly distinguished himself by his investigations upon it.

Replies to Objections

Now I think it is time that I should reply, or attempt to reply, to some objections that have been made to these views. So far as I can gather, the serious objections which have been made are not many, but some of them are objections to which considerable value should be attached. The chief one now urged is that one is misled in the conclusions that one draws from these observations of spots and storms by the fact that the solar lines corresponding with the lines which we consider to be common to two substances are really double, and that the lines common to two substances appear common simply because we have not sufficient dispersion to separate them. Now that is a very important objection indeed, but let us examine it. It has been pointed out that of the 62 iron lines which remained as the result of the purification of the first part of the map (between wave-lengths 39 and 40) only 18 were left; all the rest being found common, not only to two substances, but in a great many cases to four or six substances, and we found also that our rough observation-book, as we went on, suggested that the solar line was double; but if we had gone on in that way we should not have been able to produce a map at all, because there would have been few lines which were not complex, so that it would have been a piece of cowardice to remain there and not attempt to find out what it meant. Now let us suppose a great many of the solar lines are double. It is fair to assume that these double lines would be irregularly distributed throughout the spectrum. We cannot imagine some spiteful freak of nature choosing out to be double a particular set of solar lines which some one should eventually find common to two substances; we must imagine an impartial distribution of double lines if we are to consider them as double. Now the argument we can bring against that is founded on this, that two things always hang together, the alleged complex nature of the line and the fact that this particular line is picked out for special prominence in spots and storms. For instance, take the line 4923·2 referred to in Fig. 41. If that line is double, and is one of two lines seen in flames, the probability that that line should be double, supposing that the solar line is double, would be as 2 to 1, but that line is picked out as 50 to 1 in the case of prominences. The betting in favour of the coincidence is not so great in the case of the spots, but when we come to the prominences, when we are dealing with 100 lines with the probability therefore of 50 double lines if they are equally distributed, and that every other line is double—when we come from 100 lines to 1, it is 50 to 1 against this particular line being double; and yet this is the particular line which we always find to be common to two substances when we really discuss the observations of the flames. I think then that the idea that these basic lines are simple creatures of the imagination, simply chance coincidences, will really not stand at all.

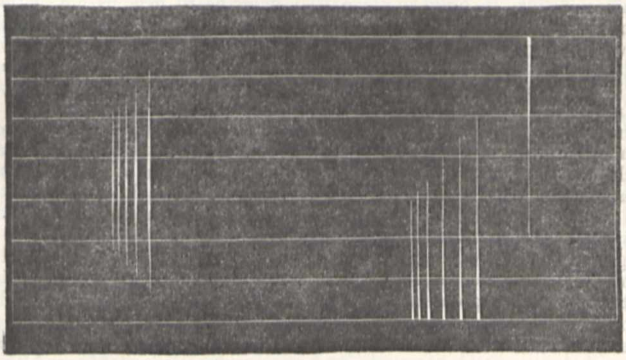


FIG. 44.—Diagram showing the action of three different temperatures on a hypothetical substance assuming three stages of complete dissociation; and also of intermediate temperatures at which the vapours are only partially dissociated.

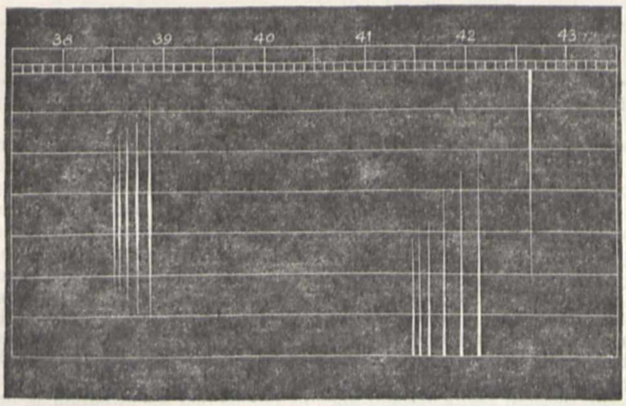


FIG. 45.—The photographed spectra of some carbon compounds.

Prof. Young has lately brought forward this objection, although in 1872 he was the first to point out the very extraordinary fact as it appeared to him, and as it still appears to every one else, that an enormous number of coincident lines which he got from the tables extant at that time seemed to cluster round the bright lines seen in his observations. The credit of that is undoubtedly due to Prof. Young, and although he has lately seen cause to withdraw somewhat from this first view of his, he is still driven to think that two of the most important lines in the solar spectrum, H and K, are really due to two substances—hydrogen and calcium. There is one more thing which must be said with special reference to this: the work is not to be negatived by a mere assertion that the line is double; it must in a great many cases be shown that it is a double-double or a treble-treble line. For instance, take the case of *b*4, which may very well be double for aught we know; that line is coincident, so far as we can make out with the means at our command, with magnesium, iron, nickel, uranium, chromium, and cerium. It will not do to limit oneself to the statement that these lines are double—they must really be perfect families of lines in order to prevent this explanation being, as I think it is, the more probable.

Another objection, again, a very important objection, is of a somewhat different nature, and may shortly be called the bell hypothesis. It is to the effect that these molecules, or atoms, are very extraordinary things indeed, as we can well imagine them to be, and that the spectrum which they produce depends entirely upon the manner in which they are struck; so that in fact it would seem at first useless to construct a map of any spectrum at all, for fear the substances we wish to observe in our laboratories should be struck in different ways and should render the map perfectly useless. I say the idea is really that the molecules struck differently would give us different spectra. Now if the difference were only slight, that would not much matter; it would be very difficult to withstand that hypothesis. But it must be remembered that in this work we are dealing with this extraordinary fact, that over the region which we have been specially studying there are no lines of iron common to spots and flames; so that if we had not any iron at all to experiment with we should be perfectly justified in asserting the iron lines in flames were produced by one substance and those in spots by another, because no two lines agree in *these* two spectra. The spectra are as distinct as the spectrum of magnesium and the spectrum of iodine, or any other two bodies. Now if the bell hypothesis is to explain that, it explains too much, because if it is true of any two bodies, it must be true of all bodies, and therefore all spectra are the result of the same thing struck differently, and spectrum analysis would then cease to be spectrum analysis, for it would simply record changes rung on the same molecule by the various methods of striking. Then again there is another thing to be said, that no statement of this bell hypothesis which I have heard gets us out of the difficulty that we are sinning against the law of continuity in advancing it, for the reason that if you begin with a known compound body, let us say, a salt of calcium, the change from a salt of calcium to calcium is the same in kind and about the same in degree as the change from one form of calcium to the other, if we can talk of different forms of calcium on the mere strength of spectroscopic work. I mean that there are more important changes to be got out of the observations of the metal calcium than there are to be got by passing from a salt of calcium to calcium; so that if the bell-hypothesis proves anything it proves that a compound body is a simple one.

