THURSDAY, SEPTEMBER 23, 1875

HELMHOLTZ ON TONE

On the Sensations of Tone as a Physiological Basis for the Theory of Music. By Hermann F. Helmholtz, M.D. Translated with the author's sanction by Alex. J. Ellis, F.R.S., &c. (London: Longmans and Co., 1875.)

IN the general advance of scientific knowledge which has taken place during the last half-century, the science of acoustics has hardly received its fair share of attention. Founded on principles originated by the ancients, and afterwards extended by Galileo, Newton, Taylor, Sauveur, Bernouilli, Euler, Smith, Young, and others, the first great and complete work on it was "Die Akustik," of Chladni, published in Germany in 1802, but which is chiefly known by its French translation.

It acquired a high and wide reputation, and it has ever since been a standard authority on the subject. Sir John Herschel's celebrated treatise on Sound in the "Encyclopædia Metropolitana," carried the theoretical views of the science much further, and so supplied what was deficient in Chladni's more practical work; but nothing of importance has been added to our knowledge of the science from Chladni's time till about fifteen years ago.

It was then known that one of the most eminent physicists and physiologists of Germany, Herr Helmholtz, Professor of Physiology at Heidelberg, had been devoting considerable attention to the science of acoustics, and, if we recollect aright, some of his discoveries were brought forward by himself in lectures at our Royal Institution. In 1863 appeared a work by him, entitled "Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik," the result of eight years' investigations in acoustical science. This work not only gave much new information on acoustical subjects generally, derived almost entirely from the author's own long-continued investigations; but it published new and most important discoveries as to the nature of musical sounds, and valuable reflections on the bearing of these discoveries on the theory of music generally. The work met with high and universal appreciation among those who could understand it; it went through three editions in Germany, and was translated into French, which gave it a much wider circulation. Helmholtz's book has been followed by two popular works in English, namely, "Lectures on Sound," by Prof. Tyndall (1867), and "Sound and Music," by Mr. Sedley Taylor (1873), the chief object of both being to expound Helmholtz's discoveries and doctrines to English readers. We have now, however, a translation into English of the entire work, as mentioned at the head of our article.

In attempting to give some idea of the book, it is necessary to premise that it treats of two distinct kinds of subjects, physical and musical. In addition to being a profound and practised physicist, the author has clearly devoted much attention to the study of music, both theoretically and practically, and he has endeavoured to apply his physical discoveries and theories to the elucidation of many points connected with the art which he has found obscure. We may therefore divide our notice into these two heads.

In regard to the physical part of the subject, Helmholtz's work owes its greatest interest and its greatest fame to the entirely new light he has thrown on the nature of musical sounds, and the complete way in which he has explained and accounted for phenomena in regard to them which were previously very obscure.

A little attention will lead anyone possessing an ordinarily susceptible ear to the perception that a musical sound has three properties, each of which may be subject to a wide range of variation. These are: (1) its *pitch*, or its degree of acuteness or gravity in the musical scale; (2) its *strength*, or degree of loudness or softness; (3) the *quality*, or character of tone.

The question then naturally arises to what physical circumstances these three peculiarities are due. In regard to the two first there has been no great difficulty. It has been long known that the *pitch* of a musical sound depends on the rapidity of the vibrations which cause it, for according as the vibrations succeed each other more or less rapidly, the note produces sounds to us more acute or more grave, or, in other words, its pitch is higher or lower.

The *strength*, or degree of loudness or softness of a musical sound, has been also known to depend on the *amplitude* or extent of the vibrations, a larger amplitude giving a louder sound, and *vice versâ*.

The third property of musical sounds is their *quality* or peculiar character of tone. A violin, for example, gives a tone of a different quality from that of a clarionet, an oboe, a flute, or a trumpet, which all again differ from each other. The varieties of quality of tone that may be obtained are almost infinite; we not only possess an immense variety of musical instruments and means of producing musical sounds, all which have their individual qualities of tone, but even on the same instrument the same note may often be given several different varieties of character, independent of the mere loudness or softness. And that these infinite varieties are really objective physical differences, and not merely subjective or ideal, is proved by the facility with which educated ears can identify and distinguish between them, even sometimes to the minutest shades of difference. The stringed tribe of instruments, and still more the human voice, furnish ample examples of this.

The tone of a particular violin, or of a particular violin player, can be identified by a connoisseur among a hundred, and we all know that the varieties of quality of the human voice, even in the same register, are as easily recognised by the ear as the varieties of physiognomy are by the eye. And even in the same voice, the numerous varieties of vowel sounds producible are, when examined carefully, only varieties in quality of tone.

The nature of this property of sounds has hitherto been very obscure. Chladni, the great expounder of acoustical science in the early part of this century, says:—"Every real musical sound is capable of different modifications, whose nature is as yet entirely unknown, but which probably consist of some mixture of what is simply noise." He then explains at some length that this may arise either from peculiarities in the structure of the sounding body as regards material, &c., or from the nature of the body with which it is struck or rubbed, to produce the sound. He further adds the suggestion that such irregularities may be due to irregular tremblings of the smaller parts of elastic bodies.

Sir John Herschel ("Encyclopædia Metropolitana"), speaking of musical sounds, says · "Of their quality and the molecular agitation on which they depend, we know too little to subject them to any distinct theoretical discussion."

To put the problem clearly, suppose we have two musical sounds of the same pitch and the same degree of loudness, but of different qualities. To what physical cause is the difference in quality due? We know that the rapidity and the amplitude of the vibrations is the same in both cases; what other element of variation can enter into the phenomenon? Helmholtz is the first who has given a complete answer to the question.

It very seldom happens that a musical sound consists of one simple note; it is generally a compound of many notes combined together. To illustrate this by a simple example, suppose a stretched string, as a violin string or pianoforte wire, sounds any particular note. This note, which is called the fundamental one, will be due to the vibrations of the string as a *whole*, and if we could prevent any other kind of vibration this sound would be a simple one. But the string has a natural tendency (for reasons too recondite to enter upon here) to take upon itself other partial vibrations, and thereby to complicate the effect produced. It will divide itself spontaneously into two, three, four, five, six, or more aliquot parts, and each of these parts will set up an independent vibration of its own, giving a new note corresponding to its length. All these will sound together, and thus by the vibration of the string we get not only the fundamental note (which is usually the loudest and most prominent), but its octave, its twelfth, its double octave, its seventeenth, nineteenth, and so on, all heard in addition, and giving a sound which is a compound of them all. All the additional notes above the fundamental have been usually called in England *harmonics;* Helmholtz calls them *overtones* (obertöne).

We have given a string as a simple example of the mode of generation of a compound sound, but such sounds are produced in many different ways. A compound sound, so far as its effect on the ear is concerned, is due to a particular form of air-wave, produced in the instance given by the superposition of different sets of vibrations of the sounding body; and such a form of wave may be equally well produced by other means, such as a reed; or it may originate in the air itself, as in a flute. In every case where a given fundamental note is found, there is the same tendency for it to be accompanied by subsidiary fractional vibrations, producing corresponding overtones.

The phenomenon of compound sounds, as found by harmonics or overtones accompanying fundamental sounds, has been long known. It was mentioned by Mersenne as early as 1636, and has since been noticed by Bernouilli, Young, Rameau, Chladni, Sir John Herschel, Woolhouse, and others; but there is great difficulty in getting practical musicians, who have not been accustomed to considerations of this nature, to admit that what, judging by the practical impression on the ear, seems only a simple and single note, can really be one compounded of a great many sounds differing much in p tch, and some absolutely discordant. Helmholtz endeavours to combat this prejudice. He shows by several analogous physical and physiologica. examples that the senses are apt, in the presence of prominent facts, to ignore others which may be less prominent but equally real; and he reasons that as the fundamental note is almost always stronger than any of the others, the ear is inclined to refer the whole combination to that one note, and refuses to take the trouble of separating and identifying the various elements of the sound.

An example o. artificial compound sounds, purposely made, is furnished by a large organ. The pipes from which its sounds arise are in themselves but weak, and no multiplication of them would give tones of great power. Hence the long experience of organ builders has led them to form compound sounds by adding to each note pipes speaking its octave, twelfth, fifteenth, and other "overtones," the effect of which is, as is well known, to produce sounds of a most powerful and penetrating quality. Yet, if these overtones are well proportioned, they give, to an ordinary hearer, only the impression of one single loud sound.

By a little practice the ear may be educated to distinguish and separate the various notes which make up a compound sound, and when the habit of doing this is acquired, the illusion disappears. But that no proof may be wanting of this important principle, Helmholtz has contrived mechanical means by which any sound may be analysed, like a ray of light or a chemical compound, and its component parts exhibited separately. He has contrived certain instruments called resonators, each of which will, like a chemical reagent, test the presence of a particular overtone, and by submitting these in succession to the vibrating influence of the compound tone, they at once show whether the sounds they are tuned to are present or absent therein.

We have dwelt at some length on this phenomenon of the compound nature of musical sounds, because it is in reality the great fact which underlies the whole of Helmholtz's researches in this volume, and he himself has accordingly taken great pains to demonstrate and explain it, knowing that, although not a new discovery, it was yet far from being generally acknowledged. Indeed, we consider the establishment of this fact, so difficult of acceptation by practical musicians, and yet so simple and obvious when explained, is one of the most useful and important features of that portion of the complete work now under review. This establishment and explanation he afterwards uses as the basis for most of his researches, namely, the compound nature of musical sounds.

Such sounds, we have shown, consist, in almost all cases, not of a simple vibration, but of a number of vibrations of different velocities, superposed upon a fundamental one. The whole thus form a compound vibration which, though it produces on the inexperienced ear the effect of a single note, is really, when analysed, a compound of this note with a number of "overtones" harmonically related to it.

Among the many novel uses Helmholtz makes of this fact, the most important, physically, is the way in which he deduces from it the explanation of the third property we have mentioned of musical sounds, namely, their quality or character of tone.

Chladni was perfectly aware of the complex and varied nature of the vibrations producing musical sounds, but he does not seem to have attached any importance to them in this respect; for he says (p. 48, ed. 1830): "Die Verschiedenheit der Schwingungsarten trägt meistens nur wenig zu einer verschiedenen Wirkung des Klanges bei."

Sir John Herschel (Encycl. Metrop.) appears to have doubted Chladni's assertion, for he hints clearly at the probable influence on the quality of the sound, of the *form of the air-wave* (which is only the result of the complex vibration); and we may probably consider this to be the first hint on record pointing to Helmholtz's discovery.

Mr. Woolhouse, in an admirable little "Essay on Musical Intervals, Harmonics, &c.," 1835, goes further. He says (p. 77), speaking of the complex vibrations of a string: "The various combinations of these different modes of vibration must have a considerable influence on the musical quality and expression of the musical sound," which is a still nearer anticipation of the later doctrine.

Still, however, these anticipations were only guesses; it was reserved for Helmholtz to put the matter in the shape of a scientifically demonstrated fact. He has shown, by the most elaborate and conclusive investigations, that the *quality of a musical tone depends chiefly on the number and on the comparative strength of the various harmonical notes of which the tone is compounded.*

The overtones accompanying a fundamental note may be present in greater or less number, and they may vary considerably in comparative loudness or softness, and it is on the combination of these sources of variation that the quality of the tone will depend—or, to put the explanation in another and more scientific shape; as the *pitch* of a sound depends on the length or the frequency of recurrence of the air-wave, and the *loudness* on the degree of disturbance of the particles of the air therein; the *quality* of tone depends on what is called its internal *form*, or on the varieties of arrangement of expansion and compression of the air contained within one complete periodic cycle of oscillation.

Some modification in effect is often produced by a sound being accompanied by unmusical noises, such as the escaping of imperfectly used wind in a pipe, the unskilful scratching of the bow on a violin, the beating of reeds, and so on; but these are rather impurities than varieties of tone, and may be excluded from consideration.

There are very few natural sounds which are entirely simple, consisting of the fundamental note only. They are best produced artificially by means of the "resonators." The nearest approach to them may be found in the larger stopped wood pipes of an organ, an old-fashioned (not a modern) flute, and a tuning-fork after the sharp ring has subsided. The vocal sound of the Italian U (our *oo*) is also nearly a simple one. These examples will give the idea that simple tones are soft, dull, and entirely devoid of what is called brilliancy.

The addition of overtones gives this brilliancy and at the same time adds life, richness, and variety. It is to them that we owe entirely the agreeableness and pleasurable effect of musical tones. In proportion as the higher overtones predominate, so will the sound be bright and sparkling, or if in great predominance it will become metallic, thin, and wiry. If, on the other hand, the upper tones are weakened and the lower strengthened, the tone becomes more full, rich, and mellow. All the qualities of tone most esteemed and most useful in music are rich in overtones.

