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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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THE PROJECTED CHANNEL RAILWAYS

II.

TO connect England and France by a railway through a submarine tunnel is not a novel idea. From time to time English and French engineers have revived the plan of tunnelling under the Straits of Dover with some modification or other. Among these the latest and most carefully considered proposition is that by Messrs. Hawkshaw, Brunlees, and Low, in connection with Messrs. Talabot, Chevalier, and Gamond. The scheme of these engineers will be best understood from their report to a committee of promoters, and we will give those passages of the report which are essential for the clear comprehension of the plan :—

"The undersigned engineers, some of whom have been engaged for a series of years in investigating the subject of a tunnel between France and England, having attentively considered those investigations and the facts which they have developed, beg to report thereon jointly for the information of the committee.

"These investigations supported the theory that the Straits of Dover were not opened by a sudden disruption of the earth at that point, but had been produced naturally and slowly by the gradual washing away of the upper chalk ; that the geological formations beneath the Straits remained in the original order of their deposit, and were identical with the formations of the two shores, and were, in fact, the continuation of those formations.

"Mr. Low proposed to dispense entirely with shafts in the sea, and to commence the work by sinking pits on each shore, driving thence, in the first place, two small parallel driftways or galleries from each country, connected at intervals by transverse driftways. By this means the air could be made to circulate as in ordinary coal-mines, and the ventilation be kept perfect at the face of the workings.

"Mr. Low laid his plans before the Emperor of the French in April 1867, and in accordance with the desire of his Majesty, a committee of French and English gentlemen was formed in furtherance of the project.

"For some years past Mr. Hawkshaw's attention had been directed to this subject, and ultimately he was led to test the question, and to ascertain by elaborate investigations whether a submarine tunnel to unite the railways of Great Britain with those of France and the Continent of Europe were practicable.

"Accordingly, at the beginning of the year 1866 a boring was commenced at St. Margaret's Bay, near the South Foreland ; and in March 1866 another boring was commenced on the French coast, at a point about three miles westward of Calais ; and simultaneously with these borings an examination was carried on of that portion of the bottom of the Channel lying between the chalk cliffs on each shore.

"The principal practical and useful results that the borings have determined are, that on the proposed line of the tunnel the depth of the chalk on the English coast is 470 feet below high water, consisting of 175 feet of upper or white chalk and 295 feet of lower or grey chalk ; and that on the French coast the depth of the chalk is 750 fee below high water, consisting of 270 feet of upper or white chalk and 480 feet of lower or grey chalk ; and that the position of the chalk on the bed of the Channel, ascertained from the examination, nearly corresponds with that which the geological inquiry elicited.

"In respect to the execution of the work itself, we consider it proper to drive preliminary driftways or headings under the Channel, the ventilation of which would be accomplished by some of the usual modes adopted in the best coal mines.

"As respects the work itself, the tunnel might be of the ordinary form, and sufficiently large for two lines of railway, and to admit of being worked by locomotive engines, and artificial ventilation could be applied ; or, it might be deemed advisable, on subsequent consideration, to adopt two single lines of tunnel. The desirability of adopting other modes of traction may be left for future consideration."

This, then, is the great Tunnel scheme, which a committee of promoters and engineers submitted to his Majesty the Emperor of the French in June 1868 ; and we are informed "His Majesty was pleased to refer the matter to the favourable consideration of his Excellency the Minister of Public Works, who appointed a special commission to inquire into the subject in all its bearings." This special commission reported in March 1869 ; and a summary of this report on the main question is contained in the following three resolutions, viz.—

"I. The commission, after having considered the documents relative to the geology of the Straits, which agree in establishing the continuity, homogeneity, and regularity of level of the *grey chalk* between the two shores of the Channel,

"Are of opinion that driving a submarine tunnel in the lower part of this chalk is an undertaking which presents reasonable chances of success.

"Nevertheless, they would not hide from themselves the fact, that its execution is subject to contingencies which may render success impossible.

"II. These contingencies may be included under two heads : either in meeting with ground particularly treacherous—a circumstance which the known character of the grey chalk renders improbable ; or in an influx of water in a quantity too great to be mastered, and which might find its way in either by infiltration along the plane of the beds, or through cracks crossing the body of the chalk.

"Apart from these contingencies, the work of excavation in a soft rock like grey chalk appears to be relatively easy and rapid ; and the execution of a tunnel, under the conditions of the project, is but a matter of time and money.

"III. In the actual state of things, and the preparatory investigations being too incomplete to serve as a basis of calculation, the commission will not fix on any figure of expense or the probable time which the execution of the permanent works would require."

Having laid before our readers the Channel Tunnel Scheme in the words of the originators, we shall now proceed to analyse it, and for the purpose we also publish a map forming part of the engineers' report, and which we reduce to a scale of six miles to one inch.

The first important statement with which we meet in the engineers' report is that under section 2, viz., on the theory of the formation of the Straits of Dover. We admit that in all probability the Dover Channel was not produced "by a sudden disruption of the earth at that point," but we cannot endorse the hypothesis that it has been formed by gradually washing away the upper chalk.

In order that the chalk may be washed away more at that point than at others, it is necessary that the current should be stronger. To begin with the operation of washing away, we must have a current, a strong current, which could only flow in a valley or channel, previously formed either by depression of the surface or by the elevation of the land adjoining that surface. Whichever way we take it, the original channel of Dover must, it would seem probable, have been formed by a geological disturbance of the earth's crust.

The current within that geologically-formed channel may have further deepened it, although this is not probable, because at the bottom of a channel from 100 to 200 feet deep, the speed of a current can be but a small fraction of its superficial velocity, and this fraction is assumed to have abraded chalk rock.

Looking at the chart attached to the report of the engineers, we find a series of lines running from the English to the French coast, which are assumed to indi-

cate the position of the strata of various rocks—of the same rocks which compose the hills on either side: so, for example, the district A between the dotted lines is assumed to be that of the upper or white chalk; B, the lower or grey chalk; C, to be the place of a series of strata, such as the upper green sand; the gault, the Folkestone, Sandgate, and Hythe beds; Atterfield clay, &c., &c.

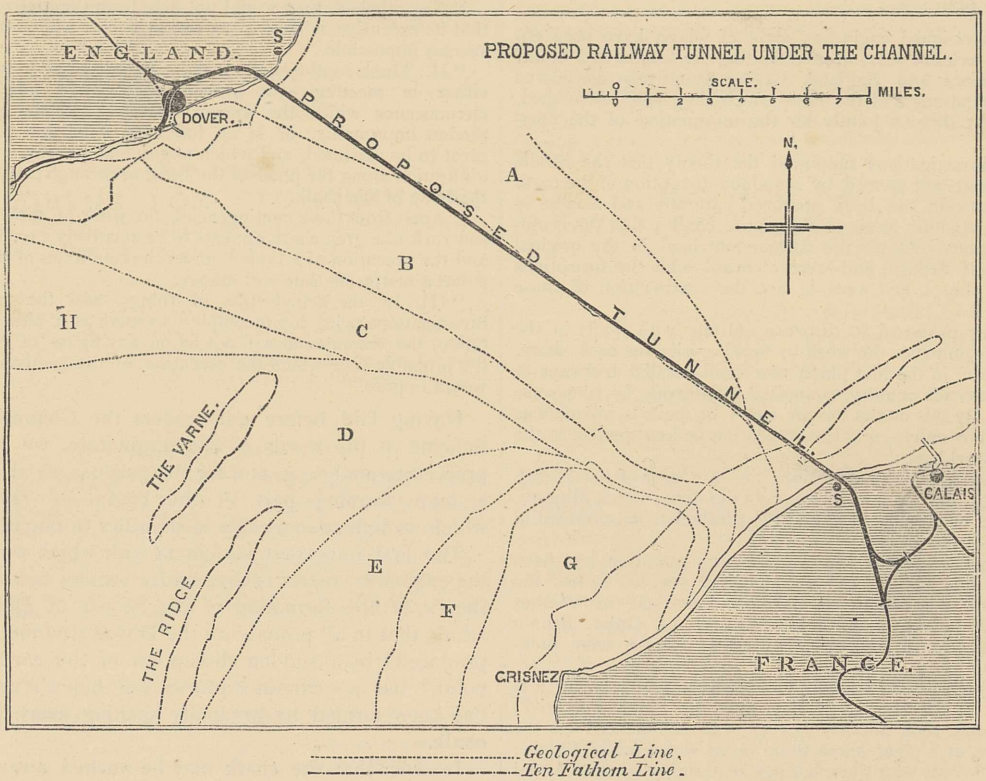
The only question that presents itself to our mind is, whether this chart be correct; whether the geological lines there indicated are the result of test and observation, or whether those lines are based on the theory of the engineers, that the strata of the hills on either side of the Channel pass "undisturbed" in plane surfaces across the Channel, assuming that the strata which formerly occupied the Channel were cut away or carried off; and whether the lines indicated on the map may not principally have been produced by constructing the intersections of those inclined surfaces with the present bottom of the Channel. We fear the geological lines of the diagram were mainly obtained on the above theory of the engineers, and will therefore in a great measure be imaginary.

It is probable that the strata which form the hills on either side will also form the bottom of the Channel, at a different elevation; but it would be rash to say how much the difference might be. It will probably vary across the Channel, the thickness of the strata remaining nearly the same; so it may be presumed that chalk will be found within the Dover Straits at certain depths below the bed of the Channel. If this be so, it would not be necessary to divert the tunnel as indicated on the plan. Nothing short of an actual test across the Channel on the line selected for a tunnel could, however, settle the question of stratification, and determine at what depth a special kind of rock, suitable for tunnelling, would be met with.

The operation of driving the tunnel would have to be carried on many hundred feet below the mean level of the Channel, and, apart from other difficulties attending the execution of such an immense tunnel, for which we have no precedent even on a small scale—the first question would be, whether it may be reasonably expected that tunnelling would be practicable under such circumstances without meeting with an insurmountable influx of water.

That we should meet with many fissures and cracks is a matter of course, for it would be difficult to select on a rocky surface a few square yards without indications of fissures. That water will find its way along cracks we know from experience, and as in this instance no water-tight strata intervene between the sea and the rocky material through which it is proposed to drive the tunnel, so, consequently, we must expect that the work of driving the driftways and the tunnel would be "wet." Although the work may be very wet, it will still remain a question of quantity, and not only one of quality; and, accordingly, we must ascertain whether the quantity of water that might find its way into the workings would be unmanageably large.

On this point a great deal of misconception may be found in "professional" papers. It is asserted, that the



A, upper or white chalk. B, lower or grey chalk. C, upper green sand, gault, Folkestone, Sandgate, and Hythe beds. D, region probably occupied by the Hastings sands. E, Neocomian sand. F, Portland stone and sand. G, Kimmeridge clay. H, Weald clay. SS, Borings.

pressure due to a column of 400 or 500 feet of water —which is equal to about 200lb. per square inch— would not only cause infiltration through fissures and cracks, but would in a short time enlarge them, abrade the very rock, and so day by day increase the flow into the driftways. As a general assertion, this evinces a confusion of the static with the dynamic laws of hydraulics.

If we stop the flow from a crack, and if the water cannot be diverted, the full hydrostatic pressure due to the whole column will be established at the very margin of the crack where it was artificially closed. But if we do not stop the flow from it and allow the discharge, the pressure at the margin within the tunnel will by no means be equal to the full hydrostatic column; for, in passing water along a fissure with a certain velocity, by far the largest part of that column will, under ordinary circumstances, be destroyed or consumed in overcoming the friction of the water over the large area of the fissure, and, as a rule, the effective pressure at the end of a fine crack within the tunnel would be only a small fraction of the whole column of water—probably not one per cent., so that no abrasion could take place, and the flow of the water, whatever it might be, would not increase day by day.

This being a very important question in reference to the proposed tunnel, let us take an example.—Assume a fissure 1 yard long and $\frac{1}{1000}$ part of an inch wide—about the thickness of tissue paper. Let this fissure continue in a vertical direction for a distance of 100 yards, to the bottom of the channel, and assume above that fissure 30 fathoms of water. What quantity of water will find its way through that fissure, and what will be the pressure at its margin within the Tunnel? We assume a clear opening throughout, and yet that fissure could only pass 4 gallons per hour into the tunnel, and the effective pressure at its margin would only be $\frac{1}{300}$ lb. per square inch. The effect of such a crack or opening would therefore be quite insignificant. Let all the circumstances remain the same, but assume the fissure 10 times as wide, viz., $\frac{1}{100}$ part of an inch in the clear: it would then pass into the tunnel 136 gallons per hour, with a marginal pressure of $\frac{1}{25}$ part of a pound per square inch; let it be $\frac{1}{10}$ inch wide in the clear, and it will pass 4,392 gallons per hour, with a marginal pressure of $\frac{2}{5}$ of a pound —and if we assume the fissure 1 inch wide in the clear, it will pass 139,905 gallons per hour, with a marginal pressure of $4\frac{1}{5}$ pounds per square inch.

It then becomes chiefly a question of the *nature* of the fissures with which we may have to deal, and while a fine crack one yard long would only admit an insignificant volume of water into the tunnel, a similar crack one inch wide would be a serious matter, very few of which would drown the drift. But even in the latter case no abrasion of the rock could take place, for a pressure of four pounds per square inch has no effect upon chalk.

But though the fissures within the driftways might be very fine, each passing or oozing out a comparatively small quantity of water, easily to be dealt with within a moderate length of the driftways, their number within a distance of twenty-two miles would be legion, and, we believe, would overpower all appliances. These fissures should not be permitted jointly to send water into the driftways; they should be closed as soon as practicable. How is that to be done? By keeping a water-tight main tunnel close upon the face of the driftway. As soon, however, as the cracks would be closed by the main water-tight tunnel, the full hydrostatic pressure of many hundred feet would be resting on it. The main tunnel would collapse unless it could bear a pressure of about fifteen tons per square foot on its superficial area. This circumstance determines the form and the material of the tunnel. It could not be an ordinary tunnel in any sense. Its form must be circular, and its principal material iron. No brickwork could stand that pressure at whatever thickness, within practicable limits, it might be assumed, because by increasing its thickness its superficial area would also increase. Nor could any brickwork be water-tight against such pressures, and, unless it be so, water will find its way into the tunnel very nearly as fast through the lining as without it.

We cannot, therefore, help differing from the resolution of the engineers that—

> "In respect of the execution of the work itself, we consider it proper to drive preliminary driftways or headings under the channel, the ventilation of which would be accomplished by some of the usual modes adopted in the best coal-mines."

We consider this resolution fraught with danger. The driftways could never be accomplished without the aid of the main tunnel; nor would the proposed ventilation through the driftways be adequate. Why should the ventilation be similar to that adopted in our best coal-mines? Is the proposed tunnel to be driven through the coal measures? No, it is to be through the grey chalk, and a ventilation which may be adequate in coal measures would certainly fail in the chalk, from which a large amount of an irrespirable gas would exude. This is another reason why the main tunnel should be kept closely behind the face of the driftway, viz., to exclude the surface of the chalk. And why have two driftways to begin with? Moreover, there is no difficulty in sending into the tunnel through ordinary piping, to the face of the heading, ten times as much pure air by mechanical power as the best mode of ventilating coal-mines could possibly ensure.

The tunnel could only be worked by pneumatic pressure: this is obvious from recent investigations of eminent engineers; it could not be for a double line, because that system is not applicable to it; and the old atmospheric plan failed above ground.

We are of opinion that it is not an unreasonable proposition, to drive a tunnel under the Channel, but that in some measure it must be a venture. If we are to undertake such a venture to gain a magnificent prize, of immense value to the English and French nations, we must be prepared to meet all ordinary eventualities, and we must not fail in the attempt by want of foresight, energy, and by dint of proper means. It will cost a great deal, but not too much, with proper management and a little good fortune. The first step towards accomplishing the object would be to obtain a geological section across the Channel, on one, or perhaps on several lines; not a section constructed upon certain theories and assumptions, but one obtained from actual test of the materials which compose the bed of the Channel, following Mr. Hawkshaw's steps on shore also across the Channel, for which purpose the assistance of the Governments of England and France may be confidently expected.

