THURSDAY, AUGUST 19, 1886

PHYSICAL HYPOTHESES

Le Ipotesi Fisiche. Analizzate da Giannantonio Zanon. (Venezia: Lorenzo Tondelli, 1885.)

AT its sitting of July 17, 1881, the Royal Venetian Institute of Sciences proposed as the subject of a prize an examination of recent hypotheses regarding the causes of luminous, thermal, electrical, and magnetic phenomena. The volume now before us is one of seven competing treatises produced by the end of March 1883. As to the vastness of its scope, and the extent of erudition displayed in it, we can fully ratify the sentence of the examiners officially deputed to pronounce upon its merits. It is an "attack all along the line," and one conducted with no despicable array of mustered forces. The author has read and pondered much on the subjects he treats of; he is a mathematician, and is hence alive no less to the value of mathematical evidence, than to the worthlessness of a mere hollow show of mathematical formulæ; while the hypotheses he criticises have usually been tried by the severe test of a serious endeavour to realise their consequences. Many of his objections we at once admit to be valid; indeed, no universal explanation of physical phenomena has yet been proposed of which the structure was not riddled with visible absurdities. The late Prof. Challis devoted his considerable abilities and his best energies to the elaboration of a hydrodynamical theory of the universe, in which physical effects of all kinds were referred to forms of pressure of a continuous elastic medium. But the suggestion that an indefinite ascending series of such media might, after all, be necessary to produce the required results, cannot be looked upon otherwise than as a confession of failure. Father Secchi's heroic effort, in his "Unità delle Forze Fisiche," to see right down to the very bottom of things, was scarcely more successful. The reasonings upon which it was founded (as our author, among others, rightly points out) were vitiated at the root by a misapplication of Poinsot's theorems on the resilience of rotating bodies; and the cosmical machinery put together with such ingenuity, and set going with such heedful solicitude, came at once to a deadlock. Nor do we anticipate any better results from the scheme which Prof. Zanon himself promises to expound in a forthcoming work. The glimpses of his views afforded in essays already before the public are not encouraging. There is absolutely nothing gained in devolving the responsibility of our ignorance upon phrases, and taking their obscurity for illumination. If we can find no adequate explanation of the activities manifested in the wonderful "frame of things," of which we are at once spectators and participators, let us, in the name of candour and common-sense, acknowledge our impotence; but let us not imagine that we in any sense repair or qualify it by talking of inherent qualities, "virtues," propagated "influences," "molecular tensions," and the like. This would be to fall back into the rut out of which Molière did something to help us with Argan's famous diploma-examination:—

"Mihi a docto doctore
Domandatur causam et rationem quare
Opium facit dormire.
A quoi respondeo,
Quia est in eo
Virtus dormitiva,
Cujus est natura
Sensus assoupire."

Undeniable; but uninstructive.

The work we are at present concerned with is divided into an historical and a critical section. Of hypotheses as to the constitution of matter and the causes of physical action, the human mind has in all ages been prolific. Their procession, lengthy as that which defiled before Bradamante in the grotto of Merlin, is here carefully, and as expeditiously as may be, marshalled for our benefit. From the aqueous world of Thales to the vortex-atom world of Sir William Thomson, we are enabled to trace the progress, or rather the vicissitudes, of thought. For it is not in this field that the amazing advances of modern science have been made. The subject of Prof. Zanon's sketch is, not the onward march of natural investigation, but the "stations and retrogradations" of speculative physics. More than once, indeed, he is forced to exclaim with Horace, "Multa renascentur, quæ jam cecidere." The incidental inclusion of some fragments of genuine scientific theory, legitimately verified by experience, does not alter this general character.

The narrative is disfigured by a few inadvertencies. At p. 44, for example, the origination of the wave-theory of light is ascribed to Malebranche; but in the very next page, and with better reason, to Hooke, whose "Micrographia," containing the assertion that "light is a very short vibrative motion," propagated in spherical waves of agitation through "an homogeneous medium," appeared in 1665, nine years before the first-fruits of Malebranche's meditations were given to the world. At p. 50, Lavoisier's *rationale* of combustion is inextricably involved with his *rationale* of the decomposition of water; and at p. 118 the point is strangely missed of Prof. Tyndall's illustrative explanation of the change from the liquid to the gaseous state. It is worth while to point out these slips in a book representing a large amount of painstaking original research.

No notice is taken in it of Isenkrahe's recent speculations concerning the cause of gravity. They belong to the same family with those of Le Sage, justly condemned as "a mere effort of imagination, defiant alike of the dictates of reason and the laws of nature" (p. 229). Shelter from molecular bombardment, in one form or the other, is the key offered by them to the standing riddle of the cosmos. They explain gravity as a *push*, not a *pull*. Central forces are replaced by the preponderant external impacts of "mundane" or "ultramundane" particles. Such theories write their own sentence. They include their own condemnation. For, as M. Isenkrahe with the utmost candour points out, the very form of his fundamental equation implies a contradiction of the law that gravity varies with mass. It is obeyed only when the value of the equation becomes equal to nothing—that is, when there is no action of the kind postulated.

In the critical section of Prof. Zanon's volume, theories of the constitution of matter are examined separately from theories of the physical forces. That perplexities exist in both departments of research is a statement equivalent to the as yet undisputed assertion that human faculties are limited and human knowledge imperfect. Nevertheless, some of the difficulties raised by him are well worthy of thoughtful consideration. There can be no doubt, for instance, that the results of M. Gustav Hirn's experiments on the resistance to motion of air at different temperatures are gravely embarrassing to the kinetic theory of gases. The resistance *ought* to vary as the square root of the absolute temperature; it continues, nevertheless, practically unchanged at all temperatures, so long as the density remains the same (*Comptes Rendus*, t. xciv. p. 379).

Our author's strictures, however, although couched in terms of praiseworthy moderation and fairness, are somewhat too indiscriminate. His respect for thoroughgoing scientific work is evident and unfeigned; but it is almost impossible to take up the position of a sceptic *ex professo*, without at times incurring the reproach of perversity. We are totally unable to see the force of his reasons for preferring the obscure notion of a threefold spectrum containing chemical rays specifically distinct from thermal, and thermal from luminous, to the simple and intelligible view which substitutes for intangible differences of quality in radiations, measurable differences of wave-length. Nor can we believe that he would, on mature consideration, attempt to maintain the opinion that the planetary movements would remain unaffected by the progressive transmission of gravity. It is demonstrable that a species of aberration would thence ensue involving accelerative effects the insensibility of which so far proves (as Laplace has shown) that gravitative influence travels *at least* fifty million times faster than light. We owe the remark to Mr. William B. Taylor (*Smithsonian Report*, 1876, p. 212), that if one minute were spent on the journey from the sun to the earth, the consequent slight obliquity of traction, represented by an angle of 2″·4, would produce a shortening of the year perceptible while the sun was finishing a single round of the Signs.

Prof. Zanon appears to have bestowed much intelligent study on spectroscopic science. He carefully examines Mr. Lockyer's discovery of the independent "behaviour" of iron lines (amongst others) in spots and prominences, and though rejecting the inference of dissociation, he admits the necessity of assuming an *ad libitum* number of allotropic forms of the substance in question in order to explain anomalous appearances. He published moreover in 1880 a detailed analysis of Mr. Lockyer's "Studies in Spectrum Analysis," deriving therefrom confirmation in many particulars of his own views as to the nature of matter. Whatever we may think of these, there can be no doubt that our hope of penetrating the mysteries of molecular constitution must in future rest on the disclosures of the spectroscope. We do not in the least believe that these point towards the conclusion deprecated by Prof. Zanon of a fundamental unity of substance. On the contrary, Mr. Lockyer's striking observations indicate (if we may be allowed the expression) divergent, rather than convergent, simplifications.

It is perhaps inevitable in a work of the character of that now under consideration that the value to science of "working hypotheses" should be taken little account of. Yet the story of man's progressive knowledge of nature is a story of tentative efforts to represent facts to thought. Without some method of provisionally co-ordinating phenomena, indeed, the advance from a lower to a higher stage of induction, which we call discovery, could scarcely at all be effected. The true investigator is he who is never weary of collecting particulars to fit into his framework of theory, and of reconstructing his framework of theory to match fresh particulars. Without the power of thinking appearances into shape, no diligence in amassing the details of those appearances is of the slightest avail.

There is, it is true, the danger of a working hypothesis hardening unduly or prematurely into a theory—of a mere track, struck out for surveying purposes, turning into a fenced highway, without exit save in a quagmire. Against this peril the activity of such critics as Prof. Zanon is a very effectual safeguard. Their assaults may sometimes appear vexatious, but are really directed in the best interests of science, as hindering false security, and leading to a juster estimate of probabilities.

A. M. CLERKE

A MANUAL OF MECHANICS

A Manual of Mechanics; an Elementary Text-Book, designed for Students of Applied Mechanics. By T. M. Goodeve, M.A. (London: Longmans, 1886.)

THIS little work is evidently meant for readers whose knowledge of mathematics is small. The amount of mathematical knowledge supposed may, perhaps, be best inferred from an example quoted from the book itself. In p. 32 we have the problem to find the resultant of two equal rectangular forces, P. If R is the resultant, $R = P\sqrt{2}$. "Now $\sqrt{2} = \sqrt{\frac{50}{25}} = \sqrt{\frac{49}{25}}$ very nearly; ∴ $\sqrt{2} = \frac{7}{5}$ very nearly. Whence $R = \frac{7}{5} P$." When the angle between the lines of action of the forces is 120°, R should be equal to P; but it is proved in p. 33 to be equal to $P\sqrt{3}$, by an unaccountable error in the application of an elementary formula. However, $\sqrt{3}$ is found by the same process as before. Thus $\sqrt{3} = \sqrt{\frac{48}{16}} = \sqrt{\frac{49}{16}}$, very nearly, $= \frac{7}{4}$.

Now what naturally strikes one about beginners who have to be spoon-fed in this fashion is that it would be very much better to spend some time in teaching them trigonometry and algebra, than to push them into dynamics with the very slender knowledge of arithmetic and geometry which they possess. In our system of national education there is far too much effort employed in bringing branches of dynamics and physics *down* to the level of the ignorant, and too little in bringing students *up* to the standard of mathematical attainment required for a really intelligent knowledge of these subjects. The result of this system is that nearly all our elementary scientific books are very scrappy in character, presenting to their readers conceptions which they cannot possibly realise, and which must therefore be crammed into the mind as mere definitions devoid of real meaning.

In elementary scientific treatises it is necessary to insist, above all things, that the information given to the reader shall be above reproach, so that if he further pursues the subject treated of, he may, at least, not be hindered or misled by erroneous definitions or ideas placed before him at the outset.

The work before us, when it has been subjected to a slight revision, will fairly satisfy the conditions of an elementary text-book which is at once strictly scientific in its ideas and rich in practical applications.

Indeed, the practical portion of the work is, as was to be expected, very good. The portions dealing with mechanism are well suited to illustrate the scientific principles, and will be found very useful.

We shall confine ourselves to pointing out some of the blemishes which disfigure the theoretical portion.

P. 1. "When the tendency which force exerts to move a body is counteracted, so that the body remains at rest, the force is commonly called a *pressure*." It may be safely said that no such limited use is ordinarily made of the term *pressure*.

P. 2. The statement that two lumps of sugar placed in contact on a table do not adhere, because "the attraction emanates from their centres, which may be an inch apart," is not altogether conducive to sound notions.

In p. 19 we have the usual definition of the measure of a variable velocity, viz. "the space which would be described in a unit of time if the body retained throughout that unit the velocity which it has at the instant considered." When will people find out that this definition really defines nothing? It is truly Gladstonian in its elusiveness. Of course we find exactly the same "definition" of variable angular velocity (p. 62), variable acceleration (p. 99), and variable pressure (p. 176).

In p. 26 we have a most confusing exposition of Newton's Second Law. What, for instance, is the beginner to understand by this?—"Since we are dealing with a body in motion, it is clear that we may consider (1) the weight of the body to be constant, and its velocity to vary; (2) the weight of the body to vary, and its velocity to remain constant." The discussion of this law seems to show that its object is entirely misconceived by the author. For, while the law aims at giving a complete measure of *force*, the author merely *deduces from it* (by reasoning in a circle) a definition of momentum, his discussion ending with the words, "Hence, quantity of motion = (mass) (velocity)."

Again, in p. 28, "the explosive force of powder in a gun is *action*, and the momentum generated in the projectile is *reaction*." This is plainly open to the objection of equating force to momentum—a most mischievous notion.

In p. 48 the full meaning of the *centre of parallel forces* is by no means brought out, and the rule for finding its distance from a given line is incorrect: "Multiply each force into its perpendicular distance from a given line," &c. The rule ought to be enunciated with reference to a *plane*, and not to a *line*, and instead of the unmeaning term "distance of a force from the plane" we should say "distance of its point of application from the plane."

It is surely incorrect to say, as in p. 61, that "it is the universal practice to appropriate the Greek letter ω (omega) for expressing the unit of angular velocity."

It is very important to distinguish, in the mind of the beginner, between *force* and *power*. In p. 72 power is correctly defined as [time-] rate of doing work; but the language immediately following loosens this idea. Thus, "the number 33,000 foot-pounds is the *unit* of horse-power." There is tautology in the expression "Watt estimated that the sustained work of a horse continued for one minute would raise 33 000 lbs. through one foot in one minute." Again, the term *power* is used in p. 94 to designate a force applied to a machine.

In p. 73 the principle of work as applied to equilibrium is thus enunciated: "If a system of bodies be at rest under the action of any forces, and be moved a very little, no work will be done." It may well be doubted whether this enunciation is explicit enough to give a useful idea of the great statical principle. Indeed, the cases selected for illustration bear out in no way the proviso that the bodies must "be moved a very little."

Passing on to p. 98, we find acceleration of velocity discussed, and we have the formal definition: "the unit of acceleration adds a velocity of 1 foot in 1 second," which, as the words stand, has absolutely no meaning. The beginner ought to be taught that acceleration has a double reference to time, and it should never for a moment be spoken of otherwise than as "feet per second per second," or "miles per hour per minute," or by some other equivalent phrase. In p. 102, g is spoken of as 32·2 feet per second.

Finally, in p. 113 the beginner is introduced to the discussion of a compound pendulum formed by a straight uniform bar. Its time of oscillation depends on a radius of gyration. What idea is our beginner likely to have of this? The definition of k, the radius of gyration of the bar AB about an axis through its centre of gravity, G (p. 114), is quite erroneous—"let k^2 be the sum of the squares of the distances of each particle of AB from the point G." This sum is, of course, infinite. What is meant by k^2 is the *mean* square of distance from G. But clearly such a question is wholly unsuited to beginners.

The book concludes with a large collection of examples, taken from the Science Examinations, which will be useful to students. GEORGE M. MINCHIN

OUR BOOK SHELF

Mémoire sur les Volumes moléculaires des Liquides, avec un Avant-propos. By Hermann Kopp. Pp. 47. (Heidelberg: C. Winter, 1886.)

THIS is a *brochure* one-third of which is devoted to an "avant-propos" explaining why the paper does not appear in the *Annales de Chimie et Physique*, and making certain statements relating to M. Berthelot's recent work, "Les Origines de l'Alchimie." The remaining part contains a discussion of a paper on molecular volumes of liquids which appeared in the *Annales*. In 1869 Prof. Kopp published, in succession to his "Geschichte der Chemie," a volume of "Beiträge" to the history, explaining more fully the principal epochs in chemical history, more especially in relation to alchemy. In 1885 M. Berthelot published his work, which had occupied more or less of his attention since 1869. Its general character is very similar to the "Beiträge," though much less complete, and Kopp complains that no reference whatever is made to his book. Berthelot, in reply, states that he was unaware of the existence of the "Beiträge," yet he quotes references to works in which this "cahier" (as he calls it) is repeatedly mentioned. The oversight is a very grave one, when we remember that the "cahier" is a contribution of over

800 pages, and when we consider the position which Prof. Kopp holds as an historian of chemistry, a position which demands that due regard should be paid to his writings.

The more immediate object of the pamphlet is, however, to reply to the strictures of MM. Bartoli and Stracciati on the law of molecular volumes. These gentlemen have criticised somewhat severely Kopp's work in this department without taking into consideration the obstacles which, at the time it was carried out (1855) stood in the way of accurate and definite investigation. Singularly enough, they do not escape the same charge, having themselves in some cases made use of materials of an insufficient degree of purity. It is likewise pointed out that they are labouring under a complete misapprehension of the views held by Kopp and the significance of his deductions, nor do they seem to have appreciated the difficulties that surround the establishment of a "physical law" of general application. G. H. B.

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 insure the appearance even of communications containing interesting and novel facts.*]

Organic Evolution

ALLUDING to the first instalment of your abstract of my recently published paper on "Physiological Selection," the Duke of Argyll remarks:—

"I rejoice that the author has at last discovered that 'natural selection has been made to pose as a theory of the origin of species, whereas in point of fact it is nothing of the kind.' This has been my contention for many years" (NATURE, vol. xxxiv. p. 335).

These words seem to imply that my views with regard to natural selection have now undergone an important change, and one which brings them into conformity with those that have for many years been contended for by the Duke. It therefore seems desirable to state that such is not the case; and as I can only attribute the misunderstanding of so able and friendly a reader to some ambiguity in the condensed abstract of my paper to which he refers, I may invite him to consult the paper itself, where the matter to which he alludes is more fully explained. He will there find that my views upon the subject of natural selection are the same now as they have been during the last fifteen years; that in all essential respects they still coincide with those that were held by Mr. Darwin; and that my "additional suggestion on the origin of species, although quite independent of natural selection, is in no way opposed to natural selection," but is to be regarded as indicating "a factor supplementary to natural selection."

