THURSDAY, FEBRUARY 1, 1877

DARWIN'S "GEOLOGICAL OBSERVATIONS"

Geological Observations on the Volcanic Islands and Parts of South America visited during the Voyage of H.M.S. "Beagle." By Charles Darwin, M.A., F.R.S. Second Edition, with Maps and Illustrations. (London: Smith, Elder, and Co., 1876.)

MR. DARWIN'S important contributions to biological observation and theory have during the last seventeen years attracted so much public attention, that there is some danger—one from which, however, all geologists will claim exemption—of his valuable labours in almost every department of geological research being to some extent lost sight of. Long, however, before the publication of the "Origin of Species," its author had achieved a foremost place in the ranks of the cultivators of geological science; nor must it be forgotten that the great work itself is as much a contribution to geology as to biology. Students of Mr. Darwin's earlier geological writings must all have been impressed by the powers of minute observation, the acumen in testing, and the skill in grouping data, and the boldness and originality in generalisation which distinguish their author; for these characteristics are no less conspicuously displayed in the theory of Coral Reefs than in that of Natural Selection.

In December, 1831, Mr. Darwin sailed from England in H.M. surveying vessel the *Beagle*, having accepted an invitation from the late Capt. FitzRoy to act as volunteer naturalist to the expedition then being despatched to complete the survey of the coast of South America. After an absence of nearly five years—during which many of the islands in the Atlantic were examined, large portions of both the east and west coasts of South America fully explored, several inland traverses of that continent made, the Falkland and Galapagos Islands carefully studied, and more rapid visits paid to Tahiti, New Zealand, Australia, Tasmania, the Cape of Good Hope, and a number of the coral islands in the Indian Ocean—the expedition returned to this country in August, 1836. Not a few important scientific discoveries will be associated with the names of the vessels of the United States Exploring Expedition, with the *Novara*, the *Challenger*, and many another surveying ship that might be mentioned; but it will be long indeed, we suspect, ere any vessel attains such a proud position in the annals of science as was won by the little ten-gun brig which bore our naturalist in his now famous "Voyage Round the World." Wherever in future the sciences of biology and geology shall be cultivated, there will the name of the *Beagle* become a household word.

The ten years which followed his return to England would appear to have been mainly devoted by Mr. Darwin to the publication of the numerous and important results obtained during the voyage. Besides editing the treatises of Prof. Owen, Mr. Waterhouse, Mr. Gould, the Rev. L. Jenyns, and Mr. Bell on the different groups of vertebrate animals, of which specimens were brought home, he wrote two very important works, one addressed to general readers—the "Naturalist's Voyage Round the World"—and the other of a more purely scientific character—the "Geology of the Voyage of the *Beagle*."

Before the publication of the "Origin of Species" had made the author's name so widely famous as it is at present, the works which we have named above, with the several memoirs communicated by their author to the *Transactions* and *Journal* of the Geological Society, had become universal favourites with the students of various branches of natural science; and this, no less on account of the rich store of novel observations which they contained, than for the originality and suggestiveness of their deductions from those observations. And since the appearance of their author's *magnum opus*, we confess that these earlier writings have for ourselves acquired a strange fascination. Again and again have we perused them, only to detect valuable observations and striking suggestions before missed, and to encounter fresh traces of the germs of ideas, that, after twenty-eight years of earnest thought and study, were developed into the theory of descent with modification, which is now exercising so important an influence on the progress of the natural sciences. At the commencement of the present notice we ventured to claim for geology at least a moiety of the advantages which have flowed from the publication of the "Origin of Species;" and, on the other hand, we feel that we are putting forward no undue demands on behalf of the same science, in declaring that the theory of Natural Selection must be regarded in at least as great a degree the prize of geological observation as the reward of biological research.

Such being the case, these "Geological Observations" are well worthy to take their place in the long series of the author's contributions to the doctrine of descent, side by side with those more widely known works on different departments of zoology and botany which have been published subsequently to the "Origin of Species." Two years ago the first part of the "Geology of the Voyage of the *Beagle*"—a work which has long been out of print, and has become extremely scarce—was republished; and naturalists and geologists were alike gratified by the appearance of this revised and enlarged edition of the well-known memoir on Coral Reefs. The work now before us is a re-issue of the remaining portions of the "Geology of the Voyage of the *Beagle*," and will be equally welcome to a large section of the scientific public.

The districts described in the present work, as Mr. Darwin justly observes in his preface to the new edition, "have been so rarely visited by men of science" that very little "could be corrected or added from observations subsequently made." And on the other hand attempts to modernize the terminology could scarcely fail to detract from the minute accuracy of observations, which were clearly either recorded upon the actual spot where they were made, or at all events while the memory of them was still fresh and vivid in the mind of the author. We think, therefore, that a wise discretion has been exercised in allowing the descriptions and discussions of phenomena to remain in precisely the same form as when they were originally drawn up; though we must confess to a feeling of disappointment at the absence of notes from the author's pen, indicating how far in his own view some of these original conclusions have been strengthened or modified by his later studies and researches.

We can only permit ourselves to recall a few of the more important among the valuable contents of this book

to the memories of our readers—and in doing so we shall dwell more particularly on such as, through recent discoveries or controversies, have acquired especial interest at the present day.

Every explorer who, since the publication of the "Observations on Volcanic Islands," has been called upon to investigate districts containing extinct volcanos, has been greatly aided by the valuable store of facts and suggestions contained in that work. We very much doubt, however, whether some of the interesting questions discussed in it—and we more especially refer to those relating to the nature and origin of the banded structure in lavas, with the light which these are calculated to throw on the difficult problem of the cause of foliation in rocks—have received that amount of attention from geologists, of which they are certainly deserving.

The proofs of the long-continued elevation of the shores of South America for thousands of miles, and to the height of many hundreds of feet, yet unattended with marked disturbances of the strata, the gradual disappearance of every trace of organism in rocks which once abounded with them, the survival of a most remarkable fauna of gigantic vertebrates to post-tertiary times, and its seemingly sudden extinction at a very recent period—these are some among the many interesting facts described in the second part of the work which are of especial value to geologists seeking to interpret the records of the past. Mr. Darwin's observation of an admixture of Jurassic and Cretaceous types of life in the same deposits in South America have acquired fresh significance now that the United States geologists have shown that ammonites range up into the tertiary strata, and that Dr. Waagen has described ammonites, goniatites, and ceratites, occurring in India, in the same bed with several carboniferous species of brachiopods. Now, too, that so much has been done by Dana, Le Conte, and others, in determining the mode of origin of the Rocky Mountains, and the part played by the volcanic outbursts which occurred simultaneously with the mountain-forming movements, Mr. Darwin's clear descriptions of the sections noticed by him in his traverses of the chain of the Andes will be referred to with fresh interest by geologists: and the comparison of phenomena displayed in distant parts of the same great chain is highly suggestive. But space fails us to refer to even a tithe of the points of interest which we have noted in our reperusal of this valuable work.

A striking characteristic of all Mr. Darwin's writings, and one which is very eminently displayed in the work before us, is his scientific candour. Like his teacher and friend, the late Sir Charles Lyell, he never forgets in his discussions to look at all sides of the questions before him, and to give the fullest expression and weight, alike to the difficulties which he himself detects, and to arguments which opponents may have advanced. With superficial readers this peculiarity in the writings of Lyell and Darwin has apparently very unjustly detracted from their merits; and we are sometimes amused by finding critics boldly parading as their own, objections which it is perfectly clear that only the candour of the authors has permitted them to rehearse, but which their own knowledge has not sufficed to enable them to understand or to make adequate use of.

Perhaps at no period in the history of the science have the great facts of geology suffered so much distortion from the works of pseudo-scientific writers—through which media alone science is too often, alas! transmitted to the general public—than at present. These writers selecting a few isolated and imperfectly understood facts, in bold defiance or lamentable ignorance of a thousand unmistakable and clearly established principles, proceed to build up the most elaborate hypotheses. We cannot therefore help regarding the republication of Mr. Darwin's "Geological Observations" as a most opportune event. The able geologist, De la Beche, many years ago wrote a charming little book entitled "How to Observe in Geology." To those anxious to learn this most important art at the present time, we would recommend as a model—since example is better than precept—the work now before us. The careful study of the clear and minute descriptions of geological phenomena, and the following step by step of the fair and cautious discussion of facts and arguments contained in this book can scarcely fail indeed to teach the reader something which is even more valuable than "how to observe," namely, how to *reason* in geology.

JOHN W. JUDD

TWO "CHALLENGER" BOOKS

Log Letters from the "Challenger." By Lord George Campbell. (London: Macmillan and Co., 1876.)

The Cruise of Her Majesty's Ship "Challenger." By W. J. J. Spry, R.N. With Map and Illustrations. (London: Sampson Low and Co., 1876.)

IT was to be expected that with so carefully-selected and intelligent a staff, both naval and civilian, on board, the cruise of the *Challenger* would be productive of something more than the official literature. It will have been seen from the "Preamble" which we recently published (*antea*, p. 254) that it must necessarily take a long time to arrange the abundant scientific results that have been obtained, and the complete official accounts may not be in the hands of the public for years. The Report on the Austrian *Novara* Expedition has taken seventeen years' serious labour to complete; but we hope to be in possession of the *Challenger* Reports in a much less space of time. Meantime many readers will be glad to have in a handy form a general account of the work which the expedition has done, and some details concerning the incidents of the long cruise and the many places which the ship visited. From either of the books before us such information may be obtained.

There is a wide difference, however, between the characters of the two works. Lord Campbell's is by no means an attractive book at first sight. It is a big, plain, heavy-looking volume, with a large page well filled with type, enormous paragraphs of sometimes half-a-dozen pages in length, and with not a single picture. One is apt to sigh at first at being compelled to read it, but after perusing a few sentences the reader "puts on the garment of praise for the spirit of heaviness," and finds the real difficulty to be to stop. Lord Campbell's pages bear all the marks of being genuine letters, written with no thought of a public before him, and only for the entertainment of those to whom they were sent. He has evidently not "got up" his subject at all, the information he conveys being almost

entirely the results of his own observation. And a thoroughly good and most original observer he is, with a faculty of telling what he has seen in such a way as keeps the reader in a constant state of exhilaration. Lord Campbell makes no pretence at instruction, nevertheless in his own characteristic and irresistibly amusing way, he conveys a vast amount of information concerning all the places visited by the *Challenger*, and tells not a little of the results of the soundings and dredgings carried out by the "scientifics," as he calls the civilian staff. But apart from the genuine entertainment to be had from the work, its great value will be the observations made by the author on the various peoples among whom he sojourned for a longer or shorter period. Even well-informed ethnologists, we should think, will be able to obtain not a little important information from the work on the present condition, for example, of the inhabitants of many of the Pacific islands, such as those of the Friendly, Fiji, and Sandwich Islands, New Guinea, Admiralty Islands, &c. A very large space is devoted to Japan, and many shrewd remarks made on the present condition and future prospects of the Japanese. There are many interesting notes besides, on the physical aspect and natural history of most of the places visited, and in almost all cases it will be found that some new feature has been brought out. Even of such well-worn subjects as Teneriffe, the Azores, Cape de Verde Islands, Australia, New Zealand, Tahiti, the South American littoral, Lord Campbell manages to say something unexpectedly original. In a concluding chapter he gives an instructive summary of the *Challenger's* work and how she did it. We can only, in our space, speak in the most general way of the nature of the contents of this work. A more entertaining, a more genuinely bracing book, it would be difficult to find, and the reader who goes carefully to the end of it will have added considerably to his knowledge of the earth's surface. It is a great pity that a work so full of varied information should have been published without either an analytical table of contents or an index. A map would also have been a great help to the reader.

Mr. Spry professes simply to give a plain, straightforward narrative of the cruise, of some of the chief results obtained by sounding and dredging, with notes on the places and people visited, partly the result of his own observation and partly of reading. The book is nicely got-up, well printed, and contains a large number of interesting and well-executed illustrations, not only of people and places, but of the implements used in carrying out the work of the *Challenger*. He gives a very clear account of the various apparatus used, their construction and uses, which we commend to the perusal of the uninitiated reader who wants to know how such work as that of the *Challenger* is performed. Mr. Spry gives the curious story of the brothers Stoltenhoff, who were found living alone on Inaccessible Island, at considerable length, in the words of the elder brother. The work contains a really large amount of valuable information, and as no two men observe alike, we commend those who desire to have complete information about the cruise of the *Challenger*, to read both books. While Mr. Spry sometimes unnecessarily introduces information obtained from books, his work is, on the whole, thoroughly readable, and certainly instructive. Altogether, these preliminary "snacks" augur well for the great official feast which is being prepared for us.

OUR BOOK SHELF

The Year-Book of Facts in Science and Arts for 1876. Edited by James Mason. (London: Ward, Lock, and Tyler, 1877.)

THE "Year-Book of Facts" so long associated with the name of the late indefatigable Mr. Timbs seems to have taken a new lease of life under the present editor, who begins his duties with the volume before us. Some useful changes have been made; thus, there is a marked improvement in the arrangement and character of the contents, and the period covered by this year-book now extends from the autumn of one year to the autumn of the following, and not January to December as heretofore. The longer time thus given for preparation has been well used by Mr. Mason, who certainly has produced a volume far in advance of any of its predecessors. It is hardly necessary for us to say much about this well-known year-book, which does not pretend to be more than a popular digest of scientific scraps; and in no sense supplies the need, to which we have alluded in previous notices, of a carefully-prepared record of scientific progress—the nearest approach to which in the English language is the American "Annual Record of Science and Industry," edited by Mr. Baird. The present editor of the book before us has done his work, so far as it goes, in a comprehensive and careful manner. One or two serious omissions we notice, notably the important discovery made by Dr. Ker, and announced at the last meeting of the British Association, of the rotation of the plane of polarisation by reflection from a polished magnetic pole, certainly one of the most novel physical facts of the past year.

LETTERS TO THE EDITOR

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

Just Intonation

YOUR esteemed correspondent, Col. A. R. Clarke, declines to admit any error or oversight in his communication to NATURE under the signature of "A. R. C.," on December 21, 1876. I regret to be of a different opinion. The first error which I pointed out ought to be palpable enough to a mathematician who is acquainted with the ratios of a scale, and therefore I treated it as an oversight. Col. Clarke gives 27 to 16, instead of 5 to 3, as the ratio for A in our present diatonic scale. By his enlargement of the interval he would unknowingly convert the well-known consonance of a major Third between F and A into a Pythagorean ditone. Evidently the Colonel is not aware that F and A are notes *interposed* in the scale of C, that harmonically they belong to F, and require F as a consonant bass. All this has been explained in a pamphlet in print. I would therefore suggest to Col. Clarke an investment of sixpence in the purchase of a "Review of Helmholtz's New Musical Theories," published by Novello, Ewer, and Co., No. 1, Berners Street. To prove all the unadmitted charges would be too long for NATURE, and Col. Clarke will find most of them touched upon in that pamphlet as well as other current errors. In the meantime it is not difficult to tell that Col. Clarke derived his ratio from the harmonic scale of C, and not from that of F (which would have been right), or from the work of any modern mathematician.

It is because F and A are harmonically derived from F that our scale cannot be reduced to a common denominator. The F string exceeds the length of the C string by half, the ratio being 3 to 2, and one of the first rules of proportion is that

"ratio can exist only between quantities of the same kind." When mathematicians employ the figures of 4 to 3 in the scale of C, the interval they really obtain is from C down to G (not from F down to C, as they have supposed), and when 5 to 3, they have the major Sixth from E down to G, and not the one from A down to C.