Helmholtz gives many examples of musical sounds of different character, which have been analysed according to his method. The tones produced from strings are peculiarly adapted to this purpose, because the vibrations so produced admit not only of mathematical calculation, but of ocular observation, and so give direct means of comparing the new theory with the facts, the result in all cases being most satisfactory and conclusive. The overtones in strings depend largely on the kind of impulse and the place where it is applied. In an ordinary piano the first six overtones are all audible, the three first strong, the fifth and sixth weaker, but still clear. The seventh and ninth, which are inharmonious, are excluded by striking the string in a peculiar place which does not admit of their generation. To prove the dependence of the quality on the strength of the overtones, Helmholtz has calculated mathematically what the strength of the first six overtones ought to be when produced with hammers of different degrees of hardness, and finds they should be as follows:—

	1.	2.	3.	4.	5.	6.
With a very hard hammer	100	325	500	500	325	100
With a medium hammer	100	249	243	119	26	1
With a very soft hammer	100	100	9	2	1	0

Now, as everybody who knows anything about pianofortes is aware that the tone is full and rich with a soft hammer, and hard and jangling with a hard one, it will be seen how admirably the mathematical results correspond with the actual ones, and how both confirm the theory.

Again, it is easily shown, both mathematically and practically, that thin wires will vibrate in short lengths much more easily than thicker and stiffer ones, and will therefore be more liable to produce the higher overtones, and hence the well-known metallic jangling of thin wires. This is the scientific explanation of the improved tone from the use of thicker wires in modern pianos, inasmuch as they admit of a more powerful blow without the production of the high and unfavourable overtones that would result from such a powerful impulse on a thin string.

The peculiar tones of the violin tribe, wind instruments, wood and brass, organ-pipes of various kinds, and so on, are all satisfactorily investigated in this way.

Helmholtz devotes much attention to the phenomena of vowel sounds, which had been already investigated by Willis and Wheatstone. He has completed their investigations by bringing the vowel sounds within his theory, and his elegant discussion of the subject, and the important results he obtains, are among the best features of his book.

All the above results we have mentioned have depended on the *analysis* of musical sounds. But the author has not stopped here. In chemistry, when a discovery has been made of the constitution of a compound body, by analysing it into its constituent elements, the efforts of the chemist are naturally turned to the converse process of proving the same proposition by synthesis, or by combining the single elements and showing that they will produce the compound. This proof has not been wanting in the present case, for Helmholtz has succeeded in combining simple sounds together in such a way as to produce imitations not only of vocal sounds, but of many other peculiar qualities of tone; not perfectly, from the extreme difficulty of imitating exactly all the minute shades of difference that enter into the combination, but still with enough success to demonstrate the general argument.

We have given especial prominence to Helmholtz's discoveries on the nature of musical sounds, because this is in reality the great feature of his work, by which it first acquired its fame, and by which his name will hereafter be honoured. But the physical part of the book contains much beside this that is important and interesting. His explanations on the general phenomena of acoustics are most lucid, and often very original; and his descriptions of the mechanism and action of the organs of hearing, coming from one of the highest authorities in physiology, are exceedingly instructive and valuable.

In Part II. the author enters into an investigation of what are called *beats*—a subject which has been heretofore very obscure—and also of other acoustical phenomena called "combination tones," which, though known since the time of Tartini, have not been thoroughly understood till Helmholtz gave their explanation. Into these matters we have not, however, space to follow the author: those who are interested in them can refer to the book for themselves.

Before we leave the physical part of the work it will be only just to testify to the excellence of the translation. Mr. Ellis is so well known as a philologist and a man of science, that his competence to deal with the work in a literary and scientific point of view requires no comment, and English readers may be satisfied that in this translation they have the original faithfully put before them. His work has evidently been a labour of love, and he deserves the highest credit for the trouble he has taken over it.

At the same time all men are fallible, and when a great authority condescends to do a work that could hardly be expected from him, we must not be unprepared for some little waywardness on his part, and there are a few things which we would rather have seen otherwise done. The title of the book is unfortunate; for, although no doubt "The Sensations of Tone" is a correct translation of "Tonempfindungen," yet to many English ears it will, we fear, sound strange and unintelligible from the fact that we are hardly accustomed in our language to understand the word "tone" in the sense here intended. The English title certainly does not give to the English reader anything like the same idea as the original title, "Die Lehre von den Tonempfindungen," does to an educated German. The strict rendering of a German phrase does not always correctly represent the original; for example, in speaking of the clever little tract of Hauslick, "Ueber das Musickalisch Schöne," Mr. Ellis translates it, "On the musically beautiful," whereas, as every reader of the tract well knows, the more appropriate expression in English would be "On the beautiful in music."

But the chief fault we have to find in the translation is the rendering of a term which of all others is the most important in the whole work, and in which the translator has, we conceive, taken a liberty not altogether justifiable. Helmholtz, in describing the compound nature of musical sounds, has called all the sounds above the fundamental one by the name of "obertöne," a word exceedingly appropriate, useful, and expressive, inasmuch as it at once defines and includes all these sounds in one appellation. Prof. Tyndall, in his *résumé* of Helmholtz's discoveries, has most naturally and with great propriety translated this term by the word "overtones." It exactly expresses the German in the simplest way, and it is as perfectly admissible into English as "overcoat" or "overseer."

Unluckily, Mr. Ellis is either too proud to adopt this word or has taken otherwise a dislike to it; for, on the ground that he does not consider it good English, he substitutes for it the expression "upper partial tones." This is not only clumsy and roundabout, but it is imperfect and wrong, inasmuch as it does not include, as the original expression does, the *whole* of the sounds above the fundamental, and gives no means of distinguishing higher overtones from the lower ones. As these overtones play such an exceedingly important part in Helmholtz's work, we cannot but consider, with all respect to Mr. Ellis, this rendering a blot on the translation which we very much regret.

We must reserve our notice of the musical portion of Helmholtz's work till a future opportunity.

OUR BOOK SHELF

Guide to the Geology of London and the Neighbourhood (Geological Survey of England and Wales). By William Whitaker, B.A., F.G.S. (London: Messrs. Longmans and Co., 1875.)

It is a matter of great satisfaction to geologists that the Geological Survey are again giving to the public some of the accumulated stores of information of which they are necessarily possessed, by resuming the series of large and complete memoirs which had been in abeyance for many years before the publication of "Whitaker's Geology of the London Basin," Part I., in 1872—a series now so well continued by the works of Judd and Topley. These, however, are comparatively expensive, and enter into minute details, so that although the whole of the information contained in the small book under notice has already been given at greater length in Mr. Whitaker's work mentioned above, or will be in a similar promised publication on the "Drifts of the London Basin," it will be of great use to a large number of persons who would not care for a more detailed description. A special Geological Map of London and its Environs, with all the Drift beds indicated, has lately been published, and for the last two years the Geological Model of London on a six-inch scale has been the admiration of all visitors to the Jermyn Street Museum: the pamphlet now before us is designed as a handbook to these. It commences with a description of the construction of the model, a matter of no small difficulty, considering the accuracy of the representation. The description of the various formations which enter into the London area, with their resulting features and scenery, though necessarily short, contains the cream of all the known facts, and what is better still, the reasons for all the not self-obvious determinations of the age and relations of the beds. Another most valuable portion is the series of tables of localities where the different formations may now be studied, showing no less than 154 places worthy of a geological visit within twenty miles of London. With regard to the general structure of the district, Mr. Whitaker is careful to refute the idea that the Tertiary beds were deposited in an eroded hollow of the chalk, as is often supposed; unfortunately, however, his section gives them rather the appearance of being so. We should also notice that although, on the evidence of fragments of Ammonites and Belemnites, he prefers to refer the red beds of the Kentish Town section to the Lower Greensand, none of this formation is represented in the section as lying beneath this part of London.

This convenient little publication, so full of valuable and condensed information, for so small a sum, will be of such great use to the members of the numerous field clubs, that we fear it will soon be out of print. What are 500 copies among so many who would wish to have it?

Snioland; or, Iceland, its Jökulls and Fjalls. By William Lord Watts. (London: Longmans and Co., 1875.)

In a recent number (vol. xii. p. 333) we published a letter from Mr. Watts announcing the important fact that he had succeeded in crossing the Vatna Jökull. So far as is known, this is the first time that this jökull (which means "glacier," and is probably cognate with the latter part of our word ic-*icle*) has been crossed, and the fact is creditable to Mr. Watts's determination and perseverance. The little book before us contains a narrative of an unsuccessful attempt to accomplish the same object, made by Mr. Watts in the summer of 1874. We regret to have to say that the narrative is a disappointing one. It is in the form of a rough diary, which seems to have been sent to the press in its crude form and published with little or no revision. A large portion of the book is occupied with a statement of the many difficulties, petty and serious, which Mr. Watts and his party encountered in the journey from Reykjavik, by the Geysers, Hekla, and the Myrdals Jökull to the Vatna Jökull, and there is really very little information about the region through which he passed. The entire narrative is extremely vague and unsatisfactory, and if Mr. Watts has any literary faculty, he certainly does not show it here; the reading of his narrative is a heavy task. Mr. Watts ought to know a great deal about the region with which this narrative is concerned, and especially about the jökulls in the south of Iceland, and we would advise him to put this information into a systematic form, make but little reference to the difficulties he encountered, obtain a few photographs on a much larger scale than the insignificant things which appear in the present work, and we have no doubt he would make a substantial contribution to our knowledge of Iceland. The party succeeded in getting only about half across the Vatna Jökull, when, from want of the necessary means to go further, they were compelled to turn back, after Mr. Watts had rather unnecessarily and sensationally planted the union jack at his furthest point. Mr. Watts's carelessness, to put it mildly, extends even to his use of language. The use of "laid" for "lay" might possibly be justified by eminent precedents; "peninsular of rock" may be a misprint, but "pulverent" is unjustifiable, and "molusc" is shocking.

Perhaps the most tangible piece of information conveyed by Mr. Watts is contained in the following paragraph:—

"To sum up, this hitherto untrodden Vatna Jökull is a mountainous tract, surmounted by a rolling plateau, containing numerous volcanoes, one or more of which, upon the north, appear to be in a state of pretty constant activity, while numerous others in all probability are paroxysmal, most likely exhibiting all the phenomena characteristic of (if I may be allowed the term) *bottled up volcanoes*. This tract, together with the Odatha-hraun, and the centre of Iceland with its numerous mountains, is a new volume of Nature, the first leaf of which has only just been cut, but whose secluded fastnesses will amply repay investigation."

In an appendix Mr. Watts gives some information as to equipment, which intending travellers in Iceland will find useful. The map at the end is on too small a scale to be of much use.

The main object of Mr. Watts's narrative is to attract attention to Iceland and induce travellers to co-operate in its exploration. We hope the work will serve this laudable object, as there is no doubt Iceland presents a handy and fertile field for explorers. Mr. Watts himself deserves great credit for what he has already achieved; we hope he will continue his work, and in a future publication add something of permanent value to our knowledge of the interesting island.

Report on the Neilgherry Loranthaceous Parasitical Plants destructive to Exotic Forest and Fruit Trees. By George Bidie, M.B., Madras. (Printed by E. Keys, at the Government Press, 1874.)

Surgeon-Major Bidie has in this volume presented to the Indian Government a report on the parasitical plants which prove destructive to forest and garden trees on the Neilgherries, and on the best mode of remedying the evil. The whole of these destructive parasites belong to one natural order, Loranthaceæ, represented in this country by a single species, the Mistletoe, and to two genera, *Loranthus* and *Viscum*. The fruit of the Loranthaceæ is characterised by the envelopment of the seed in a layer of a viscid substance, described by Dr. Bidie as intermediate in character between resin and india-rubber. Outside this viscid layer is a pulpy body which serves as food for birds and squirrels. After devouring this the seed is rejected, or, in the case of squirrels, passes uninjured through the body, and then adheres to the bark of any tree on which it may be cast. If the immediate conditions are unfavourable, the seed will be preserved in a state capable of germination for a very considerable time beneath its viscid covering. With regard to the mode of germination, Dr. Bidie has nothing to add to the information already furnished by Mr. Griffith and Dr. Hooker. With reference to the mode of attachment between the parasite and the host, the author states that although very firmly attached, there is no actual interlacing of the tissues; and that in some instances, after maceration in water for a few days, the parasite could be separated from the host without much difficulty. It is noteworthy that native Indian trees and shrubs do not appear to suffer nearly so much from the attacks of the Loranthaceæ as introduced, especially Australian, species. One foreigner, however, which appeared quite exempt from their ravages, was the "blue gum," the *Eucalyptus globulus*, which has already so many other useful qualities placed to its credit. Dr. Bidie asserts that the Loranthaceæ derive their nutriment not from the descending elaborated, but from the crude ascending sap of the host; hence their need for green foliage containing chlorophyll and possessing stomata, in which other parasites are deficient. The volume is embellished by fifteen large lithographs representing the different species, and illustrating the structure of the fruit and the mode of parasitism of the order.

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.*]

Personal Equation in the Tabulation of Thermograms, &c.