It will be seen that the special import of these considerations lies in the question of the short lines; leaving all considerations dependent on long lines, by which the presence of impurities may be recognised, out of consideration altogether. But it may be further said that a method of purifying spectra and eliminating any spectroscopic defects which were due to the presence of

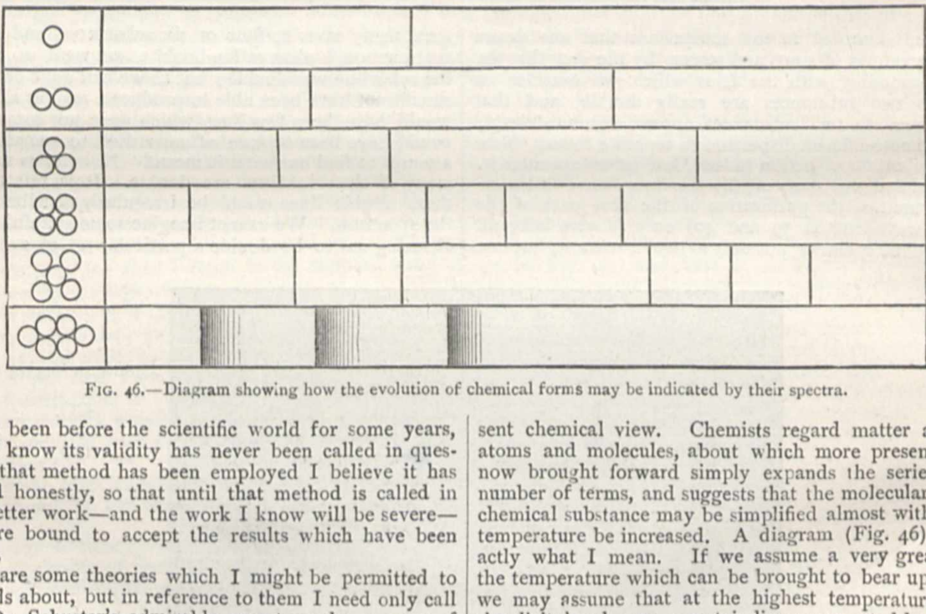


FIG. 46.—Diagram showing how the evolution of chemical forms may be indicated by their spectra.

impurities has been before the scientific world for some years, and so far as I know its validity has never been called in question. Where that method has been employed I believe it has been employed honestly, so that until that method is called in question by better work—and the work I know will be severe—I think we are bound to accept the results which have been obtained by it.

Then there are some theories which I might be permitted to say a few words about, but in reference to them I need only call attention to Dr. Schuster's admirable report on the progress of spectrum analysis, given to the British Association at the meeting at Swansea. It will be seen from that report that none of the theories which have been put forward really account for all the facts observed. It is shown that phenomena have been recorded which are not to be explained away by the bell theory or any other. Such a phenomenon, and a perfectly distinct one, is the change due to the thickness in the vapour. Changes also due to varying temperature and other causes have been seen, for which the theories in question do not account. Now changes in temperature may probably affect large reaches of the spectrum, but in the cases we have studied we got most diverse effects in lines so close together in the spectrum that it requires a considerable amount of dispersion to find out that they are really not single lines.

The New Theory of Chemical Evolution

What then is the view of the evolution of chemical species to which we are led by our study of the sun and stars? I think that after all it is but a slight expansion of the present chemical view. Chemists regard matter as composed of atoms and molecules, about which more presently. The view now brought forward simply expands the series into a larger number of terms, and suggests that the molecular grouping of a chemical substance may be simplified almost without limit if the temperature be increased. A diagram (Fig. 46) will show exactly what I mean. If we assume a very great difference in the temperature which can be brought to bear upon a substance we may assume that at the highest temperature we have, for simplicity's sake say, a certain line represented by a single circle; let us imagine the temperature reduced, we shall then get another spectrum, which we can represent by a double circle, if we like to assume that the evolution is one which proceeds by constant additions of the original unit. Coming lower down, we get another substance formed with a more complex spectrum represented by three circles; lower down still we have one represented by four circles, another by five, another by six, and so on. We might take another supposition, easier perhaps to some minds, and suppose that evolution proceeded, not by the addition of the initial unit, but by the constant doubling of the substance of the molecule itself. Instead, therefore, of our circles increasing by one, we shall have one, two, four, eight, sixteen, thirty-two, and it will be readily understood that if there are a considerable number of stages of temperature, both within our ken and beyond our ken, and if some substances form themselves perpetually by doubling, then the unit with which we can experiment at low temperature, call it the chemical atom or the chemical molecule, or what you will, must be a very complex thing indeed. If the lower spectrum represents that of a complex body such as iron, or a salt of calcium, the upper spectra will represent those due to the finer groupings brought about by higher temperatures. We pass continuously, as in the sun and the stars, from complexity to simplicity, if we begin at the lower stages, and from simplicity to complexity if we begin at the higher stages of temperature.

Now, two questions arise here which I think it is important to discuss. Are we playing fast and loose, in such an hypothesis as this, with the ordinary course of nature's operations, or are we in harmony with her? Again, is it contrary to the view expressed by the greatest minds which have studied chemical phenomena? I think really the view is not inharmonious with those other views which we have gathered from other regions of thought and work; in fact, I think it derives its whole force from the fact that along many lines it runs parallel with the evolutionary processes in the different kingdoms of nature. I have another diagram which will show what I mean (Table I.). This diagram deals with a very simple case of evolution, and it deals with this evolutionary process, going along a single line. Of course we know very well that in the organic kingdom evolution always proceeds along many lines, but to simplify the problem I have dealt with one of the simplest that I can think of. Let us assume that in a certain hottest star there shall be two substances, which we will call *a* and *b*. They will first at the transcendental temperature which I assume, exist as separate entities; the temperature being then reduced, they probably will combine, and, instead of two atoms, *a* and *b*, we shall have one group of $a + b$. If the temperature is still further reduced, we shall get *b* combining with *b*; in that case we shall have a grouping consisting of $a + 2b$. Let the same operation be performed again, we shall then have $a + 4b$, combining into two groups of 2; we shall have what we can represent, in short, in chemical language ab_4. Now, having got our ab_2, having got our temperature reduced, let us assume that ab_2 is now the substance linked on to give a greater complexity, instead of *b* or $2b$ merely. We then have this series given in the table.