Music is a much more simple science than most men suppose. All that a mathematician requires is a set of harmonic scales, with powers of 2 and 3, before him. The scales include all ratios, and all consonances with their proportions of accompanying dissonances. They are scales of aliquot parts, and those aliquot parts are the corresponding multiples of vibrations. Time spent upon calculating temperament is but thrown away, because no mathematician's figures will be adopted in practice. Practical musicians will continue to tune by listening to the beats, as they have ever done, and perhaps all other musicians before them. The recommendation to eschew temperament may be even worthy of consideration in another point of view. Mathematicians have not given sufficient attention to the musical side of the question. Have we not, of late years, heard much of proposals to divide the octave into "twelve equal semitones"? Are we to imagine that this is a mathematician's idea of "*equal* temperament"? If music were but geometry, it would be an admirable arrangement: twelve equal semitones, like twelve equal inches in a foot. But, unfortunately, a musical scale is the very reverse of a geometrical one, and there are no two intervals alike in it. As it rises, the dimensions become less at every step, in ratio and in length. For instance, the ratio of C to C sharp is 16 to 17, that of G to G sharp (G being the half-way in point of vibrations) is 24 to 25, and from B to octave C is 30 to 32, but only because we omit B sharp, otherwise it would be 31 to 32. Fancy two such extremes "tempered" to the middle note! Only one of the twelve would fit into a musical scale, and there would be eleven discordant semitones out of the twelve. The discord would not be confined to one key only, but would be the same in every key. The so-called "diatonic semitones" are really tones. E is the seventh to F, which requires F as a bass, and B is the seventh to C in the scale of C. Are these to be changed into chromatic semitones?

The diminished attraction of music, some persons even disliking it, is mainly, if not wholly due to tempered tones. The first point to be considered by mathematicians who temper scales is the meaning of the two words, "consonance" and "dissonance." The charm of music depends upon "coincident," and "non-coincident" vibration.

In justice to Col. A. R. Clarke, let me add that I find only the first error to be his own, and am still disposed to attribute it to oversight in referring to a wrong scale. All the others are after precedent, and every source might be pointed out, although he is disposed chivalrously enough to defend those upon whom he relied. My excuse for writing at all is that NATURE is a purely scientific journal, and that I share with others an earnest wish to uphold it as a fair representative of English thought. Articles such as those of Col. Clarke and my own would be distasteful to any but scientific readers. As to the "comma of Pythagoras," it is not worth discussing. In spite of his chivalry, Col. Clarke knows as well as I do, that such an array of figures, representing vibrations, as 524, 288, cannot arise in less than nineteen octaves.

WM. CHAPPELL

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The Nebula of Orion

IN NATURE, vol. xv. p. 201, in an account of the American Cambridge Observatory, it is stated that the nebula of Orion had not shown the slightest trace of resolvability under Lord Rosse's 3-feet reflector.

The authority for this statement is, I suppose, Nichol's "Thoughts on the System of the World," where, p. 52, it is said that in 1844–5, the 3-feet did not contain the vestige of a star in the nebula.

On the occasion there referred to the speculum must have been in bad order, for the resolution of parts of the nebula is quite within the reach of the instrument in its normal condition. In proof of this I may refer to Lord Oxmantown's paper on the Nebula of Orion, *Phil. Trans.*, 1861, and to an extract from my own note-books of an earlier date, February, 1848:—"With the 3-feet saw the nebula of Orion resolved as far up as the little bay and C 1 Orionis—powers 351 and 320—best with the latter which is a single lens."

I may add that Nichol also states, p. 55, that he had received from Lord Rosse, March 19, 1846 intelligence "that all about the trapezium is a mass of stars (in the six feet); the rest of the nebula also abounding with stars, and exhibiting the characteristics of resolvability strongly marked."

Observatory, Armagh, January 19 T. R. ROBINSON

Basking Shark

MY notice of Prof. Steenstrup's paper was written in the autumn of 1875, to accompany an electrotype of the woodcut in that paper of the baleen-like fringes of the basking shark, sent to me for NATURE from Copenhagen.

At the time I was quite ignorant that my friend and former master, Dr. Allman, had written on the subject, nor could the keenest bibliographer have known much of the contents of his memoir, as the only reference to it in the Fourth Annual Report of the Dublin Natural History Society for 1841–42, is "Two [papers] have been read on Icthyology; that on the basking shark (*Selachus maximus*) by Mr. Allman, caused him to notice the value of the fisheries of our southern coasts, abounding in large fishes and cetacea, whose capture would prove highly profitable to our fishermen from the quantity of oil they would yield." In June, 1876, on the arrival of the specimen in Dublin from Bofin, I had a woodcut made of a branchial arch with the fringe attached, and added a brief account of the specimen now in the Dublin Museum. About that time Dr. Allman told me that notes of his paper had been published in the *Saunders's News Letter*, but that he had forgotten the date. Guided by the notice in the Dublin Natural History Society's Report, I searched the files of that paper for the years 1841 and 1842 without success, but I fully purposed to mention what Dr. Allman had told me, from memory, of his researches, when I should get a proof of my manuscript. Unfortunately, from press of matter, my notes were not published until many weeks after they were sent, and the proof reached me during long vacation, when I completely forgot to do as I had intended. I regret this exceedingly, and hope Dr. Allman will accept my apology. I cannot, however regret, that it has induced Dr. Allman to publish an abstract of his paper (NATURE, vol. xiv. p. 368), and perhaps he may still further furnish us with the date of its original publication.

In answer to the note of Prof. Enrico Giglioli, which has called my attention again to this subject, I have simply to state that finding no notice in the Zoological Records for 1873 or for 1874 (this latter published May, 1876) of any papers on Selache, I concluded, as it now appears wrongly, that nothing had been written during these years on the subject. This was my misfortune, perhaps my fault; but regarding Italy as the mother country of all the sciences, being well aware of the advances she has made in biological researches during the last twenty years, and having gone each year, while one of the zoological recorders, to Florence to work out the Italian literature of the preceding year, I cannot accuse myself of any intentional neglect of the labours of Italian biologists. It is to be hoped that Prof. Giglioli will favour us with an abstract of Prof. Pavesi's memoir, especially of the reasons that induce Prof. Pavesi to assert that our Bofin shark is *S. rostrata* and not *S. maxima*, for to me it appears that our seas may possess both these species.

It may also be mentioned that no reference to Prof. Pavesi's memoir will be found in the account of the Pelerin appended to Prof. Lütken's "Fishes of Greenland," prepared for the use of the British North Polar Expedition, 1875.

E. PERCIVAL WRIGHT

Sense of Hearing in Insects and Birds. "Towering" of Birds

I AM glad to learn from Mr. M'Lachlan that stridulation is known to occur in several species of *Lepidoptera*; for this shows that the sense of hearing in these insects is probably of general occurrence. With regard to the sense of hearing in birds, I did not say in my previous letter that thrushes, &c., were guided to their food *exclusively* by this sense; indeed it would be a very anomalous thing if animals which possess so keen a sense of sight are not in the habit of using it, as Mr. McLachlan suggests, in any profitable way they can. But that thrushes trust *very largely* to their sense of hearing in their search for food—especially in certain conditions of the ground—no one, I think, who has observed the process can doubt. The bird runs rapidly some twelve to twenty feet in a straight line; it then stops

suddenly, elevates its head, and remains motionless in a listening attitude; after pausing thus for a few seconds, it again runs to about the same distance as before, again stops to listen, and so on. These successive excursions are usually made in the same direction; but every now and then, during the process of listening, the thrush apparently hears a sound proceeding from some point within the circle which it has last entered; immediately the course of progression is deflected at an angle from the continuous straight line in which all the previous excursions were made, and, either with a single rush or after one or two brief pauses to make quite sure of the exact spot, the bird may nearly always be seen to find a worm.

I may take this opportunity of thanking your various correspondents for the information which they have supplied with regard to the towering of birds. Some of the letters mention ducks, teals, and widgeons as birds which occasionally tower. May I ask the writers of these letters whether the action in these cases resembled that of *true* towering in the case of partridges and grouse? I ask this because one of the numerous letters by which my communication to NATURE has been answered in the *Field*, states that birds of this build never tower; and on this supposed fact the writer constructs a theory as to the mechanism of towering in general. All the correspondence taken together cannot leave any doubt that there are at least *two* kinds of towering:—viz. (1) The common kind which I described, and the cause of which is certainly pulmonary hæmorrhage; and (2) a very rare kind which I have never myself witnessed, and the immediate cause of which appears to be cerebral injury. In the case of the second, or rare kind of towering, all the correspondents are agreed that the bird is not dead when found, and that it may even fly away again when disturbed. Never having had an opportunity of observing such a case, of course any hypothesis by which I may try to explain the cause of the rare kind of towering is of no further value than a conjecture; but I may remark that both kinds of towering may possibly be due to the same cause, if the parts of the brain which are injured when the second kind of towering ensues, are the parts whose injury Brown-Sequard found to be attended in the case of mammals, with bleeding of the lungs. At any rate, it would be worth while for any sportsman who may have the opportunity, to dissect a bird which he has seen to exhibit the second kind of towering, in order to ascertain whether, in such cases also, some degree of pulmonary hæmorrhage may not have taken place.

GEORGE J. ROMANES

IN Mr. G. J. Romanes' interesting remarks (NATURE, vol. xv., p. 177) on the sense of hearing in insects, he says:—"In the case of moths, however, I believe that sounds are never emitted, except of course the Death's-head."

As I trust that insects will continue to have a place in his observations, may I be allowed to call Mr. Romanes' attention to the following species of Lepidoptera which are known to produce sounds:—

1. *Vanessa*, several species.—The sound produced—which has been compared to the friction of sand-paper—has been noticed by several observers, viz., Rev. J. Greene (*Proc. Ent. Soc. of London*, New Series, ii., p. xcviii.), Mr. Hewitson (*l.c.* iv., p. ii.), and Mr. A. H. Swinton (*Entomologists' Monthly Magazine*, xiii., p. 169, January, 1877), who describes the apparatus by which the noise is produced. On the under surface of the upper wing one of the nervures is roughened like a file, and upon this a raised nervure on the upper surface of the underwing plays; there is also a circular embossed patch of the wing-membrane destitute of scales, which Mr. Swinton thinks serves to "impress the musical tremours." The object of this stridulation, Mr. Swinton suggests, may be classed with those phenomena of rivalry and love so conspicuous in the *Orthoptera*, &c., but at the same time it is produced when the insects are disturbed, and possibly also when the sexes are coquetting in mid-air. Moreover, the development of the mechanism is greatest in the female, contrary to the usual rule. For my own part, I incline to think that the object of the sound is rather the intimidation of possible enemies than a sexual love-call. Both Mr. Greene's and Mr. Hewitson's cases occurred when insects that were hibernating were disturbed, and the sound was renewed whenever the disturbance was repeated. These butterflies hibernate in dark holes and corners, and the sound may be intended to suggest to the disturber the hiss of a snake or the note of an angry wasp or bee. As the perpetuation of the species depends for the most part on the female, she is provided with a stronger apparatus. If the sound is produced when the sexes are coquetting, it may be the butterfly expression of a playful "Get along with you."

2. The well-known case of *Acherontia* (the "Death's-head Moth").—The sound here also is probably for intimidation, and not a love-call. I cannot at present call to mind any observations on any disparity of the sound in the sexes.

3. *Setina*, several species, and

4. *Chelonia pudica*.

The sound emitted by these insects—which is compared to the ticking of a watch—is described by M. A. Guenée (*Ann. Soc. Ent. Fr.*, 4e ser., vol. iv. 1864; translated in *Ent. Month. Mag.*, i. 223) who says that it is produced by two tympaniform vesicles situated in the pectoral region, and is much more developed in the male than in the female. This, M. Guenée remarks, is rather curious, for, as the females of *Setina* can scarcely fly, it would seem that, if the organ of sound is to produce a love call, it is the female, and not the male that should have it most strongly developed. M. Guenée consequently expresses himself unable to give any plausible reason to account for the object of the sound.

A reason has occurred to me and I here give it for what it may be worth. We know that the females of several Lepidoptera (especially wingless females) have the power of emitting a scent which attracts the males, often from considerable distances. When the male of *Setina* is hunting for the female and making probably his drums vibrate loudly, the sound reaching the concealed female may excite her to give out an increased odour, and thereby more surely attract the male. In short the drums are organs of excitation.

5. *Hylophila prasinana*.—This species Mr. Swinton (*Entom. Monthly Mag.*, vii. 231) has noticed to emit a twittering sound, which he thinks is produced by a structure between the thorax and abdomen.

I have been fortunate enough to have also had an opportuuity of hearing the sound produced by this species (*Scottish Naturalist*, i. 213). The sound resembles a continuous squeaking and was heard on more than one occasion, and was audible at a distance of ten feet or upwards. All the specimens that I caught in the act of squeaking were males, so that I cannot say whether the other sex squeaks or not. The sound is emitted whilst the insect flies about the bushes, and the object of it is probably the same as I have suggested above in the case of *Setina*. The emission of the sound is quite voluntary on the part of the moth, as specimens taken in the act of squeaking and made to fly afterwards did not then give out any sound. Careful dissection revealed no structure that appeared capable of producing the noise except a tympaniform plate situated at the base of the hind body.

6. *H. quercana*.—According to Mr. Swinton (*l.c.*, vlii. 70) this species can make a "membranous sound," which he thinks is produced by the wing catching a little horny lateral thoracic plate.

There may be other recorded instances of sound-producing *Lepidoptera*, but I cannot at present recall any to mind. It is probable moreover that more species than are generally supposed emit some kind of a sound. It is therefore much to be regretted that the many collectors of Lepidoptera—whose sole aim seems to be the amassing of large collections and whose lack of anything beyond the mere desire to accumulate specimens, has made entomology a bye-word amongst the sciences—would not spend some of their misplaced energy in really studying the objects of their attention.

Mr. Romanes' observation of the sensible appreciation moths have for high-pitched notes suggests a question. Does the shrill squeaking of bats convey an intimation to moths of the approach of one of their greatest enemies?

It is to be noted moreover that in the majority of cases the sounds emitted by moths, and indeed all insects, themselves, are high pitched.

F. BUCHANAN WHITE

Perth, January 12

P.S.—Since the above was written I see that my friend Mr. McLachlan has pointed out (NATURE, vol. xv. p. 254) another record of a sound-producing moth—*Euprepia matronula*.

THE perusal of Mr. McLachlan's letter on "Sense of Hearing &c., in Birds and Insects" has recalled to my memory another instance of a Lepidopterous insect which possesses the property of emitting a marked sound when on the wing. This is a common Brazilian butterfly (*Ageronia feronia*), and attention

was long ago directed to its habits in this respect by Mr. Darwin in his delightful "Naturalists' Voyage" (p. 33). He there mentions that when watching a male and female of this species in flight, he "distinctly heard a clicking noise, similar to that produced by a toothed wheel passing under a spring catch."

This curious observation I had numerous opportunities of verifying in the course of three visits to Rio Janeiro in 1866, 1867, and 1869. ROBERT O. CUNNINGHAM

Queen's College, Belfast, January 19

I HAVE noticed that, when moles are burrowing, the worms near make their way to the surface. I have also observed that starlings gather round and under cows in pasture-fields. Their doing so I have been in the habit of ascribing to the tread and grazing-work of the cows producing tremors in the ground, which worms may mistake for mole-work, and therefore crowd to the surface; and I have offered the same explanation for the method of hunting pursued by blackbirds and thrushes. They have practically found out that (earth-tremors induced by) small hopping-runs make "the poor inhabitants below" seek safety above, and that thus the hunters most readily secure a breakfast. I am not acquainted with the habits of those hunters.

Cambuslang HENRY MUIRHEAD

Galton's Whistles

WITHIN the last few days I have had the opportunity of making observations with Galton's whistle upon a large number of people and upon some cats, and I have come to some conclusions which are curious and suggestive, even though they may not be absolutely exact. Thus, on the whistle a line is marked which is the usual limit of human hearing, and which represents, I should say, a number of vibrations somewhere between 41,000 and 42,000 per second. Out of many hundreds of persons examined I have only met with one instance, a young man, in which I was satisfied that a note higher than this was heard. As a rule the compass of the ear of women is markedly higher than it is in men, and age seems to lower it sooner in men than in women. Is this a result of the female animal always having the more intimate protection of the young as her work, the young having notes of higher pitch than the adult? The fact is at least suggestive.

Very few of the persons experimented upon seemed to have the compass of one ear exactly the same as that of the other, the right ear usually hearing a higher note than the left, and this is more marked in men than in women.