Mr. Plummer, in his letter (Nature, vol. xii. p. 395), has missed the point of the review of the work of the Meteorological

Office referring to the tabulation of temperatures (vol. xii. p. 101). From 1,283 estimations of tenths of seconds, as tabulated by the highly-trained and experienced observers at Greenwich, he shows that the whole seconds estimated were 15 per cent. of the whole number, and thereupon remarks that this is precisely the excess of whole seconds that is taken in the review of the work of the Meteorological Office as indisputably proving the carelessness of the tabulations at the Kew Observatory. This is a mistake. Kew was not singled out for criticism because the whole degrees tabulated there amounted to 15 per cent. of the whole number, but because of "the irregularity of the tabulations, more especially as regards the tabulations from day to day." An examination of the tabulations at Kew from day to day shows that there are first-class tabulators in that Observatory, but it also shows there are others whose work is inferior. Thus, in the first published sheet for Kew, viz., January 1874, on seventeen of the days the whole degrees tabulated amounted on each of these days to at least 25 per cent., and the average of the whole seventeen days reached 31 per cent., or nearly a third of the whole. On the remaining fourteen days of the month the average was 14 per cent. Hence the variations of the numbers of whole degrees from month to month, which, as stated in the review, were 172 for January, 87 for February, 127 for March, and 94 for April. It is this irregularity in the work of tabulation which has lowered the character of the work done at Kew.

The averages calculated from 6,696 tabulations showed that the number of whole degrees read off at the seven observatories were 8·4 per cent. of the whole at Stonyhurst, 15·0 at Kew, 19·5 at Aberdeen, 21·2 at Armagh, 23·7 at Falmouth, 24·7 at Valencia, and 24·8 at Glasgow. So far as the mere average numbers are concerned, the tabulations at Stonyhurst and Kew are satisfactory; not so, however, is the work done at the other five observatories, especially the last three, where, on an average of 6,696 tabulations, a fourth part of all the numbers tabulated were whole degrees. For particular months the percentages are sometimes very large. Thus, at Aberdeen during January 1875, the following are the percentages of the different decimal places of the dry-bulb readings as printed by the Office:—

Decimal places.	·1	·2	·3	·4	·5	·6	·7	·8	·9	·0
Percentages.	11	6	5	7	6	6	6	5	9	39

From this examination it is seen that 50 per cent. of the whole readings are assigned to two of the decimal places, viz. ·0 and ·1, of which 39 per cent. are whole degrees. The largest percentages are not, as in the cases adduced by Mr. Plummer, distributed in different parts of the decimal scale, but stand together, viz., ·9, ·0, and ·1. As regards the column for each particular hour, out of the thirty-one readings, nineteen whole degrees occur in the 5 A.M. column, eighteen in the 8 P.M. column, sixteen in the 5 P.M., fifteen in the 6 A.M., fourteen in the 4 P.M., thirteen in four of the columns, twelve in six, and so on, down to eight whole degrees in one column, and seven in another, than which no fewer whole degrees occur in any column. It is unnecessary to make any remark on these figures.

The Meteorological Office has published in their Quarterly Weather Reports the monthly extremes of temperature in two forms, viz. in figures, and in curves of temperature. These were compared and the results stated in the review, from which it was shown that as regards the first month's extremes, fourteen in number, there were twelve errors in the numbers as published by the Office; and as regards the first year's extremes, 168 in all, there occurred forty-one errors of temperature varying from 0°·4 to 9°·6, and twenty-two errors as regards the day and nine errors as regards the hour of occurrence. Altogether twenty-nine months have been examined with the general result of an average of fully four errors in stating each month's fourteen extreme temperatures. Now it is on the large proportion of errors made in stating the extreme temperatures (for the prevention of which one of the twenty-seven regulations for the Director of the Central Observatory was specially designed), taken in connection with such results as those given above for one of the observatories for January last, that the charge of inaccuracy in this very costly but vitally important part of the work of the Meteorological Office is based. This charge, Mr. Plummer's letter in no way meets. The simple course is to see that this department of the Meteorological Committee's work, including that of the outlying observatories, be brought under some sort of satisfactory control.

THE REVIEWER

Ocean Circulation

As the strength of Mr. Croll's conviction that he has completely demolished the "gravitation theory" of oceanic circulation by the "crucial test" to which he subjected it before the Geographical Section of the British Association, is not unlikely to influence the minds of some, I shall be glad to be allowed to point out (1) that I have never denied the existence of a horizontal "wind-circulation," and (2) that the doctrine to which he applied his test was not mine, but a creation of his own. For his whole argument was based on the assumption that the ocean is in a state of static equilibrium; whereas the theory I advocate, which was originally advanced by Lenz, and which Sir William Thomson (in commenting upon Mr. Croll's paper and my reply to it) pronounced to be a matter "not of argument, but of irrefragable demonstration," is, that the ocean never is and never can be in a state of equilibrium, so long as one part of it is subjected to polar cold, and another to equatorial heat; but that it is in a state of constant endeavour to recover the equilibrium which is as constantly being disturbed.

If the boiler and water-pipes of a heating apparatus be filled with water whose temperature is that of the building in which it is placed, the whole mass of fluid *is* in a state of equilibrium; but the lighting of the fire beneath the boiler disturbs that equilibrium, and produces a circulation, which will be maintained as long as the water is being alternately heated in the boiler and cooled by the atmosphere of the building.

Suppose that the elongated basin of the Mediterranean, instead of lying E. and W., were to be turned N. and S., so that its water, instead of being exposed (as at present) to a practical identity of thermal influences, should be subjected at one end to arctic cold and at the other to almost tropical heat: instead of remaining in its present state of nearly perfect equilibrium, it would have a circulation like that which I have exhibited in the trough-experiment.

The only objection raised by Mr. Croll which has even a show of validity, is based on the supposed "viscosity" of water, which he asserts to be sufficient to prevent the disturbance of thermal equilibrium from exerting the effect which the "gravitation theory" attributes to it. This assertion has now been completely disproved by the masterly investigations of Mr. Froude; who has demonstrated experimentally—what the "wave-line theory" of Stokes, Rankine, and Sir William Thomson had rendered probable—that in the resistance to the motion of a ship through the water, the viscosity of the water itself is so small an element that it may be practically thrown out, water behaving as a nearly "perfect fluid," except where it moves over solid surfaces. Mr. Froude (in conversation with me) not only sanctioned my conclusion that a constantly renewed disturbance of thermal equilibrium *must* produce an oceanic circulation, but mentioned as an instance of the very small difference of downward pressure necessary to sustain such a circulation, that he had ascertained by repeated observation at the mouths of harbours, lochs, and fiords, that wherever the water within has its salinity at all reduced by a mixture with fresh water, there is an underflow of sea-water setting inwards, precisely as in the Baltic and Black Sea Straits.

Mr. Croll attempted to draw a further disproof of the "gravitation theory" from the *Challenger* observations on the temperature of the upper strata of the Antarctic Sea, at and near the ice-border. These observations show that a stratum of water of from 32° to 29° overlies a stratum of from 34° to 32°; which is considered by Mr. Croll as a death-blow to my assumption that the coldest water sinks to the bottom. Now, since I have repeatedly pointed out that the water of melting field-ice, and *à fortiori* that of melting icebergs, will float on ordinary sea-water colder than itself, in virtue of its inferior salinity, and since Capt. Nares distinctly speaks of the cold surface-stratum as having this origin, it does seem to me not a little strange that Mr. Croll should have overlooked this consideration. It is obvious that, for the reason just stated, the descent of the cooled surface-stratum cannot take place in the polar summer *at or near the margin of the ice*: but that it takes place wherever and whenever the surface-cold is sufficient to check surface-liquefaction, and to cool down water of ordinary salinity to a temperature below that of the subjacent stratum, it will be hard for Mr. Croll to disprove.

I cannot but greatly regret that Mr. Croll abstains from subjecting his conclusions on this subject to the test of *personal discussion*. For if he would bring them (as I have brought my own) under the criticism of the Mathematicians and Physicists of Section A, he would find that, notwithstanding the acceptance which his endeavour to solve the climatal problems of past epochs by astronomical computation has very deservedly met with on the part of Geologists, his denial of the possibility of a thermal circulation in the ocean is utterly repudiated alike on mathematical and on experiential grounds, by those whose authority as physicists ought to make him feel less confident in his own conception of the question. W. B. CARPENTER

Source of Volcanic Energy

A FEW words of explanation are necessary concerning my letter which appeared in NATURE, vol. xii. p. 396. Mr. Mallet's *prime* source of energy for producing tangential pressures is the force of cohesion developed in a cooling globe, gravitation giving only partial assistance; and when I spoke of "gravitation of the whole mass to itself," I wished to convey that, setting aside altogether the force of cohesion and its accompanying motions, there still remains the force of gravitation, which, acting in a globe of such size as the earth, and composed of heterogeneous materials, must of itself produce enormous local pressures.

Mr. Fisher objects to my supposing the possibility of the development of heat without room being left for motion, but so far as I understand the doctrine of energy, it is only necessary to have *force* for the production of heat when motion is impossible.

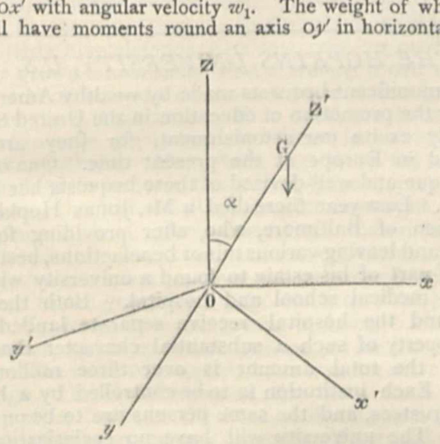
In Mr. Fisher's interesting paper his objection appears to be to the localisation of *fusing*, and not to the localisation of heat, fusing in some cases being prevented by the accompanying pressure. But in my little diagram I attempted to explain that the forces producing the high temperature might act in one set of strata, the neighbouring strata above and below at the same time being under much lower pressure, the pressure upon them being equal to the pressure of the rocks doing the work, minus the cohesion of said rocks; this difference of pressure being sufficient to allow one set of rocks to melt while others are crushed.

Kenmare, Co. Kerry WM. S. GREEN

Gyrostat Problem: Spinning-top Problem

IN vol. xi. p. 424 is given the solution, by Sir W. Thomson, of his gyrostat problem at p. 385. I venture to send a slightly different method* of obtaining the result (far inferior to Sir W. Thomson's in elegance and simplicity), in which Euler's equations for the motion of a rigid body about a fixed point are employed.

1. Take point of suspension for origin; the string for axis of z. The axis of the wheel Ox' revolves in horizontal plane xOy with uniform angular velocity Ω, and the wheel revolves round its axis Ox' with angular velocity w_1. The weight of wheel and axis will have moments round an axis Oy' in horizontal plane



perpendicular to Ox'. Let w' = weight of wheel and axis; A, B, B, moments of inertia round Oz, Ox', Oy'; w_2 angular velocity round Oy' at time t; a = the distance of C. G. from Oz, $xOx' = \phi$ = angle described by Ox' in time t. Taking moments about Oy', we have

$$B\frac{dw_2}{dt} + \overline{A-B}\,w_1\Omega = w'ag \quad . \quad . \quad . \quad (1)$$

(Pratt, "Mech. Phil." 446). Also since there is no velocity about an axis in horizontal plane perpendicular to resultant axis of $w_1 w_2$,

$$w_1 \sin.\phi - w_2 \cos.\phi = 0 \quad . \quad . \quad . \quad . \quad (2)$$

where
$$\phi = \Omega t.$$

$\therefore \frac{dw_2}{dt} = w_1\Omega \sec.^2\phi = w_1\Omega$ for $t = 0$ in (1), since w_1, Ω are independent of the time; whence (1) becomes

$$A w_1 \Omega = w' a g,$$

where
$$A = w k^2, \Omega = \tfrac{1}{4} \quad . \quad . \quad . \quad . \quad \text{Q.E.D.}$$

2. A similar question (concerning a spinning top) was proposed in the Senate House, Cambridge, in 1859, of which indeed the preceding is a particular case.

A uniform top spins upon a perfectly rough horizontal plane, its axis being inclined to the vertical at a constant angle α, and revolving about it with constant angular velocity Ω. Prove that the velocity of rotation of the top about its axis must be $\frac{(a^2+k^2)\Omega^2\cos.\alpha + ga}{k_1^2\Omega}$, where a is the distance of the centre of gravity from the extremity of the peg, $k'\,k$ the radii of gyration about the axis of figure, and about an axis through C. G. perpendicular to it respectively. Take O, the extremity of the peg, which remains fixed, as origin, and let Oz' be position of axis at any time t; OG $= a$; $zOz' = \alpha$. Let M = mass of the top; A, C, C, moments of inertia about Ox', Oy', Oz' (rectangular axes moving with the top); $w_1 w_2 w_3$, angular velocities about Ox', Oy', Oz' at time t.

The intersection of planes xOy, $x'Oy'$ will move round Oz with angular velocity Ω. Let ϕ = angle which Ox' makes with this line.