TABLE I.

$a \quad b$
$a+b$
$a+bb$
$a+(bb)(bb)$
$= \ldots ab_4$
$+ab_2 = \ldots\ldots a_2b_6$
$+ab_2 = \ldots\ldots a_3b_8$
$+ab_2 = \ldots\ldots a_4b_{10}$
$+ab_2 = \ldots\ldots a_5b_{12}$
$+ab_2 = \ldots\ldots a_6b_{14}$
$+ab_2 = \ldots\ldots a_7b_{16}$
$+ab_2 = \ldots\ldots a_8b_{18}$
$+ab_2 = \ldots\ldots a_9b_{20}$
$+ab_2 = \ldots\ldots a_{10}b_{22}$
$+ab_2 = \ldots\ldots a_{11}b_{24}$
$+ab_2 = \ldots\ldots a_{12}b_{26}$
$+ab_2 = \ldots\ldots a_{13}b_{28}$
$+ab_2 = \ldots\ldots a_{14}b_{30}$
$+ab_2 = \ldots\ldots a_{15}b_{32}$
$+ab_2 = \ldots\ldots a_{16}b_{34}$

Now, that is an ideal scale. The question is, Is it absurd? How can we honestly answer that question? By asking whether we are or are not on the lines on which nature works in the region of the known, in the region which we can get at?

TABLE II.

C H
C+H
C+HH
C+(HH)(HH)
$= \ldots CH_4$
$+CH_2 = \ldots\ldots C_2H_6$
$+CH_2 = \ldots\ldots C_3H_8$
$+CH_2 = \ldots\ldots C_4H_{10}$
$+CH_2 = \ldots\ldots C_5H_{12}$
$+CH_2 = \ldots\ldots C_6H_{14}$
$+CH_2 = \ldots\ldots C_7H_{16}$
$+CH_2 = \ldots\ldots C_8H_{18}$
$+CH_2 = \ldots\ldots C_9H_{20}$
$+CH_2 = \ldots\ldots C_{10}H_{22}$
$+CH_2 = \ldots\ldots C_{11}H_{24}$
$+CH_2 = \ldots\ldots C_{12}H_{26}$
$+CH_2 = \ldots\ldots C_{13}H_{28}$
$+CH_2 = \ldots\ldots C_{11}H_{30}$
$+CH_2 = \ldots\ldots C_{15}H_{32}$
$+CH_2 = \ldots\ldots C_{16}H_{34}$

We will now refer to another diagram; we will pass from the ideal to the concrete, and it will be seen that there is, if one can invert the term in such a way, a distinct precedent for such a table as the last; for here are the absolute facts with regard to one evolutionary series of the combination of carbon and hydrogen. We have the gases CH_4, C_2H_6, C_3H_8; we have as liquids from C_4H_{10} to $C_{15}H_{32}$; each of them formed, not by the addition of my hypothetical ab_2, but by a concrete CH_2, and we have connected with that a beautiful order and exquisite regularity in the way in which the boiling-points and specific gravities of these things increase.

TABLE III.—*Hydrocarbon Series*

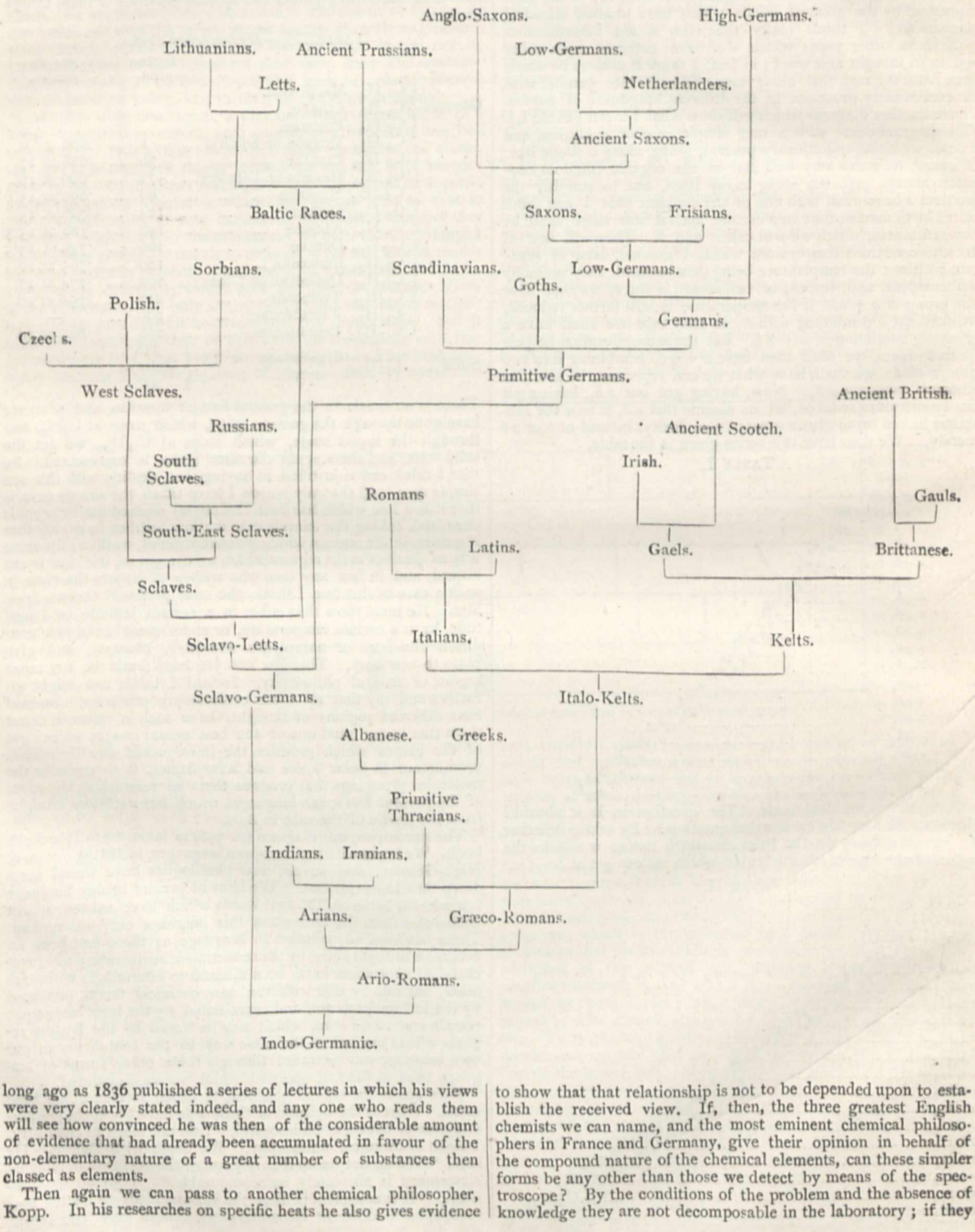
			Boiling point.	Specific gravity.
Gaseous	CH_4	Marsh Gas.		
	C_2H_6	Ethane.		
	C_3H_8	Propane.		
Liquid	C_4H_{10}	Tetrane or Diethyl ...	1°	·600 at 0°
	C_5H_{12}	Pentane	38	·628 at 17
	C_6H_{14}	Hexane or Dipropyl..	71	·669 at 16
	C_7H_{16}	Heptane	99	·699 at 15
	C_8H_{18}	Octane	124	·726 at 15
	C_9H_{20}	Nonane	148	·728 at 13·5
	$C_{10}H_{22}$	Decane	166	·739 at 13·5
	$C_{11}H_{24}$	Endecane	180	·765 at 16
	$C_{12}H_{26}$	Dodecane or Dihexyl.	202	·774 at 17
	$C_{13}H_{28}$	Tridecane	218	·792 at 20
	$C_{14}H_{30}$	Tetradecane	230	—
	$C_{15}H_{32}$	Pentadecane	258	·825 at 16
Solid ...	$C_{16}H_{34}$	Hexdecane or Dioctyl	278 Melts at 21°	