The sense of direction of the sound in the human ear seems to be lost at a very much lower point than appreciation of the note, but this is not the case with cats; for until the instrument ceases to produce a note altogether, or at least one within their compass, they turn their faces to the source of it the moment it is produced. These facts are also suggestive. The cat still depends to a large extent for its food supply on the appreciation of high notes, and quite as much on the appreciation of the direction from which they come. The power of hearing a note of a pitch beyond the limits of our sense of direction is suggestive that that sense has been blunted by disuse; and it would be extremely interesting to know if the compass of direction is higher in savage than in civilised peoples. From facts known concerning their other senses, I should say it is likely to be higher.

This difference in the two compasses is further indicative that the appreciation of direction is the work of a separate organ, and Dr. Crum Brown's experiments suggest the semicircular canals, or the utricle or succule in association with them, as the seat of this sense. If, as Dr. Brown seems to have shown, the semicircular canals are the organs of the general sense of position and direction, it would not be a far-fetched idea, that the utricle has to do with the sense of the direction of sound and that the canals are additions to it. An analogous relation of the cochlea to the saccule is suggested by the mere facts of anatomy. If it be, as Helmholtz believes, that the cochlea is the organ for the appreciation of *pitch*, the relations of the three divisions of the organ of hearing are to be easily understood, and these relations offer, at first sight, a singularly strong evolutionary argument. There is, first, the organ for the perception of sound vibrations, having a comparatively limited compass. To this is added an organ for the appreciation of the direction of the sounds, and another for the appreciation of highly-pitched notes; and a part of the first of these becomes so modified as to be capable of interpreting position and direction generally, independently of sound. The facts of the development of the ear support such a view, and we may conclude that the sense of direction is more important than the appreciation of high notes; for the semi-circular canals appear, or at least one exists, in the Myxine, whilst a very rudimentary cochlea does not appear till we get high up in the fishes.

Birmingham LAWSON TAIT

Atmospheric Currents

MR. CLEMENT LEY (in vol. xv. p. 157 of NATURE) asks me for the absolute proof which I suppose to exist (1) that the upper current return trades "flowing from the equator descend again to the surface of the ocean on the polar sides of the calms of Cancer and of Capricorn," and (2) that "these equatorial currents subsequent to their descent on the polar sides of Cancer and of Capricorn are known as the westerly winds of the temperate zones"; (3) he further asks "what proof exists that the upper currents from the polar depressions and those from the equatorial depressions cross one another in the calms of Cancer and of Capricorn so as subsequently to become the trades and anti-trades respectively," and suggests that it is more reasonable to suppose that their currents intermingle, and that their mixed volume is then drawn off north and south, as required, to restore the equilibrium of the atmosphere, as suggested by myself with reference to the equatorial calms. Mr. Clement Ley's three questions may, I think, be fairly answered as one, all depending upon the same proof.

The correctness of my assertions with reference to the atmospheric currents flowing from the equator can be referred to the one crucial test, viz., Are the atmospheric currents which descend to the surface of the ocean on the equatorial and on the polar sides of the two zones of high pressure, similar in their constituents (*i.e.*, when they first become established as winds on the surface of the ocean)? is their degree of electricity the same? is their degree of saturation the same? If these questions could be answered in the affirmative it would show that Mr. Ley's supposition with reference to the mixed volume of the upper currents was possible, but if, on the other hand, they are answered in the negative, Mr. Ley can hardly hold, I think, that I have put my statements forward too strongly.

Though I believe that the north-east and south-east trades meeting at the belt of equatorial calms are thrown upwards from the surface of the ocean, and in ascending do mix their volumes, the conditions of atmospheric currents meeting many thousand feet above the sea-level are entirely different, as they have not the ocean as a *point d'appui*, and there is no more difficulty in accounting for their currents passing one another and the heavier underrunning the lighter, than there is for the Labrador, augmented by the East Greenland current, meeting and underrunning the Gulf Stream.

At Teneriffe, and other mountainous regions, in the latitudes of the trades, observations have been made with reference to the height of the trade winds, and of the neutral strata intervening between them and the upper current, as also of the height of the lower portion of the equatorial return current, which flows at heights varying from 12,000 feet upwards above the sea-level.

Prof. C. P. Smyth, H.M. Astronomer for Scotland, in his very interesting work, "Teneriffe," gives us some very important data with reference to these currents, showing—

1. The extreme dryness of the north-east wind.
2. Its very moderate electricity.
3. The greater saturation of the south-west wind.
4. The descent of the south-west upper current.
5. The chemical difference between the two currents.

Though there is much that I might quote with advantage, I shall content myself with the following four paragraphs:—

Page 110. "If we must live in a wind by all means let it be the south-west, and not the north-east, that effete unwholesome and used-up polar stream. As to the chemical and sanitary qualities of the two winds there could be no comparison between them."

Page 170. "And so indeed we found before we had finished with our expedition, when the south-west wind descended to the very surface of the sea."

Page 184. "In short, whatever the north-east wind did, its electricity was always moderate."

Page 206. "The trade wind is undoubtedly a poor one for

bringing water, but its position in Teneriffe during summer is favourable for making it deposit any which may be present."

I think from these extracts, which are supported by other passages in Prof. Smyth's work, I am quite justified in arguing that the trades and counter-trades are not similar in their constituents, that their degrees of electricity and of saturation are not the same, and that therefore it is not reasonable to suppose that their upper currents intermingle at the belts of tropical calms, and that their mixed volume descends and is then drawn off north and south as required, to restore the equilibrium of the atmosphere.

As these opposite currents flowing in the northern hemisphere from the north-east and the south-west (approximately) do not intermingle, and their mixed volume does not descend in the calms of Cancer it must necessarily follow that the south-west or return equatorial current, does descend to the surface of the ocean on the polar side of the calms of Cancer, and equally that the north-east upper current does descend on the equatorial side.

I have by no means exhausted what I have to say on this subject, but Mr. Ley will doubtless understand that I am unable to treat it at greater length in your columns. The same line of argument would have enabled me to answer Mr. Ley's questions separately had space permitted. DIGBY MURRAY

Mind and Matter

PERMIT me to correct a mistake on the part of Mr. Tupper (NATURE, vol. xv. p. 217), who, though starting with a correct notion that my letter (NATURE, vol. xv. p. 78) was intended to solve a problem, immediately fell into the error of regarding it as intended to prove an alleged fact.

The fact alleged, that consciousness depends on nervous organisation I assumed to be a fact, and undertook to indicate *how* the dependence might be conceived, or regarded, to exist.

First, I alleged that the hypothesis of matter being as susceptible of consciousness as spirit, was quite *conceivable, as a hypothesis*, whether or not it should be proved afterwards to be a wrong hypothesis.

Second, the connection of two so dissimilar entities as matter and subjectivity had not the objection of being anomalous or unique; for energy and matter were equally dissimilar and yet invariably united. The parity of mystery was not intended to establish "parity of probability as to the facts," but merely *parity of conceivability*. For it is surely some help to our entertaining a new conception if we can point to an existing similar conception.

Third, if such a mysterious entity as energy could be divided and combined (using the words in a loose sense) why should there be a difficulty in conceiving of the division and combination of subjectivity. By this I meant that as division of matter involved division of energy, as to *amount*, so division of matter might be conceived involving division of subjectivity, as to *amount:* so with combination.

Thus far, however, I had only cleared away difficulties "real or apparent" in the way of our *conceiving* the relation of consciousness to matter from the "materialistic" stand-point.

The essential part of my solution which indicated roughly the *modus* of the connection between matter and consciousness and which dealt with the great difficulty of the question—How to account for the *two* aspects of matter, the unconscious and conscious?—has not been touched by Mr. Tupper. This portion he excused himself from examining because he regarded it as based on the assumption that "the probability of subjectivity being a property of matter equals the fact of energy being related to matter," whereas it is based on the fact, or alleged fact, or assumption, that "the dependence of consciousness on nervous organisation seemed by the science of nerve-physiology to be fairly established." To mistake allegations of the conceivability of a notion for assumptions or intended proofs that the notion is true, as has been done by Mr. Tupper, is surely not equivalent to pointing out fallacies in the solution of a problem.

Will he admit that, if a "pointer" could "tell us" he scented a fox and immediately thereafter follow the scent of a hare, such would be an admirable analogy of how to practise "sound logic by the old *à priori* method?"

Stafford, January 17 W. S. DUNCAN

Pre-Glacial Man in America

DR. ABBOTT, in his interesting letter on the traces of pre-glacial man in America, supposes that it may be correct that the American aborigines migrated from the Old World. This may be the case with the Red Indians, but we know that they drove out an earlier people—the mound-builders. However, both mound-builders and Red Indians were certainly post-glacial in their occupation of the northern parts of America, and the oldest traces of their existence may not date back to an earlier time than a late stage of the Neolithic period in Europe.

Palæolithic man in America holds the same relative position to these later peoples as he does in the Old World, and we have so far obtained no evidence to show whether he occupied Europe or America first. The position of his remains in the auriferous drift of California is the same as in that of Siberia; in the löess of the Mississippi as in that of the Danube and the Rhine; in the caves of Brazil along with extinct mammalia as in those of Europe; and in the lowland gravels of Virginia as in those of France and England.

The question of the post or pre-glacial age of palæolithic man depends in America as it does in Europe on that of the age of the deposits in which they are found, and this is at present a matter of inquiry and discussion which might be set at rest, as I have pointed out in the *Quarterly Journal of Science* for July of last year, by a thorough examination of the brick clays at Hoxne where palæolithic implements were first found in England.

Cornwall House, Ealing, January 27 THOMAS BELT

Holly Berries

REPORTS of the scarcity and abundance of holly berries have appeared in NATURE from the south-east of England and west of Scotland respectively. It may be interesting to note the condition of the holly crop at a point somewhere about midway between these two places. In North Staffordshire and Derbyshire the holly berries are by no means scarce. They are not so plentiful as they were last year, but there is a fair average crop.

I have seldom seen such crops of them as I saw in several places in South Wales about a month ago. It may be also worth adding that the most teeming bush I saw was at a place in Cardiganshire, which was as far as I could learn—and I made diligent inquiries—between four and five miles from the nearest hive of bees. I questioned closely several children on the spot who were intelligent enough to give me a minute description of most of the common birds and insects; not one of them had ever seen a bee. D. EDWARDES

Denstone College, Staffordshire

The Meteor of January 7

AMONG the "Notes" in NATURE, vol. xv., p. 244, there is a description of a large meteor, of which I was fortunate enough to secure a good observation; but on comparing the apparent path, as observed by myself, with that recorded in the paragraph, I find the latter somewhat imperfect; the apparent path, as seen from near London, seems to have been curtailed both at beginning and at end of flight; probably the observer in question could further amplify his remarks, or some other correspondent send an observation. The following is an abstract from my note-book:—

"Birmingham, January 7, 10·31 P.M. G.M.T.—Meteor pear-shape, deep yellow merging into ruby-red towards the tail; commenced as a luminous point near η Hydræ, gradually increased in size, motion very slow and unsteady, appeared to force its way with difficulty, and slight undulation. Near α Leonis it attained the apparent size of Venus, the forward hemisphere now showing signs of internal commotion by the projection of ebullition prominences, which were swept back towards the tail, then 8° long, and vaporous. The latter portion of its flight was intercepted by houses, but on emergence it burst with a flash below β Leonis at A.R. 182°, D.N. 16°. Length of path, 52°; time of flight, five to six seconds; radiant point (in Fluvius Eridanus), No. 96 Tupman, or No. 164 of the B. A. Catalogue. W. H. WOOD

Balsall Heath Road, Moseley Road, Birmingham

Spectrum of New Star

THE spectrum of the new star in Cygnus is changeable, and is now very unlike Cornu's representation of it in a recent number of NATURE (vol. xv., p. 158). Your readers may not be aware that it is easy to see several of the bright lines without a powerful instrument, though not to measure them accurately. As observed with a Browning's "miniature spectroscope" attached to a 4½-inch refractor, the brightest line is now about at

wave-length 503, and is probably that described by Cornu as sixth in order of brightness, at wave-length 500. At the end of last month the brightest line was about 484, probably the F hydrogen line. Since December 27 the new star has always appeared to me orange. Has not this star received any name yet?

Sunderland, January 26 T. W. BACKHOUSE

KÜHNE'S RESEARCHES ON PHOTO-CHEMICAL PROCESSES IN THE RETINA

ON January 5, Dr. W. Kühne, Professor of Physiology in the University of Heidelberg, read before the Natur-historisch-Medicinisches Verein, of Heidelberg, a paper entitled "Zur Photo-chemie der Netzhaut," so full of interest to the physicist and physiologist, that I think an abstract of it will be acceptable to the readers of NATURE.

A short time since, Boll (a pupil of Max Schultz and Du Bois-Reymond, who now occupies the chair of Physiology in Rome) communicated to the Berlin Academy the remarkable fact that the external layer of the retina, *i.e.*, the layer of rods and cones, possesses in all living animals a purple colour. During life, according to Boll, the peculiar colour of the retina is perpetually being destroyed by the light which penetrates the eye; darkness, however, restores the colour, which vanishes for ever almost immediately after death.[1]

The wonderfully suggestive nature of Boll's discovery led Kühne to repeat his observations; in doing so, whilst he has confirmed the fundamental statement of Boll, he has ascertained a number of new facts of great interest.

Kühne's observations were made on the retinæ of frogs and rabbits. In the first place, implicitly relying upon the statements of Boll, he examined, as soon as possible after death, the retinæ of animals which had been kept for some time in darkness. He soon found that the beautiful purple colour persists after death if the retina be not exposed to light; that the bleaching takes place so slowly in gas-light, that by its aid the retina can be prepared and the changes in its tint deliberately watched; that when illuminated with monochromatic sodium light the purple colour does not disappear in from twenty-four to twenty-eight hours, even though decomposition have set in.

These first observations of Kühne on the vision-purple (*Sehpurpur*), as he terms it, whilst they showed that the disappearance of the colour is not, as Boll had asserted, a necessary concomitant of death, removed many of the difficulties which stood in the way of a careful investigation. Carrying out his preparations in a dark chamber illuminated by a sodium flame, Kühne was able to discover the conditions necessary to the destruction of the vision-purple as well as some facts relating to its restoration or renewal.

As long as the purple retina is kept in the dark or is illuminated only by yellow rays, it may be dried upon a glass plate without the tint changing; the colour is not destroyed by strong solution of ammonia, by saturated solution of common salt, or by maceration in glycerine for twenty-four hours. On the other hand, a temperature of 100° C. destroys the colour, and alcohol, glacial acetic acid, and strong solution of sodium hydrate produce the same effect.

Kühne's next observations were directed to the discovery of the influence of light of different colour upon the vision-purple. It would appear that the more refrangible rays of the spectrum have the greatest action, and that the red rays are as inactive as the yellow.

Kühne now found the incorrectness of Boll's assertion that the retina of the living eye exposed to ordinary daylight does not exhibit the vision-purple, for on preparing the eyes of animals which had just been exposed to light, as rapidly as possible in the chamber illuminated by sodium light, he discovered that the retina was of a beautiful purple. It was only when eyes were exposed for a considerable time to the direct action of the sun's rays that a fading of the purple colour was perceived.

A most suggestive experiment now threw some light upon the circumstances which retard the decolorisation, and which restore the vision-purple. The two recently extirpated eyes of a frog were taken; from one the retina was removed, whilst an equatorial section was made through the other eye, so as to expose the retina and still leave it *in situ*. Both preparations were exposed to diffuse daylight, until the isolated retina had lost its purple colour. On now taking the other preparation into the yellow chamber and removing the retina, it was found that its colour yet remained: it was *dark red*, but was bleached when exposed in its naked condition to daylight.

This experiment was confirmed by others, in which the effect of strong sunlight was substituted for that of diffuse daylight.

But the most curious results of Prof. Kühne's experiments have reference to the restoration of the vision-purple. If an equatorial section be made through a recently extirpated eye, and a flap of retina be lifted up from the underlying choroid and exposed to light, the purple colour of the flap will be destroyed, whilst the colour of the rest of the retina persists. If, however, the bleached portion of the flap be carefully replaced, so that it is again in contact with the inner surface of the choroid, comple restoration of the vision-purple occurs. This restoration is a function of the *living* choroid, probably of the living retinal epithelium (*i.e.* of the hexagonal pigment cells, which used formerly to be described as a *part* of the choroid), and it appears to be independent of the black pigment which the retinal epithelium normally contains. As it is absolutely dependent upon the life of the structures which overlie the layer of rods and cones, it is natural that it should be observed to occur for a longer time after somatic death in the frog than in the rabbit.