The state of matters, then, is simply this. Mr. Darwin himself has freely acknowledged that his theory of natural selection is not in itself a sufficient explanation of the origin of species. He therefore supplemented the natural causes which are together comprised under this term[1] by sundry other causes of a similarly natural kind. Of these he attributed most importance to use, disuse, sexual selection, correlated variation, and prolonged exposure to uniform conditions of life. To these supplementary causes Moritz Wagner added independent variation in the absence of intercrossing with parent forms, while I have myself added physiological selection. Now, the whole body of Mr. Darwin's followers have agreed with him in holding that the theory of natural selection is not in itself a sufficient explanation of the origin of species. But many of these followers differ from Mr. Darwin, and also differ among themselves, as to the proportional part which the principle of natural selection is to be considered as having played in the evolution of species. Mr. Darwin thought that in this respect natural selection plays a more important part than any other principle [therefore it is hard to see how in this respect any of "the successors of Darwin" can possibly "have run quite wild from the teaching of their master"], while in the opinion of many of his followers this principle should be regarded as of a value subordinate to the others. Of all the writers who have taken the latter view, the most clear-headed, as well as the earliest and most persistent, is Mr. Herbert Spencer. He more than any other author has been instant, both in season and out of season, in giving reasons for the scepticism that is in him. I confess, therefore, to not understanding the Duke of Argyll when he says that in the two articles recently published by Mr. Spencer "we have for the first time an avowed and definite declaration against some of the leading ideas on which the Mechanical Philosophy depends." So far as I can see, these two articles convey little more than a reiteration of the characteristically Spencerian view that, in the course of organic evolution the processes of "direct equilibration" have been of more importance than those of "indirect equilibration." By the first of these terms Mr. Spencer means use, disuse, and all other causes tending directly to the production of adaptive structures, while by the second he means natural selection. Now, from the time when Mr. Darwin first published his "Origin of Species" the main point of difference between his views and those of Mr. Spencer has uniformly consisted in the estimates which they have formed of the relative importance of these two kinds of equilibration. Surely, therefore, this cannot be the respect in which it is said that Mr. Spencer has "now for the first time opened his eyes and his mouth." Yet, if not, I do not understand the allusion. The Duke seems to imagine that, in some way or another, Mr. Spencer has taken a new and important "point of departure" in the course of his speculative thinking, and one which is "fatal to the adequacy of the Mechanical Philosophy as any explanation of organic evolution." It would be a matter of great interest to me—and I am sure to many others who have read the articles in question—to be told in what respect Mr. Spencer has committed himself to so great a change of doctrine; and it would certainly be a matter of profound astonishment to all evolutionists if Mr. Spencer can be shown to have so much as insinuated that his "direct equilibration" differs from Mr. Darwin's natural selection in not belonging to the system of so-called Mechanical Philosophy—or that those factors of organic evolution on which he has mainly relied differ from those on which Mr. Darwin has mainly relied in lending better countenance to the Supernatural Philosophy of Design.

My own attitude with regard to all these questions is perfectly plain and simple. In common with Darwin, Spencer, and the great majority of evolutionists, I believe that in the origin and development of adaptations—whether structural or instinctive—two sets of strictly natural causes have been at work: I agree with Mr. Darwin in thinking that of these two sets of causes the "indirectly equilibrating" have been of more importance than the "directly equilibrating": but I differ from other evolutionists, both of the Darwinian and Spencerian schools, in expressly drawing a marked distinction between causes of either kind which have been operative in the evolution of *adaptations* and those which have been operative in the evolution of *species*; and, lastly, I claim to have shown that when once this distinction is recognised, the theory of natural selection ceases to be, properly speaking, a theory of the origin of species; that it is thus liberated from all the difficulties with which it has hitherto been entangled on account of its having been made to "pose" as such; and that it is therefore placed in a position of greater

[1] In common with many other critics of Mr. Darwin's work, the Duke of Argyll has always contended that the theory of natural selection is "fundamentally erroneous" in that it assumes "variations to arise by accident," and merely "groups under one form of words, highly charged with metaphor, an immense variety of causes, some purely mental, some purely vital, and others purely physical or mechanical." This, however, is no valid criticism of the theory, which for the first time did comprise under one general point of view all the causes which together go to produce the results. In the opinion of the Duke, the weakest element of the theory consists in its inability to explain the causes of those variations on the occurrence of which the theory depends (NATURE, *loc cit.*, p. 336). But it is clearly no valid objection to a theory which explains one set of causes that it is unable to explain another and ulterior set. So long as variations of all kinds are known to be matters of fact, they are available for the theory of natural selection, even though the ulterior or physiological causes of variation should never be discovered. And for all the purposes of this theory it makes no difference whether the variations which are seen to take place in all directions, *with and without respect to utility*, are spoken of as "accidental" or as due to hidden causes. All that this theory has to do is to take the principle of promiscuous variation in all directions as a datum supplied by observation, and from this fact to show how the further principles of heredity, struggle, and survival are competent to *select* the variations which happen to be useful from those which are not. One might as well object to the physical explanation of specific gravity in selecting or sorting the different materials of a sea-shore, on the ground that we do not know the causes either of gravity in general or of the variations that are observable among specific gravities in particular.

logical security than it has ever occupied before. Far, then, from joining the "contention" of my critic in seeking to depose natural selection as a theory of the genesis of adaptive structures and instincts, I have expressly sought to fortify that theory as a "mechanical" explanation of these phenomena. Whether or not I have been successful I must leave others to judge, *after they have done me the justice to read my original paper*. But, be this as it may, the ambiguity of that paper must indeed be extraordinary, if it leads any one to suppose that my argument is precisely the opposite of what it is intended to be.

Geanies, Ross-shire, August 16 GEORGE J. ROMANES

Meteorology and Colliery Explosions

THE catastrophe at Woodend Colliery on Friday last again directs attention to the connection generally believed to exist between explosions of fire-damp and atmospheric changes. The real nature of this connection is but little, if at all, understood. From time to time observations have been taken with the view of throwing some light on the subject, but as the observations in one mine were discussed without reference to what may have been occurring in other districts, the results have not added much to our knowledge. Meteorology, however, is now sufficiently advanced to permit the adoption of another and more satisfactory method of dealing with the question.

The idea so long prevalent that certain fixed points on the barometric scale indicated certain kinds of weather has now been discarded, the examination of synchronous weather charts showing conclusively that the weather changes are not so much dependent upon the height of the barometer at any one place as upon the relations existing between readings over a tract of country; the direction in which the highest and lowest readings lie, and the difference of pressure (the barometric gradient) between neighbouring places—these form the basis of our modern weather knowledge. Nothing of this kind has hitherto been attempted when dealing with observations from collieries. If the presence of gas in mines is in any way regulated by changes of atmospheric pressure, it would be well to see if, like the weather, it is distributed in areas, and whether in these areas again some localities would have more gas than others, dependent more upon the distribution of pressure than upon local readings. With the object of discovering some law or laws governing the presence of gas, I appeal to colliery officials in every coal-field in Great Britain and Ireland to supply me with the few observations detailed below for a period of four months—from September 1 to December 31, the best part of the year for such work.

The Meteorological Office Weather Charts issued daily (Sundays included) at 8 a.m. and 6 p.m. show the distribution of pressure, winds, temperature, and weather. To these I propose adding the information supplied from mines *at the same hours*. Those who cannot arrange for two observations daily, to give preference to the morning set. The gas observation being the most important, I would be glad if precise information can be given. Absolute uniformity cannot be expected, but I would suggest that, where possible, a disused gallery favourable to the object in view should be used, one where the changes in the quantity of gas can be stated in yards or feet, thus turning the gallery into a gas barometer somewhat similar to the one at Seaham Colliery after the explosion of a few years ago. Those who have not the facilities for such measurements can still give valuable data if they do no more than note the increase or decrease of gas as "slight," "moderate," or "great." The appended specimen of the form for recording the observations

Form for Recording Observations

.... Colliery, near ... .

Date, September 1886	Top of shaft, ... feet above sea-level		In underground workings, ... yards from shaft ...... feet below pit-bank			Gas in gallery, yards	Remarks
	Temperature in the shade	Weather	Barometer	Temperature	Quantity of air passing		
1st, 8 a.m.							
6 p.m.							
2nd, 8 a.m.							
6 p.m.							

has columns for the air-temperature in the shade at the pit-bank, and the weather; while underground, in addition to the gas-record, the reading of a barometer and thermometer, and quantity of air passing at a fixed point some distance in the workings; also, remarks on the variations of the gas and ventilation at other than the regular hours. A sheet of close-ruled foolscap, arranged as indicated, will contain the data for one month, and, as soon as filled up, should be forwarded to me at the Meteorological Office, London, S.W. On the back of the first return particulars are required of the geographical position of the mine, the name and address of the manager, whether the barometer is a mercurial or an aneroid, together with the readings of the same at the pit-bank at 8 a.m. daily for a week before taking it underground, and describing the plan adopted in measuring the gas. It must be understood that I am undertaking the discussion as my own work, for which the Meteorological Council is not responsible. HY. HARRIES

August 16

Railway Weather Signals

WITH reference to the notice given in NATURE, vol. xxxiv. p. 347, of the ingenious plan adopted by the Norwegian Meteorological Institute for disseminating its weather reports, it may not be known to all of your readers that a similar system of signals has been in use for some time on the railroads in Ohio, Pennsylvania, and Canada. The day signals there consist of sheet-iron disks about three feet in diameter, and are displayed on the side of baggage-cars. The signals are shaped like the sun, a crescent, or a star, and differ in colour, being red or blue. The red colour refers to the temperature, and the blue to the state of the weather, as rainfall or snow. This system of signals was first brought into practical operation by Prof. T. C. Mendenhall, Chief of the Ohio Meteorological Bureau.

A system of night-signals for railways is also under trial in Pennsylvania: they are in the form of rockets or an exploding cartridge, which, when fired, may be seen from six to ten miles. CHAS. HARDING

August 17

Tornaria and Actinotrocha of the British Coasts

THREE species of Balanoglossus are known to occur on the shores of North-West Europe. *Balanoglossus kupfferi* was taken by Willemoes Suhm at Helleback, in the Sound, that is, on the coast of Zealand (*Zeit. f. wiss. Zool.* vol. xxi. 1871); *Balanoglossus salmoneus*, Giard, and *B. robinii* occur, according to Mr. Bateson's last paper in the *Quarterly Journal of Microscopical Science*, at Concarneau, in Finistère, and I believe also at the Channel Islands. But no Balanoglossus has yet been found on the shores of Britain. It will therefore be of some interest to British naturalists to learn that in August 1884 I obtained in the tow-net a larva which seemed to possess the distinctive characters of Tornaria. I had not leisure at the time to study the specimen with much attention, but I made a hurried sketch of it, which shows the presence of two parallel longitudinal bands of cilia anteriorly, and a single transverse band posteriorly. At the posterior end is a conical protuberance resembling the adhesive organ described by Bateson in his creeping larva. The position of the mouth was not ascertained, but was probably between the two anterior bands of cilia. The water vessel and tuft of cilia at the apex of the præoral lobe were not observed. This larva may not have been Tornaria, but I think it really was that form; and naturalists who are spending an autumn holiday at the sea-side would probably, if they undertook the search, succeed in finding Balanoglossus in the littoral sands, and its larvæ in the shore waters.

Phorenis is also at present, I believe, excluded from the British littoral fauna, but is certainly present on our shores, though no adult specimens have been taken. I took large numbers of Actinotrocha in the tow-net, close to the shore, in September 1883, a little to the north of the mouth of the Cromarty Firth. If I am mistaken in supposing that adult Phorenis and Balanoglossus have never been found on the coast of Britain, I shall be glad to be corrected. J. T. CUNNINGHAM

Mock Suns

As I observe the omission from my letter on the parhelia or mock suns of last month (p. 313) of the diagram which was intended to describe the real phenomena, may I ask you to allow me a few lines of space for the following :—

The real sun was surrounded at a short distance by a halo or rainbow circle of great beauty, with a mock sun of the same apparent magnitude and brightness on the right and left ; and *partially* formed suns above and below the ring : all of them being slightly opalescent. From the two perfected suns proceeded cones of intense light, about 3 diameters of the sun in length, and with their apices pointing east and west. These were rather more opalescent than the mock suns from which they seemed to originate. A second rainbow ring at a considerable distance outside of these extended to the zenith. The period of greatest beauty and brightness, when they were as rich in colouring as a real rainbow, lasted about 5 minutes. I was able to watch the whole of the phenomena from a little after 4 to nearly 6 o'clock.

ROBERT H. F. RIPPON

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PHYSIOLOGICAL SELECTION: AN ADDITIONAL SUGGESTION ON THE ORIGIN OF SPECIES[1]

III.

ARGUMENT from the Prevention of Intercrossing.—This argument is the same from whatever cause the prevention of intercrossing may arise. Where intercrossing is prevented by geographical barriers or by migration, it is more easy to prove the evolution of new species as a consequence than it is when intercrossing is prevented by physiological barriers ; for in the latter case the older and the newer forms will probably continue to occupy the same area, and then there will be no independent evidence to show that the severance between them was due to the prevention of intercrossing. Nevertheless, all the evidence I have of the large part that geographical barriers have played in the evolution of species by preventing intercrossing with parent forms goes to show the probable importance of physiological barriers when acting in the same way. Hence it will be better to postpone this line of argument in favour of physiological selection until the appearance of my next paper, where I shall hope to show, from evidence furnished by the geographical distribution of species, how predominant a part the prevention of intercrossing has played in the evolution of species. Here, therefore, I will merely remark that wherever intercrossing with parent forms is prevented, in the proportion that it is prevented a better opportunity is given to natural selection for seizing upon any beneficial variations that may happen to arise. On this account physiological selection probably lends important aid to natural selection, thus becoming indirectly instrumental in the evolution of useful as well as of useless structures.

There is also another respect in which these two kinds of selection probably co-operate. For Mr. Darwin shows that "it would be clearly advantageous to two varieties, or incipient species, if they could be kept from blending, on the same principle that, when man is selecting at the same time two varieties, it is necessary that he should keep them separate." But he proceeds to show that this advantage cannot be conferred by natural selection, and hence that the sterility which is so generally characteristic of species cannot be attributed to this agency. We have, however, just seen that this sterility is in all likelihood due to physiological selection ; and therefore, if it be true, as Mr. Darwin thought, that "it would profit an incipient species if it were rendered in some slight degree sterile with its parent form," physiological selection and natural selection may mutually assist one another. For, although the benefit of this sterility could not have been initially conferred by natural selection, yet when it once arises from an independent variation in the reproductive system, there is no reason why it should not forthwith be favoured by natural selection, just as is the case with advantageous variations in general.

[1] Abstract of a Paper read before the Linnean Society on May 6, by George J. Romanes, M.A., LL.D., F.R.S. &c. Continued from p. 340.

Feeling how grave a difficulty was presented to his theory of the origin of species by the general sterility of species, Mr. Darwin was extremely anxious to find some way in which natural selection might be seen to have brought about this result. Had it occurred to him that this result was probably nothing more than the necessary expression of a particular kind of variation on the part of the reproductive system, I cannot doubt that he would have felt the theory of natural selection to have been relieved of one of its greatest disabilities.

Argument from the Inutility of Specific Differences.—After what has already been said on this subject, I will here only deal with one question, namely, Why is it that apparently useless structures occur in such profusion among species, in much less profusion among genera, and scarcely at all among families, orders, and classes? It may be answered that the points wherein species differ from species are usually points of smaller detail than those which distinguish genera, families, &c., and thus may well actually be as a rule less useful, although still not absolutely useless : natural selection, it may be urged, is better able than is the naturalist to diagnose utility. But here again we have a most unwarranted appeal to the argument from ignorance ; whereas, according to my view, it is quite intelligible that when a varietal form is differentiated from its parent form by the bar of sterility, isolation, or migration, any little meaningless peculiarities of structure (or of instinct[1]) should at first be allowed to arise, but should eventually be eliminated as so much surplusage in the struggle for existence, by economy of growth, or even by independent variation when undirected by natural selection. A greater or less time would in different cases be required to effect this reduction, and thus we can understand why they are sometimes allowed to persist into genera, but rarely into families.

Again, if apparently useless specific characters (whether these be new structures or modifications of old ones, slight changes in form, colour, and so forth) are thus regarded as really useless, we should expect that they ought to be of a kind which do not impose much physiological tax upon the organism, since otherwise natural selection would not have allowed them to become so much as specific characters. Well, I have applied this test, and find it is a most general rule that specific characters the utility of which cannot be perceived are such as do not impose any considerable demand for nourishment: either on account of their small size or of their organically inexpensive material, they do not impose much tax upon the organism. Now it is obvious that there can be no connection between utility as disguised and smallness of size or inexpensiveness of material ; while it is no less obvious that there is a close connection between these things and a real inutility.

Lastly, our domesticated varieties occasionally exhibit well-marked and more or less constant characters of a useless kind. Here there can scarcely be any question about the genuineness of the inutility, seeing that the characters have arisen only under domestication, or in the absence of any struggle for existence. Yet these structures are sometimes of the most curious and complex morphology—even more so than innumerable apparently useless structures in the case of natural species.[2]

Argument from Divergence of Character.—Any theory of the origin of species in the way of descent must be prepared with an answer to the question, Why have species *multiplied?* Why have they not simply become transmuted in linear series instead of ramifying into branches? This question Mr. Darwin seeks to answer

[1] For instances of useless instincts see Mr. Darwin's posthumous essay published in my "Mental Evolution in Animals." It is suggestive in the present connection that, just like useless structures, useless instincts, so far as I can find, only occur in species and genera : never in families, orders, or classes.

[2] For a good instance of this see "Variation of Plants and Animals under Domestication," vol. i. pp. 78-79.

"from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers."[1] And he proceeds to illustrate this principle by means of a diagram, showing the hypothetical divergence of character undergone by the descendants of seven species. Thus, he attributes divergence of character exclusively to the influence of natural selection.

Now, this argument appears to me unassailable in all save one particular; but this is a most important particular: the argument wholly ignores the effect of intercrossing with parent forms. Granting to the argument that intercrossing with parent forms is prohibited, and nothing can be more satisfactory. The argument, however, sets out with showing that it is in limited areas, or in areas already overstocked with the specific forms in question, that the advantages to be derived from diversification will be most pronounced. Or, in Mr. Darwin's words, it is where they "jostle each other most closely" that natural selection will set a premium upon any members of the species which may depart from the common type. Now, inasmuch as this jostling or overcrowding of individuals is a needful condition to the agency of natural selection in the way of diversifying character, must we not feel that the general difficulty from intercrossing previously considered is here presented in a special and aggravated form? At all events, I know that, after having duly and impartially considered the matter, to me it does appear that, unless the swamping effects of intercrossing with the parent form on an overcrowded area is in some way prevented to begin with, natural selection could never have any material supplied by which to go on with. Let it be observed that I regard Mr. Darwin's argument as perfectly sound where it treats of the divergence of *species* from one another—*i.e.* of the rise of genera, families, &c.; for then physiological barriers are present to prevent intercrossing. But in applying the argument to explain the divergence of *individuals* into *varieties* it seems to me that here, more than anywhere else, he has lost sight of the formidable difficulty in question. For in this particular case so formidable does the difficulty seem to me, that I cannot believe natural selection alone could produce any divergence of character so long as all the individuals on an overcrowded area occupy that area together. Yet if any of them quit that area, and so escape from the unifying influence of free intercrossing, these individuals also escape from the conditions which Mr. Darwin names as those that are needed by natural selection in order to produce divergence. Therefore it appears to me that, under the circumstances supposed, natural selection alone could not produce divergence; the most it could do would be to change the whole specific type in some one direction (the needful variations in that one direction being caused by some general change of food, climate, habit, &c., affecting a number of individuals simultaneously), and thus induce transmutation of species in a linear series—each succeeding member of which might supplant its parent form. But, in order to secure diversity, multiplication, or ramification of species, it appears to me obvious that the primary condition required is that of preventing intercrossing with parent forms at the origin of each branch—whether the prevention be from the first absolute, or only partial. And, after all that has been previously said, it is needless again to show that the principles of physiological selection are at once the only principles which are here likely to be efficient, and the principles which are fully capable of doing all that is required. For species, as they now stand, unquestionably prove the fact of ramification; and it appears to me no less unquestionable that ramification, as often as it has occurred, can only have been permitted to occur by the absence of intercrossing with parent forms. But apart from geographical barriers (which, according to Mr. Darwin's argument, would be inimical to the divergence of character by natural selection), the ramification can only take place as a consequence of physiological selection, or as a consequence of some change in the reproductive system which prevents intercrossing with unchanged (or differently changed) compatriots. But when once this condition is supplied by physiological selection, I have no doubt that divergence of character may then be promoted by natural selection, in the way that is explained by Mr. Darwin.