There is no break in the general line of increase, and after we have gone through the gaseous stage, which stops at C_3H_8, and through the liquid stage, which stops at $C_{15}H_{32}$, we get the solid state, and there again the same series is represented. So that I think one is justified in saying that, dealing with this one simple case (and the only reason I have taken the simple case is that it is a line which has been thoroughly worked out by organic chemists), taking this simple case we are justified in saying that if nature, in the regions which we cannot get at, works in the same way as she does in the regions which we can get at, the view is not absurd, and in fact any one who wishes to dispute the view in such a case as this has, I think, the *onus probandi* thrown upon him. He must show that either in a certain latitude or longitude, or at a certain temperature, or under some unknown condition the laws of nature are absolutely changed, and give place to new ones. That has not yet been found in any other region of natural philosophy. Indeed I think one might go further and say that all these evolutionary processes, obtained from different regions of thought, have such a oneness about them that to my mind one of the best mental images we can get of the causes which produce the lines picked out for special prominence in solar spots and solar flames, is to consider the molecular groupings that produce them as resembling the roots of the present European languages which our ancestors brought from the cradle of the race in Asia.

The accompanying diagram (p. 396) is taken from Haeckel's book. We begin with the European languages, including our own, High-German, and so on, and philologists have traced them down into Indo-German. We have at present in our language few and far between, the root-words which have existed almost unchanged from the time when this language only was spoken. There has been an evolution in language as there has been an evolution brought about by the reduction of temperature wherever chemical substances have been allowed to intermingle with each other, but side by side with the new chemical forms produced by the low temperature, and represented by the later languages, remain true root-forms, which may be traced to the hottest regions within our ken in the same way as the root-forms in our own language can be traced through these other forms of language back to the first one with which we are acquainted.

Now comes the second question, to which reference has been made. What is the opinion of those who have given the greatest attention to chemical philosophy? I do not mean to chemistry, I mean to chemical philosophy. We begin with Dalton. He says, "We do not know that any one of the bodies denominated elementary is absolutely indecomposable." I go on to Graham: "It is conceivable that the various kinds of matter now recognised in different elementary substances may possess one and the same element or atomic molecule existing in different conditions of movability. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies." I now come to Sir Benjamin Brodie, whom we have so lately lost, the last of the great English triumvirate to whom I mean to refer. His views I have already stated in his own words, but I may here again state that as early as 1867, almost, we may say, before the spectroscope had been applied to the sun, except in the general way, which was started by Fraunhofer and Kirchhoff, he prophesied that the solar facts would be as I think we have found them. That is to say, he stated his belief that at the solar temperature the constituents of our elementary bodies would be found existing in independent forms. The greatest chemical philosopher now living, M. Dumas, so

PEDIGREE OF THE INDO-GERMANIC LANGUAGES.



long ago as 1836 published a series of lectures in which his views were very clearly stated indeed, and any one who reads them will see how convinced he was then of the considerable amount of evidence that had already been accumulated in favour of the non-elementary nature of a great number of substances then classed as elements.

Then again we can pass to another chemical philosopher, Kopp. In his researches on specific heats he also gives evidence to show that that relationship is not to be depended upon to establish the received view. If, then, the three greatest English chemists we can name, and the most eminent chemical philosophers in France and Germany, give their opinion in behalf of the compound nature of the chemical elements, can these simpler forms be any other than those we detect by means of the spectroscope? By the conditions of the problem and the absence of knowledge they are not decomposable in the laboratory; if they were they would cease to be elementary bodies at once, and would be wiped out of our tables. Nor do I think it possible that in the present stage of our knowledge they can be revealed to us in any other way than by the spectroscope. It is unfortunate that none of these chemists who have given us this view have helped us by showing in what way the possibility, which all of them suggest, and which many of them intensely believe in, could be absolutely demonstrated, but it is obvious that if dissociation is the thing which time out of mind has made compound bodies simpler, in their minds the condition of higher temperature must have been present. The only difficulty was the way in which the effects of that high temperature could be measured and weighed, and I think that if the spectroscope had been introduced earlier they would probably have left some hints behind them which would have been of the greatest value to those who work with that instrument.