Kühne's researches, though suggested by the interesting observation of Boll, have not only corrected many errors which that observer had committed, but have led to the discovery of facts which add immensely to the importance of the newly-observed vision-purple.

They have shown that the living retina contains a substance which under the influence of light undergoes chemical changes, which vary in intensity according to the intensity and character of the luminous rays, and they point to the existence of structures in connection with the retina which as long as they are alive are able to provide fresh stores of substance sensitive to light.[1]

Since the above account of Kühne's researches was written, he has published in the *Centralblatt der medicinischen Wissenschaften* (January, 1877, No. 3) a short paper, dated January 15, in which he announces the startling confirmation to his previous researches afforded by his *having been able to obtain actual images on the retina which corresponded with objects which had been looked at during life* (!).

The discoveries of Boll and Kühne must, as the latter remarks, have led to the thought that after all there might be some truth in the stories which we all have heard of images of things seen in death being left imprinted upon the eye. After his first researches Kühne endeavoured over and over again to observe on the retina of rabbits bleached spots corresponding to the images of external objects, but his endeavours failed. As Kühne remarks, and as all readers who have understood his experiments will allow, in order to obtain a permanent photograph, or, as he terms it, *optogramme*, the effect of the light would have to be so prolonged or so intense as to destroy the balance between the destruction of the vision-purple and the power of the retinal epithelium to restore it.

Kühne took a coloured rabbit and fixed its head and one of its eye-balls at a distance of a metre-and-a-half from an opening thirty centimetres square, in a window-shutter. The head was covered for five minutes by a black cloth and then exposed for three minutes to a somewhat clouded midday-sky. The head was then instantly decapitated, the eye-ball which had been exposed was rapidly extirpated by the aid of yellow light, then opened, and instantly plunged in 5 per cent. solution of alum. Two minutes after death the second eye-ball, without removal from the head, was subjected to exactly the same processes as the first, viz., to a similar exposure to the same object, then extirpation, &c.

On the following morning the milk-white and now toughened retinæ of both eyes were carefully isolated, separated from the optic nerve, and turned; *they then exhibited on a beautiful rose-red ground a nearly square sharp image with sharply-defined edges; the image in the first eye was somewhat roseate in hue and less sharply defined than that in the second, which was perfectly white. The size of the images was somewhat greater than one square millimetre.*

Prof. Bunsen was amongst the witnesses of this beautiful experiment. ARTHUR GAMGEE

[1] This account of Boll's researches is taken from Kühne's paper. The latest number of the *Monatsberichte* of the Berlin Academy which has yet reached Manchester, which includes the Proceedings for September and November, does not contain Boll's communication, which is of later date (November 12).

[1] I have repeated all the more important observations of Kühne with the eyes of several *Ranæ temporariæ*, and with those of two rabbits, of which one was an albino, and can entirely confirm all his interesting facts. In ordinary daylight, the purple-red colour of the frog's retina, and its subsequent decolorisation, may be most satisfactorily demonstrated. The use of the dark chamber illuminated by sodium is, however, useful in cases where the dissection of the eye has to be conducted with care.—A. G.

ON THE PRECESSIONAL MOTION OF A LIQUID[1]

THE formulas expressing this motion were laid before the meeting and briefly explained, but the analytical treatment of them was reserved for a more mathematical paper to be communicated to the Section on Saturday. The chief object of the present communication was to illustrate experimentally a conclusion from this theory which had been announced by the author in his opening address to the Section, to the effect, that, if the period of the precession of an oblate spheroidal rigid shell full of liquid is a much greater multiple of the rotational period of the liquid than any diameter of the spheroid is of the difference between the greatest and least diameters, the precessional effect of a given couple acting on the shell is approximately the same as if the whole were a solid rotating with the same rotational velocity. The experiment consisted in showing a liquid gyrostat, in which an oblate spheroid of thin sheet copper filled with water was substituted for the solid fly-wheel of the ordinary gyrostat. In the instrument actually exhibited, the equatorial diameter of the liquid shell exceeded the polar axis by about one-tenth of either.

Supposing the rotational speed to be thirty turns per second, the effect of any motive which, if acting on a rotating solid of the same mass and dimensions, would produce a precession having its period a considerable multiple of $\frac{1}{2}$ of a second, must, according to the theory, produce very approximately the same precession in the thin shell filled with liquid as in the rotating solid. Accordingly the main precessional phenomena of the liquid gyrostat were not noticeably different from those of ordinary solid gyrostats which were shown in action for the sake of comparison. It is probable that careful observation without measurement might show very sensible differences between the performances of the liquid and the solid gyrostat in the way of nutational tremors produced by striking the case of the instrument with the fist.

No attempt at measurement either of speeds or forces was included in the communication, and the author merely showed the liquid gyrostat as a rough general illustration, which he hoped might be regarded as an interesting illustration of that very interesting result of mathematical hydro-kinetics the quasi-rigidity produced in a frictionless liquid by rotation.

P.S.—Since the communication of this paper to the Association, and the delivery of my opening address which preceded it on the same day, I have received from Prof. Henry No. 240 of the Smithsonian Contributions to Knowledge, of date October, 1871, entitled "Problems of Rotatory Motion presented by the Gyroscope, the Precession of the Equinoxes and the Pendulum," by Brevet Major-Gen. J. G. Barnard, Col. of Engineers, U.S.A., in which I find a dissent, from the portion of my previously-published statements which I had taken the occasion of my address to correct, expressed in the following terms :—

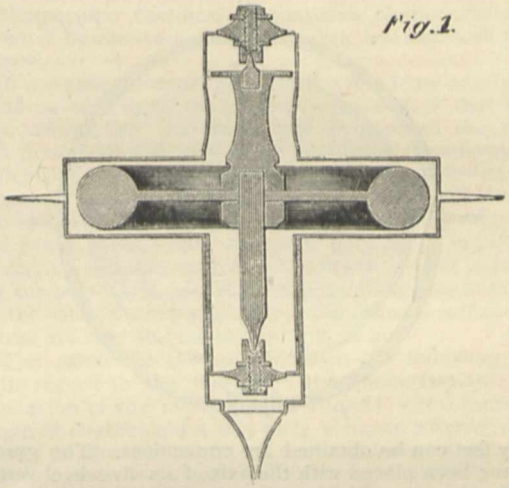
"I do not concur with Sir William Thomson in the opinions quoted in note p. 38, from Thomson and Tait, and expressed in his letter to Mr. G. Poulett Scrope (NATURE, Feb. 1, 1872). So far as regards fluidity, or imperfect rigidity, within an infinitely rigid envelope, I do not think the rate of precession would be affected."

Elsewhere in the same paper Gen. Barnard speaks of "the practical rigidity conferred by rotation." Thus he has anticipated my correction of the statements contained in my paper on the Rigidity of the Earth, so far as regards the effect of interior fluidity on the precessional motion of a perfectly rigid ellipsoidal shell filled with fluid.

I regret to see that the other error of that paper, which I corrected in my opening address, had not been corrected by Gen. Barnard, and that the plausible reasoning which had led me to it had also seemed to him convincing. For myself, I can only say that I took the very earliest opportunity to correct the errors after I found them to be errors, and that I deeply regret any mischief they may have done in the meantime.

Addendum.—Solid and Liquid Gyrostats.—The solid gyrostat has been regularly shown for many years in the

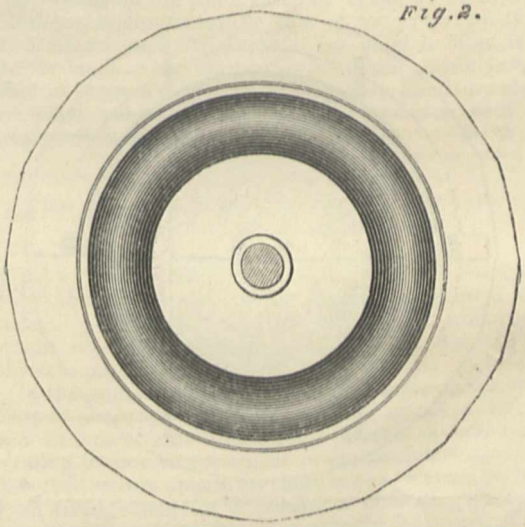
Fig. 1.



Natural Philosophy Class of the University of Glasgow as a mechanical illustration of the dynamics of rotating solids, and it has also been exhibited in London and Edinburgh at conversaziones of the Royal Societies and of the Society of Telegraph Engineers, but no account of it has yet been published. The following brief description and drawing may therefore even now be acceptable to readers of NATURE :—

The solid gyrostat consists essentially of a massive fly-wheel possessing great moment of inertia, pivoted on the two ends of its axis in bearings attached to an outer case which completely incloses it. Fig. 1 represents a section

Fig. 2.



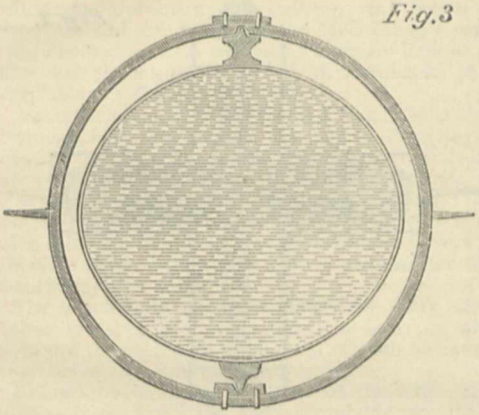
by a plane through the axis of the fly-wheel, and Fig. 2 a section by a plane at right-angles to the axis and cutting through the case just above the fly-wheel. The containing case is fitted with a thin projecting edge in the plane of the fly-wheel, which is called the bearing edge. Its boundary forms a regular curvilinear polygon of sixteen sides with its centre at the centre of the fly-wheel. Each side of the polygon is a small arc of a circle of radius

[1] Communicated to Section A of the British Association, Thursday, September 7, 1876.

greater than the distance of the corners from the centre. The friction of the fly-wheel would, if the bearing-edge were circular, cause the case to roll along on it like a hoop, and it is to prevent this effect that the curved polygonal form described above and represented in the drawing is given to the bearing-edge.

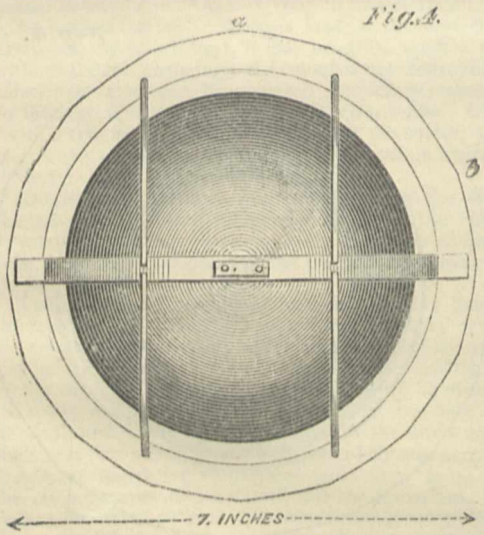
To spin the solid gyrostat a piece of stout cord about forty feet long and a place where a clear run of about

Fig. 3



sixty feet can be obtained are convenient. The gyrostat having been placed with the axis of its fly-wheel vertical, the cord is passed in through an aperture in the case, two-and-a half times round the bobbin-shaped part of the shaft, and out again at an aperture on the opposite side. Having taken care that the slack cord is placed clear of all obstacles and that it is free from kinks, the operator holds the gyrostat steady so that its case is prevented from turning, while an assistant pulls the cord through by running, at a gradually increasing pace, away from the instrument, while holding the end of the cord in his hand. Sufficient tension is applied to the entering cord to prevent it from slipping round on the shaft. In this way a very great angular

Fig. 4.



velocity is communicated to the fly-wheel, sufficient, indeed, to keep it spinning for upwards of twenty minutes.

If when the gyrostat has been spun it be set on its bearing edge with the centre of gravity exactly over the bearing point, on a smooth horizontal plane such as a piece of plate-glass lying on a table, it will continue apparently stationary and in stable equilibrium. If while it is in this position a couple round a horizontal axis in the plane of the fly-wheel be applied to the case, no deflection of this plane from the vertical is produced, but it rotates slowly round a vertical axis. If a heavy blow with the fist be given to the side of the case, it is met by what seems to the senses the resistance of a very stiff elastic body, and, for a few seconds after the blow, the gyrostat is in a state of violent tremor, which, however, subsides rapidly. As the rotational velocity gradually diminishes, the rapidity of the tremors produced by a blow also diminishes. It is very curious to notice the tottering condition, and slow, seemingly palsied, tremulousness of the gyrostat, when the fly-wheel has nearly ceased to spin.

In the liquid gyrostat the fly-wheel is replaced by an oblate spheroid, made of thin sheet copper, and filled with water. The ellipticity of this shell in the instrument exhibited is $\frac{1}{10}$, that is to say, the equatorial diameter exceeds the polar by that fraction of either. It is pivoted on the two ends of its polar axis in bearings fixed in a circular ring of brass surrounding the spheroid. This circle of brass is rigidly connected with the curved polygonal-bearing edge which lies in the equatorial plane of the instrument, thus forming a frame-work for the support of the axis of the spheroidal shell. In Fig. 3 a section is represented through the axis to show the ellipticity, and Fig. 4 gives a view of the gyrostat as seen from a point in the prolongation of the axis. To prevent accident to the shell when the gyrostat falls down at the end of its spin, cage bars are fitted round it in such a way that no plane can touch the shell.

The method of spinning the liquid gyrostat is similar to that described for the solid gyrostat, differing only in the use of a very much longer cord and of a large wheel for the purpose of pulling it. The cord is first wound on a bobbin, free to rotate round a fixed pin. The end of it is then passed two-and-a-half times round the little pulley shown in the annexed sectional drawing, and thence to a point in the circumference of the large wheel to which it is fixed. An assistant then turns the wheel with gradually increasing velocity, while the frame of the gyrostat is firmly held, and the requisite tension applied to the entering cord to prevent it from slipping round the pulley.

WILLIAM THOMSON

REMARKABLE PLANTS

I.—THE COMPASS-PLANT.

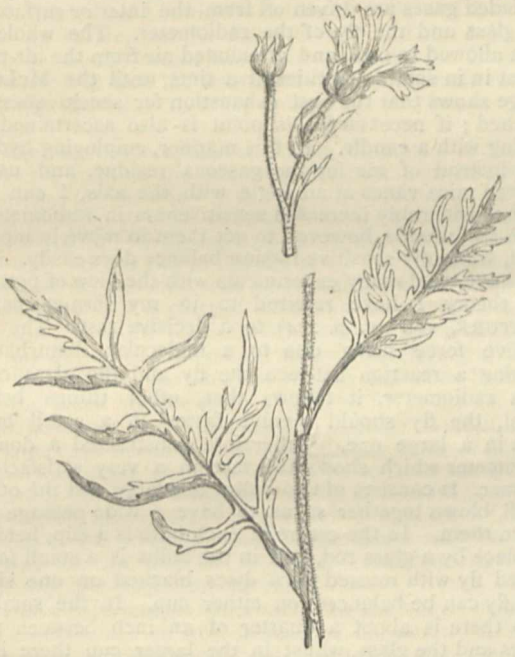
"Look at this delicate plant that lifts its head from the meadow,
See how its leaves all point to the north, as true as the magnet;
It is the compass-plant that the finger of God has suspended
Here on its fragile stalk, to direct the traveller's journey
Over the sea-like, pathless, limitless waste of the desert"

LONGFELLOW'S *Evangeline*.