If we take moments about Ox', we have by Euler's equations (Pratt, art. 446)—

$$\frac{A\,dw_1}{dt} + \overline{C-A}\,w_2 w_3 = Mga\cos.zy^1 \quad . \quad . \quad (1)$$

Also $w_1 = \Omega\sin.\phi\sin.\alpha$, $w_2 = \Omega\cos.\phi\sin.\alpha$, $w_3 = \frac{d\phi}{dt} + \Omega\cos.\alpha$

$$\cos.zy^1 = \cos.\phi\sin.\alpha \text{ (ibid. 447)};$$

$$\therefore \frac{dw_1}{dt} = \Omega\cos.\phi\sin.\alpha\frac{d\phi}{dt} = \Omega\cos.\phi\sin.\alpha\,(w_3 - \Omega\cos.\alpha).$$

Substituting in (1) and reducing, we get—

$$C\Omega w_3 = Mga + A\Omega^2\cos.\alpha. \quad . \quad . \quad . \quad (2)$$

But $A = M(k^2 + a^2)$, $C = Mk_1^2$;

$$\therefore w_3 = \frac{ga + (a^2+k^2)\Omega^2\cos.\alpha}{k_1^2\Omega}.$$

If $\alpha = 90^\circ$ in equation (2), we get the solution of the preceding question as a particular case. F. M. S.

Arnesby

* A comparison of this method with Sir W. Thomson's (which is virtually the same as that adopted by Airy in his tract on Precession and Nutation) is instructive as illustrating the dynamical meaning of Euler's equations.—ED. NATURE.

OUR ASTRONOMICAL COLUMN

THE MASS OF JUPITER.—M. Leverrier has made a special communication to the Paris Academy of Sciences with reference to the bearing of his researches on the motion of Saturn, in a period of 120 years, on the value of Jupiter's mass. Laplace, in the *Mécanique Céleste*, had fixed $\frac{1}{1067\cdot09}$ making use of the elongation of the fourth satellite as determined by the observations of Pound, the contemporary of Newton, observations of which it appears we have no knowledge, except from the reference to them in the "Principia;" subsequently Bouvard, comparing Laplace's formulæ with a great number of observations, discussed with particular care, constructed new Tables of Jupiter, Saturn, and Uranus, in which important work he formed equations of condition, wherein the masses of the planets entered as indeterminates, and by the solution of which their values adopted in the Tables were obtained. The denominator for Jupiter's mass, expressed as a fraction of the sun's taken as unity, is 1070·0, and Laplace stated that on applying his theory of probabilities to Bouvard's equations it appeared to be nearly a million to one against the error of the mass thus deduced, amounting to one-hundredth part of the whole. M. Leverrier then refers generally to the discussion of the observations of several of the minor planets with the view to correcting the mass of Jupiter, and to the observations of elongations of the fourth satellite by the present Astronomer Royal at Cambridge, which last assigned for the denominator of the fraction 1046·77. He then remarks upon the circumstance of Bouvard having deduced from his comparison of the theory of Saturn with seventy-four years' observations a mass so nearly identical with that of the *Mécanique Céleste;* Bouvard left no details of his work behind him; it is only known that he adopted at the outset the value of Jupiter's mass admitted at the time, that of Laplace, and M. Leverrier explains that on the method of procedure adopted, Bouvard could not do otherwise than reproduce at the termination of his calculations the value he had assumed at starting. This is illustrated by the result of Leverrier's solution of his own equations of condition, founded upon the much longer period of 120 years, which proved wholly insufficient for the correction of Jupiter's mass. He remarks, with respect to Bouvard's work, that any value of the mass taken arbitrarily within certain limits will allow of a tolerable representation of the observations of Saturn, on the condition that this same arbitrary value is introduced throughout in the functions representing the mean longitude, mean motion, excentricity and longitude of perihelion; the elements obtained by Bouvard are therefore found represented by these functions of his arbitrary quantity, and he reverts to the mass assumed at the commencement of his work.

In conclusion, M. Leverrier insists that the use of the elongations of the fourth satellite for the determination of the mass of the Jovian system, has at present an incontestable superiority over the employment of the theory of Saturn, on account of the too short period over which the observations as yet extend, but in the lapse of time this superiority of the former method will diminish and the use of the perturbations will become the more advantageous. It is really, he adds, the same question as that which presents itself with regard to the solar parallax, which is determinable on two methods: the one, geometrical, the method by transits of Venus; the other, mechanical, depending for instance on the large inequalities in the motion of Mars. The method by transits, so important in 1760, but limited in its means of application, must eventually give way to the method of perturbations, the accuracy of which will increase unlimitedly with the course of time.

The first evaluation of the mass of Jupiter is that of Newton in the Cambridge edition of the "Principia" (1713), inferred from Halley's observation of an emersion of Jupiter and his satellite from the moon's limb, giving for the denominator of the fraction (whereby it is usual to express the mass) 1033. In the later editions of the "Principia" the mean distance of the fourth satellite resulting from Pound's observations, to which allusion is made above, was substituted in the calculation of the mass, which was found to be 1067. (It may here be mentioned that from later observations by Pound with a micrometer on a telescope of 123 feet focus, on the mean distance of the third satellite, Bessel found for the mass 1066). The next attempt in this direction appears to have been made by Triesnecker, Director of the Observatory at Vienna. In 1794 and 1795, making use of a Dollond object-glass micrometer, he obtained a series of measures of distances of all four satellites, the notice of which appears in the Vienna Ephemeris for 1797. Bessel deduced from them, by a mean of the four values, 1055·68. Then follow Bouvard's investigations already mentioned. It is understood that Gauss was the first to bring the perturbations of the minor planets to bear upon the determination of the mass of Jupiter, and that from the perturbations of Pallas he perceived the necessity of an increase to the mass adopted by Laplace. The circumstance, so far as we know, rests upon the authority of Nicolai, who, following in the same steps, discussed observations of Juno at fifteen oppositions, between the year 1804 and 1823, and (in the *Berliner Astronomisches Jahrbuch* for 1826) deduced for Jupiter's mass 1053·92. Encke, from fourteen oppositions of Vesta, between 1807 and 1825, made its value 1050·36, in a paper published by the Berlin Academy of Sciences in 1826.

Sir George Airy's observations at the Cambridge Observatory, alluded to by M. Leverrier in his recent notice, are next in order of time. They were commenced in 1832 and continued till 1836. The final result appears in vol. x. of the Memoirs of the Royal Astronomical Society; it is 1046·77, and depends upon observations on thirty-three nights. Details of the earlier Cambridge observations will be found in vols. vi. and viii. of the same memoirs. Sir George Airy considered it very improbable that there could be an error of a single unit in the denominator of the fraction expressing the mass, being led to this opinion by the close agreement of the separate results.

In the year 1835 Prof. Santini, the present venerable director of the Observatory of Padua, by sixteen nights' measures of the distance of the fourth satellite from both limbs of Jupiter, obtained for the mass 1049·2 (*Ricerche intorno alla Massa di Giove*, Modena, 1836).

Bessel's elaborate series of measures of distances of the four satellites commenced in October 1832 and were completed in the middle of 1839. They are fully discussed in his very valuable memoir, *Bestimmung der Masse des Jupiter*, in vol. ii. of his *Astronomische Untersuchungen:* the definitive value of the mass (p. 64) is 1047·879. Bessel's mass, which has been generally adopted in the calculation of the perturbations of minor planets and comets, and which is so close a confirmation of that deduced by the Astronomer Royal, has received much additional support from recent and, as regards method, essentially different investigations. Thus Krueger, of Helsingfors, from the perturbations of Themis, one of the minor planets which approaches nearest to Jupiter, assigns 1047·16; Axel Möller, by his masterly researches on the motion of Faye's Comet, 1047·79; while Von Asten, from his last investigations relating to Encke's Comet, finds 1047·61.

THE HOPKINS UNIVERSITY, U.S.

THE munificent bequests made by wealthy Americans for the promotion of education in the United States frequently excite our astonishment, for they are unparalleled in Europe at the present time. One of the most unique and well-devised of these bequests has lately occurred. Last year there died a Mr. Jonas Hopkins, a rich citizen of Baltimore, who, after providing for his relatives and leaving various minor benefactions, bestowed the chief part of his estate to found a university with an affiliated medical school and hospital. Both the university and the hospital receive separate landed and other property of such a substantial character that the value of the total amount is over three millions of dollars. Each institution is to be controlled by a board of nine trustees, and the same persons are to be on both boards. The university will have no ecclesiastical or political character or supervision, and will be modelled as far as possible after all that is best in similar American and European institutions. It is intended to give the highest instruction that can be obtained, and the trustees are to act in accordance with the most enlightened experience of the day. The scientific and literary departments will first be organised, and then will follow the departments of Medicine and Law.

No permanent buildings will be erected till all the Faculties are in working order and the wishes of each professor can be carried out; meanwhile a building has temporarily been secured in Baltimore, on the outskirts of which city are the grounds Mr. Hopkins has left for the hospital and university which in future will bear his name. The trustees have already selected the President of the University, and an admirable head they have found in Mr. Henry Gillman, formerly the Principal of the San Francisco University. Mr. Gillman is now in England, maturing his plans and gaining information from various universities in Europe. The dominant wish of the new president is to gather round him a body of professors and lecturers devoted to original research in their different spheres. Only one chair has yet been filled, namely, that of Mathematical Physics, and to this Mr. H. A. Rowland has been appointed. Though still quite a young man, the good work Mr. Rowland has already done in magnetism has made his name well known among English physicists, and in his new position a brilliant career lies before him. It is hoped that students will be received in 1876, and we heartily wish Mr. Gillman every success in his noble work.

SCIENCE IN GERMANY

(From a German Correspondent.)

MUCH as may have been written about bone-formation, yet this theme seems still to be inexhaustible, as in the current series of the "Archiv für mikroskopische Anatomie" (of which we gave the contents in a former report) no less than three papers are published on this subject. Two of these, those by Strelzow and by Stieda, speak of the ossification of cartilage and of bone-growth, and arrive at quite contradictory results. The older view on bone-growth starts from the supposition that the bones once formed undergo no further plastic change, that their single parts cannot displace each other, that therefore an insterstitial growth cannot be imagined. If the growing bone, as usual, does not merely show a uniform increase in size, but little by little changes its shape too (the bent bones for instance, the bends of which change during growth), this naturally leads to the supposition that besides the deposit of fresh material, a solution or absorption of those older materials took place, which did not fit the new shape. In opposition to this view, which Stieda also defends, Strelzow tries to prove that the bone grows interstitially, that therefore it can change its shape in an outward direction without reabsorption of any of its parts, that it is useless therefore to suppose the latter to take place, and that there is no reason for such a supposition. Now, with regard to the change from cartilage to bone, it has certainly been proved, for most cases, that the cartilage is first destroyed before in its place a bone grows from fresh materials. But while Stieda thinks this the case everywhere, Strelzow observes that the lower jaw and the shoulder-blade form exceptions to the general rule, the cartilage there passing immediately from its softer state to bone. Hertwig's observations, which he makes with regard to his investigations of the teeth of Reptilia, have a much more extensive range. In Hemibatrachia the teeth form earlier than any other bones of the head, and starting from this basis those bones in the oral cavity are destroyed, which only cover the exterior of the original cartilage skeleton, and are therefore called covering bones. In frogs these bones certainly form without the help of the teeth, which only appear at a later stage; but as frogs (Batrachia) and salamanders (Hemibatrachia) are of the same order, and particularly as the former are the more recent family, Hertwig thinks that in their ancestors the formation of teeth took place in the same way as in the salamanders now, but that in course of time they lost the primitive bone-forming teeth and retained only the bones resulting from them. The formation of teeth now observed in frogs is therefore a secondary phenomenon. Just as the bones of the oral cavity have their origin in the teeth, Hertwig supposes the covering bones on the exterior of the head to result from scales, and states that this is still very evident with certain fishes. What is a rule for lower vertebrata may also be applied to the higher orders, so that *all* covering bones may be derived from scales or teeth, which in sharks and rays are still equivalent and homologous formations. Therefore sharks and rays must be looked upon as the oldest forms of Vertebrata provided with bones; they are succeeded first by salamanders, then by frogs, and finally by the remaining reptiles, birds, and Mammalia.

It is a well-known fact that the gland-cells only absorb certain materials from the blood in order to convey them, more or less changed, into the hollow interior of the gland organ, and thus to furnish useful substances to the organism (secretions), or to remove useless ones from the same (excretions). Wittich demonstrates these relations in a particularly clear manner ("Archiv für mikroskopische Anatomie," 1875). After the injection of differently coloured solutions (carmine ammonia, indigo-sulphate of soda) into the blood of living rabbits, these colours are again excreted by the kidneys. If the animals are killed during this excretion, and the glands are examined, the carmine is only found in the gland vessels, not in their cells; the indigo, however, in the cells also. Such experiments evidently show that the gland-cells have a sort of selective affinity for the two colouring materials, letting the one pass entirely, and partly retaining the other in their interior.