DR. BALFOUR STEWART'S METEOROLOGICAL BLOCKADE

IMAGINE a line drawn round any district, and consider all the air that passes over that line outwards and inwards in any time. Let the whole quantity of vapour of water carried across the boundary line by this air be determined. If during any particular interval of time there is just as much vapour carried outwards as inwards, there must be in that interval either no rainfall on, and no evaporation from the district, or there must be just as much rainfall as evaporation. If more vapour is carried out than in, there must be more evaporation than rainfall. Or, if more vapour enters than leaves, the difference falls in excess of rain above evaporation.

Dr. Stewart proposes to establish a *cordon* of meteorological stations, and to arrange a reduction of observations taken at them, so as to keep, as far as possible, an exact account of the quantity of vapour entering and leaving the space over the surrounded district. This appears to me a most valuable proposal, which, if well carried out, must have a very important influence, tending to raise meteorology from its present empirical condition to the rank of a science. The object of the present notice is to suggest that the same system of account-keeping ought to be applied to electricity.

Whatever we may think as to the nature of electricity, it certainly has, in common with true matter, the property of being invariable in quantity. This property is conveniently enough expressed, as it were, mechanically, by the one-fluid hypothesis, which asserts that positive or negative electrification of any piece of matter consists in the presence of more or less than a certain quantum of the electric fluid; that quantum being the amount possessed when the matter in question exercises no attractive or repulsive force, varying with artificial variations of the electric condition of the testing body presented to it. On this hypothesis, "quantities of electricity," positive and negative, are excesses of the quantity of the hypothetical fluid above or below the "quantum" corresponding to zero of the electric tests.

The ordinary fair-weather condition in our latitudes presents us with negatively electrified air in the lowest stratum, extending at least as high as our ordinary houses above the surface of the earth; and positive electricity of greater amount, on the whole, in the higher regions. The atmospheric electrometer indicates in absolute electrostatic units the total quantity of electricity in the atmosphere, over a certain area of the place of observation; being the excess of the amount of positive above the amount of negative electricity in the whole column. This excess in fair weather is generally positive. The fact that it is not the electricity in the lower regions alone, but an effect depending on the whole electricity of the atmosphere from lowest to highest, that is the thing observed in the ordinary observation of atmospheric electricity, renders this subject more suitable even than moisture for the application of Dr. Stewart's blockade. Thus, the hygrometrical blockade is complete only if both moisture and the effective component of wind are known at all heights above the surface; the electrical blockade is complete when, besides the electrometer measurements at the observatory, the effective component of the wind at all heights is known. But among the many unknown quantities involved, the two departments of the blockade combined will give means for eliminating some and estimating others for which the hygrometrical blockade alone, or the electrical one alone, would be insufficient.

WILLIAM THOMSON

THE SCENERY OF ENGLAND AND WALES

The Scenery of England and Wales, its Character and Origin. By D. Mackintosh. (Longmans and Co.)

READERS of some of the geological journals are sufficiently acquainted with the name of Mr. Mackintosh. They know him as a writer who, for the last four or five years, with wonderful perseverance, has kept on enunciating certain views of denudation which he has adopted. His papers, however, do not suggest that their author possesses the large grasp of the subject, the range of acquirement in geological studies, and the gifts of style which must necessarily belong to the man who would successfully expound the history of the scenery of the country. The task is a difficult one—much more difficult than even geologists themselves usually believe—and the announcement that it had been undertaken by Mr. Mackintosh probably took a good many readers by surprise. They could not but award him the credit of great boldness, whether or not they found, on examination, that he deserved also the praise of success.

Mr. Mackintosh, as a writer on geological subjects, is under the influence of one dominant idea. He believes in the Sea, the whole Sea, and nothing but the Sea. It seems to haunt him like a nightmare. To his ear all sounds in Nature are drowned in the thunder of waves, the rush of ocean-currents, the beating of multitudinous billows, that batter and grind the solid substance of the land. A mist of ocean spray is ever in his eyes. He can but dimly see that any other powers are at work around him save the everlasting breakers. The fall of rain, the roar of rivers, the silent majesty of glaciers and snowfields, have little charm for him, and can never wean his affections from his first love. He can be brought to turn his thoughts neither to the right hand nor to the left, but keeps his eye stedfastly fixed on the one grand object of his faith and adoration. If, indeed, for a moment the contending claims of some other power in Nature obtrude too pertinaciously upon his notice, he clings to some bigger wave, or dashes into some stronger ocean-current until the temptation has passed away.

It is seldom that we meet with one governing idea so unflinchingly followed to the exclusion of anything and everything which might modify it; seldom that we encounter mental colour-blindness so thorough as to admit of the discrimination of apparently but one hue. Not merely does he everywhere and always reiterate that "the greater part of the land surface, at any given time, must present the form given to it by the sea," but he traces this marine sculpturing down to the minutest details of contour, even though rains and frosts and streams seem to rise up in protest to his face. The tendency unduly to exalt the power of the sea as a geological agent has always characterised the writings of English geologists, probably from the greater prominence which the sea acquires in the eyes of islanders. But the tendency never received such an extreme development as it has done in Mr.

Mackintosh's pages. He complacently remarks that his views "more or less agree with opinions held by Sir R. I. Murchison, Sir Charles Lyell, Professor Phillips, Mr. C. Darwin, and others. This *argumentum ad hominem*, we imagine, will hardly be relished by some at least of these gentlemen. Mr. Mackintosh should remember that if his views are sound, the citation of such names is not necessary, while, if they are erroneous, not all the big names in England can save them from the oblivion to which they are doomed.

The author of this volume has evidently never travelled. He tells us, too, that he purposely refrained from reading until his views were formed, "lest a bias should be given to his opinions." And his reading, since these unbiassed opinions were elaborated, appears to have consisted mainly of the recent journals and magazines in which he himself has been writing. And yet a man without travel, and without reading, sits down to write a book on the character and origin of the scenery of a country! He has taken his own little corner of the earth and framed his theory out of it, forgetful of the wide world outside, which must be studied as a whole, if one would well understand any part. While reading the book with laudable perseverance, we have had four lines of "Don Juan" constantly suggesting themselves, where the poet describes a certain easy-minded old gentleman as one who

> "Saw with his own eyes the moon was round,
> Was also certain that the earth was square,
> Because he had journeyed fifty miles and found
> No sign that it was circular anywhere."

We presume that Mr. Mackintosh's views would "more or less agree" with those of this philosopher as to the shape of the moon, and generally as to matters of fact regarding which there can be no dispute. Like the same worthy, too, he does not take things on trust. He uses his own eyes and draws his own inferences. He is certain of his convictions, because he has journeyed so many miles and found no sign that these convictions could be anything but true. Unfortunately, however, he has either made his journeys with a foregone conclusion in his mind, or they have been too limited to enable him fully to understand what the subject really is of which he proposes to treat; or, what is probably the truth, he lacks that grounding in exact knowledge of geological structure which is absolutely necessary to one who wishes to elucidate the history of a land surface.

So much for the general purpose of the volume. Its plan and execution are sadly unmethodical. It is divided into three books; but, in spite of the explanation of this arrangement given in the preface, it is not easy to trace any clear distinction in the subjects of the divisions. The reader is jerked from one topic to another, getting of some of them the merest glimpse, so that after a little he begins to experience something of the feeling of unrest which comes over him at another time when he tries to pick out the details of a landscape from the window of an express train. There is no index to the book, and unless a passage is specially noted down at the time of perusal its subsequent recovery is troublesome. Altogether the volume suggests to the reader the idea that he has here a multifarious collection of jottings and excerpts from note-books, strung together with not much reference to their connection. For example, the First Book, according to the table of contents, consists of "Introductory remarks on the causes of denudation and origin of natural scenery in various parts of the world." Under this comprehensive title come just twenty-one pages, in which a word or two are said about faults and fractures, the fact that rains, frosts, rivers, and glaciers are denuding agents is alluded to and not denied, while about fourteen pages, or two-thirds of Book I., are devoted to desultory remarks on sea-bottoms, Irish eskers, oceanic currents, waves in Norway, waves in Shetland and Caithness, waves on the coast of Ireland, sea escarpments in Ireland, remarks on Irish cliffs, sea-coast cwms in Ireland, beachless shores, denudation of Norway, denudation of South America and Australia. This introduction is really a very fitting one to the rest of the book. It shows how the hue of the author's spectacles not only colours all his own observations, but will not let him see the true tint of the observations of other people. For instance, he professes to condense from Forbes's work an "account of the most striking physical features of Norway, *excepting those resulting from glacial action*." *Hamlet* with the Prince of Denmark omitted was nothing to this. If there is one country in the world more marvellously eloquent of glacial action than other countries it is Norway. That is the one grand physical feature of the peninsula; every fjord, every fjeld points to the grinding power of ice. Yet a writer on denudation, anxious as to the impartiality of his views, ventures to write about the sculpturing of the present physical features of Norway, and to pass the glaciers and snowfields by. Again, he cites Australia as a recently emerged area, where there cannot have been any "long-continued action of rains and streams." But he takes no note of the old river gravels, with cappings of basalt, hundreds of feet above the level of the present streams, which point to the passage of a vastly protracted period of subaërial erosion.

Perhaps no passage in the book shows better the author's obliquity of vision, and his consequent (though no doubt unintentional) unfairness, than one in this same introductory chapter, where, in lieu of giving the reader a general view of what denudation is, he states, on the authority of Mrs. Somerville, that a tidal current in the Shetlands runs at the rate of fifteen miles an hour, while "*the average velocity of the river Rhone is not a mile and a half an hour*." No one unacquainted with the subject would be likely to escape the inference which the writer probably intended should be drawn, that even a rapid river like the Rhone has not more than a tenth of the erosive power of some marine currents. Now, though there can be no doubt that some of the most rapid currents known flow among the Orkney and Shetland Islands, yet we take leave to question whether any of them ever reach by any means so high a velocity as fifteen miles an hour. Yet even granting that they do, they are quite exceptional, and it was long ago shown by Dr. Fleming that their erosive power over a surface of rock is nearly, if not wholly, *nil*, for they cannot even rub off the crust of tender barnacles. But to set down the average velocity of the Rhone as "not a mile and a half an hour!" Shade of old Rhodanus! have mercy upon Mr. D. Mackintosh if that gentleman ever gets near enough the "rushing of the arrowy Rhone." A mile and a half an hour! why, that is a very feeble velocity

for most of our comparatively sluggish British rivers. The Thames flows at the rate of two or three miles an hour, the Severn at three or four miles, and most of our upland streams run still faster. Surely there is just cause of complaint against a writer who, in support of a theory which he has adopted, cites facts which are wholly exceptional without saying that they are so, while at the same time he is led in his ardour unconsciously to misstate the facts which do not tell in his favour.

We have read this book with a feeling of regret that it should ever have been published. Its author is evidently a hard-working observer, and perhaps had he been less ambitious, and been content to wait some years longer until his experience had widened, and he had found time to mature and methodise his opinions, our verdict upon his labours might have been different. As it is, he has hastened before the world with a book of which, in a year or two hence, no one will see the crudity more keenly than he will do himself. A work which aims at giving a popular version of any branch of scientific inquiry should be eminently clear, accurate, and readable. Mr. Mackintosh's volume fulfils none of these conditions, and we only fear that its effect may be to discourage readers from seeking to learn more of what is in reality one of the most fascinating fields of geology.

OUR BOOK SHELF

Medical Chemistry. — *Manuel de Chimie Médicale et Pharmaceutique.* Par Alfred Riche. 8vo., pp. 771, figures 104. (Paris : Germer-Baillière, 1870. London : Williams and Norgate.)

WE have here a comprehensive text-book of chemistry, in which the medical and pharmaceutical applications of the science are specially noticed. The author is one of the professors in the School of Pharmacy at Paris, and the "mineral chemistry" of the manual is essentially a reproduction of his course of lectures. The "organic chemistry" is based on the course of Professor Berthelot, and the toxicological portions of the work reflect the teaching of Professor Bouis. Though designed for the use of students of medicine and pharmacy, this manual is primarily a systematic exposition of the fundamental facts of chemistry, and its technical character is revealed only in incidental explanations. Thus, sulphuric acid is noticed with the oxygenated sulphur compounds, and its properties, preparation, commercial manufacture, purification, and chemical constitution, receive adequate treatment before its medical employment and its action as an irritant poison are considered. Again, in the section on the natural alkaloïds we get the chemical history of each of the more important opium bases before we obtain any information respecting the assay of opium, the action of the drug on the human system, or the symptoms of poisoning by opiates. The officinal processes for preparing the chemical substances used in medicine are plainly but briefly described, and practical directions are given for testing the purity and estimating the strength of commercial products. The author has devoted 150 pages to "biological chemistry," an important section of the science which receives scant notice in most manuals, and has minutely described methods of analysing milk, blood, urine, and calculi, which may be adopted by the physician to obtain trustworthy indications of the progress of disease or the effects of medicines. Professor Riche's manual has so many good qualities that we reluctantly call attention to a characteristic which detracts from its value as a treatise on general chemistry. While admitting that the modern or molecular notation is preferable to the notation based on the old equivalents, the author deliberately rejects the former "because it is not yet recognised in the official programmes." Consequently, the book is filled with symbolic formulæ which do not accord with accepted theories. In the introduction to the "organic chemistry," M. Riche attempts to adapt the exoteric notation of his school to modern ideas by doubling the symbols of oxygen, sulphur, and carbon ; but English students will look with little favour on the hybrid formulæ thus produced. We trust that the author will burst his bonds of red tape before the second edition of his manual is issued. J. C. B.

Heads and Tales ; or, Anecdotes and Stories of Quadrupeds and other Beasts, chiefly connected with Incidents in the Histories of more or less Distinguished Men. By Adam White, late Assistant in the Zoological Department, British Museum. (London : Nisbet and Co. 1870.)

THE idea of bringing together the anecdotes of animals recorded in the biographies of great or well-known men is a good one ; but the notion of interlarding these anecdotes with facetiæ selected chiefly from Mr. Mark Lemon's "Jest Book" can hardly be commended. Mr. White is sadly mistaken if he supposes that well-written stories of animal life require to be made palatable to school-boys by the addition of puns and shallow witticisms. Schoolboys may well be amused at the solemn way in which the author announces his discovery (in the pages of Macaulay) of the cause of the death of William III. ; but they will think it beyond a jest when they themselves discover that six pages which should have been filled with anecdotes are occupied by Macaulay's account of the last days of King William. Mr. White does not exclude man from what he is pleased to term his anecdotical treatment of the great order Mammalia. Three stories of a ludicrous character, however, suffice to illustrate the human species. The schoolboy reader is duly warned against the theory of the genesis of man by natural causes in the following terms :—"Let us never for a moment rest in such fallacious theories, or accept the belief of Darwin and Huxley, with a few active, agitating disciples, that animals, and even plants, may pass into each other. Darwin and Huxley cannot change nature. They may change their minds and opinions, as their fathers did before them. It is, we expect, only the old heathen materialism cropping out." This extract is, perhaps, sufficient to show the author's mental calibre. It is not necessary to praise the illustrations of this book ; it is quite sufficient to mention that they are from the drawings of J. Wolf.

Facts and Dates ; or, The Leading Events in Sacred and Profane History, and the Principal Facts in the various Physical Sciences : the memory being aided throughout by a simple and natural method. By the Rev. Alex. Mackay, LL.D. (Blackwood and Sons.)

A SO-CALLED system of mnemonics may possibly be of use to young men cramming history and geography for competitive examinations ; but we see no room for it in science-teaching. The scientific facts capable of being expressed by numbers, and which it is desirable to commit to memory, are really very few. Indeed, we believe they are so few in any given science, that it would take most people longer to master Dr. Mackay's system of artificial memory than to learn the numbers themselves. The author has spent a good deal of labour in so framing his mnemonic sentences as to contain ideas having some connection with the fact intended to be remembered. This is all very well ; but the essential fault of his system, and of every such system, is, that it is not sufficient to remember the *idea* contained in the mnemonic sentence : every word of it must be accurately retained. We are sorry to see that Dr. Mackay is sadly afflicted with the Great Pyramid craze. The introduction of such a matter as the pretended scientific revelation into a book intended for school use is very much to be deprecated.

SENSATION AND PERCEPTION

II.