IT has long been known that there grows on the prairie-lands of the south-western part of the United States of America, especially Texas and Oregon, a plant which has the peculiar property of turning its leaves towards the north, and which hence serves as a magnet to the traveller when no other means is available of ascertaining the points of the compass. It is probable, however, from Longfellow's description of it as a "delicate plant" on a "fragile stalk," that he never saw it growing. The Compass-plant is a member of the enormous natural order Compositæ, known to botanists as *Silphium laciniatum*. It is described as a stout perennial plant from three to six feet in height, with ovate, deeply-pinnatifid leaves and large yellow heads of unisexual flowers, the ray-florets strap-shaped and female, the disc-florets tubular and male. It is also known as the pilot-weed, polar-plant, rosin-weed, and turpentine-weed, the two last names being derived from the abundant resin exuded by the stem; and is occasionally to be seen in English gardens.

The "polarity" of the leaves of this singular plant has long been familiar to hunters and other denizens of the prairie, who, "when lost on the prairies in dark nights,

easily get their bearings by feeling the direction of the leaves." But the first occasion on which it was brought under the notice of scientific men appears to have been in communications addressed to the National Institute at Washington in August, 1842, and January, 1843, by General Alvord. The accuracy of his statement having been questioned, the General presented another communication at the second meeting of the American Association for the Advancement of Science, held at Cambridge, Mass., in August, 1849, in which he confirms his own observations by those of other officers, all agreeing in the conclusion that the radical leaves of the plant really present their edges north and south, while their faces are turned east and west, the leaves on the developed stems of the flowering plant, however, taking rather an intermediate position between their normal or symmetrical arrangement and their peculiar meridional position. For the following particulars respecting the phenomena exhibited by the compass-plant we are indebted mainly to a paper by Mr. W. F. Whitney, read before the Harvard Natural History Society and printed in the *American*



The Compass-plant (*Silphium laciniatum*).

Naturalist for March, 1871, and to some subsequent notes by Prof. Asa Gray and Mr. Thos. Meehan.

The cause of the ordinary position of the leaves of most plants, one surface being directed towards the sky and the other towards the earth, is generally believed to be a difference in the sensitiveness to light of the two surfaces, the epidermal tissue of the upper being generally denser and less pervious to light than that of the under surface. It is possible also that something may be due to the fact that the under surface of the leaf is almost always more copiously furnished with stomates or "breathing-pores," as they are often inaccurately termed, minute orifices, which serve to promote a diffusion of gases between the external air and the intercellular cavities within the tissue, and especially an abundant exhalation of aqueous vapour. A microscopical examination of the leaves of the compass-plant shows that the structure of the epidermal tissue of the two surfaces is similar, and also that the number of stomates in each corresponds, affording in this respect a contrast to other allied species of the genus *Silphium*, which do not exhibit the phenomenon of polarity, and in whose leaves the stomates were found to be from two to three times as numerous on the under as on the upper surface. If, therefore, the object to be gained is an equal sensitiveness to light, it is obvious that the two surfaces will receive an equal mean amount of light during the twenty-four hours, if they face the east and the west, rather than if they face the north and the south, or the earth and the sky. An attempt has also been made to explain the phenomenon of polarity by currents of electricity induced by the peculiar chemical composition of the substances secreted by the stem and the leaves, but not with much success.

In a recent communication to the Academy of Natural Sciences at Philadelphia, Mr. Meehan says that those who affirm that the leaves are directed to the north, and those who say that there is no such tendency, are both right. He watched a plant in his own garden, and observed the unmistakable northern tendency in the leaves when they first came up, and until they were large and heavy, when winds and rain bore them in different directions, and they evidently had not the power of regaining the points lost. It would appear, therefore, to depend on the season when the observation is made whether the leaves are seen to bear northwards or not.

The same observer records also the following facts with regard to the flowers of the compass-plant. The expansion of the ray-florets in August was observed to begin at daybreak, 4 A.M. ; forty minutes afterwards the florets of the disc began to open, and the whole of the corollas were expanded in about three-quarters of an hour, after which there was no further growth in the corollas. The stamens and pistils were gradually protruded beyond the corolla, and the lengthening of the stamens ceased at 6 A.M. After 6.20 no further growth was noticed in the flowers. Later on they were visited by insects, causing the detachment of the florets of the disc, and the scattering of the pollen on to the ray-florets, which were thus fertilised. There appear, in fact, to be three phases of growth, with a slight rest between each, the pistil taking the most time, then the stamens, and the corolla the least ; but the whole growth of the day is included within two hours.

The geographical range of the plant is stated to be from Texas on the south to Iowa on the north, and from Southern Michigan on the east to 300 or 400 miles west of Missouri and Arkansas. Its chief habitat is rich prairie land. Our illustration is taken partly from the plate in Jacquin's "Eclogæ," the only good drawing of the plant published, assisted by comparison with dried specimens in the Kew Herbarium. A. W. B.

EXPERIMENTS WITH THE RADIOMETER[1]

II.

HAVING ascertained that the action of the radiometer was due to the internal movement of the molecules of the residual gas, it became important to obtain as much information as possible respecting the physical properties of this residue.

In the apparatus constructed for this purpose a vertical plate is suspended by a glass fibre, which it twists in opposite directions alternately, instead of continuously rotating in one direction, as in the ordinary radiometer. Attached to this apparatus there is :—*a*, a sprengel pump ; *b*, an arrangement for producing a chemical vacuum ; *c*, a lamp with scale, on which to observe the luminous index reflected from a mirror ; *d*, a standard candle at a fixed distance ; and *e*, a small vacuum tube, with the internal ends of the platinum wires close together. I can therefore take observations of—

1. The logarithmic decrement of the arc of oscillation when under no influence of radiation.

[1] Continued from p. 227.

2. The successive swings and final deflection when a candle shines on one end of the blackened bar.

3. The appearance of the induction spark between the platinum wires.

1 measures the viscosity of the gas ; 2 enables me to calculate the force of radiation of the candle ; and 3 enables me to form an idea of the progress of the vacuum according as the interior of the tube becomes uniformly luminous, striated, luminous at the poles only, or black and non-conducting. The movement is started by rotating the whole apparatus through a small angle, and the observation consists in noting the successive amplitudes of vibration when the instrument is left to itself, a mirror and spot of light being employed for this purpose. The amplitudes form a decreasing series, with a regular logarithmic decrement. Up to the point at which the vacuum is apparently equal to a Torricellian vacuum, the logarithmic decrement is nearly constant ; but as the exhaustion proceeds beyond this point, it becomes smaller, and the force of repulsion approaches a maximum ; when the logarithmic decrement is about one fourth of what it was at the commencement, the force of repulsion begins to diminish, and at much higher exhaustions it nearly ceases.

I have experimented with different gases in the apparatus, and by means of a McLeod gauge attached to a mercury-pump, I have been able to measure the atmospheric pressure at any desired state of exhaustion. From the results of the measurements of the force of repulsion and of the viscosity of the residual gas, I have plotted the observations in curves, which show how the viscosity of the residual gas is related to the force of repulsion exerted by radiation.

I have supposed my scale to be 1,000 metres long, and to represent one atmosphere. Each millimetre, therefore, stands for the millionth of an atmosphere.

When the residual gas is air, the viscosity, measured by the logarithmic decrement of the arc of oscillation, is practically constant up to an exhaustion of 250 millionths of an atmosphere, or 0·19 millim. of mercury, having only diminished from 0·126 at the normal pressure of the atmosphere, to 0·112. It now begins to fall off and at 0·1 of a millionth of an atmosphere the logarithmic decrement has fallen to about 0·01. Simultaneously with this decrease in the viscosity, the force of repulsion exerted on a black surface by a standard light varies. It increases very slowly till the exhaustion has risen to about 70 millionths of an atmosphere ; at about 40 millionths the force is at its maximum ; and it then sinks very rapidly till at 0·1 millionth of an atmosphere it is less than one-tenth of its maximum.

When the residual gas is oxygen the logarithmic decrement is 0·126 at the atmospheric pressure, and at 2 millionths of an atmosphere it is 0·02. The force of repulsion in oxygen increases very steadily up to an exhaustion of about 40 millionths of an atmosphere ; it is at its maximum at about 30 millionths, and thence declines very rapidly.

It is not necessary to get so high an exhaustion with hydrogen as with other gases to obtain considerable repulsion. The viscosity at the normal pressure is measured by a logarithmic decrement of 0·063 ; at 50 millionths it is 0·046, when it rapidly sinks. The force of repulsion is very great in a hydrogen vacuum being, in comparison with the maximum in an air vacuum as 70 to 41.

Carbonic acid has a viscosity of about 0·01 at the normal pressure, being between air and hydrogen, but nearer the former. The force of repulsion does not rise very high and soon falls off.

A long series of observations have been taken, at different degrees of exhaustion, on the conductivity of the residual gas to the spark from an induction coil. Working with air I find that at a pressure of about 40 millionths of an atmosphere, when the repulsive force is near its maximum, a spark, whose striking distance at the normal pressure is half an inch, will illuminate a tube having terminals 3 millimetres apart. When I push the exhaustion further the ½-inch spark ceases to pass, but a 1-inch spark will still illuminate the tube. As I get nearer to a vacuum more power is required to drive the spark through the tube, but, at the highest exhaustions, I can still get traces of conductivity when an induction coil, actuated with five Grove's cells, and capable of giving a 6-inch spark, is used.

When so powerful a spark is employed it sometimes happens that the glass is perforated, thus causing a very slight leakage of air into the apparatus. The logarithmic decrement now slowly rises, the repulsive force of the candle increases to its maximum and then slowly diminishes to zero, the logarithmic decrement continuing to rise till it shows that the internal and external pressure are identical.

In preparing experimental radiometers I prefer to exhaust direct to one or two millionths of an atmosphere. By keeping the apparatus during the exhaustion in a hot air-bath heated to about 300° C. for some hours, the occluded gases are driven off from the interior surface of the glass and the fly of the radiometer. The whole is then allowed to cool, and attenuated air from the air-trap is put in in small quantities at a time, until the McLeod gauge shows that the best exhaustion for sensitiveness is reached ; if necessary, this point is also ascertained by testing with a candle. In this manner, employing hydrogen instead of air for the gaseous residue, and using roasted mica vanes at an angle with the axis, I can get very considerably increased sensitiveness in radiometers. I am still unable, however, to get them to move in moonlight, while my sensitive torsion balance does easily.

I have tried many experiments with the view of putting the theory I have referred to in my former paper (NATURE, vol. xv. p. 224) to a decisive test. The repulsive force being due to a molecular disturbance causing a reaction between the fly and the glass case of a radiometer, it follows that, other things being equal, the fly should revolve faster in a small bulb than in a large one. I therefore constructed a double radiometer which shows this fact in a very satisfactory manner. It consists of two bulbs, one large and the other small, blown together so as to have a wide passage between them. In the centre of each bulb is a cup, held in its place by a glass rod, and in the bulbs is a small four-armed fly with roasted mica discs blacked on one side. The fly can be balanced on either cup. In the smaller bulb there is about a quarter of an inch between the vanes and the glass, whilst in the larger cup there is a space of half an inch. The mean of several experiments shows that in the small bulb the fly rotates about 50 per cent. faster than in the large bulb when exposed to the same source of light.

One of the arms of another radiometer was furnished with roasted mica discs blacked on alternate sides. The other arm was furnished with clear mica discs. The two arms were pivoted independently of each other, and one of them was furnished with a minute fragment of iron, so that by means of a magnet I could bring the arms in contact, the black surface of the mica then having a clear plate of mica in front of it. On bringing a lighted candle near the instrument and allowing it to shine through the clear plate, on the blackened mica, the clear plate is at once driven away, till the arm sets at right angles to the other.

Two currents of force, acting in opposite directions, can exist in the same bulb. I have prepared a double radiometer in which two flys are pivoted one over the other, and having their blackened sides turned in opposite directions. On bringing a lighted candle near the flys rotate in opposite directions.

Experiment shows that the molecular disturbance

which constitutes the force can be reflected from a plane surface in such a manner as to change its direction. A two-disc radiometer was made having flat opaque mica discs blacked on one side. In front of the black surface of the mica and about a millimetre off, is fixed a larger disc of thin clear mica. On bringing a candle near, the molecular pressure streaming from the black surface is caught by the clear plate and thrown back again, causing pressure behind instead of in front, and the result is rapid rotation in the negative direction, the black side now moving towards the light.

The above actions can be explained on the "evaporation and condensation" theory, as well as by that of molecular movement. I therefore devised the following test to decide between these two theories. A radiometer has its four discs cut of very clear and thin plates of mica and these are mounted in a somewhat large bulb. At the side of the bulb in a vertical plane, a plate of mica, blacked on one side, is fastened in such a position that each clear vane in rotating shall pass it, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black plate the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near. This could not happen on the evaporation and condensation theory, as this requires that the light should shine intermittently on the black surface in order to keep up continuous movement.

The experiments with the double radiometer of different sizes showed that the nearer the absorbing surface was to the glass the greater was the pressure produced. To test this point in a more accurate manner a torsion balance was fitted up with a glass suspending fibre and a reflecting mirror. At one end of the beam is a disc of roasted mica blacked on one side. In front of this black surface, and parallel to it, is a plate of clear mica so arranged that its distance from the black surface can be altered as desired at any degree of exhaustion without interfering with the vacuum. This apparatus has proved that when light falls on the black surface molecular pressure is set up, whatever be the degree of exhaustion. A large series of observations have been taken with it with the result of not only supplying important data for future consideration, but of clearing up many anomalies which were noticed, and of correcting many errors into which I was led at earlier stages of the research. Among the latter may be mentioned the speculations in which I indulged as to the pressure of sunlight on the earth.

I now tried similar experiments to the above, using the best conductors of heat instead of the worst. A radiometer, the fly of which is made of metallic plates perfectly flat and lampblacked on one side, is much less sensitive to light than one of mica or pith; but, as I proved in a paper sent to the Royal Society in January last year, it is more sensitive to dark heat, which indeed causes the black face of a metal radiometer rapidly to rotate in a negative direction, the black continuing to advance until the temperature has become uniform throughout; but as soon as the source of heat is removed the fly commences to revolve with rapidity the positive way, the black retreating as it would if light shone on it.

Experiments with discs of aluminium, mounted diamond-wise and turned up and folded at different angles, show that shape has even a stronger influence than colour. A convex bright surface is strongly repelled, whilst a concave black surface is not only not repelled by radiation, but is actually attracted.

Carefully-shaped cups of gold, aluminium, and other metals, have been tried, as well as cones of the same materials. If a two-disc, cup-shaped radiometer, facing opposite ways and both sides bright, is exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3·37 seconds. A screen placed in front of the concave side so as to let the light shine only on the convex surface, the latter is repelled, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off so as to let the light strike only on the concave side, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being attracted.

These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action gradation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

With a two-disc, cup-shaped aluminium radiometer as above, lampblacked on the concave surfaces, the action of light is reversed, rotation taking place, the bright convex side being repelled and the black concave attracted. When the light shines only on the bright convex side no movement is produced, but when it shines only on the black concave side this is attracted, producing rotation.

Light shining on a cup-shaped radiometer similar to the above, but having the convex side black and the concave bright, causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

With a cup-shaped radiometer like the above, but blacked on both sides, a candle causes rapid rotation, the convex side being repelled. On shading off the light from the concave side the rotation continues, but much more slowly; on shading off the convex side the concave is strongly attracted, causing rotation.

Radiometers have also been made with cups and cones of plain mica, roasted mica, pith, paper, &c., and they have been made either plain or blacked on one or both surfaces. These have also been balanced against each other, and against metal plates, cups, and cones. The results are of considerable interest but too complicated to explain without numerous diagrams. The broad facts are contained in the above selections from my experiments.

Some of the phenomena produced by the action of light on the cup-shaped vanes of a radiometer may be explained on the assumption that the molecular pressure acts chiefly in a direction normal to the surface of the vanes. A convex surface would therefore cause greater pressure to be exerted between itself and the inner surface of glass than could a concave surface. But it is not easy to see how such an hypothesis can explain the behaviour of those instruments where the action of the bright convex surface more than overcomes the superior absorptive and radiating power of the concave black surface; and the explanation appears to fail to account for the powerful attraction which a lighted candle is seen to exert on the concave surfaces in other instruments.