[1] "Origin of Species," p. 87.

From which it will be seen that the theory of physiological selection has this advantage over the theory of natural selection in the way of explaining what Mr. Darwin calls diversification of character, or what I have called the ramification of species. This diversification or ramification has reference chiefly to the secondary specific distinctions, which, as we have seen, the theory of natural selection supposes to be the first changes that occur, and, by their occurrence, to induce the primary distinction of sterility. My theory, on the other hand, inverts this order, and supposes the primary distinction to be likewise (in most cases) the primordial distinction. Now, the advantages thus gained are twofold. In the first place, as just shown, we are able to release the principle of natural selection from what appears to me the otherwise hopeless difficulty of effecting diversification of character on an overcrowded area with nothing to prevent free intercrossing. And, in the next place, as we can now see, we are able to find an additional reason for the diversification of character, over and above the one that is relied upon by Mr. Darwin. For, by regarding the primary distinction of sterility as likewise the primordial distinction, we are able to apply to an incipient variety, inhabiting even an overcrowded area, the same principles which are known to lead to diversification by geographical barriers or by migration, as previously explained. In other words, if once we regard the primary distinction of sterility as also the initial distinction, instead of the incidental result of secondary distinctions, Mr. Darwin's argument touching the causes of diversification is not merely saved: it is notably extended by the addition of an independent principle, which, as we know from other evidence, is a principle of high importance in this respect.

Argument from Geographical Distribution.—The body of evidence under this head is too large to be given in an abstract; but the following are some of the chief points.

Mr. Darwin took a great deal of trouble to collect evidence on the two following facts, namely, (1) that "species of the larger genera in each country vary more frequently than the species of smaller genera"; and (2) that "many of the species included within the larger genera resemble varieties in being very closely, but unequally, related to each other." By larger genera he means genera containing many species, and he accounts for these general facts by the principle "that where many species of a genus have been formed, on an average many are still forming." But how forming? If we say by natural selection alone, we should expect to find the multitudinous species differing from one another in respect of features presenting utilitarian significance; yet this is precisely what we do not find. For Mr. Darwin's argument here consists in showing that "in large genera the amount of difference between the species is often exceedingly small, so that in this respect the species of the larger genera resemble varieties more than do the species of the smaller genera." Therefore the argument, while undoubtedly a very forcible one in favour of the fact of *evolution*, appears to me scarcely consistent with the theory of *natural selection*. On the other hand, the argument tells strongly (though unconsciously) in favour of physiological selection. For, the larger a genus, or the greater number of species it contains, the greater must be the opportunity afforded for the occurrence of that particular kind of variation on which the principle of physiological selection depends. All the species of a genus may be regarded as so many varieties which have already been separated from one another physiologically: therefore each of them may now constitute a new starting-point for a further and similar separation—particularly as, in virtue of their previous segregation, many of them are now exposed to different conditions of life. Thus, it seems to me, we can well understand why it is that genera already rich in species tend to grow still richer; while such is not the case in so great a degree with genera that are poor in species. Moreover, we can well understand that, multiplication of species being in the first instance determined by changes in the reproductive system alone, wherever a large number of new species are being turned out, the secondary differences between them should be "often exceedingly small"—a general correlation which, so far as I can see, we are not able to understand on the theory of natural selection.

Another general fact mentioned by Darwin, and now well recognised by all naturalists, is that closely allied species, or species differing from one another in trivial details, usually occupy contiguous areas; or, conversely stated, that contiguity of geographical position is favourable to the appearance of species closely allied to one another. Of course this fact speaks in favour of evolution; but where the question is as to method, I confess that the theory of natural selection appears to me wholly irrelevant. For, in most of the numberless cases to which I allude, the points of minute detail wherein the allied species differ in respect of secondary distinctions, are points which present no utilitarian significance. And, as previously argued, it is impossible to believe that there can be any general or constant correlation between disguised utility and insignificance of secondary distinction.

Now the large body of facts to which I here allude, but which I have not space to detail, appears to me to constitute perhaps the strongest of all my arguments in favour of physiological selection. Take, for instance, a large continental area, and follow across it a chain of species, each link of which differs from those on either side of it by the most minute and trivial distinctions of a secondary kind; but all the links of which differ from one another in respect of their reproductive systems, so that no one member of the series is perfectly fertile with any other member. Can it be supposed that in every case this constant primary distinction has been superinduced by the trivial secondary distinctions, distributed as they are over different parts of all these kindred organisms, and yet nowhere presenting any but the most trifling amount of morphological change? Or, even if we were to suppose this, we have still to meet the question, How were all these trifling changes produced in the face of free intercrossing on the continental area? Certainly not by natural selection, seeing that they are all useless to the species presenting them. Let it then be by changes in the conditions of life, whether of food, of climate, or of anything else. I can conceive of no other alternative. Yet, if we accept this alternative, we are but espousing—in a disguised and roundabout way, to be sure—the theory of physiological selection. For we are thus but hypothetically assigning the causes which have induced the primary distinction in each case, or the causes which have led to the mutual sterility. For my own part, I believe that the assignation would be, in the great majority of such cases, incorrect. That is to say, I do not believe that in the great majority of such cases the trivial secondary distinctions—however these were caused—can have had anything to do with the great primary distinction. What I believe is that all the closely-allied species inhabiting our supposed continent, and differing from one another in so many points of minute detail, are but so many records of one particular kind of variation having taken place in the reproductive systems of their ancestors, and which, so often as it did take place, necessarily gave birth to a new species. The primary distinction thus became the constant distinction, simply because it was in virtue of this distinction—or in virtue of the variation which first originated this distinction—that the species became species; and the secondary distinctions thus became multitudinous, minute, and unmeaning, simply because they were of later origin, the result of spontaneous variability, unchecked by intercrossing with the parent forms, and, on account of their trivial (*i.e.* physiologically harmless) nature, unchecked also by natural selection, economy of growth, or any other principle which might have prevented spontaneous variability of any other kind.

There are many other general facts relating to geographical distribution which lend the strongest countenance to the theory of physiological selection—in particular I may mention the difficulty which Mr. Darwin experiences in accounting for the absence or rarity of transitional varieties between species inhabiting contiguous areas (*loc. cit.*, p. 134), which is just what might have been expected on my theory—but it is time that this abstract should draw to a close.

Relations between the Theories of Natural Selection and Physiological Selection.—The two theories resemble one another in the kind of evidence by which they are each supported. For in neither case is this evidence that of direct observation of the transmutation of species under the influence of the agency supposed: the evidence in each case consists in first proving the facts on which the principle depends, and then showing that the phenomena of organic nature are such as they ought to be if the principle in question has had any large share in their production. But the two theories differ in that while natural selection is a theory of the origin of genera, families, orders, and classes even more than it is of the origin of species; the theory of physiological selection is almost exclusively a theory of the origin of species. Again, the latter theory differs from the former in that the variations on the occurrence of which it depends are variations of a comparatively unuseful, or non-adaptive, kind. Nevertheless, physiological selection must be quite as vigilant as natural selection, and it seizes upon the comparatively unuseful variation of sterility with even more certainty than natural selection can seize upon any useful variation. Lastly, as will have been gathered from the foregoing abstract, the two theories are in no way opposed to one another: they are, in fact, complementary, and the principles with which they have to deal co-operative. For, on the one hand, without the assistance of physiological selection, natural selection would, I believe, be all but overcome by the adverse influences of free intercrossing—influences all the more potent under the very conditions which are required for the multiplication of species by divergence of character. On the other hand, without natural selection, physiological selection would be powerless to create any differences of specific type other than those of mutual sterility and trivial details of structure, form, or colour—differences wholly without meaning from a utilitarian point of view. But in their combination these two principles appear to me able to accomplish what neither can accomplish alone—namely, a full and satisfactory explanation of the origin of species.

Conclusion.—It has not been possible to do justice to the theory of physiological selection within the limits of this abstract. But perhaps enough has been said to show that there is a great deal of evidence in its support; that by regarding mutually sterile species as records of variation in reproductive systems, we are at work, so to speak, on the foundation of the matter; and that we are thus able to explain a number of general facts which do not admit of being explained by any previous theory. It only remains to add that, if true, the present theory ought to admit of experimental verification. Let well-marked natural varieties of plants growing on the same area be systematically tested with regard to their relative *degrees* of fertility, first within themselves, and next towards one another: let these experiments be made in successive years over a number of natural varieties, by carefully-conducted artificial fertilisation, and by counting the seeds and tabulating the results. In this way experimental evidence would probably be obtained of degrees of sterility between even slight though constant varieties growing on the same areas; and, if so, such evidence would serve as further proof of the present theory. But experiments of this kind, in order to be satisfactory, ought to be conducted by a number of observers in different geographical areas; and my object in publishing so lengthy an abstract of my views in this periodical is that of inducing naturalists in other parts of the world to co-operate with me in carrying out this research. The paper itself, which furnishes fuller particulars as to the way in which such experiments should be carried out, is published in a separate form by the Linnean Society.

THE WOODEND COLLIERY EXPLOSION

QUI s'excuse s'accuse will occur to the minds of many who have followed the details of the disastrous explosion which took place at Woodend or Bedford Colliery on Friday last. We read in the *Times* of the 16th inst.:—"The Four-foot or Crombonke Mine is a very dusty one, and it is considered that at the Woodend pit the dust has increased the extent of the damage." "But to water the mine, as suggested by the Commission, would here be a very difficult operation, because the floor of the mine consists of a species of fire-clay which, as it absorbs the water, causes a lifting of the ground, and so prevents mining operations being conducted." Inasmuch, however, as the floor of perhaps ninety-nine out of every hundred mines consists of the same kind of material, the same argument against watering would hold equally good in most cases, and, if it is allowed to pass, this recommendation of the Commissioners is likely to come to nothing. It has been pointed out more than once in NATURE that the amount of water required to lay the dust is very small—far less than would be necessary to materially affect the floor of a mine in the manner suggested, and it would perhaps be wiser to try the effect in the first place and judge by results rather than to meet the proposition with a simple *non possumus*. We speak thus plainly here, because many of the witnesses who gave evidence before the Commissioners brought forward the very same argument with the same degree of plausibility, and we have reason to believe without having put the matter to a practical test. Many of those who now water regularly, for the express purpose of laying the dust on floors consisting of fire-clay, admit that the water produces no appreciable difference when properly and carefully distributed.

The bursting of the gauze of a safety-lamp, described by one of the survivors, is so contrary to all reason and experience that it cannot be accepted as an explanation of the origin of the explosion. Hundreds, if not thousands, of safety lamps are placed in explosive gas every day when the mines are being tested for the presence of fire-damp, and yet no parallel case has ever been recorded. Under these circumstances we prefer to attribute it to some other still unknown cause. We have yet to learn whether shots were fired in the mine, and if so we have probably not far to look for the explanation.

Up to the present all we know with certainty is that the mine produced very little gas, that it was dry and dusty, and that the explosion was violent but not universal. It would be most interesting, as well as instructive, to ascertain whether any natural local dampness curtailed its extent; but as this is a feature that has not hitherto attracted or received much attention, we are not sanguine that it will be carefully inquired into in the present case. We shall, however, watch the future course of the inquiry, and perhaps again comment upon it for the benefit of our readers.

W. G.

ON THE DIFFERENTIAL EQUATION TO A CURVE OF ANY ORDER

TO Mr. Samuel Roberts (see Reprint of *Educational Times*, vol. x. p. 47) is due the credit of having been the first to show that a direct method of elimination properly conducted leads to the differential equation for a cubic curve: but he has not attempted to obtain the general formula for a curve of any order. By aid of a very simple idea explained in a paper intended to appear in the *Comptes rendus* of the Institute, I find without calculation the general form of this equation. The eft-hand member of it may be conveniently termed the differential *criterion* to the curve. One single matrix will then serve to express the criteria for all curves whose order does not exceed any prescribed number. For instance, suppose we wish to have the criteria for the orders 1, 2, 3, 4:—

Let $m.\mu$ be used in general to denote the coefficient of

$$h^m \text{ in } \left(\frac{1}{1.2}y''h^2 + \frac{1}{1.2.3}y'''h^3 + \frac{1}{1.2.3.4}y''''h^4 + \ldots\right)^m.$$

Write down the matrix—

2·1	3·1	3·2	4·1	4·2	4·3	5·1	5·2	5·3	5·4
3·1	4·1	4·2	5·1	5·2	5·3	6·1	6·2	6·3	6·4
4·1	5·1	5·2	6·1	6·2	6·3	7·1	7·2	7·3	7·4
5·1	6·1	6·2	7·1	7·2	7·3	8·1	8·2	8·3	8·4
6·1	7·1	7·2	8·1	8·2	8·3	9·1	9·2	9·3	9·4
7·1	8·1	8·2	9·1	9·2	9·3	10·1	10·2	10·3	10·4
8·1	9·1	9 2	10·1	10·2	10·3	11·1	11·2	11·3	11·4
9·1	10·1	10·2	11·1	11·2	11·3	12·1	12·2	12·3	12·4
10·1	11·1	11·2	12·1	12·2	12·3	13·1	13·2	13·3	13·4
11·1	12·1	12·2	13·1	13·2	13·3	14·1	14·2	14·3	14·4

The determinant of the entire matrix, which is of the tenth order, is the criterion for a quartic curve. The determinant of the minor of the sixth order, comprised within the first six lines and columns is the criterion for a cubic. The determinant of the third order, comprised within the first three lines and columns (subject to a remark about to be made) will furnish the criterion for a conic, and the apex of the matrix is the criterion for the straight line. By adding on five more lines and columns, according to an obvious law, the matrix may be extended so as to give the criterion for a quintic; then six more lines and columns a sextic, and so on as far as may be required.

The remark to be made concerning the determinant of the third order referred to is that it contains the irrelevant factor 2·1, *i.e.* $\frac{y''}{2}$, so that the criterion for a conic (Monge's) is this determinant divested of such factor. It is *certain* that the next determinant is indecomposable, and is therefore the criterion for a cubic. There is no reason that I know of to suppose that any other determinant except that one which corresponds to the conic, is decomposable into factors. If this is made out, then, observing that the single term which is the criterion for the right line is indecomposable, we have another example of what may be called, in Babbage's words, a miraculous exception to a general law.

A well-known similar case of such miraculous exception I had occasion many years ago to notice in connection with the criteria for determining the number of real and imaginary roots in an algebraical equation. Such criteria may, with one single exception, be expressed by means of invariants. The case of exception is the biquadratic equation, for which it is impossible to assign an invariantive criterion that shall serve to distinguish between the case of all the roots being real and all imaginary.

It is proper to notice that it follows, from the definition of the symbol $m.\mu$, that its value is zero whenever m is less than 2μ. Thus, in the matrix written out above, the symbols 3·2, 4·3, 5·3, 5·4, 6·4, 7·4 may be replaced by zeros.

The above general result for a curve of any order is actually obtained by a far less expenditure of thought and labour than was employed by Monge, Halphen, and others to obtain it for the trifling case of a conic. I touch a secret spring, and the doors of the cabinet fly wide open.[1] J. J. SYLVESTER

New College, Oxford, August 6

CAPILLARY ATTRACTION[2]

III.

IN these other diagrams, however (Figs. 13 to 28), we have certain portions of the curves taken to represent real capillary surfaces shown in section. In Fig. 13 a solid sphere is shown in four different positions in contact with a mercury surface; and again, in Fig. 14 we have a section of the form assumed by mercury resting in a circular V-groove. Figs. 15 to 28 show water-surfaces under different conditions as to capillarity; the scale of the drawings for each set of figures is shown by a line the length of

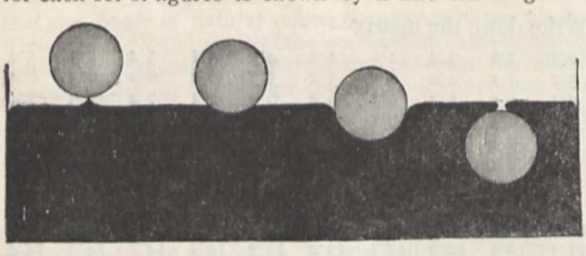


FIG. 13.—Mercury in contact with solid spheres (say of glass).

which represents one centimetre; the dotted horizontal lines indicate the positions of the free water-level. The drawings are sufficiently explicit to require no further reference here save the remark that *water* is represented by the lighter shading, and *solid* by the darker.

We have been thinking of our pieces of rigidified water as becoming suddenly liquified, and conceiving them inclosed within ideal contractile films; I have here an arrangement by which I can exhibit on an enlarged

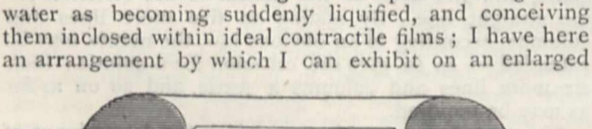
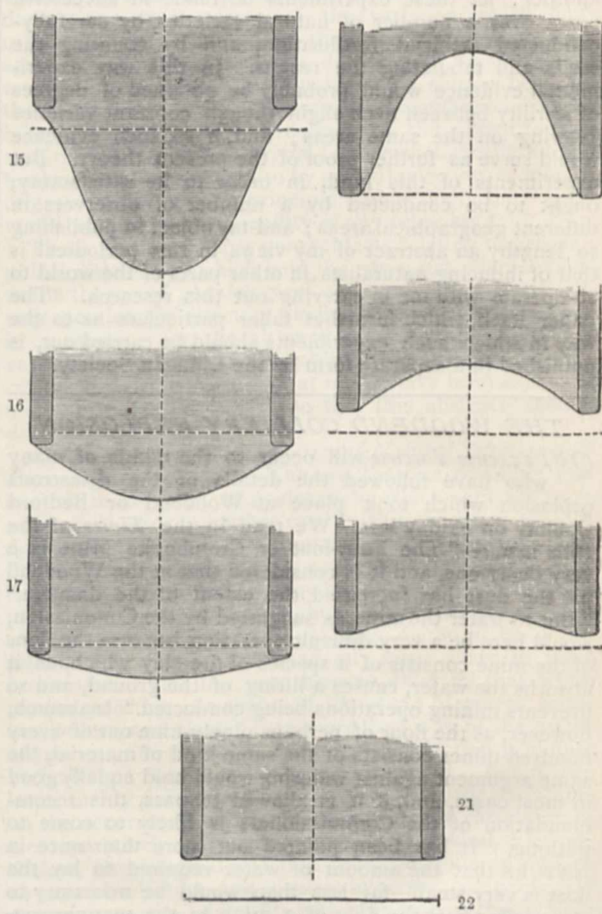


FIG. 14.—Sectional view of circular V-groove containing mercury.

scale a pendant drop, inclosed not in an *ideal* film, but in a *real* film of thin sheet india-rubber. The apparatus which you see here suspended from the roof is a stout metal ring of 60 centimetres diameter, with its aperture closed by a sheet of india-rubber tied to it all round, stretched uniformly in all directions, and as tightly as could be done without special apparatus for stretching it and binding it to the ring when stretched.