THE doctrine that there is a distinct organ for the realisation of Sensations only, apart from that for Perception, has been very generally taught, and has been insisted on by no one more strongly than by Dr. Carpenter in his otherwise most able and suggestive expositions of nervous physiology. He says:* "The general rule of action appears to be that the impressions made by external objects upon the afferent nerves, when transmitted to the spinal cord, ascend towards the cerebrum without exciting any reflex movements in their course. When such an *impression* arrives at the sensorium,† it excites the consciousness of the individual, and thus gives rise to a *sensation;* and the change thus induced being further propagated from the sensory ganglia to the cerebrum, gives occasion to the formation of an *idea*." And that Dr. Carpenter here means by the word 'idea' what we have previously spoken of as that complex intellectualised sensation generally called a 'perception,' seems obvious from the following passage occurring on another page, where the same author says: "It is further important to keep in mind the distinction between the *sensations* themselves and the *ideas* which are the immediate results of those sensations when they are perceived by the mind. The ideas relate to the *cause* of the sensation or the object by which the impression is made" (p. 711). But since, in Dr. Carpenter's view, the sensory ganglia constitute the *sensorium*, in which impressions become conscious sensations; and because he naturally thinks it very improbable that there are two distinct organs of consciousness, he is compelled to adopt the hypothesis that the superficial grey matter of the cerebral hemispheres, in which intellectual operations are principally carried on, is not itself endowed with the function of consciousness. Thus he assumes—as the most probable inference to be drawn from various kinds of evidence—"that the sensory ganglia constitute the seat of consciousness not merely for impressions on the organs of sense, but also for changes in the cortical substance of the cerebrum; so that until the latter have reacted downwards upon the sensorium we have no consciousness either of the formation of ideas or of any intellectual processes of which these may be the subject."‡ And, although we are quite unable to agree with the conclusions themselves as to the absence of consciousness in connection with the activity of the cerebral hemispheres, and as to its presence as a functional attribute of the sensory ganglia alone, still it is sufficiently interesting, in a philosophical point of view, to find Dr. Carpenter declaring so confidently in favour of a distinct organ of consciousness, even altogether separate from those parts of the cerebral hemispheres in which what we have called § *potential knowledge* is produced—meaning by this term what is called knowledge, so far as it can exist *minus* the attribute of consciousness. The elaboration of this potential knowledge is, in fact, a process the possibility of which has been ably discussed by Dr. Carpenter in the section in which he speaks of "unconscious cerebration."

We must, however, briefly inquire into the reasons which have induced Dr. Carpenter to regard these so-called sensory ganglia as the seat of consciousness; though, before doing so, it will be well to draw the reader's attention to the following considerations. As it is quite true that different nerves, coming from the sense organs or surface of the body generally, do pass through the sensory ganglia on their way to the higher centres in the cerebral hemispheres, it is obvious that the impressions made upon any one of these lower centres must be *qualified* to a certain extent, inasmuch as they are the middle terms of a series, and therefore are related to their antecedents and to their consequents in the same way that these are related to one another—the antecedents being the external impressions, and the consequents the resulting perceptions. For, when an impression of a certain kind is made upon any given part of the surface of the body, this impression traverses definite nerve-fibres, in order to reach functionally related portions of the cerebral hemispheres, and so we may well suppose that the fibres, on their way, must necessarily pass through *definite parts* of the sensory ganglia, and produce, in certain of the ganglionic elements there situated, impressions of a *definite kind.*

Thus, therefore, although we may believe that no state of consciousness is aroused by this molecular action taking place in these lower sensory ganglia, the impressions made upon them may be, nevertheless, definite enough in kind and place to ensure a partial transference of such molecular movement along given and accustomed outgoing motor channels; such organic possibilities of motorial response having been slowly built up and elaborated, in past time, under the guidance of then co-existing and related conscious states. Thus, movements may at times be produced in every way similar, as regards mode of origin, to those automatic or reflex movements occurring through the intervention of the spinal cord alone; though they may be as much more complex, and apparently purposive, as these higher centres are more complex than the spinal centres. And it is, we think, because the movements are produced by reflections from the highest motor centres, *whose complexity renders the most purposive movements possible*, that such movements have been supposed to be invariably the sequences of conscious impressions or *sensations*, and have hence received the appellation of *sensori-motor.* This name, however, begs the question in dispute—as to whether impressions reaching thus far would be revealed in consciousness or not; and from what has been already said it will be seen that in the settlement of this question we must not rely too much upon the purposive nature of the movements as evidence that they are the results of conscious impressions. Reasonings of this kind led Pflüger to suppose that the spinal cord was also a seat of consciousness.

We quite agree with Dr. Carpenter* and others, however, in the opinion that the organic possibilities of executing all combined muscular movements of which the individual is capable, reside in the spinal cord and medulla, and also still higher in the motor centres in immediate connection with these so-called sensory ganglia—by virtue of definite nerve connections therein established. So that all the facts with which we are acquainted, as he says, "tend strongly in favour of the view that even voluntary movements are executed by the instrumentality of the automatic apparatus, and that they differ only from the automatic or instinctive in the nature of the stimulus by which they are excited." This doctrine may be aptly illustrated by reference to the act of coughing, since this is an instance in which a complicated set of movements usually produced automatically may nevertheless be incited by a voluntary determination. When so produced, the will is directed to the production of the result as a whole; no attempt being made to single out the different movements, and then to combine them; so that, as Dr. Carpenter also says, "the will thus seems obviously to take the place of the laryngeal or tracheal irritation as the *primum mobile* of the series, which, in its actual performance, is as automatic in the latter case as in the former." In each case, the same organised set of nerve connections in the higher motor centres (constituting the organic representatives of the combined muscular movements of the act of coughing) are called into activity; now by a volitional incitation descending from the cerebral hemispheres, and, at another time, as a result of an afferent stimulus reaching the

* Principles of Comparative Physiology, fifth edition, p. 707. 1854.

† Constituted by certain ganglionic structures at the base of the brain, in relation with the various sensory nerves, and usually spoken of as the *sensory ganglia.*

‡ Loc. cit., p. 546.

§ See paper on "Consciousness," in *Jour. of Ment. Science*, p. 512.

* Human Physiology, fifth edition, p. 516.

related sensory centres from some part of the respiratory passages themselves.

Bearing these facts in mind, and also the psychological view of the essential unity in the mode of evolution of all sensations or conscious states—whether simple or complex—we shall find that the performance of many acts of the so-called *sensori-motor* type do not necessarily lend such support to the supposition that the sensory ganglia are the seats of consciousness as they have been supposed to do. That the movements of an infant or of an idiot should appear to be automatic in nature, is only to be expected if we consider that they are responses to conscious impressions excited in quite undeveloped cerebral hemispheres, in which, as yet, the possibilities of intellectual and volitional action of the lowest type only are organised. Thus, the only action which at this time could possibly emanate from the operation of the intellectual centres would be such as Dr. Carpenter has supposed to depend upon the stimulus of mere sensations ; and it does not at all follow, as he seems to suppose, that such movements are excited by sensations, realised as such in the sensory ganglia and thence reflected without the intervention of the cerebral hemispheres. Neither do we think his doctrine receives any stronger amount of support from the fact of our ability to perform certain *habitual* movements whilst the cerebrum is occupied with some engrossing train of thought.

It is, we think, an altogether improbable assumption to suppose that the so-called "secondary automatic" acts take place, as a general rule, altogether without the intervention of the cerebral hemispheres. Taking the instance cited by Dr. Carpenter, of the individual who, falling into a deep reverie whilst making his way through the streets, nevertheless walks with ease along accustomed routes, though his attention may be entirely absorbed by some particular train of thought, it is supposed that these movements are characteristic instances of *sensori-motor* acts, that they are brought about solely by reflections from some of the sensory ganglia, and without the intervention of the hemispheres. But it seems to us much more reasonable to suppose that the cerebral hemispheres have been concerned to some extent, even though the consciousness of the individual has been otherwise monopolised. In proportion to the frequency of the repetition of such movements—to the degree in which they have become habitual, so can we the more easily understand that the cerebral action involved may take place without arousing consciousness, and so quite independently of trains of thought which are monopolising the person's attention. A motor incitation now really volitional, may, though similar in all other respects, at another time be purely reflex and unconscious, even though passing over from regions of the cerebral hemispheres themselves;* the consciousness or the unconsciousness of the incitation depending upon the particular direction of the attention of the individual at the time being. This view seems rendered all the more probable if we consider what are the effects on man of even small injuries of parts of the cerebral hemispheres above the level of the sensory ganglia. If, in the instance above alluded to, the person maintains the erect position, and even walks perfectly well, through the intervention of nerve-centres no higher than the sensory ganglia, how is it that the same man would be completely paralysed on one side of his body for months or perhaps years by an effusion of blood into, or a softening of, a portion of brain tissue quite above these sensory ganglia—by a lesion, for instance, of a portion of the opposite hemisphere outside its *corpus striatum?* We may set such an individual upon his legs as often as we choose, but no amount of mere sensory impressions are capable of exciting the supposed *sensori-motor* movements ; the paralysed limb is utterly powerless, and even the mere attitude of standing, when the individual is unsupported, is found to be impossible.

The experimental evidence which has been appealed to is also capable of receiving different interpretations ; and, indeed, physiologists themselves have already expressed directly contrary opinions upon this subject. The evidence is, in fact, of such an uncertain nature as to be quite incapable of leading to a very definite conclusion, unless we have formed some decided opinion as to the real nature of a sensation, and as to how this differs from what is called a perception. Flourens denies, for instance, that birds or mammals whose cerebral hemispheres have been removed are any longer capable of appreciating sensory impressions ; whilst Magendie, Longet, Vulpian, and many others maintain that such animals are capable of feeling simple sensations, and that they are therefore, to a certain extent, in possession of consciousness. But it may be fairly maintained that the way in which such animals respond to external influences acting upon them are explicable without postulating the existence of consciousness, if we bear in mind that they do still retain their sensory ganglia and all the related motor centres in organic connection with these ; and if we bear in mind also what has been already said as to the degree in which impressions, reaching only so far as these sensory ganglia, and not revealing themselves in consciousness at all, are, nevertheless, *qualified*, and therefore capable of exciting those purposive movements which exist potentially in the related motor centres in the form of definite nerve connections. The molecular mobility of these centres has only to be disturbed in order to bring about, with machine-like precision, the natural movements themselves. Longet and Vulpian look upon the *pons Varolii* as the seat of *general sensibility*,* and there certainly seems much more evidence in favour of this view than in support of the doctrine of Dr. Carpenter that such is the function of the optic thalami. Evidence of different kinds seems quite opposed to the latter view ; but much *may* be said—even though, as we think, to little purpose—in favour of the opinion of Longet and Vulpian. The *corpora quadrigemina* are similarly supposed to be the seats of perception for optical impressions. And one of the strongest facts that has been recorded in favour of the supposed *sensori-motor* nature of certain movements was observed in a pigeon, which lived eighteen days after its cerebral hemispheres alone had been removed by Longet. When this animal was taken into a dark room, every time a light was brought near its eyes the irides contracted, and often even winking occurred ; "mais chose remarquable," Longet says,† "aussitôt que j'imprimais un mouvement circulaire à la bougie enflammée, et à une distance assez grande pour qu'il n'y eût point sensation de chaleur, le pigeon exécutait un mouvement analogue à sa tête." Nevertheless, it is said that this same animal sometimes seemed to avoid obstacles, and sometimes knocked himself against them. So that when we consider how closely the movements of the eyes are bound up with visual impressions generally, this following of the strong light, which seems so much to suggest consciousness, may be capable of explanation without having recourse to such a supposition. Dr. Carpenter has called attention to the fact that when the eyelids are closed, if we attempt to move the eye-balls in any given direction, we can only do this with considerable difficulty, and with an extreme sense of effort—"This sense being the result of the state of tension in which the muscles are placed by the effort to move the eyes without the guiding visual sensation." He then adds :—"Now, on the other hand, the will may determine to fix the eyes upon an object ; and yet this

* See article on "Physiology of Thinking," *Fortnightly Review*, Jan. 1869.

* These conclusions were arrived at from an observation of birds and mammals whose cerebral hemispheres, corpora striata, thalami, corpora quadrigemina, and cerebellum had been all removed, leaving within the cranial cavity only the *pons*, or *tuber annulare*, and the medulla. (Longet, "Traité de Physiologie," 3rd ed. t. iii. p. 156.)

† Loc. cit. p. 428.

very fixation may be only attainable by a muscular movement, which movement is directly excited by the visual sense without any exertion of voluntary power over the muscles. Such is the case when we look steadily at an object whilst we move the head from side to side; for the eyeballs will then be moved in a contrary direction by a kind of instinctive effort of the external and internal recti, which tends to keep the retina in their first position, and to prevent the motion of the images over them." These are said to belong to the category of voluntary movements, and yet we are not ourselves conscious that they are taking place; we know of it only, as Dr. Carpenter says—or rather learn to infer the existence of such movements in ourselves—by observing what takes place in other persons. We must make due allowances, therefore, for facts like these, when attempting to interpret what takes place in animals from whom a part of the brain has been removed; and we must also bear in mind that the endowments of the lower nerve-centres are different amongst different classes of animals, before we come hastily to the conclusion that movements of the kind mentioned in the pigeon were indicative of consciousness on its part; and still more before we conclude from such phenomena that the sensory ganglia in man are also seats of consciousness. As regards motor power, the differences are most notable amongst different groups of animals. Thus, after complete removal of the cerebral hemispheres, fish, reptiles, birds, and the lower mammalia experience extremely little diminution in their powers of movement. Carp and frogs continue to swim as well as before; a pigeon when abandoned in the air flies to the ground, settling lightly on its feet; whilst a rabbit runs away when irritated, performing these movements with no appreciable difficulty, and with only a slight evidence of weakness. The weakness becomes much more notable when the operation is performed upon a dog, though it is less marked in proportion as the animal is a young one. An adult dog deprived of his cerebral hemispheres is, however, no longer capable of maintaining the erect position, though it can still move its legs freely whilst lying down.

The effect in man, of even limited injuries to one or other hemisphere, in producing paralysis of the opposite side of the body, has been already referred to. Such variations must be taken into account in our interpretations of Longet's experiments with the pigeon. But even Longet himself, though he makes the *pons* the centre for general sensibility and the *corpora quadrigemina* the centre for visual sensations, seems, after all, to entertain considerable doubts or to whether he is warranted in making use of the word "sensation." Thus he says: "Certes en prenant le mot *sensation* dans son acceptation rigoureusement métaphysique, et ne l'appliquant qu'à tous les cas d'exercice de la sensibilité avec *conscience*, on devra admettre que la protubérance, siége de la sensibilité générale, et les lobes cérébraux, siége de l'intelligence, doivent nécessairement mettré, pour ainsi dire en commun leur activité, et concourir au même acte." But then he adds, "Mais à la rigueur, ne pourrait-on pas permettre aux physiologistes de distinguer la *perception* simple (en quelque sorte *brute*) des impressions, de l'attention qui leur est accordée, de l'aptitude à former des idées en rapport avec elles?" To this question we would reply that the power of simple perception with which Longet wishes to endow these lower centres is probably not attended by Consciousness, as he himself seems to intimate, and therefore such a word is altogether unsuitable to express that *unconscious discrimination* of impressions, which may be followed by apparently purposive movements, resulting from the excitation of these lower centres. How this unconscious discrimination may occur, and how it may result in definite movements, have been shown.

Thus, we think the experimental evidence leads us to the conclusion that *unconscious* or *organic discrimination* takes place in the sensory ganglia, just as it takes place in the spinal cord; only that the discrimination in the former is of a higher type, and results in the more purposive movements, because it takes place in nerve-centres of higher rank. For the production of a distinct state of feeling or sensation, however, even of the simplest kind, conscious intellect is needed, and this cannot be brought into operation without the conjoint activity of the cerebral hemispheres.

We do not consider that such a conclusion is in the least shaken by the evidence furnished by comparative anatomy, notwithstanding what Dr. Carpenter* says to the contrary. He writes as follows:—"Thus we are led by the very cogent evidence which comparative anatomy supplies, to regard this series of ganglionic centres as constituting the real *sensorium*, each ganglion having the power of rendering the mind conscious of the impressions derived from the organ with which it is connected. If this position be denied, we must either refuse the attribute of consciousness to such animals as possess no other encephalic centres than these, or we must believe that the *addition* of the cerebral hemispheres in the vertebrated series alters the endowment of the sensory ganglia,—an idea which is contrary to all analogy." We feel most surprised that Dr. Carpenter should have expressed this latter view; in the first place, because it is quite adverse to the general doctrines of Von Baer, or, in other words, to the doctrines of Evolution which he has done his best to elucidate; and, secondly, because such a notion is opposed to the information afforded by actual experiment as to the alteration in the endowment of the motor centres (to which we have already referred) in ascending the vertebrate scale. As specialisation of structure occurs, so must we get specialisation of function; and we are glad to find that an idea, long familiar to ourselves, has also occurred to Mr. Herbert Spencer, and has been thus clearly expressed by him†:—"It does not follow, as it at first sight seems to do, that feelings are never located in the inferior nervous centres. On the contrary, it may well be that in lower types [of animals] the homologues of these inferior centres are the seats of consciousness. The true implication is, that in any case the seat of consciousness is that nervous centre to which mediately or immediately the most heterogeneous impressions are brought; and it is not improbable that in the course of nervous evolution, centres that were once the highest are supplemented by others in which co-ordination is carried a stage further, and which, therefore, become the places of feeling, while the centres before predominant become automatic."