These experiments, interpreted by the light of the dynamical theory given in my last communication, explain very clearly how it was that I obtained such strong actions in my earlier experiments when using white pith, and employing the finger as a source of heat; and how it happened that I did not discover for some time that dark heat and the luminous rays were essentially different in their actions on black and white surfaces. Rays of high intensity pass through the glass bulb without warming it; they then, falling on the white surface, are simply reflected off again; but, falling on the black surface, they are absorbed, and raising its temperature, produce the molecular disturbance which causes motion. Rays of low intensity, however, do not pass through the glass to any great extent, but being absorbed, raise its temperature. This warmed spot of glass now becomes the repelling body through the intervention of the mole-

cules rebounding from it with a greater velocity than that at which they struck it. The molecular pressure, therefore, in this case streams from the inner surface of the warm spot of glass on which the heat rays have fallen, and repels whatever happens to be in front of it, quite irrespective of the colour of its surface.

WILLIAM CROOKES

THE SPONTANEOUS GENERATION QUESTION

TWO contributions have recently been made to this subject through the Royal Society by Dr. W. Roberts and Prof. Tyndall. Dr. Roberts's communication is as follows[1]:—

In a recent communication to the Royal Society, Dr. Bastian[2] brought forward some experiments to show that while an acid urine usually remains barren after being boiled a few minutes, the same urine becomes fertile when similarly treated, if previously neutralised or rendered alkaline by liquor potassæ, especially if it be afterwards maintained at a temperature of 115° F. or 122° F. In this respect urine only conforms to the general rule observed by myself and formulated in my previous communication to the Society[3]—that "slightly alkaline liquids were always more difficult to sterilise (by heat) than slightly acid liquids."

This difference came out strongest in my own experiments in the case of hay-infusion—the acid infusion invariably remaining barren after a few minutes' boiling, and the neutralised infusion invariably becoming fertile after a similar boiling. Accordingly I utilised hay-infusion to determine the cause of the difference in question. It could evidently only be due to one of two things—either (1) the change of reaction enabled germs pre-existing in the infusion to survive the ebullition; or (2) the addition of the alkali exercised a positive influence in exciting a *de novo* generation of organisms. To decide which of these two interpretations was the true one, an experiment was contrived in which the liquor potassæ could be added to the infusion, not before, but after it had been boiled, and thereby rendered permanently sterile. When added in this way, I found that liquor potassæ had not any power to excite germination. The infusions invariably remained barren when the alkali was added to them after they had been sterilised. I therefore concluded that the effect of the change of reaction consisted simply in enabling pre-existing germs to survive a brief ebullition. Dr. Bastian, in repeating this experiment in the case of urine, arrived at an opposite conclusion: he found that whether the alkali was added before or after ebullition he obtained the same result—the urine in both cases became fertile; and he concluded that the alkali had a positive power of promoting the origin of organisms in the urine.

This experiment, if properly performed, is obviously a crucial one, and it is recognised as such by Dr. Bastian. But two conditions are essential to the validity of the experiment. In the first place it must be ascertained beyond doubt that the boiled acid fluid has been really deprived of its germs—in other words, that the ebullition has been sufficiently prolonged to render it permanently barren; and secondly, that in adding the liquor potassæ due care is taken that no new germs are introduced at the same time. In repeating my experiment, Dr. Bastian appears to have departed from my procedure in two points, and he has thus possibly laid himself open to the two sources of fallacy just mentioned. In my own experiments, the acid infusion, after it had been boiled, was set aside in a warm place for a fortnight in order to test its sterility; and the liquor potassæ was not added to it until the lapse of time had satisfied me that it had been rendered permanently barren. In Dr. Bastian's experiments the liquor potassæ was added as soon as the vessels had cooled, so that he had no certainty that their contents would not have germinated without the addition of the alkali.[4] In the second place, instead of heating the tubes containing the liquor potassæ (as I had done) to 150° F., and thus ensuring the destruction of all germs contained in the air imprisoned therein with the alkali, he contented himself with subjecting them for an inconsiderable period to the heat of boiling water.

Seeing these two possible sources of fallacy, I determined to repeat Dr. Bastian's experiments with urine, but taking care to avoid these defects. I proceeded as follows:—

A flask with a longish neck was charged with an ounce of normal acid urine. The due quantity of liquor potassæ requisite to exactly neutralise this (as ascertained by previous trials) was inclosed in a sealed glass tube drawn to a capillary portion at one end. The tube was then heated in oil up to 280° F., and maintained at that temperature for fifteen minutes. The tube was then introduced into the body of the flask. The neck of the flask was next drawn to a narrow orifice; then the urine was boiled for five minutes, and the orifice sealed in ebullition. Ten such flasks were charged and treated in the same manner. They were then set aside in a warm place (from 70° F. to 80° F.) for a fortnight. At the end of this time the contents of the flasks were found perfectly transparent; the urine was therefore assumed to be permanently sterilised. The liquor potassæ was then liberated by shaking the tubes against the sides of the flasks and thus breaking their capillary points. The previously acid and barren urine was thus neutralised. The flasks were then placed in an incubator and maintained at a constant temperature of 115° F. At the end of two days it was found that the urine in each flask had deposited a sediment of earthy phosphates, but the supernatant liquor was perfectly transparent. The flasks were again placed in the incubator and maintained at a constant temperature of 122° F. for three days. At the end of this period they were withdrawn and opened for examination. Not one of them showed the slightest evidence of living organisms; the supernatant liquor was perfectly transparent, and no Microphytes could be detected under the microscope. The precipitated phosphate in some of the flasks presented a granular appearance, which might, by the unwary, be mistaken for Micrococci, but any such illusion was at once dissipated by adding a drop of hydrochloric acid, which instantly dissolved the phosphate and restored the perfect transparency of the urine. This acid has no effect on the turbidity caused by Microphytes.

These experiments therefore negative the conclusion that liquor potassæ, or a temperature of 115° F. to 122° F., or both conditions combined, have the power of exciting the generation of organisms in sterilised urine.

The effect of elevated temperature was also tested in another way. I had by me twenty-nine preparations of fermentible liquids which had remained over from my previous experiments in 1873–74. These consisted of

15 alkalised hay-infusons,
5 pieces of boiled egg-albumen in water,
1 piece of turnip in water,
2 diluted ascitic fluid,
1 blood with water,
1 albuminous urine,
4 pieces of meat or fish in water.

These had all been sterilised by the heat of boiling water two or three years ago, and were contained in large bulbs with long necks. Ten of the hay-infusions were hermetically sealed; the rest were all open to the air, under the protection of a plug of cotton-wool. All possessed perfectly transparent supernatant liquors, and showed no signs of containing organisms, nor of having undergone any fermentive or putrefactive changes.

These twenty-nine preparations were introduced into the incubator, and maintained at a constant temperature of 115° F. for two days, and then at a temperature of 122° F. for three days. At the end of this period not one of them showed any signs of fertility. The supernatant liquid in each bulb was quite transparent, and some of them, which were opened for microscopic examination, showed no traces of living organisms.

I can, however, fully confirm the statement of Dr. Bastian, that Bacteria, or certain kinds of them, grow and multiply freely in (unsterilised) urine, both acid and neutralised, when exposed to a temperature of 115° F. to 122° F.

The following is Prof. Tyndall's paper[1]:—

The communication "On the Influence of Liquor Potassæ and an Elevated Temperature on the Origin and Growth of Microphytes," which, at Dr. Roberts's request, I have had the pleasure of presenting to the Royal Society, causes me to say earlier than I should otherwise have done, that the subject which has occu-

[1] "Note on the Influence of Liquor Potassæ and an Elevated Temperature on the Origin and Growth of Microphytes," by W. Roberts, M.D. Communicated by Prof. Tyndall, F.R.S., December 21, 1876.

[2] "Researches Illustrative of the Physico-Chemical Theory of Fermentation," &c., read before the Royal Society, June 15, 1876.

[3] Studies on Biogenesis. Phil. Trans. vol. lxiv. p. 457.

[4] It is not sufficient to rely in such a case on a central flask or retort. Each flask or retort should have its own individual sterility tested, because it is practically impossible to apply the heat exactly in the same degree in any two cases.

[1] "Note on the Deportment of Alkalized Urine," by Prof. Tyndall, F.R.S. Communicated December 21, 1876.

pied Dr. Roberts's attention has also occupied mine, and that my results are identical with his.

In some of the experiments the procedure described by Dr. Roberts was accurately pursued save in one particular, which has reference to temperature. Small tubes with their ends finely drawn out were charged with a definite amount of caustic potash, and subjected for a quarter of an hour to a temperature of 220° Fahr. They were then introduced into flasks containing measured quantities of urine. The urine being boiled for five minutes, the flasks were hermetically sealed during ebullition. They were subsequently permitted to remain in a warm place sufficiently long to prove that the urine had been perfectly sterilised by the boiling. The flasks were then rudely shaken so as to break the capillary ends of the potash tubes and permit the liquor potassæ to mingle with the acid liquid. The urine thus neutralised was subsequently exposed to a constant temperature of 122° Fahr., which is pronounced by Dr. Bastian to be specially potent as regards the generation of organisms.

I have not found this to be the case, for ten flasks prepared as above described towards the end of last September, remained perfectly sterile for more than two months. I have no doubt that they would have remained so indefinitely.

Three retorts, moreover, similar to those employed by Dr. Bastian, and provided with potash tubes, had fresh urine boiled in them on September 29, the retorts being sealed during ebullition. Several days subsequently the potash tubes were broken and the urine neutralised. Subjected for more than two months to a temperature of 122° Fahr. they failed to show any signs of life.

These results are quite in accordance with those obtained by Dr. Roberts. His potash tubes, however, were exposed to a temperature of 280° Fahr., while mine were subjected to a temperature of 220° only.

With regard to the raising of the potash to a temperature higher than that of boiling water, M. Pasteur is in advance both of Dr. Roberts and myself. In a communication to the French Academy, on July 16 last, M. Pasteur showed that when due care is taken to add nothing but potash (heated to redness if solid, or to 110° C. if liquid) to sterilized urine, no life is ever developed as a consequence of the alkalisation.[1]

M. Pasteur has quite recently favoured me with sketches of the simple, but effectual apparatus, by means of which he has tested the conclusions of Dr. Bastian. Since his return from his vacation at Arbois, he has carefully gone over this ground with results, he reports to me, not favourable to Dr. Bastian's views.

I may add that I have by no means confined myself to the thirteen samples of urine here referred to. The experiments have already extended to 105 instances, not one of which shows the least countenance to the doctrine of spontaneous generation.

It gives me pleasure to refer to the skill and fidelity with which here, as in other cases, Mr. Cottrell has carried out my directions.

OUR ASTRONOMICAL COLUMN

THE NEW STAR IN CYGNUS.—In No. 2,115 of the *Astronomische Nachrichten*, Prof. Schmidt publishes the results of his observations on the intensity of light exhibited by this star between November 24, the date of its discovery, and December 15, when it was last perceptible to the naked eye. Having laid down his estimates of magnitude graphically on a large scale, he reads off therefrom the magnitude for every sixth hour, the differences showing a marked uniformity except about November 28, when the diminution of brightness was much more rapid. The magnitudes at midnight are as follow :—

	m.		m.		m.
Nov. 24 ...	2·97	Dec. 1 ...	5·27	Dec. 8 ...	6·44
,, 25 ...	3·03	,, 2 ...	5·47	,, 9 ...	6·55
,, 26 ...	3·14	,, 3 ...	5·65	,, 10 ...	6·64
,, 27 ...	3·38	,, 4 ...	5·81	,, 11 ...	6·71
,, 28 ...	4·06	,, 5 ...	6·00	,, 12 ...	6·79
,, 29 ...	4·74	,, 6 ...	6·16	,, 13 ...	6·86
,, 30 ...	5·06	,, 7 ...	6·32	,, 14 ...	6·92

In the forty-eight hours following November 27^{d} 18^{h}, there was a diminution to the extent of nearly 1½m. It is remarked that on the night of discovery its brightness was such as to render its near neighbour, 75 Cygni, invisible, while on December 14 and 15, 75 Cygni (6·4m.) in its turn nearly obliterated the new one ; at 10 P.M. on the latter date it was only by great exertion of the eye that a trace of the star could be discerned. Prof. Schmidt did not remark any decided change of colour : it was at no time decidedly of an orange tint, but less ruddy than γ, ϵ, and ζ Cygni, yet of a full yellow, 5·6 to 5·8 on his scale.

The curve resulting from the Athens observations accompanies Prof. Schmidt's description, and for comparison with it similar curves are added to show the law of diminution to the limit of unassisted vision, in the cases of the so-called new stars of 1848 (Hind, April 27) and 1866 (Birmingham, May 12). The descent was slowest in the former case and quickest in the latter, but the curve for the star of 1848 appears to be drawn from a small number of observations. Prof. Schmidt assigns for the interval between discovery and disappearance to the unaided vision, twenty-five days in 1848, nine days in 1866, and twenty-one days in 1876 ; the writer is able to state that the star of 1848 was just perceptible without the telescope as late as May 27, four days after the termination of Prof. Schmidt's observations and thirty days after its discovery, and there was a decided check in the star's descent between May 1 and 6 ; on May 1 it was a little less than 20 Ophiuchi, and on May 6, certainly a little brighter than that star. On April 29 it was so nearly equal to ν Serpentis that close attention was necessary to decide which was the brighter ; ν was found to be in a very trifling degree superior.

VARIABLE COMPONENTS OF DOUBLE STARS.—A suspicion of variability in the small companion of δ Cygni has been entertained by several observers, Prof. Secchi, among others, having remarked that the star has appeared single at times when the atmospheric circumstances would not afford an explanation. But the case of 72 Ophiuchi, No. 342 of the "Pulkova Catalogue" of 1850, is a much more suspicious one. The discoverer, M. Otto Struve, says, "I have very often looked at this star, and have many times noted it single. Yet on three occasions I have seen it double, always in about the same direction, and at a distance of 1″·5. I do not know how to explain these discordances, except on the supposition that the satellite is very variable." Secchi found the star single at the epochs 1856·53 and 1857·71 ; at 1857·57 a doubtful companion was noted in the direction 345°·9, but at the epoch 1859·61 he records it "certainly double, and well separated," the measures giving the position 3°·75, and distance 0″·608. This star does not occur in the more recent revision of the Pulkova list, by the Baron Dembowski.

To such cases may be added those of α Herculis and β Cygni, where the companions do not vary to such an extent as to cause the objects at times to appear single.

THE BINARY STAR γ CENTAURI.—While awaiting further measures of this fine double star, it may be remarked that admitting the measures of Sir John Herschel in 1835-36, to require an alteration of 180°, in order to render them comparable with those of Capt. Jacob and Mr. Powell, the latest published angle measured by the latter observer in 1860, indicates a motion in a retrograde direction of upwards of 160° between 1835·85 and 1860·68, the distance having increased about 0″·4. The star will be in all probability one of comparatively short period, and, as such, deserves attention at the hands of southern observers. The alteration of 180° in Sir John Herschel's measures is quite justified by the near equality of the stars.

THE MINOR PLANETS.—M. Perrotin, of the Observatory at Toulouse, met with a small planet on January 10, in a region of the sky where it is probable that Nos. 77 and 149, Frigga and Medusa, are at present situated, and the same planet was detected some ten days later by Prof. Peters at

[1] That alkaline liquids are more difficult to sterilise than acid ones was announced by Pasteur more than fourteen years ago. See *Annales de Chimie*, 1862, vol. lxiv. p. 62.

Clinton, U.S. On January 13, M. Borrelly, at Marseilles, also found a small planet distinct from that of Perrotin and Peters. It is remarked in M. Leverrier's *Bulletin*, that the first of these planets is unlikely to be Frigga, since the rough ephemeris in the *Berliner Jahrbuch* gives a contrary motion in declination. The object found by M. Borrelly, however, presents indications of identity, though a considerable correction of the elements of Frigga, brought up to 1874, December, by Dr. Powalky, would be required. If we employ these elements it will be found that with $\delta M = -3^\circ 17'\cdot67$, the computed and observed longitudes of Borrelly's planet on January 13 will agree, but there is an outstanding difference of $+1^\circ 39'$ between the latitudes. The comparisons with the observation on this date and one on January 15, are as follow:—

	Long. c − o	Lat. c − o
January 13 ...	0'·0 ...	$+1^\circ 39'\cdot1$
„ 15 ...	− 4'·4 ...	$+1^\circ 36'\cdot9$

NOTES

THE eminent physicist, Prof. J. C. Poggendorff, for many years professor in the Berlin University and editor of *Poggendorff's Annalen*, has died in Berlin, in his 81st year. We hope to be able to give a memoir of Prof. Poggendorff next week.