I now pour in water, and we find the flexible bottom assuming very much the same shape as the drop which you saw hanging from my finger after it had been dipped into and removed from the vessel of water (see Fig. 16).



FIGS. 15-21.—Water in glass tubes, the internal diameter of which may be found from Fig. 22, which represents a length of one centimetre.

I continue to pour in more water, and the form changes gradually and slowly, preserving meanwhile the general form of a drop such as is shown in Fig. 15, until, when a certain quantity of water has been poured in, a sudden change takes place. The sud-

FIG. 23.—Water resting in the space between a solid cylinder and a concentric hollow cylinder.

den change corresponds to the breaking away of a real drop of water from, for example, the mouth of a tea-urn, when the stopcock is so nearly closed that a very slow dropping takes place. The drop in the india-rubber bag, however, does not fall away, because the tension of the india-rubber increases enormously when

[1] Adopting the convention for degree and weight of a differential coefficient usual in the theory of reciprocants the deg : weight of the differential criterion of the nth order will be easily found to be—

$$\frac{n.n+1.n+2}{6} : \frac{n-1.n.n+1.n+2}{8}$$

except that for $n = 2$ it is 3 : 3 instead of 4 : 3.

[2] Lecture delivered at the Royal Institution. Revised and extended by the Author. Continued from p. 294.

the india-rubber is stretched. The tension of the real film at the surface of a drop of water remains constant, however much the surface is stretched, and therefore the drop breaks away instantly when enough of water has been supplied from above to feed the drop to the greatest volume that can hang from the particular size of tube which is used.

I now put this siphon into action, gradually drawing off some of the water, and we find the drop gradually diminishes until a sudden change again occurs and it assumes the form we observed (Fig. 16) when I first poured in the water. I instantly stop the action of the siphon, and we now find that the great drop has two possible forms of stable equilibrium, with an unstable form intermediate between them.

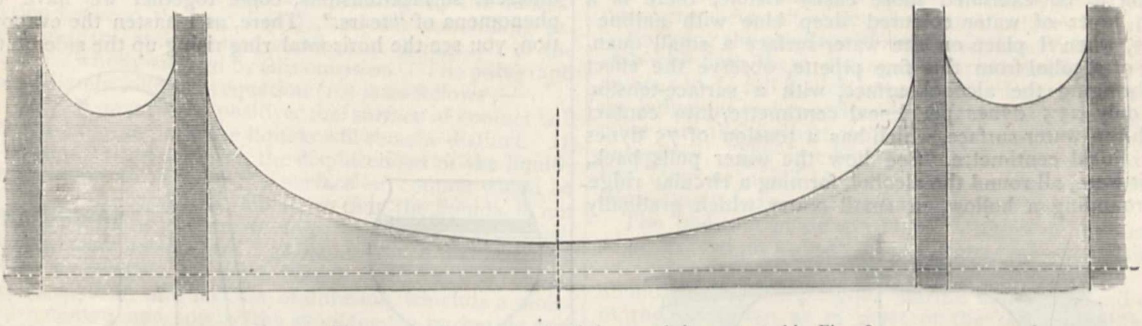


FIG. 24.—Water resting in two co-axial cylinders; scale is represented by Fig. 28.

Here is an experimental proof of this statement. With the drop in its higher stable form I cause it to vibrate so as alternately to decrease and increase the axial length, and you see that when the vibrations are such as to cause the increase of length to reach a certain limit there is a sudden change to the lower stable form, and we may

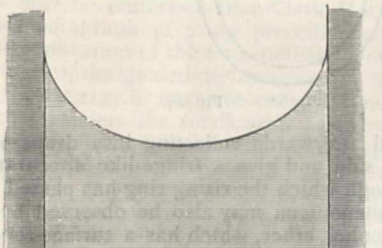


FIG. 25.

now leave the mass performing small vibrations about that lower form. I now increase these small vibrations, and we see that, whenever, in one of the upward (increasing) vibrations, the contraction of axial length reaches the limit already referred to, there is again a sudden change, which I promote by gently lifting with my hands, and the mass assumes the higher stable form, and we have it again performing small vibrations about this form.

The two positions of stable equilibrium, and the one of unstable intermediate between them, is a curious peculiarity of the hydrostatic problem presented by the water supported by india-rubber in the manner of the experiment.

I have here a simple arrangement of apparatus (Figs. 29 and 30) by which, with proper optical aids, such as a cathetometer and a microscope, we can make the necessary measurements on real drops of water or other liquid, for the purpose of determining the values of the capillary constants. For stability the drop hanging from the open tube should be just less than a hemisphere, but for convenience it is shown, as in the enlarged drawing of the nozzle (Fig. 30), exactly hemispherical. By means of the siphon the difference of levels, h, between the free level surface of the water in the vessel to which the nozzle is attached, and the lowest point in the drop hanging from the nozzle, may be varied, and corresponding measurements taken of h and of r, the radius of curvature of the drop at its lowest point. This measurement of the curvature of the drop is easily made with somewhat close accuracy, by known microscopic methods. The surface-tension T of the liquid is calculated from the radius, r, and the observed difference of levels, h, as follows:—

$$\frac{2T}{r} = h;$$

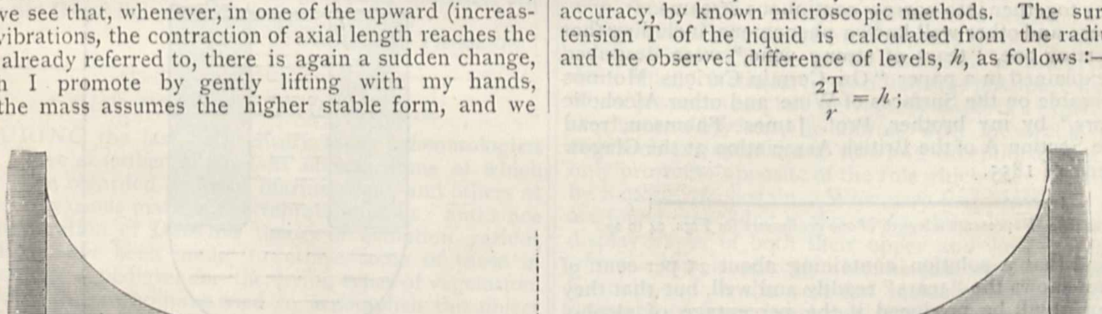


FIG. 26.

FIGS. 25 and 26.—Water resting in hollow cylinders (tubes); scale is represented by Fig. 28.

for example, if the liquid taken be water, with a free-surface tension of 75 milligrammes per centimetre, and $r = \cdot 05$ cm., h is equal to 3 centimetres.

Many experiments may be devised to illustrate the effect of surface-tension when two liquids, of which the surface-tensions are widely different, are brought into contact with each other. Thus we may place on the surface of a thin layer of water, wetting uniformly the surface of a glass plate or tray, a drop of alcohol or ether, and so cause the surface-tension of the liquid layer to become smaller in the region covered by the alcohol or ether. On the other hand, from a surface-layer of alcohol largely diluted with water we may arrange to withdraw part of the alcohol at one particular place by promoting its rapid evaporation, and thereby increase the surface-tension of the liquid layer in that region by diminishing the percentage of alcohol which it contains.

In this shallow tray, the bottom of which is of ground glass resting on white paper, so as to make the phenomena to be exhibited more easily visible, there is a thin layer of water coloured deep blue with aniline; now, when I place on the water-surface a small quantity of alcohol from this fine pipette, observe the effect of bringing the alcohol-surface, with a surface-tension of only 25·5 dynes per lineal centimetre, into contact with the water-surface, which has a tension of 75 dynes per lineal centimetre. See how the water pulls back, as it were, all round the alcohol, forming a circular ridge surrounding a hollow, or small crater, which gradually

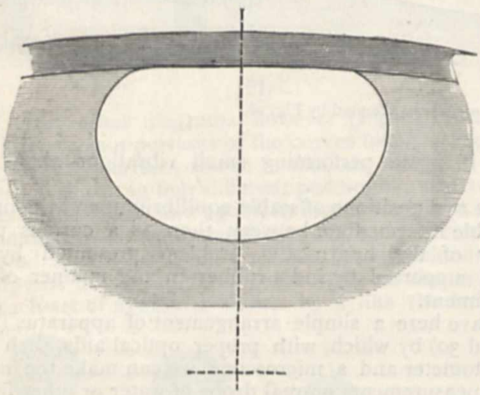


FIG. 27.—Section of the air-bubble in a level tube filled with water, and bent so that its axis is part of a circle of large radius; scale is represented in Fig. 28.

widens and deepens until the glass plate is actually laid bare in the centre, and the liquid is heaped up in a circular ridge around it. Similarly, when I paint with a brush a streak of alcohol across the tray, we find the water drawing back on each side from the portion of the tray touched with the brush. Now, when I incline the glass tray, it is most interesting to observe how the coloured water with its slight admixture of alcohol flows down the incline—first in isolated drops, afterwards joining together into narrow continuous streams.

These and other well-known phenomena, including that interesting one, "tears of strong wine," were described and explained in a paper "On Certain Curious Motions Observable on the Surfaces of Wine and other Alcoholic Liquors," by my brother, Prof. James Thomson, read before Section A of the British Association at the Glagow meeting of 1855.

FIG. 28.—Represents a length of one centimetre for Figs. 24 to 27.

I find that a solution containing about 25 per cent. of alcohol shows the "tears" readily and well, but that they cannot at all be produced if the percentage of alcohol is considerably smaller or considerably greater than 25. In two of those bottles the coloured solution contains respectively 1 per cent. and 90 per cent. of alcohol, and in them you see it is impossible to produce the "tears"; but when I take this third bottle, in which the coloured liquid contains 25 per cent. of alcohol, and operate upon it, you see—there—the "tears" begin to form at once. I first incline and rotate the bottle so as to wet its inner surface with the liquid, and then, leaving it quite still, I remove the stopper, and withdraw by means of this paper tube the mixture of air and alcoholic vapour from the bottle and allow fresh air to take its place. In this way I promote the evaporation of alcohol from all liquid surfaces within the bottle, and where the liquid is in the form of a thin film it very speedily loses a great part of its alcohol. Hence the surface-tension of the thin film of liquid on the interior wall of the bottle comes to have a greater and greater value than the surface-tension of the mass of liquid in the bottom, and where these two liquid surfaces, having different surface-tensions, come together we have the phenomena of "tears." There, as I hasten the evaporation, you see the horizontal ring rising up the side of the

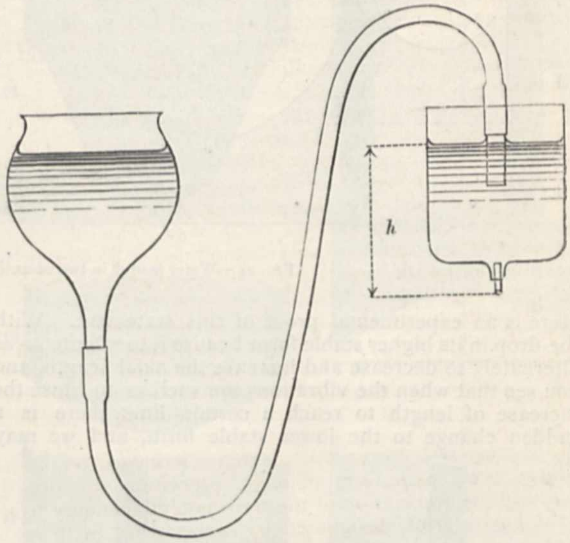


FIG. 29.

bottle, and afterwards collecting into drops which slip down the side and give a fringe-like appearance to the space through which the rising ring has passed.

These phenomena may also be observed by using, instead of alcohol, ether, which has a surface-tension equal to about three-fourths of that of alcohol. In using ether, however, this very curious effect may be seen.[1] I dip the brush into the ether, and hold it near to but not touching the water-surface. Now I see a hollow formed, which becomes more or less deep according as the brush is

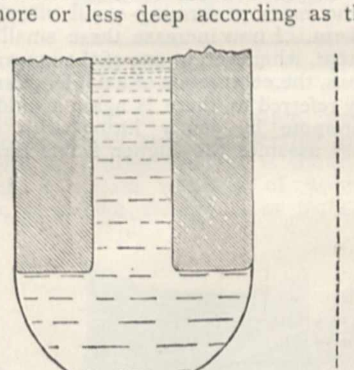


FIG. 30.

nearer to or farther from the normal water surface, and it follows the brush about as I move it so.

Here is an experiment showing the effect of heat on surface-tension. Over a portion of this tin plate there is a thin layer of resin. I lay the tin plate on this hot copper cylinder, and we at once see the fluid resin drawing back from the portion of the tin plate directly over the end of the heated copper cylinder, and leaving a

1 See Clerk-Maxwell's article (p. 65) on "Capillary Attraction" ("Encyclopædia Britannica," 9th edition).

circular space on the surface of the tin plate almost clear of resin, showing how very much the surface-tension of hot resin is less than that of cold resin.

Note of January 30, 1886.—The equations (8) and (9) on p. 59 of Clerk-Maxwell's article on "Capillary Attraction" in the ninth edition of the "Encyclopædia Britannica" do not contain terms depending on the mutual action between the two liquids, and the concluding expression (10), and the last small print paragraph of the page are wholly vitiated by this omission. The paragraph immediately following equation (10) is as follows:—

"If this quantity is positive, the surface of contact will tend to contract, and the liquids will remain distinct. If, however, it were negative, the displacement of the liquids which tends to enlarge the surface of contact would be aided by the molecular forces, so that the liquids, if not kept separate by gravity, would become thoroughly mixed. No instance, however, of a phenomenon of this kind has been discovered, for those liquids which mix of themselves do so by the process of diffusion, which is a molecular motion, and not by the spontaneous puckering and replication of the boundary surface as would be the case if T were negative."

It seems to me that this view is not correct; but that on the contrary there is this "puckering" as the *very beginning* of diffusion. What I have given in the lecture as reported in the text above seems to me the right view of the case as regards diffusion in relation to interfacial tension.

It may also be remarked that Clerk-Maxwell, in the large print paragraph of p. 59, preceding equation (1), and in his application of the term potential energy to E in the small print, designated by *energy* what is in reality exhaustion of energy or negative energy; and the same inadvertence renders the small print paragraph on p. 60 very obscure. The curious and interesting statement at the top of the second column of p. 63, regarding a drop of carbon disulphide in contact with a drop of water in a capillary tube would constitute a perpetual motion if it were true for a tube not first wetted with water through part of its bore—". . . if a drop of water and a drop of bisulphide of carbon be placed in contact in a horizontal capillary tube, the bisulphide of carbon will chase the water along the tube."

Additional Note of June 5, 1886.—I have carefully tried the experiment referred to in the preceding sentence, and have not found the alleged motion.

WILLIAM THOMSON

OUR FOSSIL PSEUDO-ALGÆ

DURING the last half-century many palæontologists have described anomalous objects, some of which have been regarded as fossil marine Algæ, and others as tracks of various marine invertebrate animals; and since the publication of Darwin's theory of evolution various attempts have been made to utilise some of these in formulating a pedigree for the living types of vegetation. Amongst those who have tried to accomplish this object my distinguished friend the Marquis of Saporta, and his colleague, M. Marion, occupy the most prominent position. They have in several publications described and figured many objects which they believe to have been true marine Algæ, and out of which they have constructed the lower roots of their genealogical tree. But meanwhile there has grown up an enlarging school of palæontologists who look with strong suspicion upon these genealogies; who refuse to recognise the vegetable character of these objects; who believe most of them to be casts of various ridges and furrows, most of which have been tracks produced by creeping invertebrate animals or by the still more mechanical agencies of wind and water. At the head of this school Prof. Nathorst, of Stockholm, stands preeminent. An animated controversy sprang up some time ago between M. Nathorst and M. Saporta relative to this subject. Blast and counterblast have succeeded one another, and the latest discharge of palæo-botanical artillery has just been fired off by M. Nathorst in the form of a memoir entitled "Nouvelles Observations sur des Traces d'Animaux et autres Phénomènes d'Origine purement méchanique décrits comme 'Algues Fossiles.'"

Enjoying the privilege of an intimate and valued friendship with both these distinguished palæontologists, I am anxious to do full justice to both. But I must admit that my judgment and experiences bring me into closer agreement with the northern naturalist than with his French antagonist. The interesting subject discussed by them has long occupied my attention, and my conclusions respecting it have not been hastily arrived at.

The question in debate is not whether or not marine Algæ existed in Palæozoic and later geological epochs: on this point Nathorst and Saporta are agreed. The abundance of phytophagous marine mollusks found even in the Cambrian, as in most of the other fossiliferous strata, clearly demonstrates that there must have been submarine pastures upon which they could feed. The question is, are numerous objects, found in strata of marine origin, and believed by some to be fossilised marine Algæ, really such? To this query Saporta answers *Yes;* Nathorst's reply is an emphatic *No.* Hence the elaborate controversial literature of which these two *savants* are the authors. To condense their several articles into an abstract is not easy, but such an abstract of M. Nathorst's latest publication may be attempted, illustrating the general features of the discussion.

Throughout his memoir M. Nathorst rests prominently upon two general propositions which appear to me to be unanswerable. The first is that all or nearly all these debatable pseudo-Algæ stand out in bold demi-relief from the *inferior* surfaces of the rocky slabs to which they are attached, and that beyond their sculptured surfaces they as constantly consist of a mere extension of the rock of which they form a part. Hence Nathorst insists that they are merely convex casts of what were primarily concave grooves or channels on the surface of the subjacent stratum.

In reply to this opinion M. Saporta publishes figures of casts of vegetable fragments in demi-relief, the positions of which on the inferior surfaces of slabs are identical with those of the pseudo-Algæ under discussion. One of these is a fragment of what appears to be a twig of a Conifer, of which the lower side alone is preserved in demi-relief. Nathorst freely admits the possible existence of such specimens, but he regrets Saporta's explanation of them. *Imprimis,* he affirms with inexorable logic, that such examples are so rare and exceptional that they only prove the opposite of the rule which they are alleged by Saporta to sustain. Whenever fragments like these are found embedded in the rocks, they almost invariably display traces of both their upper and lower surfaces; whereas this is scarcely ever the case with the disputed Fucoids, and in the very few instances where such are supposed to have been met with, their entirely exceptional character suggests a very different explanation of them from that proposed by M. Saporta.