The conclusions at which we have arrived have an important psychological bearing. Thus, Herbart in Germany, followed by Sir William Hamilton in our own country, made Cognition or discrimination the fundamental fact of Mind, rather than Sensation or mere feeling (which is regarded as its basis by many others), and it must be confessed that physiological evidence accords with the former rather than with the latter view. In the first place, because no consciousness in the form of sensation can take place without the aid of intellectual activity under the form of cognition or discrimination; whilst, on the other hand, cognition or intellectual action may take place under the form of a mere *organic* or *unconscious discrimination*, without the intervention of consciousness. Thus, in the individual, consciousness or feeling comes to be superadded as an additional accompaniment to certain mere organic discriminations; so that consciousness, without which sensation cannot exist, is secondary, whilst cognition, in the form of unconscious discrimination, is primary. Out of this primary undifferentiated organic discrimination, such as alone pertains to the lowest forms of animal life, there has been gradually evolved that which we know as feeling and consciousness.

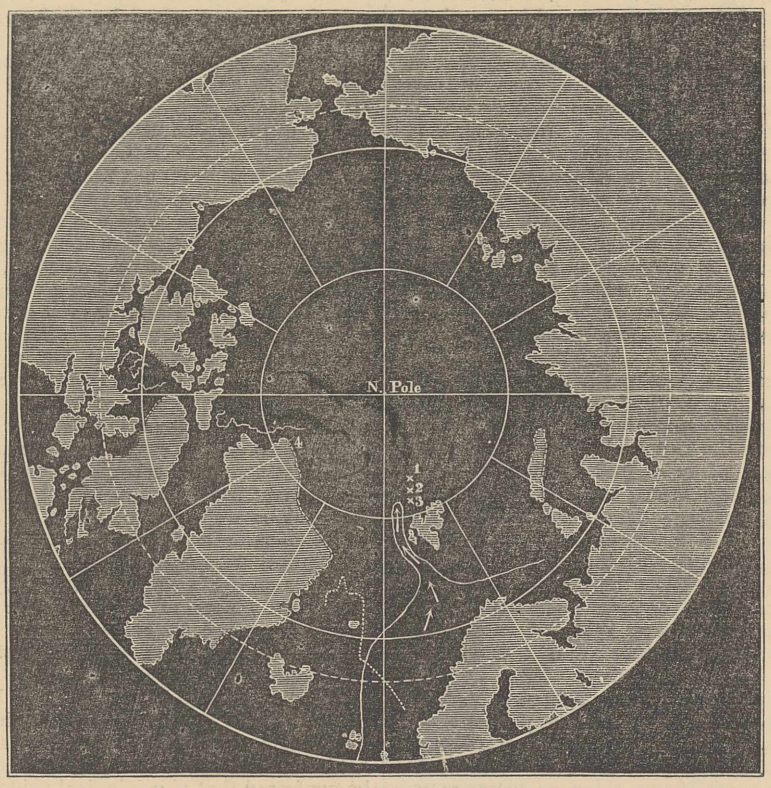
H. CHARLTON BASTIAN

* Op. cit., p. 503. † Principles of Psychology, Oct. 1868, No. 2, p. 105.

THE GERMAN AND SWEDISH EXPEDITIONS TO THE ARCTIC REGIONS

ENGLAND seems ready to resign the position she once held as chief of all the competitors in Arctic exploration. Our flag has been carried within 7½ degrees of the North Pole; our seamen have forced from the ice-bound straits which lie to the north of America the secret of the North-Western Passage; and from the days of Scoresby until those of Franklin we have been foremost in scientific researches within the dreary Arctic wastes. But now the answer to all who would emulate the deeds of a Parry or a Ross, a Beecher or a Franklin, is the stereotyped *cui bono*. A business account of the probable gains of an Arctic journey must be rendered before England will send men or ships to the Polar seas.

In the meantime, Swedish and German explorers are pushing their way boldly into the regions where England won her Arctic laurels—perhaps we ought rather to say, ice-wreaths. Already the most northerly spot reached by our seamen has been all but attained, and there is yet room for supposing that this very year the second German expedition may push its way to the Pole itself. Scientific results of extreme value have also been attained. The course of the Gulf Stream (if Mr. Findlay will permit the name) has been tracked into very high latitudes, its depth has been gauged, and the nature of the currents which run beside it, or beneath it, has been carefully inquired into. The various forms of life which people those Arctic seas have been examined with loving care (extending even to a judicious use of powder and shot, or nets and fish-hooks, as the case might be) by the naturalists who have accompanied the expedition. And lastly, a very important addition, about which I hope to make some remarks on a future occasion, has been made to our knowledge of the variations of the magnetic needle in Arctic regions.



MAP OF THE NORTH POLAR REGIONS

The primary object of all three expeditions has been to attain, if possible, the North Pole of the earth. Dr. Petermann, who had the principal part in planning the German expeditions, has a theory about Greenland, which was associated, perhaps not very fortunately, with the other objects of the German voyagers. But undoubtedly the attainment of the highest possible northerly latitude was their principal aim.

A glance at the accompanying map will show the nature of the Arctic regions, so far as they have yet been explored. The circle 10 degrees from the North Pole has hitherto been crossed in only two neighbourhoods. The figure shows (1) where Parry made his most northerly point in 1827. The Swedish and German expeditions of last year pushed their way towards the same region, and the crosses numbered (2) and (3) indicate the spots they respectively reached. In 1854 Dr. Kane crossed the circle of 10 degrees near (4), having pushed his way along the inlet above the north-western point of Greenland. And in 1865 Dr. Hayes reached the same point, after traversing a large part of Greenland in sledges drawn by Esquimaux dogs.

Notwithstanding the hopes which M. Lambert has formed of attaining the North Pole by passing through the Straits between Asia and North America (shown near the top of the map), there seems every reason for believing that if the North Pole is ever reached by man it must be either along the course pursued by Kane and Hayes, or by the path which Parry followed. In fact, it is reasonable to confine our attention wholly to the latter course, since Dr. Hayes' journey sufficed to show that without the means of crossing the sea, further passage northwards from (4) is impossible, and it is inconceivable that any suitable sea-boat could be carried to (4) either by Kane's or Hayes' route.

The Swedish expedition to (2) is perhaps the most hopeful sea-journey that has yet been made towards the North Pole. They had been engaged until late in the Arctic season in scientific researches on the coast of Norway; yet they succeeded in pushing their way within a few miles of the most northerly point yet reached, and were even then only impeded by the dangers due to the approach of winter. There seems every reason to believe, that had they started a few months earlier they might have pushed their way much further north.

The German expeditions, undertaken in the two years, seem to have followed a course less likely to be successful. Dr. Petermann holds that Greenland extends much further towards the north-west than its shores have yet been traced—nay, even past the Pole, perhaps, to the neighbourhood of Behring's Straits, between Asia and America. Both expeditions sought to reach the north-eastern shores of Greenland with the object of ascertaining whether this theory, or General Sabine's view that Greenland has some such figure as is indicated by the dotted line in the map, is the more correct.

Twice in 1868 Captain Köldewey was forced to abandon this attempt, and each time after his defeat he made for the shores of Spitzbergen. Thus, on July 18, he had

already crossed the 80th parallel, and was close to the station marked (3); but thence he again made, by a long *détour*, for the shores of Greenland. It was after his second repulse that he reached his most northerly point (3), close to the most northerly limit of the Swedish expedition.

This year, Captain Köldewey, now in a steamship, has pursued the same tactics, except that he has been less ready to accept defeat, and has persistently sought to penetrate the ice-laden seas which surround the Greenland coast. The dotted line shows the general course of the second expedition; and it will be seen that when last heard of they were close by Greenland, and far south of the 75th parallel. Captain Gray, who brought the latest intelligence of their doings in this neighbourhood, states that on August 1, although the sea was still much encumbered with ice, it was becoming rapidly clearer, so that the *Germania* was likely to have little difficulty in reaching the Greenland coast. I confess, however, that I do not share the hopes which have been expressed of the successful progress of the expedition this year. The result of the expeditions of 1868 seems to point very clearly to another course than that which the *Germania* is now seeking to pursue: and there is nothing in the whole history of Arctic expedition to encourage a hope that a way can be found so far to the west (at least in latitudes below 80°), to the neighbourhood of the North Pole.

The figure indicates the course of the mean summer and autumn isotherm of 3° Reaumer (about 39° Fahr.), in the North Atlantic. Along the course marked by arrows a branch of the Gulf Stream has been traced (in summer) as far north as latitude 81½°; the main stream making its way towards Novaia Zemlia.* Does not Nature herself seem to point out this track past Spitzbergen as the proper course for North Polar explorers? Here, in the first place, the mildest temperature is found; and in Arctic voyaging this is a matter of no small importance. Here also is an assisting current—*valeat quantum valere debet.* But the chief circumstance to be noticed is, that the course followed by the Gulf Stream shows that there is open water—ice-encumbered, no doubt, but still not ice-bound—in this direction. It is well worthy of notice, too, how deep the sea is along this part of the Atlantic. Herr von Freeden remarks, that the whole of the Bernese Oberland might be hidden, "its presence unbetrayed even by an eddy," under the ocean to the north-west of Spitzbergen. Long ago, indeed, Scoresby found no bottom with a two-mile line. Here, then, if anywhere, a ship might expect to find her way, though experience has shown again and again that that way is full of dangers.

Either along this course or along the track suggested by Herr von Freeden, the Pole, I doubt not, will yet be reached. It will be remembered that Sir Edward Parry, setting forth from Spitzbergen on his famous "boat and sledge" expedition, was foiled by an unforeseen difficulty. The whole mass of ice over which he had tracked his way for more than a hundred miles began to drift southward, so that, as fast as Parry and his party travelled northwards, they were set back by the relentless sea and wind. Now, Parry's defeat shows at once the hopefulness of the course suggested above, and of Von Freeden's proposition that an expedition like Parry's should be commenced earlier in the season, when the ice is as yet unbroken. The very fact that Parry's great ice-ship floated freely shows how wide and deep the seas must be even far to the north of the spot he reached. For not only could he see no sign of water in front—and the Arctic voyager can recognise a "water-sky" at a great distance—*but the point where he turned must have been a few days before some hundred miles at least further north*, for he and his party had been floated back more than a hundred miles. There must then have been, that year at least, a course round the floating ice-fields which would have carried a daring seaman to an open sea between the North Pole and station 1, and far to the north of the latter point. On the other hand, the ease with which Parry's party pursued their way northwards shows, as Herr von Freeden justly remarks, that it would be no very difficult matter to attain the Pole itself over the ice, if the journey were made in early summer.

RICHARD A. PROCTOR

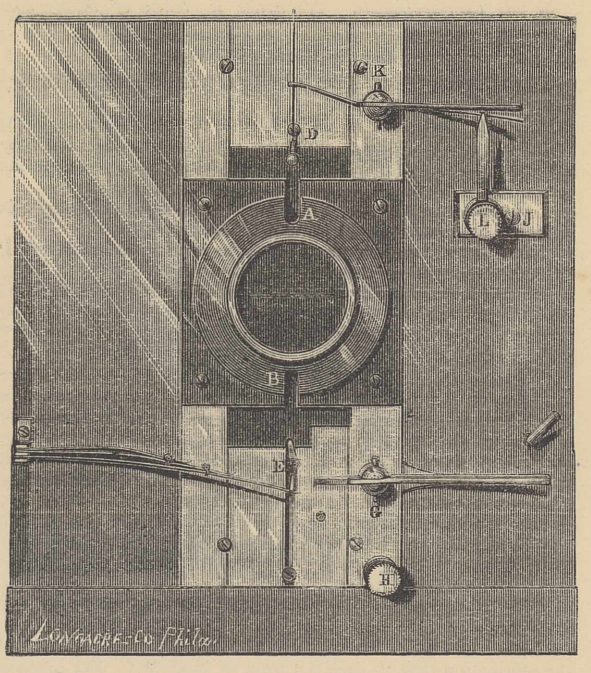
* Petermann's Geog. Mittheilungen, Part VI. The paper on "the scientific results of the first German North-polar Expedition," by Herr W. von Freeden, in this number of the Mittheilungen, will well repay careful study. In Part IX. the progress and results of this year's expedition, so far as they are yet known, are detailed in a series of letters from the people on board the *Germania*.

IMPROVED ECLIPSE CAMERA

IT is to be hoped that shortly, in view of the approaching total eclipse in December next—to observe which we trust a Government expedition will be organised—English astronomers will be making arrangements for obtaining as valuable a series of photographs as the one which rewarded the efforts of the American astronomers last year.

We therefore append a description of the important modifications successfully introduced by Professor Morton, of Philadelphia.

A B represents the face plate of the camera, to which the eye-piece tube was attached, its other end being screwed to the telescope. The diaphragm plate, D E, moved across the axis of the instrument, being drawn downwards by the combined spring, C F. The strength



of this spring could be reduced by raising the outer end of one or both the upper strips so as to disengage the forks at their end from the lower spring, and then turning them forward in a direction normal to the front of the box, out of the way.

The spring was attached to the diaphragm plate by a swivel hook.

A number of diaphragm plates were provided, with slits respectively of $\frac{1}{40}$, $\frac{1}{30}$, $\frac{1}{20}$ and $\frac{1}{10}$ of an inch in width. These plates could be readily interchanged, and, in combination with the springs, gave a very wide and yet delicate series of fixed adjustments for the times of exposure.

To make the exposure, the plate was drawn up until the projecting pin, D, could be caught on the lever, K, which would then retain it. On depressing the outer end of this lever, however, with the finger, the hold on the pin was disengaged, and the plate flashed across the axis of the tube, allowing light to traverse the narrow slit as it flew past. The plate was then arrested on the end of the second lever, G. When an exposure of some seconds was required, as during the totality, a plate having a round orifice exposing the entire field of the eye-piece was substituted for the one with the narrow slit, and was so arranged that, when caught by the upper lever, it covered the lens, but when fallen to the second lever, exposed it entirely; when, however, this lever was in turn touched, the plate descended again far enough once more to close the lens. By touching these two levers in succession, it was then possible to make a "time exposure" with great nicety and accuracy, as proved by actual experience during the eclipse.

To secure a chronographic record of each exposure, a binding screw was provided to make one connection with the general mass of the face-plate including lever K, and another at L, to carry on the circuit when the downward motion of the lever brought the spring at its side in contact with the point projecting from L. In raising the lever for a new exposure, the spring at its side was pressed back so as to pass the point without contact.

LETTERS TO THE EDITOR

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

Kant's View of Space

THE following paragraphs, I believe, faithfully render sundry passages of Kant's writings :—

"Objects are given to us by means of sense (Sinnlichkeit), which is the sole source of intuitions (Anschauungen); but they are thought by the understanding, from which arise conceptions (Begriffe)." ("Kritik," p. 55. Hartenstein's Edition.)

"The understanding is the faculty of thought. Thought is knowledge by means of conception." (*Ibid.* p. 93.)

"The original consciousness of space is an intuition *à priori*, and not a conception (Begriff)." (*Ibid.* p. 60.)

"Space is nothing else than the form of all the phenomena of the external senses; that is, it is the subjective condition of sense, under which alone external intuition is possible for us." (*Ibid.* p. 61.)

"Our nature is such, that intuition can never be otherwise than sensual (Sinnlich); that is, it only contains the modes in which we are affected by objects. On the other hand, the power of thinking the object of sensual intuition, is the understanding. Neither of these faculties is superior to the other. Without sense, no object would be given us, and without understanding none would be thought. Thoughts without contents are empty, intuitions without conceptions (Begriffe) are blind." (*Ibid.* p. 82.)