THE eminent Belgian botanist, Prof. Bellynck, died at Namur in December.

THE first four names in the Cambridge Mathematical Tripos list, are Messrs. Donald McAlister, St. John's, Frederick M. de M. Gibbons, of Gonville and Caius College, R. C. Rowe and Mr. James Parker Smith, both of Trinity College. The Senior Wrangler, who was born at Perth in May, 1854, has had a most distinguished career as a student.

THE Society of German Naturalists and Physicians holds its annual session at Munich, February 18, and celebrates at the same time its fiftieth anniversary.

THE Council of the Royal Dublin Society have elected William Archer, F.R.S., Secretary for Foreign Correspondence to the Royal Irish Academy, as head of their Library Department, and the members of the Society, as well as the literary and scientific public in Dublin are to be congratulated on the occasion.

THE Russian Archæological Society holds its Annual Congress at Kasan, July 31.

ACCORDING to a Report of the French Minister of Public Instruction, the salaries of the Inspectors-General of Public Instruction, the Professors of the Collége de France, and the Professors of the Museum of Natural History, have been raised to 10,000 francs, and of the Professors of the School of Living Oriental Languages, to 7,500 francs.

IN 1855 Napoleon III. proposed a prize of 50,000 francs for the most important improvement made in the use of voltaic electricity during the previous ten years. The prize was last awarded to M. Ruhmkorff, who, it is known, is a German physician established in Paris. M. Waddington has recently appointed a jury to award the prize for the third time. Any improvement in any industry using voltaic electricity comes within the competition, consequently the sphere is a very wide one. Regulations will shortly be issued.

PROF. NORDENSKJÖLD proposes to take command of an expedition next year which will examine the Siberian coast from the mouth of the Jenissei to Behring Straits. The return journey will be by way of China, India, and the Suez Canal.

THE remarkable entomological collections of the late Dr. Breyer are to be purchased for the Royal Museum for Natural History of Brussels, for the very low price of 240*l*. They contain above 21,000 specimens of insects, classified by the late eminent entomologist.

WE observe that the following honorary members have been elected by the New York Academy of Sciences:—In this country, Mr. G. Bentham and Prof. Boyd Dawkins; on the Continent, Profs. Brandt, De Candolle, Milne-Edwards, Hoffmann, M. de Verneuil, and Herr von Siebold.

WE have lately alluded to the very large ethnographical additions made to the Berlin Museum by Dr. Lenz, Dr. Bastian, and Dr. Jagor. A still more valuable collection has lately been presented by Dr. Nachtigal, and is now in process of arrangement. It embraces a vast variety of objects gathered amidst widely-diversified tribes by this well-known traveller during his last extensive tour through Africa, and affords a rare opportunity for comparative ethnographical study.

A CREDIT of 13,668*l*. has been requested from the Belgian Chamber of Representatives by the Minister of the Interior, for the astronomical and meteorological observatory of Brussels. Besides the construction of new astronomical and magnetical instruments, this sum will be used for the enlargement of observations upon the periodical phenomena of vegetation. Special arrangements will be made for carrying on the observations in the garden of the observatory, on plants especially selected for the purpose from the double point of view of the botanical geography of the present time, and of the study of former climates of the earth.

THE Belgian Chamber of Representatives passed, on January 26, a resolution of great importance to geologists, allowing the necessary sums for the publication, first, of the beautiful coloured maps of the soils and sub-soils of Belgium, prepared about thirty years ago by André Dumont, on the scale of 1 : 160,000, and which are long ago out of print; and second, of the MSS. of Dumont, with his numerous geological sketches and drawings, and of the numerous notes he took during his travels in Belgium. The MSS., which were purchased by Government, are already arranged for publication, and their appearance, as well as that of the maps, is expected about the end of this year.

THE Belgian Geological Society is engaged in the elaboration of a scheme for the preparation and publication of a detailed geological map of Belgium, on a large scale. The idea of such a publication being already approved by the government, the point under discussion now is the best means of engraving the work, and the Society proposes to entrust the task to two special committees, geological and cartographical; both committees will be placed under a common directorship.

AT the session of the Berlin Anthropological Society on January 20 Prof. Virchow gave an extended account of a large collection of diluvial remains found in the neighbourhood of Weimar. They consisted of the bones of such animals as the elephant, rhinoceros, arctic bear, deer, wild swine, &c. Apart from their palæontological value, they are extremely important from an anthropological point of view, as among the bones are several flints, remains of fires, and peculiarly divided fragments of bone, all indicating the presence of man in company with the animals mentioned. Dr. C. Jung described the superstitious observances and use of charms and amulets among the aborigines of Australia. The bones of animals which have been eaten, or the bones of dead relatives are regarded with peculiar reverence by almost all of the tribes. There was also a discussion on the supposed Phœnician inscription lately found on a block of stone

in North Russia, Dr. Wettstein declaring it impossible to connect it with known Phœnician characters.

In the January session of the Swedish Anthropological Society H. Torell gave the results of an interesting comparative study of the Esquimaux and Japanese. The anatomical and ethnographical resemblances are so striking that they give additional strength to the theory of the settlement of America from Asia by the way of Behring's Straits.

H. v. Schlagintweit-Sakünlünsky publishes, in connection with his report on the botany of the Himalayas, presented before the Berlin Academy of Sciences, an interesting comparison between the snow limits of the great Asiatic mountain chain and those of the Swiss Alps. The Himalaya range shows a snow limit at the height of 16,600 feet on the northern side, and 16,200 feet on the southern side. That of the Kuenlun range varies from 15,100 feet on the northern side, to 15,800 on the southern. The snow limit of the Alps shows an average height of 9,000 feet, 8,900 feet on the northern side, and 9,200 feet on the southern.

In the last session of the Austrian Meteorological Society, Prof. A. von Obermayer read a paper on the nature of fogs, strongly advocating the theory regarding them as minute drops of water, the specific gravity of which is overcome by the friction between the particles of air, according to Stokes's hypothesis.

In a communication in the *Daily News* of January 25, in reference to the Cairo Geographical Society, by the Alexandria Correspondent of that paper, some interesting details are given of papers read on the Eastern Sudan and on Darfur.

The use of rock crystal for normal scientific apparatus has recently been advocated by S. Stein, of Bonn, in a communication to the German Chemical Society. For scale-beams and scale-pans it is especially adapted, as it is entirely unaffected at ordinary temperatures by acids, bases, or the gases and moisture present in the atmosphere, while possessing nearly the same specific gravity as aluminium—2·65—and being comparatively unelastic. It is equally practicable for standards of measure, longitudinal as well as circular. Discs to be used for telescopes, theodolites, quadrants, &c., if cut at right angles to the chief axis, show an almost absolute unchangeability of form. The smallness of the coefficient of expansion renders it also eminently well fitted for normal thermometers, where accuracy and not cost is the chief requisite.

Commander Cameron having been invited by the Geographical Society of Paris to deliver a lecture on his journey across Africa did so at an extraordinary meeting held by the Society in the large hall of the Sorbonne, on January 26. The place, although fitted to accommodate 2,000 people, was crowded to inconvenience. On Saturday a banquet was given to Commander Cameron by the Fellows of the Geographical Society, about 200 being present. The President of the Republic was represented by his first aide-de-camp, the Marquis D'Abzac, and the Minister of Public Instruction by his general secretary, M. Watteville, who delivered to Commander Cameron the University gold palms and diploma. The knife and fork used by Livingstone in his African travels, and which had been purchased by MM. Rambaud, the large French traders established in Zanzibar, and presented to the Geographical Society of Paris, were used by Cameron. A magnificent album of the portraits of all the persons present at the banquet will be sent to Commander Cameron. MM. Hachette and Co. are preparing a magnificent French edition of Cameron's work.

At the session of the Berlin Academy of Sciences on January 25, Prof. Du Bois-Reymond gave a report of the investigations carried on in connection with the Humboldt foundation intrusted to the Academy. At present two travellers are supported by the funds—Dr. Hildebrandt, who is studying the snowy regions of the Kilimanjaro Mountains in Eastern Africa, and Dr. C. Sachs, engaged in researches in Brazil on the nature of the electricity of the electric eel.

A piece of burnt stone resembling a piece of partially burnt slate coal, with white sparkling specks on it, fell at Ecclefechan on the evening of the 2nd January. Two men, walking on the Glasgow road, heard a noise behind them, and on turning round they found the stone referred to embedded in the ground to the extent of half-an-inch or more. One of them attempted to lift it but got his hand burnt. The stone, which measures about four inches by two, and weighs nine ounces, took twenty minutes to cool. A volume of smoke proceeded from it.

A few years since, M. Delacour, sub-director of the Danish Meteorological Institute, invented the so-called phono-telegraphic system. Since then he has carried on an extensive series of experiments, the cost of which has been defrayed by the Danish Government, with the view of perfecting the new system. The results of his investigations were displayed a few days since to a company of electricians and members of the Danish Parliament. As is already known this system is based upon the application of vibrating currents, tuning-forks of the same number of vibrations per second being brought within the influence of current at both ends of the wire. M. Delacour made use on the above occasion of twelve different pairs of tuning-forks, all of which were connected at the same time with a single telegraphic wire. He was then able to send simultaneously twelve messages by means of the tuning-forks as well as one by the ordinary method, and most satisfactorily solved the problem with regard to the use of a single wire for the forwarding of numerous messages at the same time.

Prof. Anton Kerner, author of the "Means of Protection in Flowers against Unwelcome Visitors" (Nature, vol. xv. p. 237), has lately received from Charles Darwin the following characteristic epistle:—"Allow me to express to you my heartiest thanks for the pleasure experienced in reading your work. You have opened up an entirely new field of research, and explained many things which were previously enigmas to me. I find that I have fallen into many mistakes, in the preparation of my last book, when touching upon the subject which you have considered so fully."

The last number of the *Jahrbuch der k. k. geologischen Reichsanstalt* (vol. xxvi. No. 3) contains a very valuable elaborate paper by K. M. Paul—"Grundzüge der Geologie der Bukowina"—with a map on a scale of 1 : 280,000, reduced from that of the Geological Survey, and embodying the results of the survey, made during the last four years, together with the data furnished by former explorations. The southern, hilly corner of the Duchy is occupied by an island of crystalline rocks, bordered on one side by a zone of mesozoic limestone (Dyas and Trias). A broad zone of the so-called Carpathian sandstones (Neocomian, Gault, and Upper Chalk, and probably Eocene) follows, as is generally the case at the northern slope of the Carpathian, and crosses the land in a north-western direction. Further to the north-east we see a broad district covered with Neogene formations (Lower and Upper Mediterranean, and Garmathian stages), diluvial deposits, and loess, which district meets with the Galician plains to the west, and the Podolian to the north.

The geological structure, and especially the volcanoes of the southern parts of Luzon (Philippines) are the subject of an interesting note by Dr. Drasche, being a preliminary report upon his recent travels in the interior of the island, which appeared in Tchermak's *Mineral. Mitth.*, 1876, Heft 3. The note is

accompanied by a map on the scale of 1 : 1,000,000, constructed after that of M. Jagor ("Reise in den Philippinen") and by some views and geological sections. The volcanoes of the Taal district, the Majajai, the Monte Labo, and Sierra Corasi, the Ysarog, Buhi, and Mayon, or Albay, were visited by the author, and are shortly described. Among other interesting observations we notice the very steep slopes (32°) on which lava-floods may flow sometimes without being interrupted for distances of more than 300 feet, observed on the Mayon. The height of this last being 2,374 metres (7,787 feet), Dr. Drasche points out the error in the index of volcanoes, given in the late Mr. Poulett Scrope's work, where the height of the Mayon is given as 3,200 English feet. Another error of the Index is that an active volcano is reported to exist on "the little island Mindoro," whilst on this rather large island (250 square geographical miles) there are no volcanoes at all, neither active nor extinct.

In a communication to the Vienna Academy on the nature of gas molecules, M. Boltzmann abandons the notion that they behave like aggregates of material points (the atoms). He considers that in estimating the impact action of the molecules, we may almost regard the whole aggregate, which we denote as an individual gas molecule, and which may consist of different substances, perhaps even ether atoms, as rigid. It is found that then the ratio of the heat-capacities of the gas must be $1\frac{2}{3}$, when the gas molecules have ball-form. The ratio of the heat-capacities will be 1·4 if the molecules have the form of rigid bodies of rotation, but which are not balls; and $1\frac{1}{3}$ if they are of any other form of rigid bodies. These numbers at least seem to agree so far with those found experimentally, that one cannot say that experiment furnishes a contradiction of the theory thus modified. It is further shown that the values experimentally got for the heat-capacity, under this view, are in satisfactory agreement with the heat-capacities of solid bodies. Of course the gas molecules cannot be absolutely rigid bodies; this is already disproved by spectral analysis, but it may be that the vibrations producing gas spectra are merely brief shiverings during the shock of two molecules, comparable to the sound perceived on the shock of two ivory balls.

The current opinion as to the Ural range not bearing any traces of former glaciers, is now contradicted by M. Poliakoff. This explorer, who has had during the last ten years many opportunities of making a close acquaintance with glacial formations in Southern Finland, in the basin of Lake Onego, and on the Valdai plateau, reports that, while the lower parts of the Ural ridge are connected under large alluvial deposits, its upper parts, especially east of the water-parting, exhibit unmistakable morainic deposits with scratched boulders. The rocks bear also, sometimes, true glacial striae running from north-west to south-east, and certainly such striae would be found more numerous, were the localities more thoroughly explored than M. Poliakoff could do as he crossed the ridge on his way to the Obi. A lower secondary ridge, the last crossed by the highway before Ekaterinburg, exhibits also many *trainées* of immense boulders running in parallel directions, such as are found in Finland, Erthonia, and northern Russia. Further east, on the shores of the Obi (near to the mouth of the Irtysh), the lowest parts of the loose deposits, which are roughly stratified sands, contain a good deal of well-polished and striated boulders of glacial origin. These observations make it very desirable that the Ural were thoroughly explored by a geologist well acquainted with glacial formations.

The Göttingen Academy announces the following subject for prize competition in the physical class till November, 1878:—The question what special actions (if such there be) breathing in pure oxygen gas of the ordinary density of atmospheric air has on the animal organism, has not hitherto been answered by researches with sufficient agreement. Further researches, therefore, are desired, both on homoiothermal and, as far as practicable, on poikilothermal animals; in these should be shown, quite specially, along with the phenomena externally observable in the animals, the nature of the change of blood and material (excretion of carbonic acid, nature of urine). In the view of certain data, the purity of the oxygen from all foreign matters occurring in its preparation must be carefully looked to, while a mixture of atmospheric nitrogen within narrow limits, hardly to be avoided, would not essentially vitiate the results. In the mathematical class of the same Academy, new researches are desired on the nature of the unpolarised light-ray, calculated to bring the ideas regarding natural light from any source, near to those which theory connects with the different kinds of polarised light.

We have received the Fourth Report of the New Cross Microscopical and Natural History Society, which, we are glad to see, continues to prosper. It contains an address by the president, Dr. F. T. Taylor, on "Spontaneous Generation."

From the Seventh Annual Report of the Wolverhampton Free Library Committee, we are glad to learn that a Naturalists' and Archæological Department has been established in connection with that institution.

In 1875 a Bedfordshire Natural History Society was formed, and it has just issued its first Abstract of Proceedings. It has made a very fair beginning both as to numbers and as to the quality of the papers read. We hope it will receive more encouragement from residents in the county than it appears to have done, and that it will work diligently at local natural history. One of the papers, accompanied by a map, is "On the Botanical Division of Bedfordshire," by Mr. W. Hillhouse, F.L.S.