It is difficult to understand how a cylindrical object sufficiently dense to produce a deep concave impression upon hardening mud could do so without leaving some trace of its upper surface upon the opposite surface of the sand by which that mud became overlain. Saporta's theory explaining why it does not do so is surely untenable. That theory supposes that an organism half embedded in mud and overlain by sand began to decay at its *upper* surface, which decay ultimately reached the lower surface which rested on the mud; that, as the decay proceeded downwards, the superimposed sand would finally reach the concave mould in the mud which it would fill, and of which it would permanently preserve the form. Nathorst replies that the decay would follow no such downward course; that it would commence in the softest tissues, wherever they were; and that in such objects as Bilobites and Cruziana, which Saporta believed to be cellular and fistular objects, whilst their outer surfaces were sufficiently hard and resisting to impress their sculpturings upon the underlying mud, the decay would commence in their interior, with the result, in each instance, not of destroying all traces of the upper surface whilst the lower one was preserved, but that both surfaces of the flattened organism would be preserved, as is the case with the Carboniferous Sigillaria and Lepidodendra. Once thus flattened, the organism would no longer be capable of producing the deeply concave groove in the clay to which the specimens in bold demi-relief have been due.

This part of the controversy furnishes Nathorst with another argument. However much compressed, embedded fragments of vegetable matter almost invariably retain some traces of their primitive carbonaceous constituents, however thin the film thus preserved may be. As in the case of many of the Monte Bolca Fucoids, this may be no more than a faint brownish stain on the surface of the stone; whereas such stains, suggestive of the former permanent presence of organic matter, are almost invariably, if not wholly, absent from the pseudo-Fucoids.

In his new memoir M. Nathorst then proceeds to examine in detail the claims of several well-known genera to rank as members of the vegetable kingdom. I must refer such as are interested in the details of this controversy to the original memoir of the Swedish palæontologist. I would only observe that, whilst M. Nathorst denies the accuracy of some of M. Saporta's statements as to the facts in certain instances, in others on which the two palæontologists are agreed he shows that the acknowledged facts are capable of such explanations as lead to conclusions diametrically opposed to those arrived at by M. Saporta.

One of the most important features of M. Nathorst's new memoir is seen in his illustrations. He has invented some simple instruments, by rolling which over some plastic materials he produces impressions, casts of which recall most strikingly the objects known by the generic titles of Cruziana and Harlania.

Whatever ultimate decisions may be arrived at respecting these debated objects, I am compelled to arrive at a conclusion which I have already announced on more than one previous occasion. When it is possible for two observers so experienced as are M. Nathorst and M. Saporta to study the same objects and to arrive at such opposite determinations as to their organic or inorganic character, we must at least conclude that objects capable of receiving such contradictory explanations can have no value when we are considering the evolution of the vegetable kingdom. The evidences of the witnesses in such a controversy must be clear in their testimony and indisputable as to their antecedents.

Manchester, August 11 W. C. WILLIAMSON

NOTES

A TELEGRAM from Grenada, August 16, states that the British observing party for the eclipse of the sun on the 29th inst. has arrived there, and has divided into two. Messrs. Lockyer, Turner, Perry, and Maunder are going to Green Island and Grenville Bay, on the east side of Grenada, and to Carriacou, a small island to the north. Messrs. Darwin, Thorpe, Schuster, and Lawrence will take up their station at Prickly Point, Hog Island.

THE seventeenth meeting of the German Anthropological Society was formally opened on the 11th inst. at Stettin. The gathering is described as a very representative and distinguished one.

THE Yorkshire Naturalists' Union fungus foray will take place on Thursday, September 30. On the following day there will be an exhibition of the specimens in the Leeds Museum, kindly lent for the purpose; and in the evening the usual dinner. Several distinguished mycologists have promised to be present, and no effort is being spared on the part of the officials to make it a success.

LORD DUFFERIN is, it is stated, about to address a memorandum to the Provincial Governments of India regarding technical education, pointing out where the present system fails, suggesting remedies, advising the adoption of a more practical system, and inviting opinions from the Provincial Governments on the whole subject.

WE have received the Smithsonian reports on the progress of physics and mineralogy for the past year. The former is by Prof. Barker, the latter by Prof. E. S. Dana. Physics is treated under the heads general, mechanics (with the subtitles solids, liquids, gases), acoustics, heat (sub-titles production of heat, expansion and change of state, conduction and radiation, specific heat), light (production and velocity, reflection and refraction, dispersion and colour, interference and polarisation), electricity (magnetism, electric generators, electrical units and measurements, electric spark and electric light), obituary, and bibliography. Mineralogy, similarly, is treated under the heads general, crystallography and physical mineralogy, chemical mineralogy, new mineral localities, new minerals, papers on mineral species, bibliography, and obituary.

THE Smithsonian Report for the past year contains a most interesting paper on the "Volcanic Eruptions and Earthquakes in Iceland within Historic Times," translated and condensed from the work of Th. Thoroddsen, by Mr. George Boehmer. The original work appears to be one of enormous research and thoroughness. Mr. Boehmer divides his subject into early accounts, position of the active volcanoes, of which there are eight groups, with a sketch of each, chronological list of volcanic eruptions and earthquakes in Iceland, and finally an extensive bibliography of the volcanoes, earthquakes, and geysers of Iceland.

THREE severe shocks of earthquake occurred at Malta, the first at 8.30 p.m. on August 14, the second at 3.45 a.m. on August 15, and the third at noonday. Three fresh shocks were felt on the evening of the 17th, the first at 5.45, the second at 6.20, and the third at 7.45. They were not so violent as those experienced on Saturday and Sunday, and no damage is reported.

MR. H. B. GUPPY is completing his work on the Solomon Islands and their natives, which will shortly be ready for the press. The work will refer chiefly to the anthropology and geology of this region. It will also treat of the botany, natural history, meteorology, and general resources of these islands, and there will be appended an account of the original discovery of the group as related in the British Museum manuscript of Gallego's "Journal." The chief value of Mr. Guppy's observations will lie in the circumstance that his collections have been examined by the leading authorities on the subjects to which they relate. He hopes to illustrate the work from his own photographs.

WITH reference to Mr. Verbeek's investigations into the Krakatão eruption, which were noticed in NATURE, vol. xxxiii. p. 560, we have received a letter from Herr Retgers, Mining Engineer in Samarang, stating (as indeed Mr. Verbeek had already stated in his work) that the whole of the mineralogical investigations into the composition of the volcanic ashes then thrown out were carried out by him.

THE Committee of the Liverpool Naturalists' Field Club begin their report for the past year by observing that for twenty-five years past statistics have been exhausted and lectures also, so far as their usefulness is concerned. "Your Committee, therefore, on this occasion, will spare you figures and forbear admonition, contenting themselves with a bare record of the year's proceedings, and leaving members to draw their own conclusions as to what has been and what might have been done." With such a lugubrious commencement, one might expect that the affairs of the Club were in a "parlous" state, but this is far from being the case. One complaint is that the attendance at the excursions was not satisfactory, and therefore either the number will have to be cut down, or the distances travelled be less. The average attendance was about 60,—which many similar societies would consider an uncommonly good one, especially when it is remembered that some of the journeys were rather long. The Treasurer has the handsome balance of nearly 90*l.* in his hands; the President, the Rev. H. Higgins, delivered an excellent address on "Calcareous Sea-Weeds: an Essay in Comparative Phytology;" the lists of interesting plants noticed on some of the excursions show that the members who did go kept their eyes open; the competition for the prizes appears to have been pretty keen; and there is a tolerably long list of members,—so that, on the whole, notwithstanding the low spirits of the Committee, the case of the Society is far from hopeless. But we trust the members will attend in larger numbers when the Society next goes to the Cefn Caves, Caergwrle, or Humphry Head, notwithstanding the long distances. They will thereby not only add to their own enjoyment and instruction, but will bring relief to the minds of their depressed and anxious Committee.

THE greatest balloon in the world has been lately constructed at San Francisco by a Mr. van Tassel. It will hold 150,000 cubic feet of gas, and has been made for the purpose of traversing the American Continent from ocean to ocean. From the bottom of the car to the top of the inflated balloon will be 119 feet, and when filled the diameter will be 68 feet. The car is 21 feet in circumference and has sides 34 inches high; 15 persons can be seated in it.

FOR several years attempts have been made in Sweden to extract tannic matter from the Swedish species of pine, similar in quality, &c., to that of the American hemlock (*Pinus canadensis*), but without satisfactory results, chiefly on account of the manner in which this is done not being known. Now, however, the question has been solved by a chemist, Dr. Laudin, who, having visited North America for this purpose, has, on his return to Sweden, succeeded in producing tannic matter by a chemical process, which has been found equal to the American, though the colour of the Swedish leather produced therewith is more yellow in colour than the American. It is hoped that this discovery will have the effect of causing a great tanning industry to spring up in Sweden.

THE additions to the Zoological Society's Gardens during the past week include a Guinea Baboon (*Cynocephalus sphinx*), from West Africa, presented by Mr. C. Palgrave, F.Z.S.; an Alpine Marmot (*Arctomys marmotta*), two Tawny Owls (*Syrnium aluco*), European, presented by Mr. Lionel H. Hanbury, F.Z.S.; a Bank Vole (*Arvicola pratensis*), British, presented by Mr. G. T. Rope; two Derbian Screamers (*Chauna derbiana*) from the North Coast of Columbia, presented by Capt. H. Rigaud; a Peregrine Falcon (*Falco peregrinus*), European, presented by Mr. J. Howard; a Golden-crowned Conure (*Conurus aureus*) from South-East Brazil, deposited; three Long-fronted Gerbilles (*Gerbillus longifrons*), eight Elliot's Pheasants (*Phasianus ellioti*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 AUGUST 22–28

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 22

Sun rises, 4h. 58m.; souths, 12h. 2m. 42·2s.; sets, 19h. 7m.; decl. on meridian, 11° 44′ N.: Sidereal Time at Sunset, 17h. 11m.

Moon (at Last Quarter) rises, 22h. 6m.*; souths, 5h. 21m.; sets, 12h. 46m.; decl. on meridian, 13° 18′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	4 13	11 19	18 25	12 1 N.
Venus	2 28	10 19	18 10	19 43 N.
Mars	10 47	15 49	20 51	12 3 S.
Jupiter	8 25	14 22	20 19	1 27 S.
Saturn	1 11	9 16	17 21	21 53 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Aug.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
23	48 Tauri	6	1 23	1 54	126 186
23	γ Tauri	4	3 22	4 13	118 215
23	58 Tauri	6	4 1	near approach	348 —

Aug.	h.	
22	12	Jupiter at greatest distance from the Sun.
25	11	Mercury stationary.
27	20	Venus in conjunction with and 3° 0′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Aug. 22,	21 8	*m*
			,, 27,	20 47	*m*
Algol	3 0·8	40 31 N.	,, 22,	21 7	*m*
V Tauri	4 45·4	17 21 N.	,, 27,		*M*
W Virginis	13 20·2	2 47 S.	,, 26,	0 0	*m*
U Coronæ	15 13·6	32 4 N.	,, 22,	0 4	*m*
			,, 28,	21 46	*m*
U Herculis	16 20·8	19 9 N.	,, 24,		*M*
R Draconis	16 32·4	67 3 N.	,, 26,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 22,	23 50	*m*
			and at intervals of	20 8	
β Lyræ	18 45·9	33 14 N.	Aug. 24,	2 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 28,		*M*
δ Cephei	22 24·9	57 50 N.	,, 27,	2 0	*M*

M signifies maximum; *m* minimum.

Meteor Showers

Meteors have been observed at this time of the year from near κ Ceti, R.A. 53°, Decl. 0; near Castor, R.A. 110°, Decl. 32° N.; near ζ Draconis, R.A. 260°, Decl. 64° N.; and from near ο Draconis, R.A. 282°, Decl. 57° N.

GEOGRAPHICAL NOTES

WE have before us Nos. 5, 6, 7, and 8 of *Petermann's Mittheilungen* for the present year, and Supplement No. 82. The last is a detailed account, by the late Herr Robert Schlagintweit, of the Pacific railways of North America. No. 5 contains a paper on the Xingu Expedition (concluded in No. 6), by Herr Claus, detailing the cartographical surveys and the physical and astronomical measurements made in the course of the explorations. The paper may be regarded as a supplement to the work of Dr. von den Steinen, "Durch Zentralbrasilien," lately published by Brockhaus. Dr. Oppel, in the same number, contributes a statistical paper showing the steady and enormous increase in the population of Europe. No. 7 contains two very interesting and original geographical papers—one by Herr Engelhard on the Island of Saleijer, a Dutch settlement in the Malay Archipelago, situated immediately south of Celebes. The island is described in an exhaustive way, its climate, people, situation, &c., being discussed in some detail. In the second

Dr. Posewitz refers to recent formations in the Island of Banka, off the east coast of Sumatra. This is an instance of an island, undisturbed by volcanic activity, in which erosion and denudation are constantly at work forming the coast. Herr Strass has a paper in the same number dealing with the statistics of emigration from Germany between 1871 and 1884. No. 8, which is the last published, contains a report by Herr Pohle on the expedition sent in 1884 to that part of the coast of South-Western Africa between the Orange River and Walfisch Bay, which came at that time into the possession of Herr Lüderitz. The expedition was intended mainly to ascertain what useful minerals existed in the new territory, and also to study its fauna, flora, and soil. The report is one of considerable length, and deals with all these points. The paper on the forests of North America is based on Prof. Sargent's report, contained in the ninth volume of the United States Census Report for 1884.

The *Proceedings* of the Royal Geographical Society for August contain several papers of interest. In "Recent Portuguese Explorations in the Zambezi Region" two journeys are described—one between the Zambezi and Pungué, the other between Tete on the Zambezi and Makanga. Mr. J. W. Wells, in a short paper, contributes some information on the delta of the Tocantins, in Brazil, and there is also a summary (the first, we believe, that has appeared in English) of the Von den Steinen exploration of the Xingu. The results of this expedition confirm the conclusion with regard to the geology of the interior of Brazil arrived at by the late Prof. Hartt, and by Mr. Wells, that south of the Amazon valley the whole interior of Brazil was at one time an immense plateau, and that the changes which it has undergone are due to water denudation. But the most important communication in this month's *Proceedings* is the report of the measures adopted by the Council of the Society for the improvement of geographical education. These are of two classes, to be carried out with the co-operation or assistance of the two Universities and the Education Office respectively. Under the first head the Council offer to appoint and pay a lecturer or reader in geography to deliver courses of lectures at both Universities, arranged so as to suit students in the Honour Schools; or, in the alternative, to join with both Universities in appointing and paying a reader in geography. In addition, the Council offers to contribute the funds for an exhibition. In connection with the Education Office, the Council offers various prizes in money and books to pupil teachers. Further, a donation of 30*l*. for the present year is made in aid of the geographical lectures in the University extension courses; copies of the *Proceedings* are to be sent to various public school libraries, and travellers and geographers are to be put in communication with the head masters of public schools. The proposal for a readership in geography at the Universities is obviously the most important of these, and the result of the communications now passing between the President and the Vice-Chancellors will be awaited with much interest.

The last number (Bd. xxix. No. 4) of the *Mittheilungen* of the Geographical Society of Vienna has for its first article a discussion, by Prof. Penck, of the proportion of the areas of land and water on the surface of the globe. The writer gives at the outset an interesting sketch of the history of the subject, and of the various theories which have prevailed from time to time on the subject, beginning with Columbus, who thought the proportion of land to water was as 6 to 1. Starting from the generally accepted proportion of Wagner as that of 1 to 2·76, Prof. Penck advances various reasons for believing this to be unreliable, especially our ignorance of the regions around the North and South Poles. The blanks on our maps are still too numerous and important to permit of any reasonable approach to accuracy being made. Dr. Paulitschke writes on the hydrography of the Upper Webi, one of the two hydrographic problems of the Somâli peninsula, the other being the Juba, which Capt. Cecchi calls *complicata e misteriosa*. The writer appears to throw much light on the first from his own explorations. He thinks we must seek the source of the Webi in one of the lakes of Gurage. Prof. Blumentritt criticises that part of Dr. Montano's recent work on the Philippines which deals with the ethnology of Mindanao. Accepting for his present purpose (though he refuses to do so as a general proposition) Montano's division of the inhabitants into three main heads—Negritos, Indonesians, and Malays—he advances various reasons for holding that that writer does not arrange the tribes of Mindanao accurately under these heads. These are based chiefly on the languages; but if it does nothing else, the paper demonstrates the wide knowledge which Prof. Blumentritt possesses of these regions. Indeed, for years past he has made every department of research connected with the Philippines his own, until now he is without a living rival.

A report has been received at the Hydrographic Department of the Admiralty from Commander Moore, of the surveying-vessel *Rambler*, relative to the existence of an island lying between L'Echiquier group and Durour Island, recently discovered by Mr. Allison, commanding the British steamer *Fei Lung*, when on the passage from Sydney to Shanghai. This island is covered with trees, and appeared to be 2 or 3 miles long in a north-west and south-east direction, and 100 to 150 feet in height. Both Durour Island and this were visible at the same time from the *Fei Lung* when passing between them. The approximate position as reported is lat. 1° 25′ S., long. 143° 26′ E. The Hydrographer to the Admiralty says that, unless the positions of the islands already on the chart are more inaccurate than they are believed to be, there is little doubt that the island now reported is a new discovery.

The August number of the *Scottish Geographical Magazine* contains a translation of Col. Fontana's lecture on the Patagonian Andes to the Argentine Geographical Institute, describing his recent journey from Chubut to the slopes of the Andes. The latter are mentioned with great enthusiasm: they teem with fertility, and Nature is as exuberant there as farther north in the Gran Chaco. Finally, the sub-Andean portions of Patagonia are described as the country of the future, being another added to the long list of countries of the future.

In tome x. fascicule 5, of the *Bulletin* of the Geographical Society of Antwerp, M. van den Gheyn discusses the question whether there is unity, duality, or plurality of races in Australia, and comes to a conclusion in favour of unity. He thinks that the differences found amongst them are to be explained by mixture with the Indonesians on the one side and the Polynesians on the other.

The French Minister of Public Instruction has intrusted M. Alfred Marche with a mission to the Marianne Islands to study the geography, natural history, anthropology, and ethnography of the Archipelago.

THE AUGUST PERSEIDS

The shower of Perseids has been a fairly conspicuous one this year notwithstanding the somewhat unfavourable circumstances attending the display. On the nights of August 9, 10, and 11 the nearly-full moon was visible during the greater part of the time available for observation, and robbed the phenomenon of its chief prominence during the evening hours. Those, however, who continued to watch the heavens until after the moon set on the early morning of the 11th must have been rewarded by a tolerably rich exhibition of meteors. The number observable by one person fell little short of 100 per hour, and this rate compared with similar observations in past years proves the late display to have fully maintained its decided character. Numerically this shower of Perseids cannot be placed in the same category as the brilliant meteoric storms of November 13, 1866, and November 27, 1872 and 1885, but it must be remembered that the August shower is one which returns *annually*, and apparently without much variation in its leading features. Its frequent and regular appearances compensate for whatever it lacks in other respects, and it yields many fine meteors of the same type as the Leonids, flashing out with remarkable swiftness, and projecting lines of phosphorescence upon the background of the sky.