Passing from the chemists to the physicists, there is one, at all events, who has appreciated exactly how this decomposibility of the terrestrial elements could be established. I refer to the lamented Clerk Maxwell. In his article on atoms in the "Encyclopædia Britannica," he says: "The discovery of a particular line in a celestial spectrum which does not coincide with any line in a terrestrial spectrum indicates either that a substance exists in the heavenly body not yet detected by chemistry on earth, or" (and it is to the "or" I wish to draw attention) "that the temperature of the heavenly body is such that some substance undecomposable by our methods is there split up into components unknown to us in their separate states." Absolutely nothing could be clearer than this.

In endeavouring to discuss the bearing of this application of the hypothesis of evolution of chemical forms upon modern chemistry, we must draw a very wide distinction between chemical theory and chemical fact.

When we compare the laws given in average chemical text-books with the laws which lie at the root, let us say, of astronomy, the candid mind cannot fail to be struck by the difficulty which chemists must have encountered in endeavouring to reduce the facts of their science to order on the hypothesis they bring before us. An outsider, for instance, thinks that the basis of chemistry, or a large part of the basis of chemistry at all events, lies in the fact that the chemist has determined the existence of a certain number of elementary bodies, each of these elementary bodies having a certain atomic weight, and that this atomic weight determines all the constants of that body. Yet we read in chemical text-books that this atomic weight is fixed according to no invariable rule; indeed, with Kepler's laws and Newton's laws in one's mind one comes to the conclusion that it is not too much to say that it is determined by a series of compromises. An outsider would think that if any one of these elementary bodies were taken as a standard, the weight of an equal volume of vapour of another substance under equal conditions would bear some relationship of a definite character to the atomic weight. This however is not the case. Again, among the questions to be considered as determining the atomic weights taken, is an assumed limitation of combination power, a so-called atomicity, according to which one substance is a monad, because it will combine with that same relative proportion of hydrogen which exists in half a water-molecule. Another substance is called a dyad, because it will combine with the same relative proportion of hydrogen which exists in a whole water-molecule, and so on. When we thus begin to class the substances into monads, dyads, hexads, and so forth; in fact, when we thus effect a re-classification of elementary bodies, the solidarity at once breaks down; we find that the classification after all is useless, because the same substance may behave as a dyad, a tetrad, a hexad, a pseudo-tryad, a pseudo-octad; in fact, one feels one is dealing with something that is more like a moral than a physical attribute—a sort of expression of free will on the part of the molecules. We are, I think, justified in asking whether these various attempts to formulate a science do not break down after a certain point, because they attempt to give a fixity to what is in truth variable.

When we pass to the *facts* of the science, the key-note of which is variability from one end of the scale to the other, we find that the view of successive dissociations, the view of variable molecular groupings brought about under different conditions, is really more or less in accordance with the facts where the laws based on the fixity of the facts break down entirely. Thus, for instance, let us take the question of vapour densities. The view accounts fully for the so-called anomalous vapour densities, and in this way: it suggests that the elements may really be complex groups which break up into their constituent groups under suitable conditions of temperature, like phosphoric chloride and many other bodies do when obtained in the condition of vapour. We have dissimilar groups in the one case, and possibly similar groups in the other. In this way, that contradiction in terms, the "monatomic molecule," really becomes the evidence of a higher law.

Let us pass to allotropic conditions. The explanation of these is that there are bodies which have a large molecular range within the ordinary temperatures at our command. The substances in which allotropism is most marked are all metalloids which have not been found in the sun, and the allotropic forms give us in many cases different spectra—spectra indicating a considerable complexity of the molecules which produce them—spectra of continuous absorption, continuous radiation in the blue, continuous absorption in the red, fluted spectra, and the like. In the passage from one allotropic condition to the other, energy, without any known exception, is absorbed or given out. What becomes of this energy; what is it doing; unless it is in some way or other controlling the passage from one molecular group to another? These allotropic conditions, occurring very obviously to us in certain limits at our ordinary temperature and pressure are, I hold, but special cases of group-condensation common to all bodies, represented by Dalton's law of multiple proportions. We can indeed imagine a condition of things in which the difference between iron in Fe_6, and the iron in Fe_2Cl_6, would be as obvious as the difference between ordinary and amorphous phosphorus.

In certain classes of so-called organic substances this grouping of simpler groups to more complex actually takes place, and is recognised under the term polymerism—for instance, with cyanogen compounds of oxygen we have a simple thing like CNO say, which will form a series of compounds, and we have its so-called polymers, $C_2N_2O_2$, or $C_3N_3O_3$, which will each form a series of compounds, these groups of simpler nature forming by their combination group individuals with related but not identical properties with the simplest or fundamental group.

In many cases the amount of this condensation may be determined by the vapour densities. In others, again, a dissociation takes place at a certain limit of temperature, a simpler or fundamental group being the resolution product.

The resemblance between these cases of polymerism and especially those elementary bodies which exhibit allotropism, is at least striking.

In the one case, the organic complex bodies, the range of existence is in most cases within our easy attainment; in the so-called elementary stuffs it is less frequently the case. We can certainly convert ordinary phosphorus and sulphur into allotropic and most likely polymeric forms, but we do not know as yet how many atoms more are contained in the polymeric forms of these substances than in their simpler states.

And in other substances this range of condition of formation passes gradually out of our reach, but the phenomena are the same in kind up to the temperature of the sun. And again, when we can obtain the spectra of bodies like amorphous phosphorus we can prophesy that the relative grouping of the atoms of phosphorus in this to the ordinary form will be exhibited.

This brings us to the next point—atomicity. What are the associated phenomena? Lowest melting-point, simplest spectrum, lowest atomicity. Therefore we are justified, I think, in assuming that atomicity may after all be but the measure of the molecular groupings at work. In this way we can associate various atomicities, not with moral phenomena as regards the "*behaviour*" of the same molecule, but with different physical states—different complexities of the same substance. Thus in the same substance the more complex or allotropic the molecular grouping, the higher the atomicity. Hence the substances in which the highest atomiticities appear should, as a rule, be formed and broken up at the lowest temperature. This, I am informed, is really what happens in the majority of cases.