In the same journal Neumann acquaints us with an interesting property of the cells which coat the abdominal cavity of a frog. It is known that some of these cells in female frogs are furnished with cilia, by the motion of which the ova ejected from the ovary into the abdominal cavity are introduced into the openings of the oviduct. Waldeyer, in his book, "Ovary and Ovum," had maintained that as the essential parts of the female genital organs result from the coating of the embryonal abdominal cavity, those ciliated cells physiologically connected with them result from the same basis, viz., the germ-epithelium; while the whole remaining coating of the *later* developed abdominal cavity, with its entirely different physiological signification, must be a formation genetically different from the former. Goette had already proved ("Entwickelungsgeschichte der Unke") that all those formations, together with several others, result from the uniform cell-coating of the abdominal cavity of the embryo. Neumann now specially proves their genetic identity by the observation that these ciliated cells only occur at the time of sexual maturity in the uniform epithelium of the abdominal cavity, and that therefore they represent local transformations of the same. This again confirms the theory, which Goette (*l.c.*) defends for the whole organism, that each embryonal part is not unconditionally intended for certain formations (which has been an accepted belief since Remak), but that from one single and uniform part in the embryo quite different tissues and organs can and may result, solely depending on the locally changing conditions of development. For instance, the coating of the embryonal abdominal cavity, besides the parts already mentioned, also furnishes the fibrous tissue of the intestines, the kidneys, and the heart.

THE LAWS OF STORMS *

Recent Criticism and Contrary Theories.—The rules referred to in last article are only empirical and are derived from no theory. Mechanics ought to take them in hand and explain them; but it has not been able to do so, for the circulatory movements of both liquids and gases are as yet a closed letter to that science. They are to-day in the same position as were Kepler's laws before

* Continued from p. 403.

the theory of attraction. Why ellipses? said theorists at the beginning of the seventeenth century. And why put the sun in the common focus of all these ellipses? Are there not also other curves followed by these planets in their course around the sun? But once connected with the principle of universal gravitation, these laws, so neglected by contemporaries, became "the immortal laws of Kepler."

Such at present is the position of the Laws of Storms. Despite the adhesion of practical men, meteorologists do not recognise the essential features which *ought*, according to them, to characterise storms. On this account, the practical rules themselves which sailors have followed for thirty years must be rejected; for they are entirely founded, as we have seen, on the circular movement of the air in storms.

These criticisms, more or less direct, based on the theory of centripetal hurricanes or of aspiration, have at the present time all the greater force that mariners themselves have an innate belief in the mere idea of this theory. We even find this belief in the writings of authors who have shown themselves best acquainted with the laws of storms and with the corresponding practical rules. Two examples may be referred to.

The well-known hydrographic engineer, Keller, in his "Treatise on Hurricanes," says that in intertropical regions where cyclones originate, the atmospheric strata underneath the sun dilate and draw up the inferior air of the dilated zone; that if ordinary aspiration, due to the calorific action of the sun, is further promoted by an *electric attraction*, the affluent air will rush with more force into the interior vacuum, &c. Within this space or vacuum he conceives that the water of the sea raised by the central aspiration of a typhoon or a waterspout ascends. When the gyratory column passes from the sea on to the land, it hurls against the shore the water raised by aspiration, and the sea suddenly inundates the low coast to a considerable distance inland. Finally, on land, the force of aspiration of these phenomena exercises its ravages not only by throwing down, but by tearing up trees, and overturning even solid buildings.

M. Bridet, again, asserts that there is formed under the action of the sun, a sort of vacuum resulting from the rapid ascension of masses of heated air. This vacuum is rapidly filled up by the lower currents of air which flow towards it from all directions. These currents, flowing along the surface of the earth, acquire a gyratory motion from the daily rotation. On reaching the base of the ascending column, near the centre of rarefaction, the air carried by these currents gets heated, and expands in its turn; it follows the ascensional movement of the molecules that it replaces, and rises, preserving its rotatory motion.

Persuaded of the reality of this immense draught which the aspiration of ascending columns of heated air must exercise on the lower stratum, in the manner of a chimney, sailors themselves must say that the circular diagram which Reid and Piddington have used for cyclones is scarcely admissible from the theoretic point of view; that already the centripetal movement has been recognised in waterspouts and tornadoes, which, after all, are only cyclones in miniature; that the convergent diagrams proposed recently by Mr. Meldrum, of Mauritius, have perhaps a better foundation, more especially if, as Mr. Meldrum affirms in the cases of two storms which he has recently discussed, these convergent diagrams better represent the true features of the hurricane than concentric circles. Mr. Meldrum's "Note on the form of Cyclones in the Indian Ocean" has been published by the Meteorological Committee of the Royal Society, and is thus well known. We reproduce one of the figures (Fig. 4), and ask the reader to compare it with the circular diagrams of the hurricane in Cuba (Fig. 1); the difference of the two systems will be seen at once.

According to the first the centre is situated perpendicularly to the direction of the wind; according to the second, it will be situated (neglecting for the moment the curvature of the spirals) in that very direction. There is here a difference of nearly 90°.

What will hereafter be the position of sailors in the face of an imminent danger? This is in substance what they are told:—You feel, you see, that a danger menaces you; the aspect of the sky, the state of the sea and of the winds, the steady fall of the barometer, already tell you that there is not a moment to lose if you wish to take the step which may save all. Hitherto you have believed, in the faith of certain empirical rules, that the danger is on your left; not at all—by my theory it is before you.

The captain has no time to search the works of Reid, of Redfield, of Piddington, or to examine the theory of centripetal hurricanes. This is a question which must be quickly answered. Is it necessary, in order to this, to make one's self familiar with all that has been done during the last thirty years in order to *try*, in this repetition of the first investigation, if the centripetal diagrams represent the direction of the wind better than the circular diagrams? This is a labour which would require at least many years.

Happily there is another method of solving the question, which is to examine that theory of hurricanes of centri-

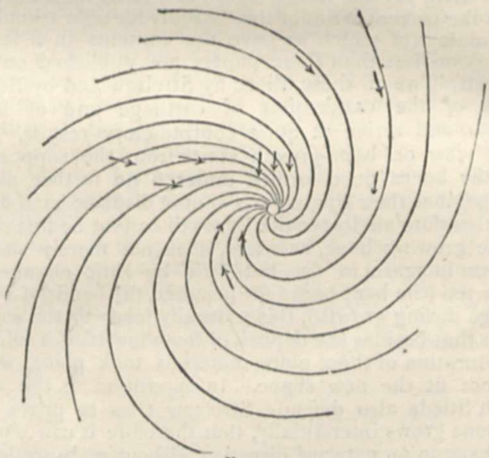


FIG. 4.

petal aspiration which has given rise to all these doubts with regard to the laws of storms. If this theory is found to be true, the authors of the "Laws of Storms" will certainly have been wrong in neglecting its indications. Let us therefore put aside their pretended circulatory movements. The air moves towards a centre of aspiration instead of turning round a point; all will thus be changed, and especially will it be necessary to promulgate practical rules altogether different. But if the theory of aspiration is proved to be false—and, to know what to believe on the subject, long years are not necessary—a rapid examination will be sufficient; if it is false, we say, sailors may continue to place confidence in the rules which have been so serviceable to them for thirty years.

By investigating the whirling movements of which the sun is the theatre, M. Faye was led some time ago to examine this theory without any reference to nautical matters. He has found it completely illusory. On the contrary, that theory which fits into the solar phenomena is found to agree thoroughly with the Laws of Storms; and we need not be astonished at this agreement, for the laws of mechanics are the same everywhere, and the gyratory movements of fluid masses will not vary more in the case of one heavenly body as compared with another than the laws of gravitation. Putting aside solar questions, which interest only astronomers, we shall treat of the purely meteorological question, and in the meantime place before the reader the conclusions of this essay:—

1. The idea of centripetal hurricanes of aspiration originates in an illusion of the sense of sight; it is an old prejudice whose history it is easy to follow from the most remote times to the present day.

2. The theory of centripetal hurricanes, suggested by this prejudice and the hypotheses which it implies, cannot be accepted. The adoption of similar ideas by enlightened minds is only to be explained by the venerable authority of this prejudice.

3. Bases of the mechanical theory of gyratory movements; agreement of that theory with the Laws of Storms. These ought to be considered as a first but excellent approximation; a means of making further advances.

1. *History of a Nautical Prejudice.*—In the midst of the profound calm which often precedes thunderstorms, the lower strata of the atmosphere are not agitated by the least breath; heavy clouds approach at a great speed and cover the sky—a clear proof that powerful currents prevail above, the influence of which does not extend to the ground. From one of these clouds a sort of bag or end of a tube or funnel is seen to issue, and which gradually descends, lengthening at the same time. It seems to be formed of the same material as the cloud; and in fact is a true fog which envelops the cloud, thus rendering it visible to our eyes.

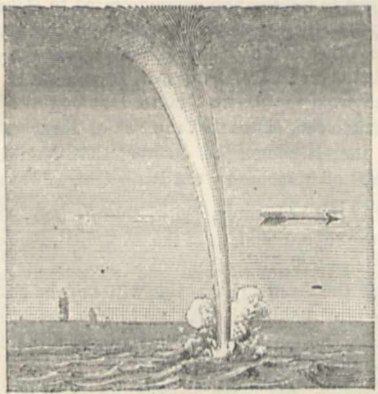


FIG. 5.

Meantime the centre of this funnel is agitated by a violent whirling movement of which the small whirlwinds of dust that are sometimes seen on our roads give a very accurate idea. When the waterspout reaches the ground and encounters obstacles in its way, it sets to work upon these after the manner of a turning machine of great speed at the end of a vertical axis. It raises around its lower extremity a cloud of dust, overturns trees, batters down walls, and unroofs houses. If, instead of land, the waterspout meets with a water surface, it acts upon it like a square-bladed scoop at the end of a vertical axis, and the churned water is thrown to a distance in foam; if it advances on a pool, is empties it in an instant; if on a lake or a sea, the water spurts out all round the foot of the waterspout in clouds of spray.

Look particularly at this long vaporous tube (Fig. 5), which extends from the surface of the earth to the clouds, to a height of from 1,600 to 2,000 feet and upwards; it appears flexible, and has an undulatory movement through its entire length: the least breath of air alters and distorts its form; and its whirling movements are felt down even to its base, which sweeps over the earth, carrying devastation in its train. If it assumes greater dimensions, it is no longer a waterspout, but a tornado. We have here in two words the history of the tornado of Jan. 20, 1854, which occurred in the county of Knox, Ohio, and which in half an hour levelled 50,000 trees with the ground, hewing for itself a pathway through the forest a quarter of a mile broad, which could not have been made in some weeks by a whole army of backwoodsmen.

The tube, which takes the form of a pillar, a funnel, the trunk of an elephant, &c., usually disappears after being as it were broken across, by the violence of its own gyratory movements. Further, the misty vapours which compose it slowly ascend, and the combination of the ascending and whirling motions gives the appearance, when seen at some distance, of a spirally ascending movement, which, however, bears no relation to the internal gyrations of the waterspout. Movements, not real but illusory, are all that are perceived. The spectator supposes he sees objects ascending in the interior of the waterspout. Thus a bit of cloudy vapour looks like a bird caught by the waterspout and rapidly whirled aloft. If the vermicular motion is continuous and along the whole length of the waterspout, the question is asked, what can in this manner ascend in a long tube whose base is plunged into the sea and which violently agitates its surface. At once and without any inquiry the logic of the imagination comes into play, and the conclusion is come to that it is the water of the sea which the waterspout is in quest of; this it pumps up and distributes among the clouds, and its ascent up the tube is plainly seen. No question is put as to how a tube composed of aqueous vapour can hold and sustain deluges of solid water. Moreover, are the clouds not seen rapidly to grow portentously heavier and bigger by the water so abundantly supplied by the waterspout?

It were idle to listen even to observations made under such impressions. For thousands of years sailors have transmitted from age to age tales of waterspouts which have lifted ships into the air, sucked up the water of the sea, and poured it down again on some hapless ship which was unfortunate enough to pass under and break the tube of the spout. Tales like these, unceasingly reproduced with ever-fresh details, powerfully aid the illusion in determining the event before it is seen.

(*To be continued.*)

NOTES

AN interesting service to astronomy has been rendered by Mr. Davidson, the head of the American Transit Expedition to Nagasaki, Japan; he has determined the exact site of Abbé Chappe d'Auteroche's Observatory in 1769, when he observed the transit by order of the French Academy of Science, at St. Joseph, California. As Abbé Chappe died soon afterwards from a fever caught while fulfilling his mission, his narration was completed by people who had never been on the spot; a blank has been left in the records of his observations, which has now been filled up 108 years after the event. The Abbé Chappe was an uncle of the celebrated Chappe who invented telegraphs during the wars of the Revolution.

M. LECOCQ DE BOISBAUDRAN, who is well known in connection with spectroscopic analysis, has just announced the discovery, by means of the spectroscope, of a new chemical element which he calls *gallium* and affirms to be closely allied to zinc. The spectroscopic character of gallium is two violet lines, one corresponding to wave-length 417, and the other to 404, but fainter. The communication was made by M. Wurtz, at Monday's sitting of the French Academy. A commission has been appointed to report on the discovery. Gallium is said to be found in a special blende from Pierrefite mining works, in the Argeles Valley.

IT appears that M. Janssen's observatory is to be built at Fontenay at the expense of 80,000 francs. A sum of 50,000 francs is to be spent on instruments, exclusive of the apparatus used in the transit of Venus. He is to have two assistants, each of them receiving 4,000 francs yearly. The instruments are to be under the direct supervision of the Minister of Public Instruction.