The importance of watching every recurrence of the leading meteor showers is acknowledged on all hands, for if we would successfully trace out the modern history and developments of these wonderful systems we must first carefully secure the materials to form the basis of such investigations.

With reference to the shower of Perseids this year, the observations were much interrupted by cloudy weather. Preparation had been made here to commence a look-out during the last week in July for *avant-couriers* of the stream, but the nights were persistently overcast, and it was not until August 2 that a good view was obtained. Clouds were, it is true, somewhat prevalent before midnight, but afterwards the firmament became very clear, and it remained uniformly serene until daybreak.

Watching the eastern sky between 10h. and 14½h., I counted 50 meteors, and of these 12 were Perseids displaying the normal features. The radiant-point was, at 33° + 55°, not very sharply defined. Some of the best observed paths were slightly discordant, and gave the impression that the focus of divergence was diffused over an area of 3° or 4° diameter.

The following night was cloudy, but August 4 came in very clear, and 58 meteors were seen between 10h. and 14¾h. Amongst these were 12 Perseids, and the radiant-point, more contracted and definite than on the 2nd, was now at 37° + 57°, having increased 4° in R.A. in the interim of 48 hours. The shower exhibited no increase in numbers between the 2nd and 4th; indeed, there appeared to have occurred a slight falling off on the latter date. But on the 4th I saw a duplicate shower of Perseids, the companion radiant being at 48° + 43°, between α and β Persei, and this position was accurately indicated from seven paths.

A cloudy period supervened between the 4th and 10th, but on the latter night the sky was very clear throughout, though the moon was up until 13h. 30m. Between 10h. and 14½h. I observed 152 meteors, though the watch was not persistent during that interval. The number seen included 122 Perseids with a radiant at 44° + 57½°. At 13h. 6 meteors were noted within 20 seconds, and after the moon had fallen below the horizon the shower developed into one of considerable activity. Between 14h. and 14h. 15m. I counted 22 meteors, so that they were coming at the rate of about 90 per hour for one observer. Some of them were unusually bright. At 13h. 34m. a splendid Perseid appeared in the northern sky, pursuing a path of 13° from 77° + 67° to 111° + 67°. It lit up the whole heavens with a momentary flash, and left a luminous streak, near the end of its path, that remained visible to the eye for nearly 4 minutes. This was by far the most conspicuous meteor seen during the night, and it will probably have been recorded at many other places.

On August 11 the sky was partly clear between 10½h. and 11¾h., and 22 meteors were noted, including 15 Perseids from 47° + 57½°. Thus the position of the radiant showed a still further displacement towards the east. The shower had declined greatly since the preceding night, and offered little attraction in the presence of the bright moonlight.

The shifting radiant of the Perseids forms one of the most curious and important details of its display. I first mentioned this feature in NATURE, vol. xvi. p. 362, and have been much interested in reobserving it on many subsequent occasions. Comparing the four positions determined this year, and one obtained on August 13, 1885 (NATURE, vol. xxxii. p. 415), the character of the displacement is well shown, and corroborates the figures given in the *Monthly Notices*, December 1884, pp. 97-8:—

1886, August	2	33° + 55°	12 meteors
	4	37 + 57	12 ,,
	10	44 + 57½	122 ,,
	11	47 + 57½	15 ,,
	13	51 + 58	6 ,,

On the whole the recent shower may be justly regarded as one fully answering to expectation. It has been quite equal, if indeed it has not surpassed, the Perseid displays as I observed them in 1869, 1871, 1874, 1876, 1877, 1878, and 1880. It is, however, somewhat difficult to institute perfectly fair comparisons. The circumstances affecting two displays are rarely if ever identical. In some years the shower escapes suitable observation owing to cloudy weather just at the important time. In others moonlight nearly obliterates it. We must also consider that, as the main richness of the stream is limited to a short interval, it will occasionally elude us by occurring in daylight. These varying conditions and hindrances render it unsafe to draw conclusions as to the relative aspect of the annual displays unless the evidence is very complete and satisfactory.

It is well known that an unusually large number of minor systems occur simultaneously with the August Perseids. The positions of many of these are now ascertained with considerable precision. The labours of Heis and Schmidt, ably supplemented by Greg, Alex. Herschel, Zezioli, and others, have furnished a multitude of observations which are satisfactorily accordant as to many of the secondary showers of the epoch. The results obtained in the present year have been extremely productive of tenuous radiants. I select five of these as affording instances of very definite showers:—

No.	1886	Radiant ° °	No. of meteors	Features
1	July 27–Aug. 11	291+51	14	Rather slow.
2	July 31–Aug. 11	350+51	11	Rather swift.
3	August 2–11	48+43	10	Swift, streaks.
4	August 2-4	26+42	6	Swift, streaks.
5	July 31–Aug. 2	20+58	7	Swift, streaks.

Nos. 1 and 2 I observed also in August 1885 (see NATURE, vol. xxxii. p. 415), when I derived their radiants at 292° + 52° and 345° + 53° respectively. No. 3, between α and β Persei, I observed in July and August 1877, and again on July 23-25, 1884 (*Monthly Notices*, December 1884, p. 107). No. 4, near γ Andromedæ, has also been pre-observed here in August 1877 and 1879; and No. 5 represents the Cassiopeiads, which have long been known as a pronounced companion shower to the Perseids.

The position No. 2 at 350° + 51° lies between Cassiopeia and Lacerta. It was the most prominent of all the minor streams of the August epoch in 1885, and in 1877 I had observed it well both in July and August. It has also been noticed by many others in recent years. Taking an average of fifteen different observations the radiant comes out at 350°·2 + 52°·1. This particular shower, by its increasing activity during the past few years, appears to have supplanted Mr. Greg's *Lacertids* at 335° + 52°, which have evidently not maintained their former strength. It is probable also that during the period of Mr. Greg's researches this August shower at about 350° + 52° was comparatively quiescent, for there is no reference to it in his catalogue of 1876. The same may also be said of the system of Cygnids at about 291° + 51° (near θ Cygni). Possibly, however, the latter may have been formerly confused with the Draconids (= Greg, No. 78). In the "Annuaire pour l'an 1885, publié par le Bureau des Longitudes" I find that two of the chief showers accompanying the Perseids on August 9-14 are stated as at 345° + 50° and 294° + 52°. My recent observations just described confirm this pair of showers in the most definite manner, and they will doubtless be similarly corroborated wherever systematic observations of the Perseids are conducted.

W. F. DENNING

THE SWISS SOCIETY OF NATURAL SCIENCES

THE annual meeting for this year of the Swiss Society of Natural Sciences opened at Geneva on the 10th instant under the presidentship of Prof. Louis Soret. This precursor of all itinerant scientific societies was founded in 1815 in Geneva, and the present is its seventh meeting in the city of its birth. The members and visitors were received on the evening of the 9th in the *salons* of the celebrated Palais Eynard, which, after being long closed, were opened specially for the occasion. After the presidential address on the 10th, a new committee for the forthcoming period of six years was appointed, with its seat at Berne, the next meeting was fixed to take place at Frauenfeld, in Thurgau, and Prof. Grubenmann was elected president.

Prof. Soret in his address first referred to the advantages offered by Geneva to men who have taken science for their vocation, and then, under the title of "*Des impressions réitérées*," developed a series of new and original ideas on æstheticism analysed by the man of science. The repetition, he said, of the same design, whether in a symmetrical form, or in lined designs, such as we see in tapestry, furniture, or buildings, whether of the same dimensions, or of dimensions regularly decreasing, gives an agreeable impression. It is the same with regular curves, but the æsthetic sensation dwells less in the sensation itself than in the intuition which it gives us of a law. This applies not only to form, but also to sound and to colours. Developing these ideas, M. Soret insisted on the part played constantly by repetitions and similitudes because they evoke by intuition the idea of a law.

M. Marcel Deprez then read a paper on the transmission of force by means of electricity, in which he described his recent experiments between Creil and Paris and the results. M. Rilliet, of Geneva, read the report of a commission appointed to investigate the depth to which light penetrates water. Dr. Heim, of Zurich, read a paper on the deformation of fossils in mountains. He described the modifications which rocks undergo in form even after induration. Under the enormous pressure of the rocks above they may become laminated without any visible solution of continuity in the mass, or any rupture.

The effect on the fossils which they contain is similar; these are sometimes enlarged into the most grotesque forms, and hence Agassiz was misled in distributing the fossil fish of the older rocks into eighty distinct species, a considerable number of which were of the same species but deformed in various ways so as to appear different.

Subsequently the members were present at the inauguration of a monument erected to the memory of Gosse, the founder of the Society. Much interest was attracted by the new geological map of the Republic exhibited in the hall. It has just been completed, and is the fruit of twenty-seven years of the labour of a number of geologists under the superintendence of M. Alphonse Favre, who has now the satisfaction of seeing the end of this great task.

On the second day, in the Botanical Section, Prof. Müller, of Geneva, spoke of his systematic researches into the lichens of the *Graphidæ* group, of which he is about to make a general revision; Dr. Fischer, of Berne, described a new fungus (*Hypocrea*); Dr. Nuesch, of Schaffhausen, read a paper on the origin of Bacteria; and Prof. Schuetzler described a curious moss which grows at a depth of 200 feet in the sub-lacustrine moraine of Yvoire. It contains grains of chlorophyll perfectly formed, and is probably a variety of *Thamnium alopecurum*. Prof. Magnus, of Berlin, recounted his observations on the fecundation amongst aquatic plants, and more especially species of the *Naias*. M. Pittier spoke of the modifications being slowly made in the Vaudois flora, certain plants having disappeared wholly, while new ones have taken their places. M. Casimir de Candolle described his investigations into the action of low temperatures in germination.

In the Section of Zoology and Physiology Prof. Auguste Forel communicated a written memoir of the perception of violet by ants. He came to the conclusion that they perceived it with their eyes, and not through the skin. The so-called photodermatic sense does not appear to exist in ants, or at least is of small importance compared with ocular vision. M. Goll read a paper recording his observations on the fauna of Lower Egypt, especially of the fishes of Fayoum. There exists a well-marked distinction between the fauna of the desert and that of the Nile, particularly in colour. Dr. Zschokke gave some details on the development of the *Scolex polymorphus*, a kind of parasitic worm which he studied at the Naples Zoological Station. He thinks that Wagener's classification of the Scolices is not a natural one. Prof. Blance, of Lausanne, continues his studies of the fauna of the Lake of Geneva, and presented a memoir on a new Protozoa which he discovered in a deep part of the lake, and which he names *Gromia brunneri*.

In the Geological Section Dr. Schmidt, of Freiburg in Brisgau, read a paper on the geological and mineralogical nature of the schists of the Grisons, which, it is now demonstrated, belong to the Jurassic. He spoke particularly of the mica which is one of their constituent elements. MM. de Fellenberg and Baltzer described the remains of great vegetable fossils found at Guttanen in the crystalline schists in the mass of Finsteraarhorn. M. Greppin exhibited a beautiful collection of fossils, not yet determined, of the oolite, found in the Jura in the neighbourhood of Basle. Prof. Renevier, of Lausanne, read a report on the excursions made by the Swiss Geological Society in the Vaudoise higher Alps during the five days preceding the meeting. M. Schardt described the geological structure of the Dent du Midi. M. Steinmann gave an account of a journey extending over two years in the Cordilleras of South America, between Bolivia and Patagonia. He sketched rapidly the characteristics of this great chain. The fossil fauna and flora are almost identical with those of European formations. The Upper Trias, Rhætian, Lias, Jurassic, and Cretaceous are all represented.

Of the pleasures as apart from the business of the meeting it is needless to speak. The Genevese authorities and people gave the members a hearty reception, and the whole town was *en fête*. Among the honorary members elected was Dr. J. H. Gladstone.

THE BRITISH MEDICAL ASSOCIATION AT BRIGHTON

THE annual meeting of the British Medical Association is anticipated not only as an occasion for the association and communion of medical men of all classes, but as an opportunity for, so to speak, taking stock of the progress of medical science and practice during the past year. From the choice of a locality near the metropolis, the meeting this year has been very successful, both as to the numbers attending it and the character of the papers read. From the tone of many of the addresses, indeed, it is easily perceived how intimately chemistry, physiology, biology, and even physics are becoming associated with medicine, and how, as a result of this, the special medical departments of pharmacology and therapeutics, pathology and hygiene, are being modified by scientific methods of investigation.

The subject chosen by the President, Dr. Withers Moore, for his address, viz. the higher education of women, was one which, though of interest to all classes of the community, did not lend much scope for the introduction of new matter. The chief argument on the medical aspect of the question brought forward by Dr. Moore was the statement that the extra tax on woman's intellectual faculties produced by this "higher education" leads to bodily degeneration and to unfitness of the individual for a woman's peculiar social duties. This is admittedly so with those who are subjected to over-pressure; still, the questions as to how far these bad effects are general among the class of women who are subjected to severe intellectual training, and how far these bad effects may be counteracted by judicious hygienic surroundings, remain yet to be solved; and the experiments in the higher education of women now being performed in America and England will no doubt yield results which will practically solve the question.

The address in Medicine, which was given this year by Dr. J. S. Billings, of the United States Army, dealt chiefly with medical politics in America, which, like our own country, needs reform in reference to medical education. It is interesting to note, from the remarks of one so well qualified to judge as Dr. Billings, the great progress made in America in the establishment of laboratories devoted to scientific medical investigation; and it may be confidently expected that by this means important contributions will be added to the stores of medical science.

It is in the departments of pathology and pharmacology that the influence of scientific thought and method is most evident. As Dr. Dreschfeld pointed out in his address before the Section of Pathology, there is in the modern study of pathology a great deal more than was comprised twenty or thirty years ago; for, besides the marvellous advances of morbid anatomy due to the improvement in histological methods and knowledge, the stimulus of experimental physiology has initiated important researches on various morbid processes. Indeed it is difficult to draw a hard-and-fast line between experimental physiology and pathology; for, in many instances, the investigation of physiological function proceeds *pari passu* with that of the loss or inhibition of that function. On the anatomical side pathology is seizing the facts discovered by purely scientific investigators, and applying them with good results. Thus, as Dr. Dreschfeld points out, the application of the researches of Flemming, Heuser, Rabl, and others, on the composition of the nucleus, to the study of the cancer-cell, has shown that this is deficient in chromatin and embryonic in character. Again, the selective action of methylene-blue for certain nervous structures when injected into the living body, as described by Ehrlich, indicates a new method of pathological research by which the condition of these structures under the toxic action of substances may be investigated. If, moreover, as Ehrlich thinks, this selective action is due to the conditions of alkalinity and oxidation in the structure, some light may be thrown by future research on the still very obscure reactions of the nucleus and cell, and, more particularly in pathology, on the chemical changes occurring in the nerves in chronic peripheral paralysis due to poisons, such as alcohol and lead. Though a strong advocate of the study of experimental pathology, Dr. Dreschfeld insisted on the necessity of an investigator having a clear idea of the object and, as far as possible, of the methods of the research which he is undertaking. This point, which is of course the basis of all useful experimentation, is very important in experimental pathology, owing to the peculiar conditions under which experiments on animals are performed in this country.

In pathology, which deals more closely with the facts of disease—disordered structure, disordered function—progress has been rapid, but not more so in its scientific aspect than pharmacology and therapeutics. A great deal of attention has of late years been devoted to this subject, as shown by the rapid accumulation of facts concerning old and new remedies. It is on such an occasion as the meeting of the Association that it is well to stand still for a moment and see the direction in which modern therapeutics is tending. Connected on the one hand with chemistry and physiology, and on the other with pathology and medicine, it is justifiable to expect that the recent advances in these departments of knowledge would have a stimulating effect on the progress of therapeutics.

Dr. Lauder Brunton, in his address (which we print in full) before the Section of Pharmacology, illustrated one aspect of this influence by discussing the connection between chemical constitution and physiological action. It will readily be seen from a study of his remarks how important an effect the line of research which he indicated will have on the progress of rational therapeutics, which is based on a knowledge of the physiological action of a drug. Dr. Brunton's address shows a hopeful sign of advance in the treatment of disease by scientific methods and not by mere empiricism.

One of the most important communications made to the Association, and deserving of mention here, was that by Prof. O. Liebreich, of Berlin, on lanolin as a therapeutic agent. This substance, which is a cholesterin-fat from sheep's wool, is much more rapidly absorbed by the skin than glycerol-fats or vaselin, this property being probably connected with the fact that in nature it is closely associated with, if not formed by, keratin-containing cells, such as those of the skin, hair, feathers, &c. Such a readily absorbable fat, which is unirritating, and will serve as a vehicle for medicaments, has long been a desideratum, and it is probable that lanolin will be a most important agent in the treatment of skin diseases and of local disorders beneath the skin, as in the joints.

Space does not admit of a discussion of the numerous other interesting subjects, chiefly technical, introduced at the meeting of the Association. The interesting questions brought forward by Dr. Taafe in his address on public medicine included the spread of scarlatina by means of milk, a subject the investigation of which has been undertaken by the Local Government Board, and will no doubt yield important results to preventive medicine.

ON THE CONNECTION BETWEEN CHEMICAL CONSTITUTION AND PHYSIOLOGICAL ACTION[1]

THE meeting of the British Medical Association is not for mutual instruction only; it is also for recreation; and, probably, many members of this Association will utilise the opportunity which a meeting at the sea-side, like the present one at Brighton, affords them of indulging in that excellent occupation for an idle man—of watching the waves on the sea-shore and speculating how far each of them will come. If one have only half an hour to spare, it is difficult to say whether the tide is ebbing or flowing; it is only by watching for a longer time that one can be certain that the water is really moving in one direction or another. Probably a great part of the charm which this occupation possesses is due to the resemblance which one involuntarily traces between the ebb and flow of waters and that of human affairs—individual, national, or racial. The life of a single man is very short in comparison with the history of race; and it is often very difficult to say whether mankind is advancing or retrograding, unless we compare his condition at epochs widely removed from one another.