"Time and space are 'mere forms of sense'" (Formen unserer Sinnlichkeit, "Prolegomena," p. 33) and "mere forms of intuition." ("Kritik," p. 76.)

With these passages before one, there can be no doubt that that thorough and acute student of Kant, Dr. Ingleby, was perfectly right when he said that Kant would have repudiated the affirmation that "space is a form of thought." For in these sentences, and in many others which might be cited, Kant expressly lays down the doctrine that thought is the work of the understanding, intuition of the sense; and that space, like time, is an intuition. The only "forms of thought" in Kant's sense, are the categories. T. H. HUXLEY

January 14

I DO not believe Professor Sylvester has been betrayed, as Mr. G. H. Lewes asserts, into any misconception of this matter by me.

When Kant, at the outset, says, "Alles Denken aber muss sich, es sei geradezu oder im Umschweife, vermittelst gewisser Merkmale, zuletzt auf Anschauungen...beziehen," it would take the veriest dunderhead not to see that all forms of intuition must be, ndirectly at least, forms of thought. I never dreamed of disputing so obvious a position. But I object to the phrase "forms of thought," as designating Space and Time, on the ground of precision. They are *peculiarly* forms of general Sense, and not forms of Thought *as Thought*. Kant, I believe, eschewed the phrase in that sense, and, for all I see, might for the same reason have disclaimed it.

Ilford, Jan. 14 C. M. INGLEBY

IT is not *my* habit "when objections are made to what I have written, silently to correct my error or silently disregard the criticism." If the objections are well founded, I think it due to the cause of truth to make a frank confession of error, and in the opposite case to reply to the objections.

With reference, then, to Mr. Lewes's strictures in NATURE'S last number, I beg to say that Dr. Ingleby has "betrayed" me into no error. If I have fallen into error, it is with my eyes open, and after satisfying myself by study of Kant, that to speak of Space and Time, whether as forms of understanding, or as forms of thought, is an unauthorised and misleading mode of expression. Space and Time are forms of sensitivity or intuition. The categories of Kant (so essentially in this point differing from those of Aristotle) do not contain Space and Time among them, and are properly called forms of understanding or thought.

To the existence of thought the operation of the understanding is a necessary preliminary.

Sensibility and intuition are antecedent to any such operation.

Can Mr. Lewes point to any passage in Kant where Space and Time are designated *forms of thought?* I shall indeed be surprised if he can do so—as much surprised as if Mr. Todhunter or Mr. Routh, in their Mechanical Treatises, were to treat *energy* and *force* as convertible terms. To such a misuse of the word energy it would be little to the point to urge that *force without energy is a mere potential tendency*. It is just as little to the point in the matter at issue, for Mr. Lewes to inform the readers of NATURE that *intuition without thought is mere sensuous impression.*

Dr. Ingleby has rendered, in my opinion, a very great service to the English reading public, by drawing attention to so serious and prevalent an error as that of confounding the categories (the proper forms of thought *as thought*) with Space and Time, the forms of intuition, the Sentinels, so to say, who keep watch and ward outside the gates of the Understanding.

Athenæum Club, Jan. 15 J. J. SYLVESTER

Correlation of Colour and Music

SOME twenty-six or twenty-seven years ago, in a lecture on Light at the London Institution, I suggested an analogy between the octaves of Sound and Light; not then knowing the view of Sir J. Herschel to which Dr. Pereira subsequently called my attention.

I endeavoured to support the hypothesis of three primary colours by supposing the intermediate colours to arise from the blending of the primary. Thus orange would result from the blending of red and yellow, green from yellow and blue, and violet from the secondary red impinging on the blue or indigo. This seemed to me a less arbitrary explanation than that of Sir D. Brewster of a superposition (in degrees of intensity chosen to suit the hypothesis) of all the primary colours throughout the whole spectrum. Spectrum analysis has now much changed our views on this subject.

The interesting article in your number for January 13, by Mr. Barrett, has recalled my attention to the matter, and induces me to ask whether he, or any of your contributors, can explain a phenomenon which I have very often observed, as have doubtless others, but which I have never seen noticed in any work on Light.

It is this. When a very brilliant solar-rainbow is seen, there is plainly visible within, and forming a continuous spectrum with the main rainbow, a repetition, but in much narrower bands, of the rainbow: the same seven colours in the same order; and within this again, I have, on certain occasions, detected a third. Are these repetitions of the spectrum as suggested by Sir J. Herschel? If so, we should have two, three, and more reds, and so of the other colours, in which light, producing the impression of the same colour on the retina, would have different wave-lengths, say, in the ratios of one, two, four, &c.; or is the phenomenon due to some other cause? W. R. GROVE

January 15

I VENTURE to call attention to a curious point in connection with the very interesting note by Mr. W. F. Barrett on the "Correlation of Colour and Music," which appeared in yester-

day's NATURE (January 13). The researches of Helmholtz and others have (as is well known) overthrown, to a certain extent, the old idea of the three primary colours—red, blue, and yellow—and have shown that if any *three* are to be selected, red, blue, and green have greater claims than the former. Now, in Mr. Barrett's diagram these correspond to the following notes :—

Red.	Yellow.	Green.	Blue-Indigo.
C.	E.	F.	G.
Tonic.	Third.	Sub-dominant.	Dominant.

The old triad, red, yellow, blue, correspond to the common chord ; but the new triad, red, green, and blue, to the *tonic, sub-dominant* (or fourth) and *dominant* (or fifth) ; or, in other words, to the three notes which constitute in music the *fundamental base* of the scale.

F. DE CHAUMONT, M.D.

Army Medical School, Netley, Jan. 14

Government Aid to Science

I CANNOT but feel flattered that my letter on this subject should have been thought so dangerous as to require a leading article in the same number by way of immediate antidote, but I must beg you to allow me to correct one or two errors into which you have fallen as to the views I really hold, and which it seems I failed clearly to express. You say, you "understand Mr. Wallace to mean that the main result of cultivating science is merely the gratification of those directly engaged in the pursuit, and that they who do not take this personal interest in it derive little or no benefit from it."

The first half of this passage does express, though imperfectly, what I believe to be the truth ; the latter half expresses the exact opposite of what I have ever thought or intended to write on the subject. The main result of the cultivation of science I hold to be, undoubtedly, the elevation of those who cultivate it to a higher mental and moral standpoint ; while the secondary, but not less certain result, is the acquisition of countless physical, social, and intellectual benefits for the whole human race. But if these are the *secondary* and not the *primary* results of cultivating science, it seems to me to be radically unsound in principle, and sure to fail in practice, if by means of any system of State support we seek to find a short cut to these secondary results.

The only logical foundation for advocating the furtherance of scientific discovery by the expenditure of public money, would be the belief that science can be most successfully pursued by those whose chief object is to make practical and valuable discoveries ; whereas the whole history of the progress of science seems to me to show that the exact opposite is the case, and that it is only those who in a noble spirit of self-sacrifice give up their time, their means, even their lives, in the eager and loving search after the hidden secrets of Nature, who are rewarded by those great discoveries from which spring a rich harvest of useful applications.

One more point. I do not admit that it is just to tax the community for all the Government institutions you name, but in the short space at my command I could not go into details. I have stated how I think some of these institutions require modification to make them accord with the fundamental principle of just government ; and if that principle is a sound one, it is easy to see in what way the others should be dealt with. As an example I may indicate, that a detailed survey, like that of the large-scale Ordnance-maps, being primarily a boon to the landowners of the country, should not be *wholly* paid for by the public.

ALFRED R. WALLACE

Food of Oceanic Animals

I FIND on my return home that Dr. Wallich is vexed at my not having given him the credit of having already answered the question which I ventured to put in the ninth number of NATURE, and that he apparently accuses me of inconsistency as regards my estimate of his observations on deep-sea life. I hasten to assure him that my opinion in that respect has never changed ; nor do the extracts which he has given from my reports warrant such an inference.

I certainly overlooked some of his remarks in the "North Atlantic Sea-bed" bearing on my question, in which he says (page 131), it may be asked "under what other conditions than exceptional ones can marine animal life be maintained without the previous manifestation of vegetable life, as must be the case if it exists at extreme depths?" And he answers this inquiry by submitting that "in the majority of the marine Protozoa—as for instance in the Foraminifera, Polycystina, Acanthometræ, Thalassicollidæ, and Spongidæ—the proof of these organisms being endowed with a power to convert inorganic elements for their own nutrition, rests on the indisputable power which they possess of separating carbonate of lime or silica from waters holding these substances in solution." But this does not appear to be a satisfactory answer to the inquiry ; because a limpet separates carbonate of lime from sea-water in order to construct its shell, yet it cannot be assumed that this animal (which is well known to be a vegetable-eater) has also the power of converting other inorganic substances for its own nutrition. Among the Protozoa, many, probably all, of the Rhizopods are animal-eaters. With regard to sponges, Dr. Bowerbank says (Mon. I., p. 122) that in the greater number their nutriment "is probably molecules of both animal and vegetable bodies, either living or derived from decomposition," and that "the fæcal matters exhibit all the characteristics of having undergone a complete digestion."

J. GWYN JEFFREYS

P.S.—In the 10th number of NATURE, Dr. Martin Duncan, under the head of "Deep-Sea Corals," opposes a statement in what he calls a postscript to my report on the "Deep-Sea Dredging Expedition in H.M.S. *Porcupine*." This statement was not part of my report, nor had I anything to do with it.

J. G. J.

MY attention has been directed to a paragraph in one of the late numbers of NATURE referring to Professor Dickie's interesting remarks on the bathymetrical distribution of Algæ, and raising the question of the mode of nutrition of the great sheet of animal life, which is now shown to extend over the bottom of the sea at all depths.

This curious problem was of course one of the first which engaged our interest when working up the results of the dredging cruise of the *Lightning*. In April last, I proposed a solution in one of the "Afternoon Scientific Lectures" in connection with the Royal Dublin Society, which was afterwards reprinted in full in the "Annals and Magazine of Natural History." I see from notices in several newspapers that this question has excited considerable interest ; I may, perhaps, therefore be allowed to quote the passage in the lecture specially bearing upon it :—

"The question of the mode of nutrition of animals at these great depths is a very singular one. The practical distinction between plants and animals is, that plants prepare the food of animals by decomposing certain inorganic substances which animals cannot use as food, and recombining their elements into organic compounds upon which animals can feed. This process is, however, constantly effected under the influence of light. There is little or no light in the depths, and naturally there are no plants : but the bottom of the sea is a mass of animal life. On what do these animals feed? The answer seems to be sufficiently simple : nearly all the animals—practically *all* the animals, for the small number of higher forms feed upon these—belong to one sub-kingdom, the Protozoa, whose distinctive character is that they have no special organs of nutrition, but that they absorb nourishment through the whole surface of their jelly-like bodies. Most of these animals secrete exquisitely-formed skeletons, sometimes of lime, sometimes of silica. There is no doubt that they extract both of these substances from the sea-water, although silica often exists there in quantities so small as to elude detection by chemical tests. All sea-water contains a certain proportion of organic matter in solution. Its sources are obvious. All rivers contain a large quantity : every shore is surrounded by a fringe which averages about a mile in width of olive and red sea-weeds : in the middle of the Atlantic there is a marine meadow, the Sargasso Sea, extending over three millions of square miles : the sea is full of animals which are constantly dying and decaying ; and the water of the Gulf Stream, especially, courses round coasts where the supply of organic matter is enormous. It is, therefore, quite intelligible that a world of animals should live in these dark abysses, but it is a necessary condition that they should chiefly belong to a class capable of being supported by absorption, through the surface, of matter in solution ; developing but little heat, and incurring a very small amount of waste by any manifestation of vital activity. According to this view, it seems highly probable that at all periods of the earth's history, some form of the Protozoa, rhizopods, sponges, or both, predominated greatly over all other forms of animal life in the depths of the warmer regions of the sea ; whether spreading, compact, and reef-like, as the Laurentian and Palæozoic eozoön ;

or in the form of myriads of separate organisms, as the globigerinæ and ventriculites of the chalk. The rhizopods, like the corals of a shallower zone, form huge accumulations of carbonate of lime, and it is probably to their agency that we must refer most of those great bands of limestone which have resisted time and change, and which come in here and there with their rich imbedded lettering, to mark, like milestones, the progress of the passing ages."

The following passage, referring to another aspect of the question, was published in the 114th Number of the "Proceedings of the Royal Society":—

"The vitreous sponges along with the living Rhizopods and other Protozoa which enter largely into the composition of the upper layer of the chalk-mud, appear to be nourished by the absorption through the external surface of their bodies of the assimilable organic matter which exists in appreciable quantity in all sea-water, and which is derived from the life and death of marine animals and plants, and, in large quantity, from the water of tropical rivers. One principal function of this vast sheet of the lowest type of animal life, which probably extends over the whole of the warmer regions of the sea, may possibly be to diminish the loss of organic matter by gradual decomposition, and to aid in maintaining in the ocean the 'balance of organic nature.'"

I cannot at present enter into detail, as the whole subject of the conditions of life at great depths will be fully discussed in the second part of the preliminary report to the Royal Society on the *Porcupine* Expedition. I may mention, however, that the much more extended researches of the past summer have increased our confidence in the general accuracy of our former conclusions; while careful analyses by the physicists who accompanied the expedition have proved the existence in the water at all depths of a very appreciable and tolerably constant proportion of organic matter in a condition suitable for assimilation by animal organisms. WYVILLE THOMSON

January 7

MY friend Mr. J. Gwyn Jeffreys informs me that he is not answerable for the postscript to his letter, a sentence in which I noticed in a letter published by you in the last number of NATURE. Will you kindly insert this statement.

Lee, January 10 P. MARTIN DUNCAN

[The postscript was editorial, and so appears on the face of it.]

NOTES

IT was announced by Mr. Lockyer at the meeting of the Royal Astromonical Society, on Friday last, that the great refractor of 25 inches aperture, constructed by the Messrs. Cooke, of York, is so near completion that it will be erected in the observatory prepared for it at Gateshead early next month. The completion of this magnificent instrument, with which Mr. Newall has endowed science, marks an epoch in astronomy. Mr. Newall has in fact done what the French Government has already done, and what our Government ought to have done; he has furnished observers with an instrument capable of grappling with the physical problems which have now to be solved—one on a level with our present requirements; and we doubt not that when it is once at work, the wish of its owner, that science may be advanced by it, will be amply fulfilled.

WE are informed that the Senate of London University have proposed to establish a Faculty of Science.

THE last number of the journal of the German Chemical Society contains a biographical sketch of the late Professor Graham, in which his scientific merits are more fully analysed than in the interesting sketch lately published by Professor Williamson in the columns of NATURE. Professor Hofmann, the author of the German biographical notice, has added a photograph of the late Master of the Mint, and an autographic copy of a letter of particular interest to the society. It runs as follows:—"4, Gordon Square, London, Dec. 28, 1868. My dear Hofmann,—I am much gratified by the receipt of your kind letter, and have since received the official intimation of my election as an honorary member of the Berlin Chemical Society over which you preside, which I esteem a high compliment and great honour. I have written a line to Messieurs the secretaries in acknowledgment, which I beg you to forward to them. There is a communication of mine before the Royal Society at present, which I believe will amuse you, or at least the hardiesse of the thing will surprise. What do you think of Hydrogenium, a white magnetic metal of the specific gravity 2?—I remain, dear Hofmann, sincerely yours, THOS. GRAHAM."—On Saturday, January 8, this society gave a dinner to Professor Hofmann, on his retiring from the presidency. The presence of a great number of the celebrities of the town added to the significance of this meeting. Professor Magnus (who acted as chairman on the occasion), Dove, Virchow, Rose, Dubois-Raymond, Kronecker, Curtius, and others, as also some of the Ministers of State, the American Ambassador (Mr. Bancroft), &c., honoured the meeting with their presence, and partly with their speeches. A great number of the foreign members sent messages from England and France, or from distant parts of Germany. The following telegram was received from M. Dumas, at Paris:—"Félicitations et vœux! Longue prospérité à la société! Longue vie à Hofmann! Votre fête est la fête de famille des chimistes du monde entier, qui tous l'admirent et l'aiment." A photolithograph representing Dr. Hofmann, the discoverer of compound Ammonium, as Jupiter Ammon, surrounded by a halo of Aniline colours, was distributed and explained by the artist, and a hymn to Aniline, composed for the occasion, gave a humorous tone to the latter part of the festival.