METEORS of unusual brilliancy have been seen from several points of late. We recently noticed one seen from the Radcliffe Observatory, Oxford, on Sept. 3, and from the same place we learn that a large meteor was observed on Sept. 7, 11h. 21m. Greenwich mean time, about twice the apparent magnitude of Jupiter, increasing to about four times that of Jupiter, with an accompanying tail of about 5° in length, from near 4 Arietis to a point near *f* Tauri, where it burst into five or six pieces. Colour, blue to green, with red at bursting. Time visible, about seven seconds. It was seen by Mr. Lucas and Mr. Bellamy. Another very peculiar one was seen from Edinburgh and neighbourhood on the 11th inst. A Burntisland correspondent, Mr. G. J. P. Grieve, writes that about 11 P.M. that evening, while pacing a gravel walk in moonlight and partly gaslight, a sudden vivid gleam from behind threw his shadow clear cut on a bright ground. Turning sharp to see the origin of the blaze, after a second or so he noticed a serpentine meteor: the glow or trace left in the path of a shooting star, whose maximum intensity, if not explosion, lay at the west end of the trace. The trace appeared in Auriga, and so close to the three leading stars next south of Capella, that he had not the least difficulty in sketching the position. The particulars are these:—Station in lat. N. 56° 3′ 57″; long. W. 3° 13′ 10″. Position of meteor, in constellation Auriga. Duration from first blaze to disappearance of trace, three to four minutes. Timed at disappearance of trace, 11.2¼ P.M. by Edinburgh gun time. Several letters on this serpentine meteor—"the sky snake" they call it in the north—appear in the *Scotsman*, all agreeing as to its peculiar form and great brilliancy. One observer near Mid Calder "was attracted by the appearance of a magnificent meteor, which was visible for about two seconds, and which, being apparently interrupted in its flight, assumed a zigzag course; and, flashing brightly at each angle thus formed, it disappeared, leaving the snake-shaped track behind it, which was visible for several minutes afterwards, finally disappearing in the form of a ring." On the night of the 14th inst. another magnificent one was visible, apparently over all England. It is noticed in the *Bradford Observer* of the 15th, and Mr. T. W. Shore writes us that he saw it while in the Southampton Water. The time of its appearance, both in the north and south of England, was 8.30 P.M. Mr. Shore, while looking towards the land on the north, observed the meteor commence its luminous course at an apparent altitude of about 30°, and travel to the horizon in a direction from S.E. to N.W. The meteor appeared to him to be about three or four times the brightness of Jupiter, and the time of its course rather more than two seconds. The *Bradford Observer* states that "all accounts agree in saying that it presented the appearance of a flying body of light of considerable size, and that during the period of its passage it lighted the whole sky. It would seem that it first made its appearance from the south-west, its course being over Bowling Park and in a north-westerly direction over Bowling, Horton, and Manningham, and a spectator describes it as an oblong body of light, several feet in length, and bearing the appearance of some solid body in a state of combustion, the sparks flying out on all sides, and a track of flame being left after its passage. Its passage was accompanied by a noise as of a loud explosion, which was plainly heard, not only by those who were outside, but by persons inside the houses who did not see the aërolite itself. All parties concur in saying that so strong a light was cast around that a newspaper could easily be read for the space of half a minute." The same meteor was seen from Manchester and London, and no doubt from various other places. In the report of the meteor of Sept. 3, δ should be λ Piscis Australis.

IN order to stimulate research, experiment, and invention, and to promote the advancement of mining enterprise in Cornwall and Devon, Mr. G. L. Basset, of Tehidy, offers prizes under the following conditions:—1. For the discovery of a new mineral, in Cornwall or Devon, which is deemed likely to become commercially valuable, a prize of 50*l*. An accurate analysis and a description of the leading physical properties and distinguishing characteristics of the mineral to be given, specimens to be handed to the Committee, and the locality and mode of occurrence to be distinctly described. 2. For the invention of a method—mechanical or chemical—of making marketable with commercial advantage, ores or minerals produced in Cornwall or Devon, and hitherto regarded as worthless or of little value. The method to be clearly described, and specimens of the product in its several stages to be handed to the Committee; or, for the discovery of some new application of a mineral substance already known to occur in Cornwall or Devon, either by itself or in combination, to some useful purpose, so as to render it of marketable value, or materially to enhance its value if already marketable to some extent—a prize of 100*l*. The prizes to be awarded at the discretion and according to the judgment of a Committee, consisting of the President and Hon. Sec. of the Miners' Association, and some other gentlemen to be nominated by Mr. Basset. All communications on this subject must be addressed, in the first instance, to Mr. J. H. Collins, F.G.S., hon. sec. of the Miners' Association of Cornwall and Devon, 57, Lemon Street, Truro.

ACCORDING to information communicated to *Aftonbladet* from Christiania, the Norwegian vessel, which in the end of August met Nordenskjöld west of Novaya Zemlya, was the yacht *Elvire Dorothea*, belonging to J. Berger, in Hammerfest. The yacht has returned from the Arctic Sea to Hammerfest. Its master, Johan Alexandersen, states that the Sea of Kara was nearly free of ice, and that it cannot be doubted that Nordenskjöld will reach the goal of his journey, the River Obi.

M. LEVERRIER has announced to the French Academy that Mr. Hind, the superintendent of the *Nautical Almanack*, intends to employ his new Tables of Saturn as soon as they are printed. He reminded the Academy that this will be the sixth table constructed by him that the British Admiralty has introduced into the almanack, and he expressed his sense of the honour thus done him by the Admiralty.

AN interesting and very useful publication comes to us from Germany, under the title of "Die Fortschritte des Darwinismus," by J. W. Spengel (Cologne and Leipzig, E. W. Mayer). This is the second number of the publication, and originally appeared as a paper in Klein's *Revue der Naturwissenschaften*. The purpose of the brochure of eighty pages is to give a brief review of all the works and articles of importance bearing on Darwinism, either *pro* or *con*, published during 1873-4. A very large number of such works, in various languages, is noticed, and their bearing on the Darwinian hypothesis pointed out. The work will be found of great use to those who have not access or have not time to consult all the various publications bearing on the important theory, and will also serve as an excellent bibliography to those who wish to make a thorough study of the subject.

THE German Scientific and Medical Association was opened at Graz on the 17th inst. Lieut. Weyprecht, of the recent Austrian Arctic Expedition, made a speech deprecating all past Arctic expeditions as adventurous and valueless because they constituted an international rivalry that resulted only in giving names to some ice-bound islands. The speaker, amid general applause, expounded a new programme for making Arctic expeditions more fruitful for natural science, and to enable poorer countries to undertake such expeditions.

IN the American *Boston Medical and Surgical Journal* for July there is a paper by Dr. H. P. Bowditch, on the course of the nerve-fibres in the spinal cord. From his experiments the author demonstrates, in opposition to the results of many other investigators, that the channels of motor and sensitive impressions lie in the lateral, and not in the anterior and posterior columns of the cord.

THE International Congress of Physicians was opened at Brussels on Sunday by the King of the Belgians with great ceremony.

IN connection with the Science and Art Department, South Kensington, the following candidates have been successful in obtaining Royal Exhibitions of 50*l.* per annum each for three years, and free admission to the course of instruction at the following institutions:—1. The Royal School of Mines, Jermyn Street, London: John Gray, engineer; Frederick G. Mills, student; Thomas E. Holgate, farmer. 2. The Royal College of Science, Dublin: C. C. Hutchinson, engineer; Henry Hatfield, student; Thomas Whittaker, clerk.

PROF. FLOWER's important monograph on the structure and affinities of the Musk-deer (*Moschus moschiferus*) has just appeared in the new 3rd part of the Proceedings of the Zoological Society for this year.

WE commend to our readers a paper in Tuesday's *Daily News* on the scientific work of the *Valorous*, by a member of the expedition. Under somewhat trying circumstances much good work was done. Many new and valuable facts bearing upon the very important question of the geographical distribution of particular forms have been added to those already obtained by the *Porcupine* and *Challenger*.

IN a letter in the *Morning Post*, signed "W. S. M.," attention is drawn to the provision in the New Code of the Privy Council Committee of Education for instruction in cooking, house management, &c., in elementary schools, and a very happy suggestion is made. The writer can see no reason why some portions at least of the subject should not at once be introduced into all schools which are in connection with the Science and Art Department. He then shows how very large a number of students attend the classes for Animal Physiology, Organic and Inorganic Chemistry, and Heat, and says: "There is thus already given, though scattered over four subjects, much of the instruction which would belong properly to the special subject of 'Food and its Preparation.' To make the subject an efficient one, all that is needed is to select certain portions from the subjects already taught, 'Physiology,' 'Acoustics, Light, Heat,' 'Inorganic Chemistry,' 'Organic Chemistry;' to group these portions as one subject, and to add to it some additional instruction that is not at all more difficult than much that is already given." We commend "W. S. M.'s" suggestion, indeed the whole of his letter, to the notice of the South Kensington authorities.

THE Cryptogamic Society of Scotland will hold its first Annual Conference at Perth on September 29 and 30, and October 1, the president being Sir T. Moncreiffe, of Moncreiffe, Bart., President of the Perthshire Society of Natural Science, and the secretary, F. Buchanan White, M.D., F.L.S., editor of the *Scottish Naturalist*. The following is the programme of the meeting:—Wednesday, September 29, field-excursions to Moncreiffe, Dupplin, and Scone. Thursday, September 30, (1) Arrangement and examination of specimens; (2) Business meeting (reading of papers and communications, &c.); (3) Fungus dinner. Friday, October 1, show of fungi and other cryptogamic plants in the City Hall, Perth. All fungi, &c., intended for exhibition must be delivered (addressed to the care of the "Keeper of the City Hall, Perth") not later than 10 A.M. on Thursday, September 30. Ferns in pots must be delivered between 8 and 10 A.M. on Friday, October 1. Botanists (especially in distant localities) who purpose attending the conference are requested to give early intimation of their intention, in order to facilitate arrangements. Further information may be obtained on application to the general secretary, Dr. Buchanan White, Rannoch, Perthshire; or the local secretary, Mr. J. Young, C.E., Tay Street, Perth.

A FRENCH blacksmith has devised a perforated plate, put in rotation by clockwork, and intended to place behind the lock of a safe. The consequence is that the safe cannot be opened except at certain times during business hours, when there is no danger of any robber intruding into the offices.

THE patrons of the Lille Catholic University are trying to get an hospital placed at their disposal in order to start a school of medicine, and they have offered a sum of 150,000 francs to the administration of public hospitals in order to have a *clinique* of their own. The answer has not yet been given, but it is doubtful whether the requisition will be complied with.

THE death of M. Duchesne de Boulogne, one of the most celebrated practitioners who engaged themselves in studying medical electricity, took place on Saturday, Sept. 18. M. Duchesne de Boulogne was the author of several cleverly written books on the subject. His death will be felt as a loss by those who are organising the International Exhibition of Electricity, which is to take place only in 1877, having been postponed owing to the amount of work required to collect all the objects relating to that immense science.

THE admirable "Report on the Progress of the Iron and Steel Industries in Foreign Countries," by Mr. David Forbes, F.R.S., has been reprinted in a separate form in the *Journal* of the Iron and Steel Institute.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Kent; a Common Raccoon (*Procyon lotor*) from North America, presented by Mr. W. Binder; a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, presented by Mrs. Barton; an Egyptian Gazelle (*Gazella dorcas*) from Egypt, a Green Monkey (*Cercopithecus callitrichus*) from West Africa, a Brazilian Hangnest (*Icterus jamaicai*) from Brazil, a Sulphury Tyrant Bird (*Pitangus sulphuratus*), two Red-rumped Hangnests (*Cassicus hæmorrhous*), three Blue-bearded Jay. (*Cyanocorax cyanopogon*) from South America, deposited; a Getulian Ground Squirrel (*Xerus getulus*) from Morocco, six Houbara Bustards (*Houbara undulata*) from North Africa, purchased; a Wapiti Deer (*Cervus canadensis*), and a Reeves's Muntjac (*Cervulus reevesi*) born in the Gardens.

THE BRITISH ASSOCIATION

REPORTS.