New Analogies between Organic and Inorganic Bodies

I have ventured in these few remarks to touch upon the relations of the new view to modern chemical facts, because I think such a discussion shows us that there are several chemical regions in which the views can be tested from a chemical point of view, although I have, from a set purpose in my lectures, dealt with them absolutely from the physical side. In fact, one such step of the highest interest has already been taken by my colleague, Capt. Abney. I will read what it is, and the language of Prof. Roscoe, the President of the Chemical Society, is so clear and so admirably put, that it is impossible for any one to improve upon it.[1] Referring to the work which Capt. Abney and Col. Festing have done together, he says: This work "is no less than a distinct physical test of the existence in organic compounds, of the organic radicals, and a means of recognising the chemical structure of an organic compound by means of the spectroscope." This result "is accomplished by photographing the absorption spectra of organic compounds in the infra-red part of the spectrum. In these invisible portions characteristic and distinct absorption lines and bands occur for each organic radical. The ethyl compounds all show one special ethyl band; the methyl compounds a special methyl band; and thus, just as a glance at the luminous portion of the spectrum satisfies us of the presence of calcium, lithium, and rhubidium, so a simple inspection of these infra-red photographs enables us to ascertain the presence of the various organic radicals. This invention is still in its infancy, but one of greater importance to chemists has seldom if ever been communicated to the Society." I have been the more anxious to give these results in Prof. Roscoe's own words, because it will be seen that, *mutatis mutandis*, these remarks touching the spectra of organic radicals are precisely the statement I have been endeavouring to make with regard to inorganic radicals. It cannot therefore be said that the nature of the principle I bring forward is one with which chemists are not familiar. In this beautiful work, then, we have an analogy between the behaviour of known compounds and assumed elementary bodies.

A new method of laboratory work which I have recently started may, I think, in the course of time furnish us with another analogy, and in connection with it there is an experiment to which reference may advantageously be made, because it will show what kind of results we expect to get. It is simply referred to as an indication of the probable fruit which will come from many new kinds of experimentation which might be adopted, provided always we bear in view the idea which it has been my duty to bring forward. This experiment is founded on the behaviour of compound bodies when they are distilled at different temperatures. If we take, for instance, a mixture of hydrocarbons, some of them very complex in their nature, and others more simple; when a low temperature is employed it is found that the simpler hydrocarbon comes over in the shape of vapour. If therefore we were fortunate enough to be able to observe the spectra of these different vapours, assume that that series of hydrocarbons, for instance, shown in the accompanying diagram (Fig. 47), had each of them a distinct spectrum, we should be able to follow spectroscopically the effect of each change of temperature, and we could in that way associate the known fact of the greatest density of the vapour which comes over at a higher temperature with a spectrum of a certain kind.

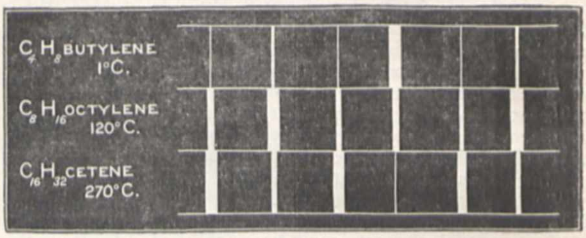


FIG. 47.—Hypothetical spectra obtained on distilling at successively increasing temperatures a mixture of light and heavy hydrocarbons.

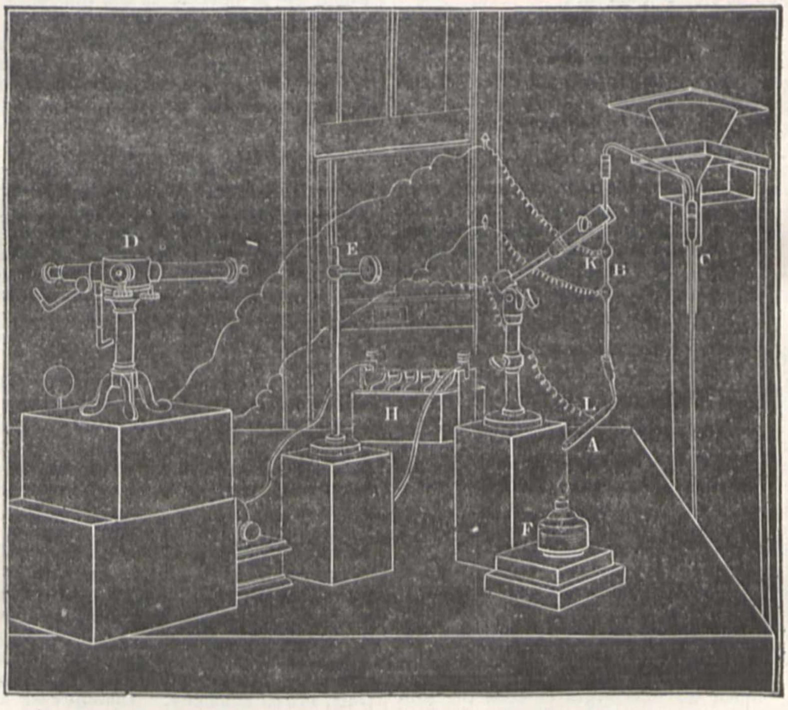


FIG. 48.—Fractional distillation of potassium. A, hard glass tube containing the potassium and connected with a Sprengel pump C by a tube B, having two bulbs with platinum electrodes sealed into them, between which an induced current may be made to pass; F, spirit lamp; H, battery; D, spectroscope; E, lens to focus image of spark on slit of spectroscope.

Now in our experiment we deal not with a compound body in the ordinary sense, but with the so-called elementary body, potassium, which we have in a hard glass tube of the form shown in Fig. 48. By means of a Sprengel pump the tube is very perfectly exhausted, and then gently warmed with a spirit lamp, the exhaustion going on during the whole process. On passing a current between the platinum electrodes we see a beautiful green glow in the tube, and obtain a certain spectrum. On replacing the spirit-lamp by a Bunsen burner we find as the result of this increased temperature that the colour in the tube is blood-red, and the resulting spectrum is entirely different. The spectrum of potassium is one which requires a very great deal of study, for the reason that it varies very much under different experimental conditions. If the

[1] *Journal* of the Chemical Society, May, 1881.

potassium, for instance, is thrown into a Bunsen burner, the chief line that one gets is a red one. Kirchhoff, in the early days of solar chemical investigation, pointed out that this red line is not to be found among the Fraunhofer lines. The flame also gives us a line in the blue. If we examine the spectrum of potassium by means of an induction-coil we find the blue line which we also see in the flame, but it is intensified in the spark. We also see some strong lines in the green and yellow, which are barely visible in the flame—which are in fact not generally recorded in the flame-spectrum of potassium, although they are really visible when considerable dispersion is employed. These lines in the yellow and green I say become prominent lines. Now, it so happens that some of these lines in the green do, it is believed, correspond with Fraunhofer lines, and we are, therefore, justified in assuming that they represent a something, whatever it may be, in the potassium, which can withstand the heat of the sun, while the red lines represent something which