ALL readers of the "Origin of Species" are aware that the theory now universally (and rightly) known as "Darwinian," was independently conceived and thought out by a naturalist who knew nothing of Darwin's views of the operation of natural selection, and who was at that time thousands of miles away from England. The English public are therefore not likely to forget that to Mr. Alfred Wallace, as well as to Mr. Darwin, belongs the distinction of having discovered "a new idea, a new genus of thought." In Germany, where Darwinism has excited such profound interest, the claims of Mr. Wallace have been somewhat overlooked by the distinguished men who have expounded the theory of natural selection. This has now been rectified by the publication of a pamphlet entitled "Charles Darwin und Alfred Russell Wallace," in which Dr. A. B. Meyer reprints the papers by which the theory was first made known; narrates the circumstances of their publication; and gives slight sketches of the lives of their authors. Dr. Meyer adds to these biographical sketches, lists of the writings of their subjects. Such lists are sometimes not brought down to so late a date as they should be; but in the case of Mr. Wallace, so far is this from being the case, that we see noted as published in the pages of NATURE an article on Geologic Time, which we regret to say we have not yet been able to lay before our readers, owing to the extraordinary pressure upon our columns.

PROFESSOR MAYER was elected, at the meeting of the French Academy on January 10, a correspondent of the section of Physics in place of the late Prof. Matteucci. Of 47 votes, Prof. Mayer received 40; of the remainder, 5 were given to Prof. Kirchhoff, and 1 each to M. Ångström and Sir W. Thomson. We are indebted to the *Revue des Cours Scientifiques* for the following information with regard to vacancies in the lists of corresponding members of the various sections of the Paris Academy of Sciences. The Astronomical section has four corresponding members to replace, namely, Enke, Admiral Smyth, Petit, and Valz; but as the most recent vacancy occurred so long ago as 1867, it is probable that the section considers that the number of places for corresponding members exceeds that of the foreign and provincial astronomers worthy of the honour. In the Physical section, into which, as our readers are aware, Prof. Helmholtz of

Heidelberg and Dr. Mayer of Heilbronn, have recently been elected, there is still a vacancy caused by the death of Principal Forbes, which took place in December 1868. In the section for Geography and Navigation no successor has yet been appointed to M. A. d'Abbadie, elected *membre titulaire* in April 1867. In the Chemical section there are two vacancies, that of Bérard of Montpellier, who died in June last, and that of Prof. Graham, the Master of the Mint, who died in September. There are likewise two vacancies in the Mineralogical section, which includes the Geologists and Palæontologists. They were occasioned by the promotion of Sir Roderick Murchison to the rank of Foreign Associate, and the death of M. Fournet of Lyons. M. Harrman has been nominated by this section for the first place; Dr. W. H. Miller of Cambridge will probably obtain the second. Mr. Dana is, however, also spoken of; likewise two French geologists, M. Lory of Grenoble and M. Leymerie of Toulouse. The Zoological and Anatomical section has three *fauteuils* to dispose of, those of Quoy of Brest, Carus of Dresden, and Purkinje of Prague. One of them will in all probability be assigned to Prof. Huxley. Lastly, there are two vacancies in the Medical section, those of Panizza of Pavia and Sir W. Lawrence. Prof. Lebert of Breslau will almost certainly be nominated by the section for one of the vacancies.

THE Chancellor of the North-German Confederation has presented to the Federal Council the report of the Scientific Commission on the best means of observing the transit of Venus in 1874. The detailed report proposes to send two expeditions to different points of the northern, and two others to different localities in the southern hemisphere.

THE third volume of Dr. Percy's admirable "Metallurgy" will shortly be issued. There will still be another volume to complete the work.

THE Royal Irish Academy at their last meeting approved of some alterations in the bye-laws proposed by the Council. By these alterations the Council for the future will be divided into two Committees: one of Science, consisting of eleven members, and one of Polite Literature and Antiquities, consisting of ten members. The Committee of Publication to consist of four members from each of the Committees of Council. This change will very much simplify the management of the Academy. The first election under the new bye-laws will take place on the 16th March next, and we trust that at least one biologist may be elected into the Committee of Science. For many years past neither a zoologist nor a botanist has been elected into the Council.

L'Institut has announced that from the beginning of the new year it will open its columns to the discussion of scientific subjects of interest to the public at large. We hold that it is an important part of the work of a weekly scientific journal to afford opportunity for instructing others than scientific workers, and also to afford opportunity for a more careful and thorough sifting of most scientific questions than is possible at the meetings of learned societies or in the pages of periodicals published monthly or quarterly. It is satisfactory to us to find that our views on this head are endorsed by so long established and ably conducted a journal as *L'Institut*. As the space which we can devote to letters is necessarily limited, we take this opportunity of begging that our correspondents will make their communications as short as is consistent with clearness.

IN a paper published in a recent number of the *Journal of the Society of Arts*, Mr. Alexander Wallace expresses his conviction that the cultivation of the silkworm may now be successfully and profitably carried out in England, as the causes which led to the failure of the attempts made by the British, Irish, and Colonial Silk Company some forty years ago have been greatly modified. He advocates the adoption of the oak-feeding silkworm of Japan, Bombex Yama-mai, and thinks that the experience of a few more seasons will show the limits of temperature and locality wherein it may be acclimatised.

HAVING adverted in our number of last week to the view recently maintained by Prof. Huxley that the difference between Celt and Saxon is merely one of language, we think it right to call attention to the rejoinder of a "Devonshire Man," printed in the *Pall Mall Gazette* of Tuesday last. The writer energetically opposes Prof. Huxley's attempt to prove identity of race between the natives of Tipperary and the men of Devonshire. He shows further, that Cæsar was fully alive to the essential difference of character between the Celts and the Teutons, and that he defined those characters in language which for accuracy and precision could not be exceeded in our day. The "Devonshire Man" did not, however, adduce any evidence depending on physical characters in support of his view of the question; probably leaving this powerful weapon to some one who has made a special study of European ethnography.

MR. W. CHANDLER ROBERTS has been appointed Chemist to the Mint. The selection of this gentleman to fill this important office is a good one: many points arise during the working of the precious metals that require careful investigation, and Mr. Roberts will doubtless also continue the valuable researches interrupted by the death of Professor Graham.

WE have received specimens of a new process of photo-mechanical printing, patented by Messrs. Edwards and Kidd, of 22, Henrietta Street, Covent Garden. It seems admirably adapted for the reproduction of the works of the great masters of etching and engraving. We are unable as yet to speak of the process as applied to photographs of landscapes.

THE Royal Danish Society of Sciences has offered the following prizes for scientific memoirs, which may be written in Latin, French, English, German, Swedish, or Danish, and must be sent to the secretary of the Society, Professor J. S. Steenstrup, before the end of the month of October next. A gold medal for a memoir establishing a mathematical theory of some branch of assurance hitherto destitute of any such foundation; a gold medal for an experimental and theoretical essay on the law of Cauchy relating to the dispersion of coloured rays, with especial reference to the number of constants necessary to be introduced into the formula; and the Classen prize of 300 rixd. (about 34*l.*), for an investigation of the movement of the air in a system of ventilation. Full details of the questions, in French, will be found in the "Oversigt" of the Society for 1869, No. 1.

MR. JAMRACH, of St. George's Street East, in a letter addressed to *Land and Water*, gives the following extraordinary list of animals now on sale at his establishment:—"1 pair of South African lions, 1 brown hyena, 1 striped hyena, 8 Tasmanian devils, 1 large rare dasyurus, 8 dasyurus maugeii, 6 ichneumons, 4 banded ichneumons, 1 musk cat, 1 African skunk, 1 pair American black bears, 1 pair zebras, 1 pair imported yaks, 6 llamas, 2 Iceland ponies, a new deer, a Brocket deer, a springboc, a harnessed antelope, 2 female and 1 male Wapiti deer, a female nylghaie, a pair fat-tailed sheep, 10 kangaroos, 1 broad-nosed wombat, 1 pair mouflon, a Formosa pig, 8 large crested porcupines, 4 Java porcupines, 1 Java hare, 4 vulpine phalangers, 2 black phalangers, 1 pacca, 4 baboons, 1 pair large drills, 1 mandrill, 2 Mona monkeys, 1 ringtail monkey, 3 green monkeys, 1 blue fox, 1 pair flying opossums, 1 pair emus, a maraboo, a pair Mexican cranes, a pair Demoiselle cranes, 5 spur-wing geese, 16 pair Carolina ducks, 1 pair Australian wild ducks, 2 pair white peafowl, 1 pair lineated pheasants, sand grouse, Mogadore partridges, a New Zealand rail, large European vulture, 1 black hawk, 1 condor, a female Bateleur eagle, a secre-

tary, 2 caracaras, 1 Australian thick-knee, 1 snapping turtle, 20 grey parrots, 12 green parrots, 20 king lories, 1 China lori, 4 Pennant's parakeets, 3 Roselle's parakeets, 12 large cockatoos, 16 rose cockatels, 5 Leadbeater's cockatoos, 3 Nasicu cockatoos, 1 China cockatoo, 10 pair cockatilles, 4 large white-crested cockatoos, 1 male bloodwing, 1 mealy rosella, 6 ringnecked parakeets, 2 Alexandrine parakeets, 1 dwarf parrot, 2 rare Amazones, 7 pair Carolina parrots, piping crow, 1 mynah, 60 pair African love-birds, 80 nonpareils, 1 hang nest, 1 Indian crow pheasant, 55 pairs St. Helena wax-bills, 60 Virginian nightingales, 20 grey cardinals, 16 popebirds, 20 pair Java sparrows, Wydah birds (yellow-backed, red-shouldered, Cape of Good Hope), Madagascar grosbeaks, zebra waxbills, chesnut finches, Napoleon bishops, common bishops, harlequin doves, zebra doves, Australian doves, necklace doves, 3 blue Australian porphyrios."

WE notice that an individual was examined on Tuesday at Worship Street on the charge of sweating sovereigns. The details of the case, which are of considerable importance to the public, will be watched with interest. It appears that the coins are dissolved by acid, aided by a battery, and that the loss in some cases equals about two shillings in the sovereign.

IN the track of vessels from Australia to China lies an island called Pleasant Island. Previous to 1865, the natives had a bad reputation. A Captain Brown reported favourably of them in that year, and mentioned that he was told that an Englishman was residing there. In August 1868, Captain Hall, of the barque *Glenisle*, was boarded by the island canoes, and two whaleboats, with two Englishmen, one of whom had been twenty-eight years on the island, and had a son eighteen years of age. They told him they tried to visit all ships passing within easy distance, and seemed anxious for it to be known that they could supply ships with pigs and cocoa-nut oil. By his advice they purposed to cultivate potatoes, and gave him *an advertisement* to put in the colonial papers. Unfortunately, this document has baffled all attempts to decipher it, from the faulty writing. The island was stated by them to be nine miles across, and twenty two miles in circumference.

ANTHROPOLOGY AND ETHNOLOGY

[We have been favoured by the Count Marshal of Austria with the following abstracts of the more important papers read at the Innspruck Congress.]

Prof. Semper on the Natives of the Pellew Islands

THESE natives have come to a comparatively rather high degree of civilisation, and have been wronged by being ranked among the primitive savages. Prof. Semper, who has lived several months among them, proves his assertion by a detailed exposition of their political, social, and religious institutions. The residence of the pontiff-king and the house where the chieftains of tribes hold their meetings, are decorated with painted basso-relievos. The traces of a commixture of the natives with the Malayan race are insignificant; they seem, however, to have mixed notably with the Papuans.

Prof. Strobel on the Paraderos of Patagonia

THESE "Paraderos" (from the Spanish verb *parar*, to stay) are accumulations of remnants of repasts, fragments of pottery, unpolished stone-knives, arrow-heads, &c., superficially covered with blown sand. In one of these accumulations a human skeleton, and several skulls of brachy-hypocephalous type, were found. The pottery had evidently been shaped by hand, and burnt hard by an open fire—not in furnaces. The distinction between a Palæolithic and a Neolithic period cannot be maintained with respect to the southern-most portion of South America; no polished stone-implements having hitherto been found south of St. Luis in the centre of the Pampas, although grinding-stones and polishable minerals are not wanting there. The polished stone-implements found at St. Luis may have been imported from Peru. The objects found in the Paraderos must be anterior to the European invasion; neither the Patagonians nor the Indians of the Pampas using at present any stone-weapons, but being armed with lassoes, bolas, and lances. Arrows and bows having gone out of use since the introduction of horses by the Europeans. The Indians of Chaco and the inhabitants of Terra del Fuego, who have refused to use horses, still use arrows as a weapon. Horsemanship seems not to have had any diminishing action on the size of the Patagonians, still conspicuous for tallness; the inhabitants of Terra del Fuego, though no horsemen, being rather of small size. Prof. C. Vogt said that the collections made in South America by Mr. Claray have safely arrived in Europe, and that among them is a human lower jaw of uncommon size. Prof. Seligmann observed that the skeleton of a giant, in the museum of Innsbruck, offers likewise an uncommonly developed lower jaw, a peculiarity mentioned by Prof. Langer as generally connected with gigantic size. Prof. Virchow remarked that the outline of the jaw in question is rather curved than angular.

Prof. Virchow on Comparative Measurements of Crania

IT is uncertain whether the great number of crania found in Denmark in sepultures of the Stone Period, belonged to the persons of rank, or to those immolated to honour them—as Mr. Worsaae concludes from the circumstances under which they have been found. At all events, they must have belonged to inhabitants of the Danish isles, or of their immediate neighbourhood. Ninety-six crania have been extracted from one single sepulture near Borreby. Those from the first part of the Bronze Period, when the dead were buried unburnt, are not abundant. Eschricht and Nilsson agree in ascribing the crania of the Stone Period to Laplanders or Esquimaux. The crania of the Greenlanders are conspicuously Dolichocephalous, not to say Scaphocephalous. The insertions of the temporal muscles are unusually distinct, the temporal ridges reaching beyond the *tubera parietalia*; the root of the nose is very narrow and the eyes very near to each other; the facial portion is more developed than in any other race. The few Dolichocephalous crania found in the sepultures are far from offering any analogy to those of Greenlanders. The Laplanders are known to be a Brachycephalous race, whose outward form is evidently the result of their mode of living. The temporal diameter of the crania of this people is ordinarily very considerable, the root of the nose is very broad, and the middle of the lower jaw conspicuously compressed. The crania from the sepultures do not bear more than a distant resemblance to the type just described. The crania of the Finnic race—abstraction made of individual variations—are relatively Brachy-hypocephalous. The Danish crania here in question may be possibly of Finnic origin, the more, as history proves this race to have spread formerly far more southward in Scandinavia than at present. Two forms may be distinguished among these crania, not so discrepant, however, as to indicate the co-existence of two distinct nations, although a commixture between two nations may have taken place. The Slavonians are nearly generally admitted to be Brachycephalous, and the Germans to be Dolichocephalous; the Poles and the Wends, however, make an exception to this rule, being anything but Brachycephalous. Crania from peat-bogs of North Germany exhibited to the Anthropological Congress, held at Paris in 1867, are decidedly Dolichocephalous, not Prognathous, without a decidedly ferocious expression, &c. Those extracted from the Danish peat-bogs exactly resemble them, and have, moreover, the greatest analogy with the crania of the Basques, a nation which (as may be historically ascertained) had spread in ancient times over Southern France and North Italy under the denomination of "Iberians." It results from all this, with some degree of probability, that the nation to which the skulls in the peat-bogs are to be referred was rather of Meridional than of Septentrional origin. Chronological dates, relating to the crania here in question, are still insufficient. At present, no trace of man's existence during the reindeer-period has been ascertained in North Germany. Remains of reindeer have been found in Mecklenburg and lately (probably) in the Uckermark, but not associated with any products of human industry. The co-existence of the Dolichocephali, whose remains have been found in peat-bogs, with the reindeer, is therefore not yet proved, although in some degree probable.