On doing this, we find a general consensus of opinion, to the effect that civilisation has steadily advanced; and this advancement is usually divided into four stages, characterised by the nature of the tools or weapons employed. In the first, or Palæolithic Age, man employed weapons or tools of flint roughly chipped into shape and unpolished. In the next, or Neolithic Age, the implements consisted of stone, but they were polished. The next age is characterised by the employment of bronze as a material, and the fourth and highest stage by the employment of iron. These stages are not all marked off from one another, for we find them together in the same country or in different countries. Thus, the age in which at present we live is recognised as the Iron Age, on account of the large employment of that metal; but we find that in various countries stone, more or less rudely fashioned, is still used in the manufacture of weapons or tools. For example, when I was in the Colonial Exhibition lately with Mr. Norman Lockyer, he pointed out a kind of threshing implement, such as is now used in Cyprus. It consists of a flat board, in the under side of which are embedded a number of stone celts exactly like those made by prehistoric man, and perhaps used by him for a similar purpose as well as for axes. In the same way that we recognise four stages in the development of the implements used by man in the arts or in warfare, we may, I think, recognise four stages in the development of the implements he has used in the treatment of disease. In the first stage crude drugs were employed, prepared in the roughest manner, such as powdered cinchona or metallic antimony. In the next stage these were converted into more active and more manageable forms, such as extracts or solutions, watery or alcoholic. In the third stage the pure active principles, separated from the crude drugs, were employed, *e.g.* morphine and quinine. In the fourth stage, instead of attempting to extract our medicines from the natural products in which they are contained, we seek to make for ourselves such substances as shall possess the particular action we desire. Now, just as we find stone and iron implements occasionally used together in the same country, so we find that drugs belonging to the different stages mentioned are used at the same time. For example, we may find crude powders, alcoholic extracts, and pure alkaloids all contained in the same pill. Nay more, we may sometimes give to the patient in addition to all these, a medicine made artificially. But, while this condition still exists, we notice that crude drugs are being less and less used, and their place is gradually being taken by pure active principles. We may say, then, that we are passing at present from the Stone Age into the Bronze Age of pharmacology; and may indeed be said to be just entering on the Iron Age. This age may be said to have begun about twenty years ago, when the researches which my predecessor in this office, Dr. Fraser, made with Prof. Crum Brown upon the connection between physiological action and chemical constitution, inaugurated a new era in pharmacology. They found that, by modifying the chemical constitution of strychnine, they could also alter its physiological action, and convert it from a poison which would tetanise the spinal cord into one which would paralyse the motor nerves.

We might perhaps date the beginning of this age from Blake's attempts to show that a connection exists between the form in which various bodies crystallise, and the mode in which they act upon an animal body. Richardson, too, had observed that, amongst various compounds of carbon, certain differences existed in physiological action which might be supposed to correspond to differences in their chemical composition. And at the same time that Crum Brown and Fraser were making their experiments, Schroff in Vienna, and Jolyet and Cahours in France, had independently arrived at somewhat similar conclusions; nevertheless, I think we may fairly say that it was the experiments of Crum Brown and Fraser which fairly started pharmacology in the new direction in which it has since been steadily advancing. It would be impossible for me to enter at all fully into the recent development of this branch of research, but I think it may be both interesting and useful to try to give you a short and popular account of the chief points already made out; and, in doing so, I may perhaps be excused for using, almost to the extent of abusing, similes which are not precisely exact, but which may be useful in giving you a rough idea of a somewhat complicated subject.

We have all heard of the "flesh-pots of Egypt"; but I find that everybody is not acquainted with the "flesh-pots of Shiloh," though "good little Samuel" has probably been frequently held up before us as an example to be followed, and possibly the naughty sons of Eli as an example to be avoided. When these sons of Eli were priests in Shiloh, their custom was, when any man offered a sacrifice, to send their servants with a "flesh-hook" of three teeth, in his hand, which he struck into the pan, or kettle, or cauldron, or pot; and all that the flesh-hooks brought up the priest took for himself.

It is obvious that what the priest's man brought up would depend very greatly on two things, viz. the contents of the pot and the nature of the hook—whether it were large or small, sharp or pointed, single-pronged or many-pronged. It is obvious, too, that a very slight alteration of the points, by the judicious application of a file or whetstone, might considerably influence the savouriness of the priest's dinner. With the small pots that they were likely to have in Shiloh, it would not matter much what the nature of the handle was; but it would matter

[1] An Address delivered at the opening of the Section of Pharmacology and Therapeutics, at the Annual Meeting of the British Medical Association held in Brighton, August 1886. By Thomas Lauder Brunton, M.D., F.R.S., Lecturer on Materia Medica and Therapeutics at St. Bartholomew's Hospital; President of the Section.

very greatly if the priests had to go fishing in the brazen sea of Solomon, for there, with a short handle, they might not be able to reach the tit-bits in the middle, and if the handle were too long, they might go plunging their hooks about the opposite side of the vessel, with the same result as if the handle were too short. Now, in the drugs which we use in medicine, we may find a certain analogy with these flesh-hooks, some part of the drugs being comparable to the hooks, and others to the handle. Perhaps the analogy would be even more correct if we were to regard the hooks as having movable points, which could be taken off and replaced by others of a different form or sharpness. If we take alkaline salts as an example, we may regard the base as the handle, and the halogen as the hook; and by modifying either of these, we may alter the parts of the body affected and the manner in which they are affected. We might, indeed, compare chloride of sodium, in which we have the chlorine attached to sodium, with the low molecular weight of 23, to a hook with so short a shank that it did not reach the big joints lying in the middle of the cauldron; while potassium, with a molecular weight of 40, was just long enough to do this; and rubidium, with a mol ecular weight of 85, was so long as to go plunging about on the other side. In fact, we find that this is very nearly what occurs in the muscles of the animal body after the administration of the chlorides of sodium, potassium, or rubidium; for, while potassium chloride is a powerful muscular poison, the action of sodium and rubidium chlorides on the muscles is very slight.

We have seen what changes would follow alterations in the shank of our flesh-hook; now let us see the effect of altering the prongs. If we put on a small one like chlorine, it may go dragging about catching everything, but bringing out nothing; a bigger one, like bromine, may lay hold of a lung or a brain; and a bigger one still, like iodine, may lay hold of a big joint. Now, what we find in the body seems to be somewhat similar. The chlorides circulate in the blood without producing any marked alteration beyond that which is due to the substance with which the chlorine is combined. The bromides attack the brain and nerve-centres, and the iodides tend more especially to affect the muscles and the glands.

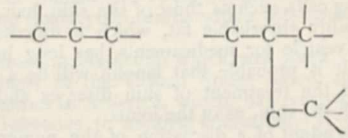
It is evident that another important factor besides the sharpness of the hooks is the number of prongs, and the three-pronged hook seems to be the generally effective one. Now, in pharmacology, there is one substance—nitrogen—which appears sometimes to have three, and sometimes five prongs, or affinities, as chemists term them, and it is a substance having a very general and powerful influence over the body. When combined with hydrogen in the form of ammonia or of ammoniacal salts, it affects nerve-centres, motor nerves, and muscles, tending first to stimulate and then to paralyse them. But, as we would expect, the effect of the ammonia is modified by its combination with iodine, chlorine, and bromine; and we find that, while the ammonium-chloride generally attacks the spinal cord and causes irritation, ammonium-iodide paralyses the motor nerves and muscles.

When nitrogen has oxygen combined with it in place of hydrogen, so as to form nitrous acid, its action is exerted more especially upon the blood and blood-vessels, so that it causes the blood to become chocolate-coloured, and the blood-vessels to dilate. This power of dilating the vessels is sometimes exceedingly useful in the treatment of disease; and we have been enabled to vary the action of our drugs so as to attain, to a great extent, the end we desire, by our knowledge that the action depends upon the nitrous acid, and not on the substance to which the acid may be attached; or, to return to our own comparison, the effect depends on the nature of the hook rather than on the kind of shank to which it is attached. Thus, where rapid dilation is desired, we use nitrite of amyl; but where a slower and more prolonged action is desirable, we employ nitrite of soda or nitro-glycerine.

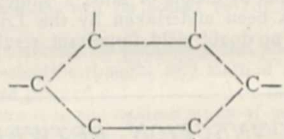
In some useful tools we have the two ends serving different purposes: one end, for example, being a hammer and the other end a claw for extracting nails; and we can easily imagine a flesh-hook constructed on the same principle, one end, let us say, having the prongs widely apart, and the other the prongs close together. With such a hook, it is evident that the viands which were fished up would be different according as one or other end was put into the pot, for the close prongs would bring up delicate little pieces which would simply slip through the wide ones. If we carry our illustration a step further, and suppose this hook to consist of two parts attached to one another by certain prongs, while others were left free, we can see that, if only one prong were left free in each part, but these prongs were of different shapes, the pieces obtained by the man using it would be of a different kind, according as the prong belonged to one end or the other. Now we seem to find something of this sort in the union of nitrogen with carbon. Carbon is a substance with four affinities, while nitrogen appears sometimes to have three and sometimes to have five. When the nitrogen and the carbon are united in such a way that four affinities of each are connected together, leaving one free affinity or prong belonging to pentad nitrogen, thus, $-N\equiv C$, the compound is exceedingly poisonous; whereas, when the free affinity or prong belongs to the carbon and the other three affinities are joined to triad nitrogen, thus, $-C\equiv N$, the compound is comparatively innocuous.

This fact shows us how very important the nature of the free affinity in the compound is in regard to physiological action.

We have just pictured to ourselves an instrument of two parts, joined together by small hooks, and consisting, in fact, of two links. In this instrument the links differ a good deal from each other; but one link—namely, carbon—has a great power of uniting with itself, so as to form long chains, straight or branching, thus—

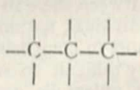


It also forms what we may possibly regard as close chains, so stiff as to act the part of a shank, to which single hooks or long open chains may be attached. We may represent it graphically thus—

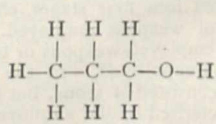


Now, if any of Eli's successors wanted to fish in Solomon's brazen sea with hooks attached to a flexible chain instead of to a stiff shank, the results of his fishing would not only depend on the hooks he used, but on the length of the chain, on the kind of chain, single or branching, and on the position of the links to which the hooks were attached.

Now, in the series of chemical substances to which alcohol belongs we have an illustration of the modifications in physiological action which are produced by the length of the chain, the kind of chain, and the position of the hooks. The links, in the case of alcohol, consist of carbon atoms attached to each other by one affinity, so that each terminal atom, or link, has three affinities, or prongs, and the intermediate links have two each unattached, thus—

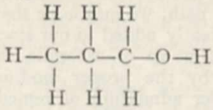


We may regard one prong of one terminal link as furnished with a sharp point, to which we give the name of hydroxyl, while all the others are furnished with blunt hydrogen points, thus—

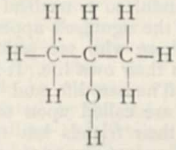


All the alcohols attack the nerve-centres, and paralyse the brain, the spinal cord, and the centres of organic life in the medulla oblongata. In large doses they all produce death, and the longer the chain the more deadly do they become, until the chain is so heavy that it can hardly be used at all, or, in other words, till the alcohol becomes so solid that it will not readily enter the body and produce its toxic action.

If we fix the sharp hydroxyl on one of the intermediate links, instead of the end one, we would naturally expect that it might simply scratch the pieces of meat instead of pulling them out, as it might do if it were attached to the terminal link; and this is exactly what we find in the case of alcohol. For example, primary propyl alcohol,—

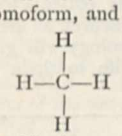


where the hydroxyl is attached to the terminal link, appears to produce steadily increasing paralysis of the nerve-centres; but secondary propyl alcohol, where the hydroxyl is attached to an intermediate link, thus—

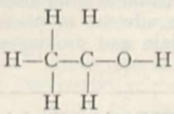


scratches up or stimulates the nerve-centres before it paralyses them (Efron Pflüger's *Archiv*, Band xxxvi., 1467).

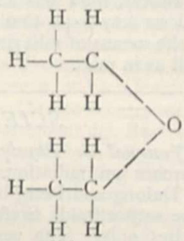
The whole of the carbon compounds, formed on the principle of an open chain, appear to have an action more or less like that of alcohol, though these are modified by the nature of the substances which "tip," as it were, the free affinities of the carbon links. Thus, marsh-gas, alcohol, ether, chloroform, bromoform, and iodoform—



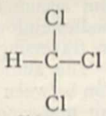
Marsh-gas



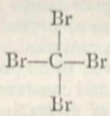
Alcohol



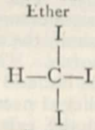
Ether



Chloroform



Bromoform

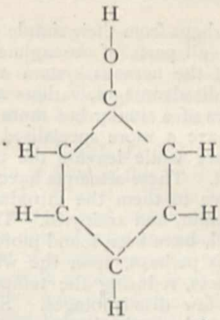


Iodoform

all tend to paralyse nerve-centres, and to exert an anæsthetic action; but the chloral in the chloroform tends to make the substance paralyse the heart more quickly than marsh-gas, alcohol, or ether, which contain hydrogen alone, or hydrogen and oxygen; and in iodoform the effect of the carbon is to a great extent swamped by the iodine.

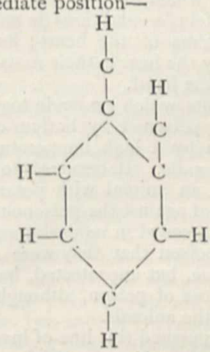
It is to Liebreich's recognition of the fact that similar carbon compounds possess a similar anæsthetic action that we owe the discovery of chloral. The knowledge of the depressing action on the heart of chlorine in such compounds led Schmiedeberg and Cervello to search for a hypnotic substance which should not contain chlorine, with the result that paraldehyde has been added to our therapeutic *armamentarium;* and the stimulant action of ammonia led Schmiedeberg to introduce a new hypnotic, urethane, which, like chloral, will produce sleep, but, instead of weakening, will stimulate the heart, and is thus admissible in cases where chloral might be dangerous.

Let us now turn to the other class of carbon compounds in which the atoms are arranged so as to form a close chain, or, as we may call it, a stiff nucleus or shank, to which either single hooks or open chains may be attached. This group of carbon compounds is termed the aromatic series. The substances belonging to it differ from those of the open chain or fatty groups, inasmuch as they tend to stimulate the nerve-centres, and produce convulsions or spasms before paralysing them. But the most marked property which they possess appears to be their power of reducing temperature, and of destroying low forms of life, so that they act both as antipyretics and as antiseptics. We have seen that in the open chains of the fatty series of carbon compounds, the increased number of links appears to increase the activity of the compound, and a condition which is similar, in some respects at least, is to be found in the aromatic series. For example, in phenol or carbolic acid, as it is usually termed, we have one hydroxyl terminal, just as in ordinary alcohol; the other carbon affinities being saturated with hydrogen—

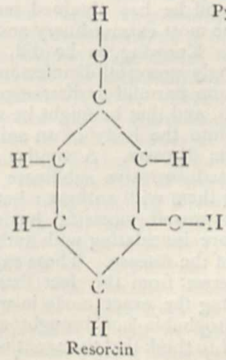


When these hydrogen atoms are replaced by methyl, the antiseptic power of the phenol is increased, and the increase appears to be in proportion to the number of methyl groups which are introduced into the compound. Turning again to our old illustration of the flesh-hooks, we might compare the benzene nucleus to the shank with six points, each of which might be armed either with a sharp hydroxyl hook, or with a blunt hydrogen one, or with a carbon chain. The more the blunt hydrogen hooks were replaced by chains, the more thoroughly would they sweep the pot; and, in fact, we may say that the more chains there are instead of hydrogen, the more thorough is the antiseptic action of the compound.

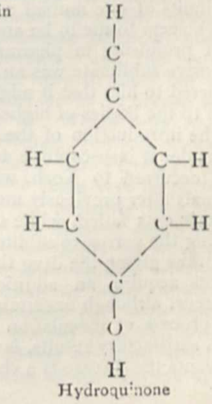
In the case of antiseptics, all that we want is to insure a thorough destruction of the microbes, which give rise to putrefaction or disease; but when we come to deal with antipyretics we have a more complicated problem before us, for we wish to reduce the temperature in man or the higher animals, while at the same time we have to avoid producing any marked action on the nervous system in the way either of spasms or paralysis, and also to avoid depressing the circulation and causing collapse. Now several bodies nearly allied to carbolic acid, and differing from it only in the fact that the benzene nucleus in them has two hydroxyl groups attached to it instead of one, as in carbolic acid, have a strong antiseptic power. These bodies are hydroquinone, resorcin, and pyrocatechin; they all have an antiseptic action, but the strength of their action is very different, resorcin having only one-third of the strength, and pyrocatechin only one-fourth of that of hydroquinone. This difference in strength shows us here, also, how important the position of the hydroxyl groups is; because, in pyrocatechin they are close together, in hydroquinone they are as far apart as they can be, and in resorcin they keep an intermediate position—



Pyrocatechin



Resorcin



Hydroquinone

But these bodies, perhaps from their simple structure, appear to be adapted to attack all parts of the animal organisation, and they are apt to affect the nervous system and circulation. In order to avoid these disadvantages, various attempts have been made to obtain bodies of a similar but more complicated structure, which should have a more specialised action, and would lower the temperature while leaving the nervous system and circulation unaffected. These attempts have been more or less successful, and we owe to them the introduction of three new remedies—kairin, thallin, and antipyrin. The former two, after a brief period of trial, have been found more or less unsatisfactory; but the latter is perhaps, upon the whole, the best antipyretic that we possess, reducing the temperature and, at the same time, having few disadvantages. Salicylate of soda is nearly allied in chemical constitution to resorcin, and as a general antipyretic it is almost equal to antipyrin, and superior to it in cases of rheumatic fever. It is possible that we may still obtain antipyretics more powerful than any we yet possess, and specially adapted to the febrile conditions arising from different causes, for these antipyretics do not appear to be equally successful in different kinds of fever. Antipyrin is best in hectic fever, and salicylate of soda in rheumatic fever, but an antipyretic which will be thoroughly satisfactory in typhoid fever is still a desideratum.

I have said that antipyrin is generally free from any disagreeable action; but this is not always so, for it sometimes may produce collapse. This shows us that in the action of all our drugs we have two factors to consider, namely, the drug itself and the body into which we introduce it. We have just been considering the alterations in physiological action which may be produced by changes in the chemical constitution of our drugs; but there is another factor which is perhaps more difficult to investigate, and still more important in the treatment of disease, namely, the condition of our patients. The failure of our drugs to produce the effects we desire is one of the most trying occurrences in medical practice. Thus, in fever, we sometimes find that drugs will not reduce the pulse as they do in non-febrile conditions, and digitalis in pneumonia sometimes appears to have lost its sedative action on the heart altogether. Some years ago I thought that possibly this might be due to the high temperature producing paralysis of the nervous apparatus which restrains the heart, and supposed that the peripheral ends of the vagus in the heart might be paralysed. I then made some experiments, which showed that I was wrong in this supposition. Several years afterwards my friend Dr. Cash and I made some further experiments, which showed that the failure of digitalis to slow the heart in febrile conditions is really due to paralysis of the regulating nerves of the heart; but the part of them which is paralysed by the heat is their roots in the medulla, and not their endings in the heart.

In other experiments which we made together we found that the muscle of a frog poisoned by barium could be restored to its normal condition by a high temperature, and also by the application of potash salts. It occurred to us that, if we could saturate the body of an animal with potassium, we should be able to render it proof against the poisonous action of barium. On trying this, we succeeded in rendering animals so far resistant to the action of the poison that they were alive and well after animals of similar size, but unprotected, had succumbed to the action of the same dose of poison, although we did not succeed in ultimately saving the animals.