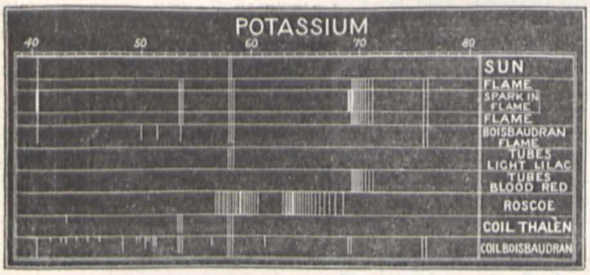


FIG. 49.—Spectra of potassium obtained under different conditions.

is broken up at the temperature of those regions of which we can determine the absorption. The interesting point of the experiment, therefore, is this: assuming for a moment that the red line does represent a complex something which cannot withstand the temperature of the sun, and that the yellow line represents a something finer which can withstand the temperature of the sun, what happens when we try to drive off the vapour of this potassium *at the lowest temperature* at which we can get it to volatilise at all, is that if the experiment is carefully performed it gives precisely those lines which are reversed in the solar spectrum alone, and of that line, which is the strongest line at the temperature of the Bunsen burner we see absolutely nothing at all. Referring to the spectrum which we get in the lilac and yellow-green part of the tube, two out of the three lines visible at all events are seen in the sun, whereas the other lines which we get in the flame and some of them which we get in the coil are not represented in that fine vapour which was produced at the lowest possible temperature.

The Bunsen burner produces some very exquisite colour-effects in the tube, and especially develops a beautiful blood-red colour which might be imagined to be the product of that molecule which gives the red line in the Bunsen burner; but that is not the fact. The line seen in the Bunsen burner is not visible as a rule in the vapour when heated in this way, the lines actually seen being more refrangible. Fig. 49 is a map of the spectrum of potassium under these various conditions. I give it simply as an indication that it is possible when other laboratory and chemical experiments are made with this view in mind that other analogies than those already obtained will be forthcoming.

The experiment then comes to this. If we assume the potassium to be a compound body and that its finer constituent molecules are those which resist the solar temperature, then it behaves exactly like a mixture of hydrocarbons is known to do, that is, the finer vapours come off in greatest quantity at the lowest temperature, and the more complex ones as the temperature is raised.

Conclusion

In concluding my lectures in this course on Solar Physics I would ask attention to the fact that the views which I have ventured to put forward, as being what I honestly believe to be the true outcome of the twenty years' work which has been applied to this subject, depend for their strength upon the convergence of very various lines of thought and work. No doubt the future progress of science will show that we, after all, are looking through a glass darkly, and that we are not yet face to face with the truth, and the whole truth. We must all of us be content to have our work criticised and expanded by future work, by researches carried on with greater skill, with more elaborate methods and higher views. But with all these reservations I do wish to draw attention to the fact that the convergence of many lines of work and many lines of thought suggest the ideas which I have put forward. Depend upon it, that we shall get a much higher and much richer truth out of further inquiries; and I quite acknowledge, although I have had a hand in the work myself, that the outcome of the work is so important that it ought to be considered honestly and carefully from every point of view. Still I consider that I am in honour bound to say, as the result of the work on solar physics, in that small branch of the inquiry into solar matters with which I am more personally connected, that my belief is that the late work has changed the views which were held say twenty years ago to this extent: whereas twenty years ago we imagined ourselves to be in full presence in the sun of chemical forms with which we are familiar here, I think in this present year we are bound to consider that that view may be modified to a certain extent, and that we are justified in holding the view that, not these chemical forms, with which we are acquainted here, but their germs really, are revealed to us in the hottest regions of the sun. J. NORMAN LOCKYER

NOTES FROM THE MALAY ARCHIPELAGO

A CORRESPONDENT in Java sends us the following:—

In 1879 I saw, at Tabu Breio, Padang Panjang, west coast of Sumatra, a child aged about one and a half years, with four legs. It was a female child with perfect organs, only the feet were clubbed and the legs bent. The added-on pair of legs were less perfect and their circulation evidently not in order, for they were not so sensitive to pain (pinches, &c.). They looked as if part of an embryo male child. The child was subject to fits; it could not walk, but crawled, using its female legs, the male (?) legs being dragged along. The spine was much dragged out of position.

During about six months of 1880 there was a child at Surabaya, Java, with two distinct heads joined to one neck. It is now with the Regent (a Javanese) of Surabaya, in spirits. Photographs of this are sold. The brains were quite independent of each other, for the one would sleep whilst the other was awake. I have not heard whether the one could articulate whilst the other slept.

Bornean Rhinoceros.—Mr. Bartlett writes to me: "We now know for certain that the Bornean is the same as the Sumatran. This comes of course from Hart Everett, and I do not doubt it for a moment." But I have strong grounds for believing that there are two kinds:—1. A Government official who recently spent a year in the deepest recesses of the island says the natives told him there were two kinds. 2. About eight years ago a small rhinoceros was killed at Bunut, about 150 miles above Sintang, on the Pontianak. This is certain, it had only one horn. I have recently spoken to an officer who spent a year and a half in the interior, and he says he always understood the animal had only one horn. Anyhow it is very rare indeed. No European I have met—and many have been a long way into Borneo—has seen it. That may be because they are phlegmatic Dutch, and not inquiring English. But the natives who killed the one at Bunut had never seen one before. At the first sight they fled in terror at such a beast. It might have been a young *R. Sum.*, as the horn was very small, and perhaps the trifling development of bud horn escaped notice.

A Dutch ship, the *Batavia*, has at length reached the point where the 141st degree cuts the west coast of New Guinea. This is considered a great feat; why, I can't precisely say. There has been a good deal of talk about sending explorers to the Dutch end of New Guinea, but directly money is asked for silence reigns. They had much better finish with Sumatra before going to New Guinea.

The cattle plague has been raging in the west end of Java, Bantam, the Preanger, and Batavia residences—during the west monsoon (now finishing) with redoubled vigour. It has now abated a little (after four years it may well do so, from want of victims) in these parts, but is extending eastward, its appearance in Krawang being the most alarming. The authorities have decided upon making a double fence right across Java at its narrowest part. This means a line from somewhere about Cheribon due south. In the interval—a considerable one—

between the two fences, no cattle will be allowed to pass or exist.