Prof. Seligmann on Exostoses in the Meatus auditorius of Peruvian Crania

SOME years ago Prof. Seligmann found on Peruvian crania of the Titicaca form (cylindrical, elongated by bandages) considerable exostoses on the external meatus auditorius, a very rare morbid affection. No traces of such exostoses were found on the other variety of Peruvian crania so nearly related to the North American form, and, like these, flattened by pressure between boards. The crania first referred to are most similar to the so-called *Avarian* skulls, to be met with from Austria as far as into France; but exostoses have never been found on any of these last. The cause of these exostoses was at last found out in the narrative of the conquest of Peru by coeval Spanish authors, all describing the solemn inauguration of the descendants of the Incas, who had come to the age of sixteen years. They were then compelled to undergo most exciting exertions of body and mind, and the hardest privations; the ceremony of piercing the ears being the term of these severe trials. The lobes of both ears were widened by introduction of metallic cylinders, so that a gold or silver disc of the diameter of an orange could find room in each of them. The Spaniards designated the wearers of this distinction as "Orejones" (Great-ears). A Spanish author says, it would scarcely be credible that the lobes of the ear could bear such a weighty ornament if they were not sustained by a loop of a finger's size. The excitement attending the trials, the comparatively late epoch of the operation, together with its rapid and violent mode of performance, may possibly have provoked a pathological process affecting the cartilaginous, and, subsequently, the osseous portion of the ears. Many tribes lengthen the lobes of their ears by introducing pieces of wood, &c., and a South American tribe is named "Orejones" on account of this custom; in these cases, however, the lengthened portion is relaxed. If the above supposition is exact, it leads to the following result: all the Peruvian crania of the Titicaca form, offering the above-described exostoses, are those of male individuals of high caste, having passed their sixteenth year; and the appellation "Inca crania," hitherto applied to flattened Peruvian skulls, is inexact. Deafness was not the necessary consequence of these exostoses, the narrowest aperture of the meatus auditorius being sufficient for the perception of sounds.

Dr. Glatter on the Influence of Race-differences on the Vital Processes

THIS influence is evident in the descendants of an Italian colony, living at Lemberg, who are in the habit of entrusting themselves for treatment to native Italian physicians, who, according to their custom, treat their patients with frequent bleedings, to the amount of 8 to 10 ounces, without any damage to their health. Poles, submitted to the same treatment, often suffer very bad consequences from it. The natives of Alpine regions established at Vienna are endowed with a high degree of vitality, and generally of longevity. The Servians are very prolific in their native country; north of Mohacs, however, the number of births among them is diminishing, so that the population of Servian places approaches extinction. Births are numerous and easy, and deaths comparatively few among Jews in every country, thus proving their strong accommodative power, and consequently their aptitude for commercial business. At Pesth, Jewish merchants reach a higher average age than Christian ones; the reverse is the case there among workmen, as tailors, &c. Suicide is rare, mental alienation frequent among Jews. Among Magyars, the number of births is moderate, the mortality rather great, and as a necessary consequence, the Slavonian population is more and more encroaching on them. Notwithstanding the very notable introduction of Turkish blood during the long occupation in the sixteenth and seventeenth century, the Finnic race and characters have suffered but little alteration, as proved by the striking similitude between Magyar and Finnic crania. The Hungarian Slovacks possess a high degree of vitality, and, notwithstanding certain noxious customs (hot wine given to women immediately after parturition, and brandy given to sucking babes), the mortality among them is anything but considerable. The Wends, living in groups in the Comitate of Wieselburg, are tall, with small heads, and, notwithstanding their irregular and excessive mode of living, generally attain to a good old age. The Germans in the Comitate of Pesth are all very prolific, but their number increases but moderately, as the rate of mortality is rather considerable. The Roumanians are reported to be generally of small size, with rather light brain, and to be subject to tuberculosis and to caries of the teeth. The Poles are more affected by epidemics than the Ruthenians; deaths among them are more numerous than births, while the Ruthenian population is constantly increasing in numbers. Prof. Sigmund has observed in the case of Italians and Spaniards, that wounds are more inclined to gangrene than in individuals of other races.

Prof. C. Vogt on a Microcephalous Subject

THE data concerning this subject have been communicated by M. de Vilanova, Professor of Geology at Madrid. His name is Vincenzo Ortis, of Codina, born at Castillon del Duca (province of Valencia), in 1813. The dimensions of his head are: facial angle, 59°; circumference of the cranium, 0·46 metres; upper arch, 0·19 m.; longitudinal diameter, 0·14 m.; transverse diameter, 12 m. His total length does not exceed 1 metre. The sternal limbs are very long, with a rudimentary sixth finger on each hand; the abdominal members are short, with a sixth toe on each foot; his whole body is covered with long hairs. His character is rather meek and timid; when irritated he tears his clothes without doing harm to others. He is unable to speak, but makes very expressive faces. His mode of progression is by leaps. The comparatively advanced age of fifty-six years, and the existence of six fingers and toes, make Ortis an exception among Microcephalous subjects.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 13.—The following papers were read:—"On the Mineral Constituents of Meteorites," by Nevil Story-Maskelyne, M.A., Professor of Mineralogy in the University of Oxford, and Keeper of the Mineral Department, British Museum. Communicated by Prof. H. J. Stephen Smith, F.R.S. [We are compelled to postpone an abstract of this paper.]

"On Fluoride of Silver." Part I. By George Gore, F.R.S. This communication treats of the formation, preparation, analysis, composition, common physical properties, and chemical behaviour of fluoride of silver. The salt was prepared by treating pure silver carbonate with an excess of pure aqueous hydrofluoric acid in a platinum dish, and evaporating to dryness, with certain precautions. The salt thus obtained invariably contains a small amount of free metallic silver, and generally also traces of water and of hydrofluoric acid, unless special precautions mentioned are observed. It was analysed by various methods: the best method of determining the amount of fluorine in it consisted in evaporating to dryness a mixture of a known weight of the salt dissolved in water, with a slight excess of pure and perfectly caustic lime in a platinum bottle, and gently igniting the residue at an incipient red heat until it ceased to lose weight. By taking proper care, the results obtained are accurate. The reaction in this method of analysis takes place according to the following equation: $2AgF + CaO = CaF_2 + 2Ag + O$. Sixteen parts of oxygen expelled equal thirty-eight parts of fluorine present. One of the methods employed for determining the amount of silver consisted in passing dry ammonia over the salt in a platinum boat and tube at a low red heat. The results obtained in the various analyses establish the fact that pure fluoride of silver consists of nineteen parts of fluorine and 108 of silver. Argentic fluoride is usually in the form of yellowish brown earthy fragments; but when rendered perfectly anhydrous by fusion, it is a black horny mass, with a superficial satin lustre, due to particles of free silver. It is extremely deliquescent and soluble in water; one part of the salt dissolves in ·55 part by weight of water at 15°·5 C.; it evolves heat in dissolving, and forms a strongly alkaline solution. It is nearly insoluble in absolute alcohol. The specific gravity of the earthy-brown salt is 5·852 at 15° 5 C.; the specific gravity of its aqueous solution, at 15°·5 C., saturated at that temperature, is 2·61. By chilling the saturated solution, it exhibited the phenomenon of supersaturation and suddenly solidified, with evolution of heat, on immersing a platinum plate in it. The solution is capable of being crystallised, and yields crystals of a hydrated salt; the act of crystallisation is attended by the singular phenomenon of the remainder of the salt separating in the anhydrous and apparently non-crystalline state, the hydrated salt taking to itself the whole of the water. The fused salt, after slow and undisturbed cooling, exhibits crystalline markings upon its surface. The dry salt is not decomposed by sunlight; it melts below a visible red heat, and forms a highly lustrous, mobile, and jet-black liquid. It is not decomposed by a red heat alone; but in the state of semi-fusion or of complete fusion it is rapidly decomposed by the

moisture of the air with separation of metallic silver; dry air does not decompose it. In the fused state it slightly corrodes vessels of platinum, and much more freely those of silver. The salt in a state of fusion with platinum electrodes conducts electricity very freely, apparently with the facility of a metal, and without visible evolution of gas or corrosion of the anode; a silver anode was rapidly dissolved by it, and one of lignum-vitæ charcoal was gradually corroded. A saturated aqueous solution of the salt conducted freely with electrolysis, crystals of silver being deposited upon the cathode, and a black crust of peroxide of silver upon the anode; no gas was evolved; with *dilute* solutions gas was evolved from the anode. By electrolysis of anhydrous hydrofluoric acid with silver electrodes, the anode was rapidly corroded. The electrical order of substances in the fused salt was as follows, the first-named being the most positive: silver, platinum, charcoal of lignum-vitæ, palladium, gold. In a dilute aqueous solution of the salt, the order found was: aluminium, magnesium, silicon, iridium, rhodium, and carbon of lignum-vitæ, platinum, silver, palladium, tellurium, gold. The chemical behaviour of the salt was also investigated. In many cases considerable destruction of the platinum vessels occurred, either in the experiments themselves, or in the processes of cleaning the vessels from the products of the reactions. Hydrogen does not decompose the dry salt, even with the aid of sun-light, nor does a stream of that gas decompose an aqueous solution of the salt, but the dry salt is rapidly and perfectly decomposed by that gas at an incipient red heat, its metal being liberated. Nitrogen has no chemical effect upon the salt, even at a red heat, nor upon its aqueous solution. Dry ammonia gas is copiously absorbed by the dry salt. In one experiment the salt absorbed about 844 times its volume of the gas. The salt in a fused state is rapidly and perfectly decomposed by dry ammonia gas, and its silver set free. A saturated solution of the salt is also instantly and violently decomposed by strong aqueous ammonia. Oxygen has no effect either upon the dry salt at 15° C., or at a red heat, nor upon its aqueous solution. Steam perfectly and rapidly decomposes the salt at an incipient red heat, setting free all its silver. No chemical change took place on passing either of the oxides of nitrogen over the salt in a state of fusion. By passing anhydrous hydrofluoric acid vapour over perfectly anhydrous and previously fused fluoride of silver, at about 60° Fahr., distinct evidence of the existence of an acid salt was obtained. This acid salt is decomposed by a slight elevation of temperature. Numerous experiments were made to ascertain the behaviour of argentic fluoride in a state of fusion with chlorine, and great difficulties were encountered in consequence of the extremely corrosive action of the substances when brought together in a heated state. Vessels of glass, platinum, gold, charcoal, gas carbon, and purified graphite were employed. By heating the salt in chlorine, contained in closed vessels, formed partly of glass and partly of platinum, more or less corrosion of the glass took place; the chlorine united with the platinum and fluoride of silver to form a double salt, and a vacuum was produced. By similarly heating it in vessels composed wholly of platinum, the same disappearance of chlorine, the same double salt, and a similar vacuum resulted. Also, by heating it in vessels composed partly of gold, an analogous double salt, the same absorption of chlorine and production of rarefaction was produced. And by employing vessels partly composed of purified graphite, a new compound of fluorine and carbon was obtained.

"Approximate determinations of the Heating Powers of Arcturus and α Lyræ." By E. J. Stone.—About twelve months ago the author began to make observations upon the heating-power of the stars. At the February meeting of the Royal Astronomical Society he first became aware of what Mr. Huggins had done upon this question: his arrangements, however, did not appear to the author to meet the difficulties. After some trials, he arranged his apparatus in the following novel manner:—He uses *two pairs* of plates of compounds of antimony and bismuth. The areas are about $(0{\cdot}075)^2$ inches, and their distance is about 0·25 inch. The poles are joined over in opposite directions to the terminals of the pile and galvanometer. The whole pile is screwed into a tube of one of the negative eyepieces of the great equatorial. This completely shuts the pile up in the telescope-tube. A thick flannel bag is then wrapt over the eye-piece and terminals. The bag is prevented from actually touching the case of the pile, and is useful in preventing the irregular action of draughts upon the case of the pile and terminals. The wires are led from the terminals of the pile to the observatory library. The two faces of the pile are so nearly alike, that the currents which are generated by any equal heating of them are exceedingly feeble. The telescope is first directed so that the star falls between the two faces, and allowed to remain thus until the needle is nearly steady at the zero. The star is then placed alternately upon the two faces, and the corresponding readings of the galvonometer taken as soon as the needle appears to have taken up its position, which usually takes place in about ten minutes. The author next referred to the way in which he refers his results to those produced by independent sources of heat at known distances. The mean result of the observations on two nights is 0°·0198 F., as a measure of the heating effect of Arcturus in raising the temperature of the plate of antimony and bismuth when the heat is condensed by the object-glass of 12·75 inches. The direct effect without object-glass would be 0°·000000685 F. The author had not yet determined the coefficient of absorption for the object-glass, but if it be provisionally taken at $\frac{2}{3}$, the direct heating effect of Arcturus = 0°·00000127 F. The result may be otherwise stated as follows:—That the heat received from Arcturus is sensibly the same as that from the face of a Leslie cube containing boiling water at 383 yards. In the case of α Lyræ the heating power is = 0°·0088 F. This result is so much smaller than those obtained from Arcturus, although the observations of Arcturus were made under more unfavourable circumstances, that the author cannot but regard it as a fact that the star Arcturus does give us more heat than α Lyræ—a result probably due to the same cause which gives rise to the difference in colour between these stars, viz., the greater absorption of the red end of the spectrum in the case of α Lyræ than in the case of Arcturus. He mentioned that on June 25, 1869, he made a comparison between Arcturus and α Lyræ. The result gave for the heat received from Arcturus: to that from α Lyræ : : 3 : 2; but on account of the observations of α Lyræ having been interrupted by cloud, they were not, however, sufficiently numerous to eliminate mere errors of reading. From these observations the author concludes that Arcturus gives to us considerably more heat than α Lyræ; that the amount of heat received is diminished very rapidly as the amount of moisture in the air increases; that nearly the whole heat is intercepted by the slightest cloud; that as first approximations, the heat from Arcturus, at an altitude of 25°, at Greenwich, is about equal to that from the cube containing boiling water at a distance of 383 yards. The heat from α Lyræ at an altitude of 60° is about equal to that from the cube at a distance of 860 yards. The form given to the pile appears likely to be useful in many inquiries respecting differences of heating power.

Ethnological Society, January 11.—Dr. Richard King in the chair. Col. Lane Fox read a note on the use of the mere or pattoo-pattoo of New Zealand, in which he showed that this weapon is used as a thrusting-instrument, and should not, therefore, be referred to the type of the club. He regards it as having had its origin from the stone celt, since a series of transitional forms may be traced connecting the two implements. The author's opinion on the use of the mere was supported by a letter from the Rev. J. W. Stack, of Kaiopoi, communicated to Dr. Hooker, C.B., in which the writer explained that the mere was always used for thrusting and not for striking.—A communication was read from Dr. Haast, F.R.S., on some stone implements discovered in Bruce Bay, New Zealand. A polished stone chisel and a sharpening-stone were found by a party of miners in an auriferous "lead." Advancing inland from the present shore of the bay, several distinct belts of land may be observed, each characterised by peculiar vegetation; and it was in the fourth belt, at a distance of 525 feet from the present high-water mark that these implements were found. They are now deposited in the Canterbury Museum, N.Z. Although these polished implements are much more highly finished than are the roughly-chipped implements hitherto found in or near moa-ovens, the author does not venture to express an opinion on the relative antiquity of the two types: indeed, he considers it probable that they may have been used simultaneously by two races co-existing in the islands—the more highly-civilised using polished tools and dwelling near the coast, while the inhabitants of the interior retained the use of roughly-chipped implements, and followed the dinornis as it retreated inland. Mr. Bonwick referred to the great antiquity of these gold-bearing terrace-deposits.—At the same meeting Dr. Gustav Oppert read a paper on the Kitai or Kari-kitai. These are a small race of about 50,000 persons, dwelling near the Caspian Sea in the Russian province of Derbend, and in the Siberian district of Guldja. They are the descendants of a race which once ruled

over China and Central Asia. One of their great princes, Yelintashe, was identified by the author with the celebrated Prester John or Presbyter Johannes. Dr. Oppert referred to the use of the names of metals by the Tatars as proper names and titles of dynasties—such as the Iron dynasty, Golden dynasty, &c. According to their own historical records, the Tatars had come from a district abounding in gold and iron. Dr. Hyde Clarke contrasted the valuable philological arguments brought forward in the present paper with the frivolous mode in which comparative philology is often employed. He alluded to the ethnological cause of the decline of the great empire of the Kitai, and referred it to the inability of any small dominant race to hold in subjection a large population composed of mixed races.