But Dr. Cash has pursued this line of investigation far beyond the limits of our mutual research, and he has obtained results which seem to me to be amongst the most extraordinary and the most promising in pharmacology. Knowing, as he did, that corrosive sublimate was an exceedingly powerful disinfectant, it occurred to him that it might be more harmful to disease-germs than to the bodies of higher animals, and that he might be able, by the introduction of the poison into the body of an animal, to render it insusceptible to zymotic diseases. A similar idea had occurred to Koch, who injected corrosive sublimate into animals after previously inoculating them with anthrax; but his experiments failed, while Cash has proved successful by introducing the corrosive sublimate before inoculating with anthrax, and thus giving the drug the start of the disease. These experiments acquire an additional interest from the fact that M. Pasteur, although uncertain regarding the exact mode in which his process of inoculation for hydrophobia has brought about such satisfactory results, is disposed to think that the agent which prevents the disease is a chemical substance, and not a microbe.

When we look back for twenty years and see how far pharmacology has advanced since Crum Brown and Fraser's experiments directed it into a new path, we may hope that twenty years more may not only have greatly added to our stock of new remedies, but will have enabled us so to ascertain the condition of our patients that, either by the proper modification of a single remedy, by the proper admixture of remedies, or by proper changes in the food or surroundings of each patient, we may insure the action we desire, and we shall not have to feel, as we painfully do at the present, that our patients often die for lack of knowledge, not on our part, but on that of our art.

Nothing is more painful to a medical man than having to answer in the negative the agonised appeal, "Oh, doctor, can you do nothing?" of those who see passing away friends who are dearer to them than their own life. It is because we medical men know the value of human life and the extent of human suffering; because we are called upon to prolong the lives of those whom not only their friends but their country and the world at large can ill spare; because we must, if possible, relieve pain sometimes amounting to extreme torture in the sufferers themselves, and felt hardly less keenly by their friends, that we consider it is not only permissible, but is our imperative duty to gain the knowledge we require to attain our object, even though we sacrifice the lives of animals, and inflict upon them some pain—never wantonly, never carelessly, and almost always slight in comparison with what we often see our patients feel. Moreover, the lower animals suffer from disease as well as men, and we may hope that the advance of pharmacology will give us the means of relieving pain and prolonging life in them as well as in man.

SCIENTIFIC SERIALS

Journal de Physique, June.—P. Garbe, experimental researches on radiation. Examination of the formulæ proposed by Dulong and Petit, by E. Becquerel, by Violle, and by Stefan. The author holds Stefan's law to be true for absolutely black bodies only. The verifications have been made by spectrophotometric measures of glow-lamps fed from accumulators.—G. Wyrouboff, the structure of crystalline bodies endowed with rotatory power. This is a remarkable paper, traversing several conclusions hitherto believed to be proven. The author states that the alleged necessary and constant relation between rotatory power and the existence of facets indicating non-superposable hemihedry is untrue, for of eighteen such substances known, only four have been proved to have such facets, while the nitrates of lead and of baryta which are cubic with facets of this kind have no rotatory power. The author now propounds the view, which he supports by the discovery of striated structures upon the facets in question and by various strong arguments, that the real physical cause of this rotatory power is that such crystals consist of superposed laminæ crossing at different angles, and possessing biaxial refraction. In fact, he holds that these substances are only pseudo-symmetrical, and that the built-up mica plates of Reusch which show rotatory power are actual types of the phenomenon in general. He particularly refers to the optical behaviour of amethyst, and further declares that he has succeeded in proving that the true crystalline form of sulphate of quinine is clinorhombic. He regards as absolutely illusory, in the vast majority of cases, the so-called measurement of the angle of rotation by these substances.—L. Laurent, practical methods for the execution of objectives intended for instruments of precision. This paper describes means for testing during process of manufacture the curvatures, &c., of lenses intended for spectroscopes, goniometers, and such instruments.—Th. and A. Duboscq, saccharimeter for white light. This saccharimeter has a Senarmont polariscope placed between the polariser and analyser. The Senarmont polariscope consists of four wedges of quartz disposed so as to show two fringes with black central band, which in the dark field are situated exactly in line with one another. On introducing any substance that rotates the plane of polarisation, the fringes move right and left. A quartz compensator is added.—J. Voisenat, influence of nature and form of conductors upon the self-induction of an electric current. A summary of the recent papers of Hughes and H. F. Weber.—K. Ångström, on the diffusion of radiant heat from plane surfaces.—Ch. Soret, researches on the refraction and the dispersion of the crystallised alums.—E. Wartmann, the compensated rheolyser. This instrument consists of a circular modification of Wheatstone's bridge with mercurial conductors.—R. Pictet, new freezing-machines. Notes on industrial applications of a new liquid, namely a mixture of sulphurous acid and carbonic acid obtained on the commercial scale by the action of sulphuric acid on carbon.—J. Maurer, influence of altitude on diurnal variations of declination.—H. Schneebeli, absolute value of coefficient of friction of air. The results, which agree with Obermeyer, were made by Graham's method.—H. Schneebeli, experimental researches on the impact of elastic bodies.

Bulletins de la Société d'Anthropologie de Paris, tome ix. fasc. 2.—Continuation of M. Topinard's paper on the cephalic index. In his revised system of nomenclature M. Topinard virtually rejects Broca's method in favour of the quintuple division adopted by Prof. Flower, and generally followed by English and American anthropologists. For his old terms of "*sus*" and "*sous*" he further adopts those of "ultra" and "hyper." Thus, for example, while he considers that the true dolichocephalic group is represented by the index of 70-74 inclusive, his ultra- and hyper-dolicho- subdivisions exhibit respectively the indices of 60-64 and 65-69. The medium group standing between the dolicho- and the brachy-cephalic limits he characterises as "mesaticephalic," with an index of 75-79 inclusive; while his brachycephalic divisions range from 80, beginning with the mean representatives of the groups, to 94 as the extreme limit of the ultra-brachycephalic index.—At a later meeting, M. Topinard drew attention to the necessity for using greater exactitude in the definition of the methods to be employed in making anthropometric determinations, those of M. Bertillon as set down in his instructions regarding anthropometric identification being, in his opinion, at once complex and inexact.—On the so-called "Lenape" stone, by M. de Nadaillac, whose opinion of the possible genuineness of the stone is, as he informs us, based only on the testimony of others.—On the occurrence of amber in the prehistoric graves of the Département des Basses Alpes, by M. Bonnemère. These finds were formerly so frequent that the peasants in some districts used amber for lighting their dwellings, and hence it was locally known as "peïra cremarela," or burning stone. This name is still applied to it at Salignac, where many of the villagers are in possession of amber, all of which is more or less red in colour.—At a later meeting, M. Bonnemère described to the Society some curious bronze disks found by M. Ollivier in graves near Salignac, and which appear to have been used to strengthen the outer surface of cuirasses and other forms of protective armour. In a cemetery in Carniola, belonging to the early Iron Age, a helmet has been found composed of similar bronze knobs fastened in rows to an inner skin lining.—Anthropology and philology, by M. Beauregard. The object of the writer is to show the importance of the comparative study of languages to determine the usages, and moral and mental status of various nations. He specially passes in review the languages of Egypt and South Africa, Mexico, Peru, and the Red Indians, indulging in many fanciful deductions regarding assumed ethnic affinities.—On the origin of life, by M. Fauvelle. The author believes that modern science justifies the theory that the simplest forms of green Algæ represent the earliest manifestations of organised beings, in which chlorophyll was the active agent.—On impregnation, and the influence exercised on subsequent offspring by the first conception, by M. Fauvelle.—On the effects of long and short periods of military service in the French army on the health and physical development of the men, by M. Lagneau.—On the origin of the fabrication of glass, by M. Mortillet.—Morphological description of the brain of Gambetta, by MM. Chudzinski and Mathias Duval. This extremely minute report of the *post-mortem* examination, undertaken at the instance of the Society, is illustrated by numerous sectional drawings of the various convolutions, which exhibit a remarkable degree of complexity, and an unusual regularity in the arrangement of the folds.—M. Beauregard laid before the Society various objects obtained from the Gauchos of the Pampas, including the curious "botas de potro," or boots made from the skin of the hind legs of horses, mules, or oxen.—On the exploration of the tumulus of Kergouret at Carnac, in 1885, by M. Gaillard. A few implements and a diorite hatchet are almost the only finds yielded by the recent explorations of these dolmens, which were nearly destroyed, and their contents almost wholly removed at the time of their original discovery about twenty years ago.—Report, by M. Hamy, of the results of the explorations, conducted by M. Charney, in the mountainous region of Popocatapetl, in Mexico. Unusual interest attaches to these researches, which have brought to light the existence of two ancient Mexican cemeteries, in one of which the remains belonged exclusively to young children. Among the numerous fragments of bones were a great mass of broken jars and vases decorated with various emblems of the divinities Chalchiutlicue and Tlaloc, to the latter of whom young children were sacrificed on high places to secure rain.

Bulletin de l'Académie Royale de Belgique, June.—On the origin of the phosphate of lime in the brown chalk phosphatic beds of Ciply, by F. L. Cornet. These beds, which have a mean thickness of 21 metres, and about 18 per cent. of phosphate, are shown to be undoubtedly of animal origin, as attested by the large proportion of nitrous organic substances contained in them. The brown chalk appears to have been deposited in a shallow sea inhabited by a numerous fauna of invertebrates, fishes, and large Saurians. The deposits were slowly formed in tranquil waters during a long geological epoch, as shown by the great thickness of the phosphatic beds, the perfect regularity of the layers, and the state of the fossil shells found in them. These deposits, which occur nowhere else, may have been caused by the periodical destruction of fish, such as at present occurs annually in the Gulf of Aden.—Note on the parallelism between the Carboniferous Limestone of North-West England and Belgium, by L. G. de Koninck. It is pointed out that the synchronism of these various systems is far from being fully established. The Tournai formation would appear to be older than the English fossiliferous mountain limestone, while the Visé rocks may be contemporary of the Yoredale series. On the other hand, the zone of *Productus giganteus* seems to have acquired a far greater development in the north of England than in Belgium.—Remarks on the law regulating the tension of fluids, by P. de Heen. The formula recently announced by the author is here shown to apply not only to stable fluids, but also to those whose physical constitution varies with the temperature.

SOCIETIES AND ACADEMIES

SYDNEY

Linnean Society of New South Wales, June 30.—Prof. W. J. Stephens, F.G.S., President, in the chair.—The following papers were read:—Note on *Ctenodax wilkinsoni*, by William Macleay, F.L.S. It is here explained that the fish described by Mr. Macleay under the above name has been ascertained by Dr. Ramsay, of the Australian Museum, to be closely allied to *Tetragonurus cuvieri*, of Risso. Some remarks are also made on the habits and affinities of the fish.—Notes on the recent eruptions in the Taupo Zone, New Zealand, by Prof. Stephens, M.A., F.G.S. In this note the author gives particulars of the late volcanic disturbances, and such information as to the geographical and geological features of the district, as may perhaps enable those living at a distance to understand more clearly the accounts of the recent outbreak which have already appeared in the newspapers.—Notes on Australian earthworms, Part I., by J. J. Fletcher, M.A., B.Sc. Up to the present time but three Australian earthworms have been described, *Lumbricus novæ-hollandiæ*, Kinberg, and *Digaster lumbricoides*, Perrier, from N.S.W., and *Megascolides australis*, McCoy, from Victoria. In this paper a fuller account is given of Kinberg's species, and descriptions are given of six new or undescribed worms from the rich volcanic soil of Burrawang and of Mt. Wilson. Of these, two species (*P. coxii* and *P. australis*) are referred to Schmarda's genus *Perichæta;* two others (*N. camdenensis* and *N. grandis*, are included in a new *intraclitellian* genus *Notoscolex;* a fifth (*Didymogaster silvaticus*) also is *intraclitellian*, but differs from *Notoscolex;* and the sixth (*Cryptodrilus*) is *postclitellian*, with eight rows of setæ, but is different from *Digaster*. Three of these, as far as is known at present, occur only at Burrawang, one at Mt. Wilson only, one is common to both localities as well as Sydney, and one occurs at Burrawang, Springwood, and Jervis Bay. Mr. Fletcher has heard of the occurrence of worms, some of them very large, in the Hunter and Manning River districts, and probably these, as well as Illawarra, the Richmond and Clarence districts, and other parts of the colony will yield, when systematically searched, a good harvest of earthworms. He therefore appeals to the members of the Society resident in these or other localities, either for information or for specimens put alive into good methylated spirit, or sent alive packed in a tin box or large bottle, with a little earth and plenty of damp moss. Information as to the existence or otherwise of earthworms in the plains of the interior would also be very valuable.—Notes on the distribution of *Ceratella fusca*, Gray, from the coast of New South Wales, by John Brazier, C.M.Z.S. A number of instances are given of the occurrence of this Hydrozoon near the Heads of Port Jackson. Mr. Brazier also mentions that a specimen sent from the British Museum to the Australian Museum as *Ceratella fusca*, Gray, is really *Dehitella atrorubens*, Gray, from Algoa Bay.

PARIS

Academy of Sciences, August 9.—M. Émile Blanchard in the chair.—On the problem of Gauss concerning the attraction of an elliptical ring, by M. Halphen. Although a clear demonstration of this well-known problem has lately been made by G. W. Hill (Simon Newcomb's "Astronomical Papers," vol. i. 1882), a fresh solution is here proposed, which has the advantage of not requiring the preliminary resolution of an equation of the third degree.—Observations on the oldest sedimentary formations in North-West France (concluded), by M. Hébert. It is shown that the clay-slates of Saint-Lô, which are pre-Cambrian or Archæan formations, were deposited in horizontal layers in a marine basin, which stretched from Wales southwards to Quimper and Alençon, and which was broken only by a few isolated masses of granite and crystalline schists. This oldest of oceanic waters lasted for a long geological epoch, as attested by the thickness of these deposits, and the transformation of the muddy sediment into hard clay-slates. The present vertical position of these rocks, which were antecedent to all animal life, was evidently due to contraction of the terrestrial crust, by which were determined the foldings, faults, and ruptures, and probably the general upheaval of the whole region.—Reply to M. Hugoniot's note on the pressure that exists in the contracted section of a gaseous vein, by M. Hirn. To M. Hugoniot's objection the author replies that he has shown by experiment that the gas flowing through a cylindrical tube into a reservoir, where it becomes very rarefied, falls gradually from the pressure P_0, which it possesses in the gasometer, to a pressure P_x, which is almost exactly that of the rarefying reservoir.—On the velocity of the flow of fluids, by M. Th. Vautier. Having in a previous communication explained his graphic method, the author here shows the process by which he has successfully applied the revolving mirror to the measurement of the velocity of fluids.—Spectrum of the negative pole of nitrogen: general law of distribution of the rays which appear in the bands of the negative pole, by M. H. Deslandres. In the luminous region, which alone has hitherto been studied, the spectrum of the negative pole is accompanied by faint traces of positive bands. But in the ultra-violet region it is prolonged only by a small number of bands, and becomes, so to say, smothered amid the powerful and numerous positive bands. The rays of the band λ 391 are disposed according to the following simple law: The intervals from one ray to another, calculated in numbers of vibrations, are arranged as nearly as possible in arithmetical progression. This appears to be a general law, not merely an isolated fact, as observed by Piazzi Smyth and Herschel between sixteen rays of the green band of the oxide of carbon.—On the temperatures and critical pressures of some vapours in liquids, by MM. C. Vincent and J. Chappuis. In a previous communication the authors announced their researches on the temperatures and critical pressures of two series of gaseous bodies at the ordinary temperature. Here they give the result of their experiments with liquid bodies at the ordinary temperature—the chloride of propyl, the series of the three amines of ethyl, and the two first normal amines of propyl.—Researches on the variations of solubility of certain chlorides in water in the presence of hydrochloric acid, by M. Guillaume Jeannel. From his experiments with the chloride of potassium the author infers that the variations of solubility of this salt are not subjected to the law recently announced by Engel. He arrives at the general conclusion that the solubility of the chlorides precipitated by hydrochloric acid varies in the presence of the acid, so that the sum of the equivalents of water, salt, and acid forming the solution remains constant at the same temperature, whatever be the chloride and whatever be the proportions of the mixture.—Combinations of ammonia with the metallic permanganates, by M. T. Klobb.—Chemical and thermic study of the phenosulphuric acids: paraphenosulphuric acid, by M. S. Allain-Le Canu. This paper is devoted to a fresh study of the three phenosulphuric acids (oxyphenylsulphonic) $C_{12}H_6S_2O_8$.—On the presence of lecithine in vegetation, by MM. Ed. Heckel and Fr. Schlagdenhauffen. The authors' researches confirm the conclusion already arrived at by Hoppe-Seyler and Kraetzschmar that this substance, known to exist in many of the animal tissues, is found also in numerous plants.—Note on fine-flavoured brandy distilled from the grape-cake of white wine, by M. Alph. Rommier.—Fresh researches on the axial nervous current, by M. Maurice Mendelssohn. It is shown that the axial current possesses the same physical and physiological properties that M. E. Du Bois-Reymond has discovered in other nervous currents; also that its direction is in the closest relation with that of the function of the nerve.—On the alterations produced in the constitution of the blood by the action of the sulphuret of carbon on the animal system, by MM. Kiener and R. Engel.—On the resistance of the virus of glanders to the destructive action of atmospheric agencies and of heat, by MM. Cadéac and Malet. It is shown that this virus loses its virulence in humours exposed to the open air after complete desiccation; also that it is destroyed rapidly in warm and dry, slowly in cold and moist weather.—On the disposition of the limestone breccias of the Alpujarras Range, Andalusia, and their resemblance to the carboniferous breccias of Northern France, by MM. Ch. Barrois and A. Offret.—On a method of volumetric analysis for the sulphates, by M. H. Quantin.

BOOKS AND PAMPHLETS RECEIVED

"Life and Labours of John Mercer," by E. A. Parnell (Longmans).—"Arc and Glow Lamps," by J. Maier (Whittaker).—"Fourth Report of the U.S. Entomological Commission," by N. Riley (Washington).—"List of Foreign Correspondents of the Smithsonian Institution," by G. H. Boehmer (Washington).—"List of Institutions in the U.S. Receiving Publications of the Smithsonian Institution" (Washington).—"Bulletin of the U.S. National Museum," No. 30, "Bibliographies of Amer. Naturalists," iii., "Publications relating to Fossil Invertebrates," by J. B. Marcou (Washington).—"Quarterly Journal of the Geological Society," vol. xlii. part 3, No. 167 (Longmans).—"Catalogue of Birds of Suffolk," by Rev. C. Babington (Van Voorst).—"Elements of Plane Geometry," part 2 (Sonnenschein).—"A New Physical Truth," by E. J. Goodwin.—"Progress in Zoology, 1885," by Prof. Gill; "Progress in Chemistry, 1885," by H. C. Bolton; "Progress in Geography, 1885," by J. K. Goodrich: "Progress in Astronomy, 1885," by W. C. Winlock; "Progress in Anthropology, 1885," by Prof. Mason; "Record of North American Invertebrate Palæontology, 1885," by J. B. Marcou; "Progress in Vulcanology and Seismology, 1885," by Prof. Rockwood (Smithsonian Institute, Washington).

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