There is a bird (native name Jallak) which follows the buffaloes about and perches on their backs. Query, can this bird have anything to do with the spreading of the plague? If so I don't see what Government can do. They can't fence him out.

In all the parts where the cattle-plague has raged the most awful fevers have been the result amongst the native population. In Bantam alone 50,000 died in 1880. In the Preanger and Batavia the death-rate was also very high. There is no doubt whatever that this is due to the imperfect interment of the carcases. The Government says it is due to the wet season; but this is a lame excuse, for why is there no fever elsewhere? In the wet season it is, of course, worse, for the heavy rains cause more miasma.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, July.—The direct manufacture of iron from ore, puddling, heating furnaces, and forge cinders, &c., by Mr. Du Puy. Discussion on steel rails.—Experiments with screw-propellers of different material and dimensions, applied to the steamer *Lookout*, with the hull coppered and not coppered, by Chief-Engineer Isherwood.—Percussion rock-drills, by Mr. Grimshaw.—Radio-dynamics, by Dr. Pliny Earle Chase.

Annalen der Physik und Chemie, No. 7.—On the forces acting on the interior of magnetically or dielectrically polarised bodies, by H. Helmholtz.—On the conductivities of metals for heat and electricity, by G. Kirchhoff and G. Hansemann.—On the same, by L. Lorenz.—The specific heat of liquid organic compounds and its relation to their molecular weight, by M. A. von Reis.—Contribution to the doctrine of induced magnetism, by E. Riecke.—On crystal analysis, by O. Lehmann.—On the contraction observed in formation of haloid salts in comparison with their heat of formation, by W. Müller-Erzbach.—Automatic mercury air-pump, by A. Schuller.—The theory of the law of saturation, by M. Planck.—The so-called self-exciting influence-machine, by P. Riess.—On K. Strecker's paper on the specific heat of chlorine, &c., by L. Boltzmann.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 5.—On curves of the third order, by M. le Paige.—On the structure of the reproductive apparatus of Teleosteans (second paper), by Mr. MacLeod.—On a registering apparatus for signals of the mirror galvanometer, by M. Samuel.

Journal de Physique, July.—Experimental researches on the capacity of voltaic polarisation, by M. Blondlot.—Fundamental equations of induced magnetism, according to Maxwell, by M. Bouty.—Pumping machines and pneumatic apparatus, by M. de Romilly.—Researches on the specific heat of mixtures of heat and the three primary alcohols $C_2H_4O_2$, $C_4H_6O_2$ and $C_6H_8O_2$, by Dr. Zetterman.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xiv., fasc. x.-xi.—Contribution to the study of Amœba, by Dr. Grassi.—On an evaporimeter with constant level, by Prof. Fornioni.—Some researches on the distillation of cadaveric alkaloids, by S. Soldaini.

Memorie della Società degli Spettroscopisti Italiani, May.—Solar observations at Palermo Observatory during the first quarter of 1881, by Prof. Ricco.—On photographic photometry, by M. Janssen.—Scientific monument to P. Secchi in Reggio Emilia.—Spectroscopy applied to investigation of some colouring matters introduced into red wines, by S. Macagno.

Atti della R. Accademia dei Lincei, vol. v. fasc. 14.—Pharmacological researches on unstriped muscles, and particularly on the bladder, by P. Pellacani.—On some compounds of the furfuric series, by G. L. Ciamician and M. Dennstedt.—On some derivatives of pyrocol, by G. L. Ciamician and L. Danesi.—Action of nascent hydrogen on apoatropine, by L. Pesci.—On the saccharifying action of neutral salts, by F. Selmi.—Some theorems in geometry of *n* dimensions, by S. Veronese.

Rivista Scientifico-Industriale, No. 11, June 15.—Theory of siphons, by Prof. Rovelli.—New application of powdered graphite, by S. Mauri.—On Elban poltuce, by S. Corsi.—On radiant matter, by Prof. Mugna.

Archives des Sciences Physiques et Naturelles, No. 7, July 15.—Essay on the periodic variations of glaciers, by F. A. Forel.—Researches on the influence of heat on respiration, by W. Marcet.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 8.—M. Wurtz in the chair.—The following papers were read:—On the heat of formation of perchlorate of potass, by MM. Berthelot and Vieille.—Specific heats and heats of dilution of perchloric acid, by M. Berthelot.—Note on the communication to last meeting, by M. Boulley, on M. Toussaint's experiments on the infection produced by the juices of heated viands, by M. Chevreul.—Researches on the anhydrous chlorides of gallium, by M. Lecoq de Boisbaudran.—The standards of weights and measures of the Observatory, and the apparatus used in their construction, their origin, history, and present state, by M. C. Wolf.—On the Fuchsian functions, by M. H. Poincaré.—On the imitation, by means of hydrodynamics, of electrical and magnetic actions, by M. C. A. Bjerknes.—On the compressibility of carbonic acid and air under weak pressure and at a high temperature, by M. E. H. Amagat.—On the action of oxygen on mercury, by M. E. Amagat.—On the heating of waggons, carriages, &c., by means of the crystallised acetate of soda, by M. A. Ancelin.—Researches on the conditions of manufacture of magnets, by M. G. Trouvé.—Dissociation comparison of formulæ by experiment, by M. G. Lemoine.—Action of sulphuric acid on bromic amylene, by M. Chatin.—On a solution, of density 3·28, suitable for the immediate analysis of rocks, by M. D. Klein.—Tuberculous infection by the liquids of secretion and the serosity of vaccine pustules, by M. H. Toussaint.—Note on hydrophobia, by M. H. Duboué.

August 16.—M. Wurtz in the chair.—On cometary appearances, by M. J. Jamin.—Researches on the anhydrous chlorides of gallium, by M. Lecoq de Boisbaudran.—Singular effects of a gust of south-west wind, by M. G. A. Hirn.—Report of the place of Claude de Jouffroy in the discovery of steam-navigation, by M. de Lesseps.—The alcamines, by M. A. Ladenburg.—On the solubility of carbonate of magnesia in water charged with carbonic acid, by MM. Engel and Ville.—On the cobaltamines, by M. Porumbaru.—On the seat of gustation in dipterous insects: anatomical constitution and physiological value of the epipharynx and hypopharynx, by MM. Künckel and Gazagnaire.—On parasitism and tuberculosis, by M. H. Toussaint.—On the shooting stars of August, 1881, by M. Chapelas.

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