Royal Microscopical Society, January 12.—The Rev. J. B. Reade, F.R.S., president, in the chair. Mr. J. Browning read a paper on a new mode of measuring spectra bands. Mr. Browning described an adaptation, by himself, of the micrometer screw to the microscope, which afforded an easy and accurate method of measuring the bands of the absorption spectrum, and the invention was accepted as a valuable improvement on the method hitherto employed for the purpose.—Mr. W. S. Kent, F.Z.S., of the British Museum, read a paper on "the *Calcareous spiculæ* of the *Gorgonaneæ*, their modification of form, and the importance of their characters as a basis for genuine and specific diagnosis." This paper was illustrated by an elaborate series of drawings of the animals and the spicula of the various species.—In consequence of the time occupied by the reading of the previous papers, a contribution from Mr. A. Sanders, M.R.C.S., "On an undescribed stage of development of *Tetrarhynchus corallatus*" was taken as read.

DUBLIN

Royal Irish Academy, January 10.—Sir Robert Kane in the chair. The Rev. Dr. Dickson read an account of some portions of the "Ars moriendi," preserved in the manuscript room of the College Library, and compared it with the photographs of the perfect copy of this work in the collection of Herr Weigel, of Leipsig. It is an excellent specimen of block-printing. The fragments in the library appear to have been portions of an early printed volume, as well as of one printed with great care, and belonged to the edition in small folio of twenty-four leaves printed on the one side.—Dr. John Barker read a paper on the "illumination of microscopic objects." One of the most important improvements of late years in object-glasses of high powers has been the immersion of objectives of a particular construction into a film of water placed on the glass covering the object, whereby it is found that the definition, light, magnifying power, and working distance are each much increased. The object of the paper was to show how the present principle could be applied with great advantage to the general illumination of objects. The results of some experiments were then detailed. Wenham's paraboloid was altered as follows: the tip was ground flat, and a film of water was introduced between it and the under surface of the glass slide containing the object, free action of the stage movements being thus allowed, and no light was lost. This form of illumination is suitable to all kinds of axial illumination, though it is but right to add that it has only practically been tried in a form corresponding to Wenham's paraboloid. Details of the construction of the paraboloid, its size, and curvature were given; and the paper concluded by the author claiming for this mode of illumination the following advantages:—1. Objects are seen by light reflected from their surfaces, and, if transparent, from their interiors. 2. No disturbing light impinges on the retina. 3. All shadows are avoided. 4. The oblique rays of light are economised. 5. The light is purely achromatic. 6. The interior of partially transparent objects can be lit up. 7. Definition is improved. 8. It is easy of application; and, lastly, it is not expensive.—Dr. Stokes presented, on behalf of W. T. De Visme Kane, Esq., a large stone celt found in Ireland. A copy of the second volume of the "Brehon Laws," just published, was laid on the table. The following were elected members:—W. Archer, Professor R. Ball, R. Day, Sir T. Esmonde, Bart., T. A. Jones, Rev. J. P. Mahaffy, and J. P. O'Reilly.

PARIS

Academy of Sciences, January 10.—M. Delaunay communicated a memoir on the physical constitution of the moon, and Father Secchi one on the constitution of the solar corona, and some peculiarities presented by rarefied gases when rendered incandescent by electric currents: we shall return to these papers.—M. Becquerel presented the second part of his eighth memoir on electro-capillary phenomena, in which he treats of the muscular, nervous, and other currents.—M. E. Becquerel communicated a note on the determination of weak electromotor forces, in which he described a method of determining the amount of force developed in organic bodies and their parts.—M. Piarron di Mondesir communicated the second part of his paper on a new method for the solution of problems in mechanics, and M. Verdeil a note indicating two experiments to be made, by means of the pendulum, to determine the variation of the resistance of the air with the velocity.—A discussion on the proposed demonstration of Euclid's postulate of parallel lines, by M. Bertrand, was raised by the opening of a sealed packet deposited by M. Lionnet on the 27th December last. M. Boillot maintained that it is impossible to get rid of the idea of infinity when we attempt to demonstrate Euclid's postulate; and M. J. Hoüel, the impossibility of demonstrating the principle of the theory of parallels by means of a plane figure. M. Fuix called the attention of the meeting to a demonstration of the postulate, independent of the idea of infinity given by him in a published work.—In a memoir on nitrous acid by M. E. Frémy, the author stated that pure nitrous acid dissolves without decomposition in a great excess of cold water, but that it is split into nitric acid and dentoxide of nitrogen by the addition of pulverulent bodies. He also referred to the reducing properties of nitrous acid, and to its behaviour and modification by substitution under the influence of hydrogenated bodies.—MM. Odet and Vignon presented a paper on the action of dry chlorine upon dry nitrate of silver, in which they described an experiment confirming their previous supposition that in the preparation of anhydrous nitric acid by this means, the reaction combines two phases, namely: 1. Production of chloride of azotyle with evolution of oxygen; and 2. Reaction of the chloride of azotyle upon the excess of nitrate of silver.—A memoir was also presented by M. A. Boillot, on the synthesis of hydrosulphuric acid by exposing flowers of sulphur to the action of the electric spark in contact with hydrogen; and one by M. T. Schlœsing, containing analyses of the mineral contents of the waters of arable lands.—M. Feil addressed a reply to a previous note by M. Gaugain on the manufacture of artificial gems; and MM. A. Riche and P. Champion a memoir on the manufacture of tom-toms and cymbals.—Of biological papers only few were communicated, the chief one being a continuation of M. Lacaze-Duthier's researches upon the morphology of the Mollusca, in which the author treats of the *Lamellibranchiate Acephala*, or ordinary bivalved molluscs.—M. Colin discussed the question whether there is any relation between the intelligence of animals and the development of the nervous centres, and showed by numerous tables that there is no exact relation between the size of the encephalon and the observed intelligence.—M. Bergeon noticed the purpose of the lachrymal glands, which he considered to be chiefly the moistening of the air passages, and of the air passing through these to the lungs, so that they are really connected with the function of respiration.—M. Drouyn de Lhuys communicated an extract from a letter noticing the attacks of an *Acarus* upon the grape-vines at the Cape of Good Hope. The parasite is said to live upon the roots, and between the bark and wood of the plants.

VIENNA

Imperial Academy of Sciences, December 9, 1869.—Dr. L. J. Fitzinger communicated the first part of a critical revision of the Rhinolophous family of Bats. It treated of the genera *Cœlops*, *Phyllorhina* and *Asellia*.—Director Tschermak presented a memoir on the form and composition of the Felspars.—Dr. J. Peyritsch communicated a memoir on abnormities of structure in the Umbelliferæ, containing the description of a series of malformations of the flowers in *Carum Carui*, *Daucus Carota*, *Torilis anthriscus*, and *Peucedanum Chabræi*.—In connection with these the author discussed the axial or carpellar nature of the fruit of the Umbelliferæ, and remarked that such cases show that the distinction between leaf and axis is not well founded in nature.—M. Schrauf noticed the occurrence of Brookite in iron-glance from Piz Cawradi, to the south of Chiamut in the Tavetsch valley of Graubündten.—The table of observations at the Central Meteorological Observatory, for the month of November, was communicated.

December 16, 1869.—Professor Reuss communicated a memoir by Dr. Manzoni "On the Italian Fossil Bryozoa," in continuation of previous papers by the same author. The present paper related exclusively to the genus *Lepralia*, of which the author described and figured 21 species, 15 of them as new

forms. Of the species described, 12 are from the middle miocene of Turin, 4 from the middle pliocene of Castelarquato, and the remaining 5 from the upper pliocene of the neighbourhood of Reggio in Calabria.—Professor A. Winckler presented a memoir on some formulæ and methods relating to the theory of definite integrals.—Dr. Tiele, of Bonn, and Dr. T. Oppolzer communicated statements of the elements of Comet III., 1869, and the latter a memoir on the determination of the path of a comet.—Professor E. Hering presented a first memoir on the influence of respiration on the circulation of the blood, in which he maintained that the system of the vasomotor nerves experiences a periodic innervation by the respiratory nervous centre, which causes periodic contractions of the muscular coat of the vessels. These he regarded as respiratory movements of the vascular system.—Dr. F. Steindachner communicated the second portion of his memoir on the Fishes of the Senegal, in which he described the species (28 in number) belonging to the families Gobiidæ, Mugilidæ, Gerridæ, Chromidæ, Anabatidæ, Pleuronectidæ, and a part of the Siluridæ. Four of these were described as new, namely: *Eleotris senegalensis*, *daganensis*, and *Lebretonis*, and *Mugil Dumerili*. The first two belong to Bleeker's sub-genus *Culius*, the previously known representatives of which occur in the Indian Ocean and Polynesia. The author regarded *Chromis mossambicus* (Peters) as specifically distinct from *C. niloticus*, *Hemichromis bimaculatus* and *auritus* (Gill), and *H. guttatus* (Günther) as varieties of one species, *Chrysichthys acutirostris* (Günther) as identical with *C. nigrodigitatus* (Lac.), and *Gerres octactis* (Bleeker) with *G. melanopterus* of the same author.

BERLIN

German Chemical Society, January 10. — The following papers were read:—Wichelhaus, "On a base isomeric with cyanide of ammonium." By the action of the tribasic formic ether $CH(OC_2H_5)_3$ on acetamide $NH_2C_2H_3O$, a base of the following composition, $CH.NC_2H_3O.NHC_2H_3O$, was obtained. This substance is converted by water into the acetate of the new base $CH.NH.NH_2$. The latter is a volatile liquid, yielding crystallised salts.—Philipp, "On perchloride of iodine;" Hansen, "On the ethylides of thallium;" P. W. Hofmann, "On the manufacture of sulphuric acid." The author, who is at the head of the manufactory of Dienze in France, accounts for the loss of oxides of nitrogen in the manufacture of sulphuric acid, by proving that these oxides are partly reduced to nitrogen, when the sulphuric acid in the lead-chambers sinks below a certain strength. The loss can therefore be avoided by carefully regulating the steam admitted into the chambers.—Schoras, (1) "On the influence of sunlight on the reduction of metallic chlorides through oxalic and tartaric acids;" (2) "On the colorisation of dry platinocyanides through traces of moisture."—Friedel and Ladenburg, "On silicopropioxic acid;" Tieman, "On derivatives of guanidine;" Junning, "Mechanical explanations of chemical reactions;" Schuchard, "On the preparation of zirconium."

BONN

Lower Rhenish Society for Natural and Medical Science—*Chemical Section*, November 13. — Professor Binz exhibited a new body, "Dihydroxylchinin," obtained by G. Kerner from quinine by treating the latter with potassic permanganate. It gives all the reactions of the alkaloid, but differs from it amongst other things in the want of basic properties and the absence of influence on the organ of taste. It likewise differs from it essentially in its physiological properties, being entirely indifferent even in large doses. Dr. Zinke gave an account of new synthesis of aromatic acids. He has obtained phenyl-acetic acid from Monochloroacetic acid, and brombenzol by treatment with finely-divided silver. Professor Kekulé communicated the results of some experiments of Dr. Thorpe, showing that bromine free from iodine enters the ethyl-group of ethylbenzole even at a low temperature, and that from the bromide thus formed various derivatives can be obtained, some of which have already been proved by Berthelot. Dr. Budde gave a preliminary report on his researches on the electric conductivity of hydrogen, oxygen, and nitrogen, at various pressures. His results agree most nearly with those of Faraday, and show a greater decrease of resistance than of pressure.

November 27.—Dr. Muck communicated his recent researches on the formation of manganic sulphide from various manganese salts and various soluble sulphides. Professor Rhitthausen likewise made some communications on the occurrence (not hitherto observed) of amygdaline in vetch seeds.

DIARY

THURSDAY, JANUARY 20.

ROYAL INSTITUTION, at 3.—On the Chemistry of Vegetable Products: Prof. Odling.

ROYAL SOCIETY, at 8.30.—On the Mechanical Performance of Logical Inference: W. Stanley Jevons.—Preliminary Paper on certain Drifting Motions of the Stars: R. A. Proctor.—On Jacobi's Theorem respecting the relative Equilibrium of a Revolving Ellipsoid of Fluid, and on Ivory's Discussion of the Theorem: J. Todhunter, F.R.S.

LINNEAN SOCIETY, at 8.—On the Flora of Iceland: Prof. Babington.—On New British Spiders: Rev. O. P. Cambridge.

ZOOLOGICAL SOCIETY, 8.30.—Descriptions of a new genus and of eighteen new species of Land and Marine Shells: Henry Adams.—On the genus Pelargopsis of the family Alcedinidæ: R. B. Sharpe.—Description of a new Fish from the vicinity of Aden: Lieut.-Colonel R. L. Playfair.

CHEMICAL SOCIETY, at 8. ANTIQUARIES, at 8·30.

NUMISMATIC SOCIETY, at 7.

FRIDAY, JANUARY 21.

ROYAL INSTITUTION, at 3.—On Haze and Dust: Professor Tyndall.

PHILOLOGICAL SOCIETY, at 8.15.

SATURDAY, JANUARY 22.

ROYAL INSTITUTION, at 3.—On Meteorology: Mr. Scott.

ROYAL BOTANIC SOCIETY, at 3.45.

MONDAY, JANUARY 24.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

ENTOMOLOGICAL SOCIETY, at 7.—Anniversary Meeting.

LONDON INSTITUTION, at 4. MEDICAL SOCIETY, at 8.

TUESDAY, JANUARY 25.

ROYAL INSTITUTION, at 8.—On the Architecture of the Human Body: Prof. Humphrey.

ETHNOLOGICAL SOCIETY, at 8.—On the Origin of the Tasmanians, geologically considered: J. Bonwick.—On a Frontier-line of Ethnology and Geology: H. H. Howorth.—The Nicobar Islanders: G. M. Atkinson.

INSTITUTION OF CIVIL ENGINEERS, at 8.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY, at 8.30.

WEDNESDAY, JANUARY 26.

SOCIETY OF ARTS, at 8.—On the Modes of Reading in Use by the Blind, and the Means for arriving at Uniformity: Thomas Armitage, M.D.

GEOLOGICAL SOCIETY, at 8.—On the Crag of Norfolk and associated Beds: Joseph Prestwich, F.R.S., F.G.S.—On the Fossil Corals of the South Australian Tertiary Deposits: Dr. P. Martin Duncan, F.R.S., Sec. G.S.—Note on a very large undescribed Wealdean Vertebra: J. W. Hulke, F.R.S., F.G.S.

ARCHÆOLOGICAL ASSOCIATION, at 8.

BOOKS RECEIVED

ENGLISH.—The Year-book of Photography for 1870: G. W. Simpson (Piper and Carter).—The Bible in India: Louis Jacolliot (J. C. Hotten).—The Body and its Health: E. D. Mapother, M.D. (Simpkin, Marshall and Co).—Natural Phenomena and Chronology of the Seasons, Part I.: E. J. Lowe, F.R.S. (Bell and Daldy).—Journal of the Statistical Society.—The Geology, Botany, and Zoology of the Neighbourhood of Alnwick: G. Tate (H. Hunter).

FOREIGN.—Note sur les Surcharges à considerer dans les Calculs des Tabliers Metalliques: M. L. Leygue.—Chênes de l'Amerique Tropicale.—Compendium der Physiologie des Menschen: Julius Budge.—Untersuchungen aus dem Physiologischen Laboratorium in Würzburg: R. Gescheidlen.—Die Praktische Markscheidekunst: E. Borchers.—Industries anciennes et moderne de l'Empire Chinois: Paul Champion.—Annales des Sciences Géologiques: Hébert et Alph. Milne-Edwards.—Cours Elémentaire de Mecanique Theorique et Appliquée: Ch. Delauny.—Cours Elémentaire d'Astronomie: Ch. Delaunay.—Recherches sur l'Ancienneté de l'Homme dans les Grottes et Monuments Megalithiques du Vivarais: J. Ollier de Marichad—Berliner Astronomisches Jahrbuch für 1872: W. Förster.—Handbuch der Chemischen Technologie: P. A. Bolley.—Ueber die Altesten Formen des Organischen Lebens: Ferd. Roemer.—Ueber den Parasitismus in der Organischen Natur: Maximilian Pertz (through Williams and Norgate).

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ERRATA.—Page 238, second column, line 34 of footnote: for "habitude" read "hebetude."—Page 289, second column, line 37: for "rectorial" read "vectorial."

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In those portions of "NATURE" more especially interesting to Scientific men are given:

V. Abstracts of important Papers communicated to British, American, and Continental Scientific societies and periodicals.

VI. Reports of the Meetings of Scientific bodies at home and abroad.

In addition to the above, there are columns devoted to Correspondence.

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