Third Report on the Sub-Wealden Exploration.—Mr. W. Topley made a statement on this subject, embodying the chief points of the report drawn up by Mr. H. Willett and himself. Up to the year 1872 nothing was known as to the beds which lie below the Wealden strata in the south-east of England. The lowest beds exposed were those on the north and north-west of Battle, long worked for limestone. The age of these beds was doubtful, some geologists correlating them with the Purbecks of Dorsetshire, others regarding them as Wealden but of somewhat exceptional character. In 1872, when the Association met at Brighton, Mr. H. Willett proposed to commence a bore hole in these doubtful strata, with a twofold object: (1) to determine the order, thickness, and character of the Secondary rocks below the Weald; (2) to prove the Palæozoic rocks which were supposed to lie beneath at a depth which could be reached. Judging from what is known of the Secondary strata near Boulogne, and comparing them with those exposed in the middle of England, it was hoped that the Palæozoic rocks would be reached at a depth not greater than 1,700 feet from the surface. In August 1874 the boring had reached a depth of 1,030 feet, and was then delayed in consequence of an accident to the rods. This hole was ultimately abandoned, and a new boring was commenced in February 1875, which has been carried to a depth of 1,812 feet. At this point the work has been stopped, in consequence of great difficulties in keeping the hole clear, and it is not proposed to continue the boring further. From the surface down to 175 feet the strata are shales and impure limestones, with gypsum in the lower part. These beds are referred to Purbecks, and with them are now classed the lowest rocks exposed at the surface, formerly called the "Ashburnham Beds." From 175 to 257 feet the strata are chiefly sand and sandstones; these are held to represent the Portland Beds. Below 257 feet there is a great series of bituminous shales and clays, with occasional bands of cement stone and sandstone. Kimmeridge Clay fossils extend down to 1,656 feet at least, possibly lower; so that this formation is here at least 1,400 feet thick. The bottom beds of the boring, just reached, are oolitic in structure, and contain bands of hard limestone. To this extent, then, the Secondary rocks have been traversed, and their order and structure ascertained. A discovery of some commercial value has been made, for two companies are in existence to work the gypsum. One of these has been for some time in operation; a shaft has been sunk and the mineral is now being raised. Scarcely less important is the knowledge now attained that no supply of water can be got by deep wells or borings into the Sub-Wealden strata. As regards the Palæozoic rocks, the boring has not had the success that was anticipated. The Secondary strata have proved too thick, and there is little or no hope of reaching the older rocks here. A boring is now in progress at Cross Ness by the Metropolitan Board of Works; this will be carried through the gault, and may possibly throw some light on this question.

Report of the Committee on Erratic Blocks, by the Rev. H. W. Crosskey.—The Committee continue their record, without attempting the more ambitious task of connecting the facts they report with theories of the history of the Glacial epoch. It will be observed, however, (1) that the facts reported increase our knowledge of the area over which erratic blocks are distributed; (2) that the boulders are connected together in more definite groups, distinctly pointing to special centres of distribution; (3) that the possibilities are increasing of obtaining a more exact history of the periods into which the great Glacial epoch must be divided from the grouping and distribution of erratic blocks. Boulders and scratched stones are reported in South Devonshire. New Red Sandstone boulders occur on the left bank of the River Dart, at Waddeton, the largest measuring 6 × 3 feet, at elevations extending from 15 to 200 feet. Are they travelled masses? If so, whence did they come? When were they lodged where they now lie? What was the agent of transportation? The boulders may have been remnants of New Red beds which once covered the older formations now exclusively overlying the district; but the different levels at which they are found, the present configuration of the surface of the country, and the great weight of some of them, indicate the possibility of their having been transported by ice from some part of the district lying between Berry Head and Galmpton Common. At Englebourne scratched blocks occur of fine grained trap over an area having slate as its subsoil. Although the size of these boulders renders their mobility under the action of waves possible, yet the grooves upon them appear to indicate ice action with considerable distinctness. A group of small boulders of mountain limestone have been found in the north-east of Hertfordshire, 100 miles from their source in Derbyshire. In Nottinghamshire remarkable boulders have been exposed by a new railway cutting, many of them finely striated, which have been described for the Committee by the Rev. A. Irving. The boulders are of lias, millstone grit, and carboniferous limestone. The boulders of lias limestone are derived from the liassic strata of the immediate neighbourhood upon which they chiefly lie. The nearest millstone grit is formed at Castle Donnington and Stanton-by-Dale in Derbyshire, on opposite sides of the Trent Valley; the former place twelve miles south of west, the latter twelve miles north of west from the deposits in which they occur. The nearest carboniferous limestone corresponding to that of the boulders is found at Ticknall in Derbyshire, about eighteen miles distant south of west. The height of the group above the sea is about 200 feet. The extent of the boulder clay and deposit is at least several square miles. In the cutting between Plumtree and Stanton the boulders are largest and most numerous, and are mingled with an immense number of quartzite pebbles, the whole being compactly bound together. In Leicestershire, there is no doubt, Charnwood Forest was a centre of distribution by ice, of blocks of all sizes. The position of various boulders is reported seven miles from their source, together with a block of peculiar millstone grit, at Hoby, near Melton, which must have come from Durham or Northumberland. In Worcestershire (Bromsgrove district) ninety-three boulders have been examined, many of them of considerable size, consisting chiefly of varieties of felspathic rock. It is impossible as yet to generalise on their distribution, but it is noticeable that no specimens of granite have been observed in this district, although they occur so abundantly around Wolverhampton. A list is given of the size and position of the principal erratic blocks, which are rapidly being destroyed. The group of felspathic boulders extends through Northfield and King's Norton to Birmingham. Isolated, and in many cases striated, boulders are reported in the neighbourhood of Liverpool, including blocks of greenstone, syenite, felspathic ash, &c. On the north-west of Bradford a few boulders are reported, similar to the rocks at Scaw Fell, Cumberland, and containing small garnets. The destruction of erratic rocks is going on so rapidly through the country that the Committee earnestly request that reports may be forwarded to them of their occurrence. Some are being buried to get them out of the way of the farmers; others are built into walls, made the foundations of houses, or blasted into fragments. In some cases they constitute the foundations of church towers. A timely record will preserve many facts of large import and assistance in the discussion of problems connected with the centres of ice action, the range of the land ice, the courses of icebergs, and the existence of interglacial epochs.

SECTIONAL PROCEEDINGS

SECTION A—MATHEMATICS AND PHYSICS

On the Measurement of Wave Motion, by Prof. Frederick Guthrie.—The rate of progression of a wave in a liquid of infinite depth and extent depends upon the wave length; scarcely at all upon its height, and not at all upon either its breadth or the density of the liquid. The measurement of rate of wave-progression in open water is difficult and at best inaccurate. Natural waves generated and supported or restrained by wind have abnormal rates of travelling. Artificial waves in ponds degenerate rapidly in height and increase in wave length, and so in wave progress-rate. The time required by a wave generated in the middle of a pond in reaching the edge, is dependent on its mean wave length. Perhaps after reflexion from the edge the conditions are sequentially reversed, and the time occupied in returning is equal to that of departure. Perhaps not. I think not, because the increase of wave length (and therefore of wave progress) is a function of the height. Be this as it may, many sources of error are got rid of by using troughs of limited surface and indefinitely great depth, by causing the original and reflected wave so to interfere as to produce one or more nodes; and instead of measuring the time required for the crest of a wave to travel in a straight line over a given distance, by measuring the number of times the crest of the wave system reappears in the same place in a given time; in other words, by transferring to liquid waves the method used to measure the rate of sound in solid bodies. As far as the method is trustworthy we get by means of a trough whose diameter is one or two feet, a more accurate method of measuring the rate of wave progress than by an experiment in an ideal pond a mile across.

Experiment shows that if a concentric binodal wave system be generated in a cylindrical trough of water of more than a certain depth (say half its diameter), the following conditions hold good. A nodal ring is formed at one-sixth of the diameter from the circumference. The amplitude at the centre is double that at the circumference unless the disturbance is very great. The rate of undulation—that is, the number of times in a given time that the crest appears in the centre—does not depend sensibly upon the amplitude, nor upon the temperature, nor upon the density of the liquid. It depends almost wholly upon the wave length of the waves formed—that is, upon the diameter of the trough—and is identical with the number of beats of a pendulum whose length is equal to the radius of the trough. Hence the rate of undulation varies inversely as the square root of the trough radius or diameter. This confirms the assertion that the rate of wave-progress varies directly as the square root of the wave length; because the rate of recurrence must vary as the rate of progression divided by the path.

Experiment shows that a wave of 1 meter wave length would travel 83·07 meters in one minute if it did not alter its wave length, and moved automatically. A cylindrical trough of water more than, say, 500 millimeters deep and 1·988 meters in diameter, will, in the latitude of London, undulate in seconds, and will remain isochronous with the London seconds' pendulum wherever they travel together.

In rectangular troughs, the wave progress is hindered. The rates of recurrence of phase in rectangular troughs are slower than in circular troughs when the wave lengths are the same; and this difference is greater when the wave length is greater. Both circular and rectangular troughs accept mononodal undulation. The rate of progress between parallel walls of a wave 1 meter long is found to be 74·7, and this is independent of the distance of the walls apart. The mononodal undulations in circular and rectangular troughs have also been examined.

The comparative empirical mean constants in minute-millimeters are—

	Circular.			Rectangular.	
	Mononodal. (a)	Binodal. (b)		Mononodal. (c)	Binodal. (d)
$n\sqrt{d}=$	1762·56	2613·24;	$n\sqrt{e}=$	1594·16	2360·04.

where d is the diameter of the circular trough and e the length of the rectangular one.

The water in a circular trough can also undulate with two perpendicular rectilinear nodes.

Taking the same trough, it is found that the number of undulations per minute, when (a) the circular binodal, (b) the mononodal, and (c) the binodal rectangular systems were established, were—

$$a = 106{\cdot}9 \qquad b = 71{\cdot}6 \qquad c = 94.$$

These numbers a and c agree well in ratio with those of a circular elastic plate in similar vibration. The details of this communication were laid before the Physical Society in June last. They will, I hope, appear in part in the *Philosophical Magazine* for October.

SECTION B—Chemical Science

Prof. Cayley read a paper *On the Analytical Forms called Trees, with application to the theory of chemical combinations*, before a good audience composed to a considerable extent of mathematicians.

The author in commencing stated that the subject he was about to consider was more mathematical than chemical, but as the results bore considerably upon the latter subject he had introduced it in this Section. The problem to be solved was to find the theoretic number of the hydrocarbons C_nH_{2n+2}.

The only assumptions are that an atom of hydrogen can link itself to one other atom, and an atom of carbon to four other atoms. A combination of n carbon atoms can then link itself on to 2_n+_2 hydrogen atoms at most, but this number is only attained when the carbon atoms are linked together without cycles, or so as to form a "tree": given the tree, the hydrogen atoms can be linked on in one way only, and the question thus is to find the number of trees which can be formed with n carbon atoms. The atoms, or dots representing them, are termed "knots," the lines joining two knots are termed "branches"—the trees in question are such that from each knot there proceed at most four branches; but this limitation is in the first instance disregarded. A tree may be considered as springing from any one of its knots as its root, and trees which are chemically the same thus present themselves under different forms. For the treatment of the chemically distinct forms it is necessary to introduce the notions of a "centre" and a "bicentre" (due to Prof. Sylvester); and the question is reduced to that of finding the number of the *central* trees with n knots: this is solved by the method of generating functions, viz., the number of the central trees of altitude N is given by a series of the form—

$$t\,x^{N+1} + \{t, t^2\}x^{N+2} + \{t, t^2, t^3\}x^{N+2} +, \&c.$$

where the numerical coefficient of any term $t^a x^{N+\beta}$ shows the number of trees of a main branches and $N+\beta$ knots. The final result as regards the carbon-trees, or say the hydrocarbons C_nH_{2n+2} is given by the following table:—

$n =$	1,	2,	3,	4,	5,	6,	7,	8,	9,	10,	11,	12,	13
Central	1,	0,	1,	1,	2,	2,	6,	9,	20,	37,	86,	183,	419
Bi-central	0,	1,	0,	1,	1,	3,	3,	9,	15,	38,	73,	174,	380
Total	1,	1,	1,	2,	3,	5,	9,	18,	35,	75,	159,	357,	799

so that theoretically for the body whose formula is $C_{13}H_{28}$ there exist 799 isomeric bodies.

It is worthy of remark that the mathematical theory agrees with experiments for the first five bodies, thus affording strong confirmation of the truth of the remainder.

The Professor also drew attention to the fact that any number is sometimes rather more and sometimes rather less than double the preceding number.

Prof. Armstrong suggested that probably a large number of these isomers would be unstable, illustrating his meaning by the two isomeric di-nitro-phenols, one whose melting-point was 76° C. readily passing into the other whose melting-point was 116° C., which was objected to on the ground that it was not fair to compare the action of bodies as complicated as the phenols with the simple hydrocarbons.

Prof. Clifford also made some remarks on the bodies represented by $C_nH_{2n+2-2x}$, and stated that it would be found that x represented the number of cycles that would occur in the trees.

Mr. P. Braham made some remarks on some further experiments on *Crystallisation of Metals by Electricity*, in which he stated that he had placed the positive and negative electrodes of a battery in a vessel containing a mixed solution of copper and zinc, and that with terminals of copper he obtained a dull crystallisation proceeding from the negative pole of mixed crystals of copper and zinc, and beyond this, crystals of copper alone. With terminals of zinc he got a mixture of crystals as before, and in front of these, crystals of zinc alone. But if terminals of brass (a compound of zinc and copper) are used, there is a dull crystallisation of zinc across the field. He also observed that with zinc terminals, by increasing the battery power, the crystallisation is broken up; but not so when the terminals are copper or brass, but then the crystallisation extends above and beyond the positive pole.

Mr. Gatehouse read a paper *On Silver Nitrite*, giving the results of some investigations into the causes of what is termed by photographers "woolliness" in their negative baths.