Among the papers in the *Proceedings* of the Belfast Natural History and Philosophical Society for 1875-6, is one by Mr. J. J. Murphy on the Glacial Climate and the Polar Ice-cap. Mr. R. Lloyd Patterson has a concluding note on some of the swimming birds of Belfast Lough.

The additions to the Zoological Society's Gardens during the past week include two Vervet Monkeys (*Cercopitheeus lalandii*) from South Africa, presented by Mr. T. G. Butler; two Arctic Foxes (*Canis lagopus*) from the Arctic Regions, presented by Sir Thomas Erskine, Bart, F.Z.S.; a Ring-Necked Parrakeet (*Palæornis torquata*) from India, presented by Miss Smith; three Silky Marmosets (*Midas rosalia*) from Brazil, purchased.

SOCIETIES AND ACADEMIES

London

Royal Society, December 21, 1876.—"On the Rotation of the Plane of Polarisation of Light, by Reflection from the Pole of a Magnet," by George Francis Fitzgerald, M.A. Communicated by G. Johnstone Stoney, F.R.S.

At the last meeting of the British Association, the Rev. J. Ker described a delicate and very remarkable experiment, from which it appeared that when plane polarised light is reflected from the polished surface of the end of a powerful magnet, the plane of polarisation is rotated. Mr. Geo. F. Fitzgerald has recently communicated to the Royal Society an explanation of this curious phenomenon, of which the following is an abstract:—

It is known from Faraday's and Verdet's experiments that when plane polarised light is transmitted through transparent diamagnetic or ferro-magnetic media, while under the influence of a powerful magnet, the plane of polarisation will be rotated, so that the plane of polarisation of the emergent beam will differ from that of the incident light. This action is most powerful when the direction of transmission coincides with the direction of the streams of magnetic influence, and in general the rotation is in opposite directions in diamagnetic and in ferro-magnetic media. Now, if we regard the incident light as formed of the two circularly polarised beams which are equivalent to it, we learn from Faraday's and Verdet's experiments that one of these beams is

retarded more than the other in passing through the medium, or, in other words, that they have different refractive indices. And of course if a powerful iron magnet were transparent, we might expect to find a much more powerful action of this kind upon transmitted light. Iron, however, is not transparent, so that the effect on transmitted light cannot be observed. But any effect which this difference between the refractive indices can produce upon *reflected* light may be made the subject of experiment, and Mr. Fitzgerald has shown that the observation made by Mr. Ker is an effect of this kind.

To show this, Mr. Fitzgerald splits the incident plane polarised light into its two equivalent beams of circularly polarised light. These circularly polarised beams are one right-handed and the other left-handed, and, before incidence, are equal in intensity to one another. If, however, their indices of refraction in the magnet are very different, as Faraday's and Verdet's experiments lead us to suppose, their intensities after reflection will be sensibly unequal, and their phases also will in general be unequally affected. Hence their united effect after reflection is to produce in general an elliptically-polarised beam, the major axis of which is inclined to the plane of original polarisation, thus producing that appearance of a slight change of the plane of polarisation which was observed by Mr. Ker.

Mr. Fitzgerald has repeated Mr. Ker's experiment, and ascertained that the reflected light is, in fact, elliptically polarised, as indicated by the theory. He has also found that when the polished pole of the magnet is gilt the observed effect disappears. This is a further confirmation of the theory, since gold is diamagnetic, and therefore too feeble to produce an appreciable effect, if the effect is due to the cause which Mr. Fitzgerald has pointed out.

January 11.—"On some Phenomena connected with Vision," by B. Thompson Lowne, F.R.C.S., Arris and Gale Lecturer at the Royal College of Surgeons, &c., communicated by Prof. Stokes.

The author arrives at the conclusion that the intensities of the sensations produced by various illuminations of a white surface vary as the square roots of the intensities of illumination, by comparing the shadows, cast in Lambert's well-known experiment, with ruled shades, as those of engravings and wood-cuts, taking the amount of reserved white in the latter to indicate directly the intensities of the sensations produced by them. He considers the number of rods and cones stimulated on a given area of the retina as the measure of sensation, and proportional to the reserved white.

He further finds, by a repetition of the experiment of MM. Delbœuf and Plateau, that the grey ring seen on a rotating disc with a black or white sector, disappears when the sector is sufficiently narrow and the rotation sufficiently rapid, and that the rate of rotation necessary to produce this result with the same disc and sector varies as the square root of the intensity of the illumination of the disc.

Lastly, he proposes a modification of Fechner's formula, which only requires a change of Fechner's convention that the liminal increment of sensation is constant. The author proposes to consider that $\frac{\Delta x}{\sqrt{x}} \propto \Delta S$. The formula then becomes, in accordance with his experiments, $2K\int\frac{dx}{\sqrt{x}} = K\sqrt{x} = S$, which he regards as a physiological instead of a psychical one.

Anthropological Institute, January 23.—Col. Lane Fox, F.R.S., president, in the chair.—A new member was announced. —Col. Fox then read his report to the Anthropometric Committee of the British Association on the Second Royal Surrey Militia. The measurements, which comprised the profession, race, origin, age, height, weight, chest measurement, colour of hair and eyes, and strength of arm, &c., of 459 individuals afforded some interesting facts concerning what might be called a fairly representative number of men from within a radius of twenty miles round Guildford. It appeared that the colour of the hair was in 391 cases brown or dark brown, and in only two cases black, and in two cases red, one of the latter being Irish. As to eyes, 311 were grey, light blue, or blue, 133 brown or dark brown. Col. Fox proposed some modifications of the existing tests of strength of arm and sight; suggesting that in the first the test should be the same as in drawing a bow, neither hand being in any way supported, and the pull being from an object not fixed. From a table of twenty comparative cases the average of strength showed in the case of pulling from a fixed point, 165·55 lbs., while the same men pulling with the one hand against the other only 81·95 lbs. From the general results Col. Fox considered that the muscular strength, vital capacity, &c., of our reserve and regular forces would show very favourably in comparison with those of the ordinary population, and so dispose of some of the frequent alarms given by the "man in the street" as to the deterioration of our forces in physique.—Mr. Street, President of the Philological Society, read a very interesting paper on the development of language, and Mr. E. B. Tylor and the President and others took part in the discussion. —Papers by Mr. Knowles, of Ballycully, Ireland, on the classification of arrow-heads, and on the "Portstewart find," were also read, and numerous objects illustrating the papers were exhibited.

Physical Society, January 20.—Prof. G. C. Foster, president, in the chair.—The following candidate was elected a member of the Society: Mr. A. G. Greenhill, M.A.—Dr. Huggins exhibited an enlarged view of a photograph, half an inch in length, of the spectrum of the star α Lyræ, which he has recently taken in a manner similar to that in which the spectrum of Sirius has already been obtained. The first results were very unsatisfactory, in consequence of the clockwork being insufficient for maintaining the image of the star on the slit for a length of time. Mr. Grubb has, however, devised a secondary control apparatus, the employment of which renders it impossible for the error to exceed one-tenth of a second. In the spectroscope employed the prism was of Iceland spar, and the lenses of quartz. Dry plates were employed, and the necessary breadth was secured by slightly changing the position of the image instead of by the use of a cylindrical lens. Dr. Huggins has also been engaged in taking a series of photographs of the moon, and hopes to obtain some information in regard to the question of a lunar atmosphere of small extent. In the spectrum of α Lyræ a line occurs corresponding with H_1 in the solar spectrum, and several more refrangible ones which he is at present unable to explain.—Mr. Lockyer considered the results which Mr. Huggins is obtaining to be of extreme importance, and he pointed out how he hopes a large series of photographs of stellar spectra will afford valuable information in regard to the constitution of certain substances now supposed to be elementary, such as calcium. Some time ago he communicated a paper to the Royal Society on the spectrum of this metal, and he considers that it is not a simple substance, but that the H lines are due to two elementary substances of which it is composed, and this supposition is confirmed by the fact that in the photographs exhibited by Dr. Huggins only one of the H lines is present, that is, only one of the constituents of the metal calcium is present in the star α Lyræ.—Mr. W. C. Roberts read a paper on the artificial production of columnar structure. He gave an account of the several theories which have hitherto been given as accounting for this phenomenon and that of cross-jointing, as observed in the Giant's Causeway, and he dwelt specially on the views of Mr. R. Mallet and Prof. James Thomson. He found as the result of experiment, that when certain masses of clay and sand are heated to about 1300° C. they contract to about the same amount as a basalt does in passing from the molten to the solid state, and that beautiful columnar forms are produced. He had hoped, by accumulating a number of specimens, to have been able to establish a relation between the strains at the point of rupture and the dimensions of the hexagons, but in the small masses employed the strains were so numerous that it was impossible to apportion their influences. He had, however, obtained a number of specimens which possessed much interest.—Mr. Lecky referred to a very fine columnar cliff in the island of Bedness, in Valencia Harbour, which he has examined in the hope of finding cross-joints, but, although some breaks exist, there are none at all comparable to those in the north of Ireland.—Prof. Guthrie showed an arrangement he has recently devised, in the hope of making the mercurial as sensitive as the water barometer. It consists of an ordinary siphon barometer in which the two vertical tubes are united by means of a long uniform horizontal tube having a diameter considerably less than that of the main tubes. The instrument is filled in the same manner as the ordinary siphon barometer, except that a bubble of air or dilute acid is left in the narrow tube. For a given rise of pressure the absolute amount of mercury which passes from the shorter to the longer tube depends upon their diameters, and as these are great in comparison with the tube uniting them, the motion of the bubble will be considerable in comparison with that of the summit of

the mercurial column. In the instrument exhibited, the horizontal tube was formed into a spiral, in order that the vertical tubes might be in close proximity. He then exhibited a number of thin india-rubber balloons, filled with water, which he has arranged with a view to illustrate the nature of jellies. When a jelly sets, it is assumed that the solid matter collects in the form of cells containing liquid, which burst on the application of heat. By weighing at intervals one of these india-rubber bags, he has found that evaporation takes place from its surface, thus with a bag weighing initially 749·4 grms., there was a loss of 0·95 grms. in the course of twenty-four hours. He is also examining a bag filled with salt water and immersed in water, in order to ascertain whether salt as well as water is capable of traversing the septum. Lastly Prof. Guthrie exhibited a large series of Chladni's rings, rendered permanent on cardboard by pressure, in contact with the plate which had been caused to vibrate, in a copying press. The sand and lycopodium were caused to adhere firmly by dilute gum.

GÖTTINGEN

Royal Academy of Sciences, December 2, 1876.—Remarks on some surfaces of a constant degree of curvature, by M. Enneper.—On the anatomy of Rhizocrinus lofotensis, by M. Ludwig.—On phenoxylic acid, by MM. Hübner and Buchka.

ROME

R. Accademia dei Lincei, December 3, 1876.—The following, among other papers, were read :—Mechanical experiments on the resistance of the principal metals used for fire-arms, by M. Rosset.—On the graduation of the Palmieri electrometer modified by Cantoni.—On Crookes's radiometer, by M. Marco.—Researches on picrotoxine, on cumosphenol, on action of chloride of acetile on santonic acid, on santonic chloride, on the chloride and the bromide corresponding to santonic acid, by M. Paterno, Cannizzaro, and others.—Petrographic studies on Latium, by M. Strüver.—Studies on the minerals of Latium, by the same.—On the muscular structure of the ventricle of the human heart, by M. Todaro.—The theoretic velocity of sound and the molecular velocity of gas, by M. Betti.—On fluoride of magnesium, by M. Cossa.—On the distribution of subterranean water in the district of Iglesias.—On the small motions of an entirely free rigid body, by M. Cerruti.—On the anatomy and physiology of the retina, by M. Boll.

PARIS

Academy of Sciences, January 22.—M. Peligot in the chair.—The following papers were read :—Craniology of the Negrito and Negrito-Papuan races, by MM. De Quatrefages and Hamy. The authors presented the fifth number of their *Crania Ethnica*. In this note they point out the differences between the two races named, and sketch their distribution. They are beginning the study of the Tasmanian type.—Memoir on electro-capillary actions ; in which are treated :—(1) The depolarisation of the electrode and the electric effects produced on contact of the skin and various liquids ; (2) Relations between electromotive forces, quantities of heat liberated during their production, and diffusive power, by M. Becquerel. Acids in contact with the skin take positive electricity, while alkalies take negative. With an alkaline liquid and the finger, *e.g.* the pores in the interior of the finger play the part of negative poles, those in the exterior of positive ; the liquids within the finger tend to be deoxidised, those without to be oxidised. Soup and wine in the stomach being in contact with venous blood through the vessels, the exterior surface of the latter is the negative pole of electro-capillary couples, the positive poles being within. Thus the liquids of the stomach are reduced and the blood oxidised. M. Becquerel finds the greater hydration of acids has always a less influence on the electromotive force than on the amount of heat liberated. There does not seem to be any relation between diffusion and the production of electromotive force.—Researches on substituted eugenols, by M. Cahours.—Contemporaneous formation of zeoliths (chabasie, christianite) under the influence of thermal springs in the environs of Oran (Algeria), by M. Daubrée.—On the structure of the calcareous shells of eggs and the characters which may be inferred from it, by M. Gervais. This inquiry was suggested by the discovery of a few egg-like fragments in some beds of detritus at Rognac in Provence, by M. Matheron. From comparison the author thinks these fossil eggs did not belong to a bird but to a reptile ; the structure of the shell closely resembles that of certain Emydosaurians. And this reptile, if really M. Matheron's Hypselosaurus, as seems likely, had more resemblance to Chelonians than the few fragments of its skeleton found there would indicate.—Observations of eclipses of Jupiter's satellites at the Observatory of Toulouse, by M. Tisserand.—On the advantage there would be in replacing quinine by cinchonidine in treatment of intermittent fevers, by Mr. Weddell. Cinchonidine can be obtained at a third (or less) of the cost of quinine ; its effect is as good, and some patients can take it more easily than quinine.—On the transmission of excitations in nerves of sensibility, by M. Bert. The end of the tail of a young rat was skinned, turned over and inserted in the back, and held there by sutures till union occurred. Eight months after, this "handle" was cut. On the dorsal stump being pinched the rat evidently felt pain. In this fragment, then (says M. Bert), the excitation of the sensitive nerves is propagated from the thick end to the thin, or the inverse direction to the normal. But this sensibility of the dorsal stump diminished from the second day and soon disappeared. The nerves, separated from their trophic centres, had degenerated. Perhaps after a longer interval the influence of new trophic centres might be sufficient, and sensibility would persist after section.—On the communication which must have existed, in historic epochs, between the chotts of Tunis and the Mediterranean, by M. Roudaire.—On the capacity of saturation of manganous acid, by M. Gorgeu.—On the normals which may be drawn to a given point in a conic, by M. Laguerre.—Note on a manometric appara us, *à propos* of a recent communication of M. Cailletet, by MM. Mignon and Rouart. It consists of a metal reservoir containing a liquid, and a glass tube indicating movements of the liquid ; the two parts are connected by a suitable joint.—Action of heat on quercite, by M. Prunier.—On the fermentation of urine—reply to M. Pasteur—by Dr. Bastian.—On the characters of the electric discharges of the torpedo, by M. Marey. He shows that the voluntary discharge of the fish is formed of the addition of a series of successive currents, and resembles, in complexity, muscular contraction, which consists of a series of shocks, the effects of which combine to produce the contraction.—On the return of contractility in a muscle, where this property has disappeared in consequence of strong induction currents, by M. Carlet. The contractility returns while the muscle is subjected to weaker currents.—On the physiological and therapeutical properties of glycerine, by M. Catillon.—On the nidification of the Aye Aye, by MM. Milne-Edwards and Grandidier. In this it approaches the lowest order of Lemurians. The higher carry their young attached to their back or to their breast.—On the modification of the floral envelopes of Gramineæ, according to the sex of their flowers, by M. Fournier.—On the theory of ventilation, by M. Chaumont. A change of 1 per cent. in the moisture produces as much effect on sensation as 2·32° C.—On seven favourable cases of transfusion of defibrinated blood, by Dr. Ladislas de Bellina.

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