The five methods given of preparing the nitrite were as follows:—

1. By mixing solutions of potassium nitrite and silver nitrate.
2. By sensitising a collodion film and evaporating to dryness a mixture of nitrite and nitrate is obtained.
3. By fusing silver nitrate with organic matter.
4. By electrolysis of silver nitrate with platinum electrode.
5. By means of metals placed in neutral solution of silver nitrate.

By this last method he found that metals which produced reduction, viz., K, Na, Bi, Hg, As, Th, did not produce nitrite, but those which did not produce reduction, viz., Fe, Ni, Co, Mg, Zn, Cu, Pb, Sn, Sb, did produce nitrite. The former, it was observed, have an uneven equivalency, and the latter an even equivalency, with the exception of Hg and Sb, the latter of which may, like Fe, be tetratomic. The physical forms of the crystals were observed to vary from modular masses to filiform crystals.

Mr. A. H. Allen, in making some remarks *On a Method of effecting the Solution of difficultly-soluble Substances*, stated that he had found that many so-called insoluble substances could, when heated with fuming hydrochloric acid in sealed combustion tubes, be either completely dissolved or decomposed with separation of silica. In some cases where hydrochloric acid failed, sulphuric acid succeeded. The heating of the tubes was generally done by means of a water bath, but for some substances a chloride of calcium bath must be used.

Mr. J. C. Melliss read an account of the method of purification of a river by precipitation, at present adopted at Coventry. He stated that 2,000,000 gallons of sewage liquor, contaminated by dye, refuse, &c., were daily passed through these works and completely purified. The process employed is briefly the following:—The water of the river, after being mechanically strained from solid impurities, is passed into tanks, where it is mixed with sulphate of alumina; it is then passed to a second set of tanks where it is mixed with milk of lime, and thence on to a field or filter bed 4½ acres in extent, which ejects 80,000 gallons of water per hour, pure enough for fish to live in. The greatest difficulty to be contended with was the freeing of the precipitated matter from the water, of which it contained 80 per cent.; this quantity, however, was considerably reduced by means of mechanical appliances, which reduced the water to such a percentage that it could either be dried (and so rendered portable) by heat, or by mixing it with some substance which increased its manurial value. In conclusion, the author stated that the primary object was to secure sanitary rather than commercial success, and that this certainly had been achieved at a cost of about sixpence per head per annum for a population of 40,000.—Some discussion ensued as to the relative merits of the method of irrigation and the method just described.—In reply, Mr. Melliss said that he was not prepared to say that the Coventry method was the best in all localities; the physical characteristics of the land in neighbourhood must always be taken into account, as of course it would make a great difference whether the soil consisted of clay or of sand.

Prof. Debus read a paper *On the chemical theory of Gunpowder*, in which he stated that nothing illustrated in so striking a manner the molecular changes produced by chemical action as the explosion of gunpowder. He said that some years ago the eminent French chemist Berthelot showed that if CO_2 be passed into a mixture of BaO and CaO in insufficient quantity to precipitate the whole of the barium and calcium as carbonate, then neither is the whole of the barium precipitated nor the whole of the calcium, but they are precipitated in a certain definite proportion, which is a multiple of their molecular weights. Hence, in general, if a mixture of the salts A and B be decomposed by some other substance, C, in insufficient quantity to decompose the whole of both, then the bodies formed will be $AC_1 + BC_2$ where $C_1 + C_2 = C$; and moreover, if the quantity of B is doubled or trebled, &c., the quantity C_2 will be increased in a definite proportion. After making some further remarks of a like nature on the decomposition of a mixture of $BaCl_2$ and $CaCl_2$ by CO_2, and also of the explosion of mixture of H and CO with an insufficient supply of oxygen (as investigated by Bunsen), the Professor went on to show that the same arguments might be applied to the explosion of gunpowder which was a mixture of carbon, sulphur, and nitrate of potash.

He then placed upon the black board the result of one of a large number of analyses of one grain of powder.

Compound.	Grain.	
(1) K_2CO_3	·3098	·00224
(2) $K_2S_2O_3$	·0338	·000177
(3) K_2SO_4	·0658	·000378
(4) K_2S	·1055	·00096
(5) CO	·0473	·00170
(6) CO_2	·2770	·0629 *

In addition to these were also formed in *small* quantities the following: potassium sulphocyanide, potassium nitrate, ammonium carbonate, sulphur, sulphuretted hydrogen, marsh gas, hydrogen, and nitrogen, most of which appear to have been the result of gaseous impurities in the carbon.

Referring to the table it will be seen that by adding up the total molecular value of the sulphur salts we get ·00151, which bears to the molecular weight of potassium carbonate (·00224) the ratio 2 : 3 nearly. Hence it is inferred that at the first moment of combustion the potassium in the saltpetre divides itself into five parts, two of which go to unite with the sulphur, and three to form the carbonate. Again, it will be seen that the carbonic oxide bears to the potassium very nearly the simple ratio 3 : 4. The CO_2 must have been formed in more than one reaction, because it does not give any simple molecular ratio. The conclusions thus arrived at are, that in the first moment of explosion the sulphur existed either as sulphite or as sulphate, and that the carbonic oxide must have been formed simultaneously with the potassium carbonate. The equations of the decomposition of gunpowder would then be the following:—

(1) $24KNO_3 + C_{35} + O = 12K_2CO_3 + 9CO_3 + 14CO_2 + 12N_2$
(2) $16KNO_3 + C_8 + S_8 = 8K_2SO_4 + 8CO_2 + 8N_2$
(3) $6K_2SO_4 + C_{11} + S + O = 5K_2S + K_2S_2O_3 + 11CO_2$.

The first two reactions taking place simultaneously.

* The third column is the number found by dividing each quantity by the corresponding molecular weight.

Prof. Thorpe, in giving some account of a *New Compound of Fluorine and Phosphorus*, said that having had some occasion recently to make a considerable quantity of the terfluoride of arsenic, by heating calcium fluoride with arsenious acid in the presence of Nordhausen sulphuric acid, he was induced to study the behaviour of this body with various other substances. When this terfluoride of arsenic is dropped into a solution of the pentachloride of phosphorus, such an immense amount of heat is evolved that it is necessary to keep the vessel surrounded with a freezing mixture, and dense white fumes are given off, while only chloride of arsenic remains in the solution. This gas is decomposed by water, but may easily be collected over dry mercury, in which condition it may be kept, but after some time the glass is observed to become dim. The specific gravity of the gas answers to the formula PF_5, and its molecular weight is 63. It acts readily upon alcohol, but the substance formed quickly corrodes glass. It is believed that it will be found to be a condensable gas under a pressure of six or seven atmospheres. It is not impossible that when decomposed by the electric spark it may give *fluorine*. It is remarkable as the only known pentatomic compound of phosphorus.

Mr. B. J. Fairley, F.R.S.E., read a paper *On New Solvents for Gold, Silver, Platinum, &c., with explanation of so-called Catalytic Action of these Metals and their Salts on Hydrogen Dioxide*, in which he stated that it was perfectly easy to dissolve silver in dilute acids, as acetic, sulphuric, or hydrochloric, provided hydrogen dioxide were present in the solution, and that if under the same circumstances the silver were dissolved in nitric acid no lower oxides were evolved. Repeating the experiments with gold, it was found that acetic and nitric acids scarcely dissolved it at all, but hydrochloric acid readily, and without the evolution of free chlorine. Some remarks were also made on the great liberation of heat observed when two unstable compounds of oxygen react upon one another so as to produce more stable compounds, especially with reference to the heat evolved during the decomposition of ozone and hydrogen dioxide, the author stating that this great *heat* must correspond to a great *force* of union.

The same gentleman also made some remarks *On the Use of Potassium Dichromate in Grove's and Bunsen's Batteries to ensure constancy*, in which he stated that he had used a small quantity of that substance dissolved in the nitric acid, and had found that the battery remained constant so long as any chromic acid remained to be reduced, and that no red fumes appeared.

Two other papers were also communicated by the same author: (1) *On a New Process for the separation of Lead, Silver, and Mercury (Mercurous) Salts*; (2) *On a Process for the Preparation of Periodates, with their application as a Test for Iodine and Sodium.*

Dr. J. H. Gladstone read a paper *On the relation of the Acids and Bases in a mixture of Salts to the original manner of combination.* In a former set of experiments the author had shown that if a molecule of copper nitrate and a molecule of potassium sulphate be dissolved in any quantity of water, and two molecules of potassium nitrate with one molecule of copper sulphate be dissolved in an equal quantity of water, then the colour produced is the same: and similarly for other sets of salts. The author, however, thought that the colours of these mixtures being comparatively faint, it would be better to try mixtures of colourless salts, and add to these mixtures some substance such as ferric sulpho-cyanide, ferric mæconate, or bromide of gold, whose colour is easily reduced. Accordingly, he mixed together potassium sulphate and magnesium nitrate, and the corresponding salts potassium nitrate and magnesium sulphate; also acetate of potassium and nitrate of lead, and the corresponding salts, &c.; in every case these were found to reduce the colour of ferric sulpho-cyanide equally. All the experiments united to confirm the supposition that the effect of a mixture does not depend upon the position of the acids and bases in it, so long as the proportions of each remain the same.

Dr. Russell asked if the amount of colour would indicate a small change in the nitrate, and also if the element *time* had been taken into the experiments.

Dr. Tilden preferred the old method, on the ground that by adding a reagent new conditions are introduced.

In reply, Dr. Gladstone said that the ferric sulpho-cyanide was much more delicate than the solutions of copper salts.

Dr. J. H. Gladstone read two notes, *On the Copper-Zinc Couple*, by himself and Mr. Alfred Tribe. In the first he showed that whereas a piece of zinc in dilute sulphuric acid (3½ in 1,000 parts of water) gave off seven volumes of hydrogen in one hour, the same piece of zinc, when covered with spongy copper, gave off eighty volumes in one hour, which showed an elevenfold increase for an addition of the negative element of only 0·11 per cent. In the second note he showed that if a quantity of arsenical zinc foil was "coupled," washed and heated with water, and two litres of hydrogen evolved therefrom were passed through a tube heated to redness, not a trace of arsenic was observed; but when a portion of the same arsenical zinc was treated with dilute sulphuric acid, and two litres of hydrogen evolved by the action were passed through a heated tube as before, ·0019 gramme of arsenic was deposited in the cool part of the tube. Arsenical zinc, when covered with spongy copper and acted upon with dilute sulphuric acid, also gave arseniuretted hydrogen. This appears to show that it is not the copper, but the inability of the arsenic to get into solution when hydrogen is made from water and the "couple," which is confirmed by adding an aqueous solution of arsenic to the same couple, when the mirror immediately appears.

The same gentleman also read a paper by the same authors, in which it was shown that if aluminium be "coupled" with more negative metals, such as copper or platinum, then at the ordinary temperature of the air in the latter case, 4 c. c. of hydrogen are evolved in twenty-two hours, and if the temperature be raised to 100° C., in the first six hours 484 c. c. are evolved. Aluminium alone, according to Deville, only decomposes water at a white heat.

The President read a paper *On an apparatus for estimating Carbon Bisulphide in Coal Gas.* The principle upon which the success of the method depends is the following:—When carbon bisulphide is heated in the presence of hydrogen, sulphuretted hydrogen is formed.

The apparatus consists of a flask filled with pebbles and asbestos (to expose a large surface to the action of heat), and surrounded by fire-clay cylinders, in which gas is kept burning. This flask is connected through a solution of lead with an aspirator. There are other connections also by means of which gas from the source requiring to be tested circulates through the flask and is burnt. When the flask has been heated for about twenty-four hours continuously (to expel all moisture), a measured quantity of water is drawn off from the aspirator, which causes the same volume of gas to bubble through the lead solution, and on account of the presence of sulphuretted hydrogen to produce a decolorisation of the lead solution. A similar vessel containing the same quantity of lead solution and a *known* quantity of sulphuretted hydrogen is placed beside it, the gas being allowed to bubble through the first until the colour is judged to be equally intense; the amount of sulphuretted hydrogen in a known volume of the gas is thus found, and hence the amount of carbon bisulphide. Having once got the apparatus started, gases from several different sources may be tested.

Prof. A. Oppenheim made some remarks on *oxyuvitic acid*, which he stated belonged to the aromatic series, and said he was able to show that it could be prepared from its elements, thus making the fifth of that series which could be prepared by synthesis. The formula of the acid he showed to be

$$C_6H_2\left\{\begin{array}{l}CH_3\\OH\\COOH\\COOH\end{array}\right.$$ thus making it a derivative of benzole. It gives a reddish brown colour with ferric chloride in the presence of alcohol. If it is slightly heated it changes its composition to

$$C_6H_3\left\{\begin{array}{l}CH_3\\OH\\COOH\end{array}\right.$$ which gives a violet colour under the same circumstance: if it is still further heated it is converted into

$$C_6H_4\left\{\begin{array}{l}CH_3\\OH\end{array}\right.$$ or cresole.

The Professor also made some remarks on the derivatives of mercaptan, which were founded on some researches of Dr. Williamson on the action of chloroform.

Mr. Chas. T. Kingzett read a paper *On the Oxidation of Essential Oils*, which he observed was a continuation of papers which had previously been communicated to the Chemical Society. The object of the paper was to give some results on the limited oxidation (by air) of terpenes of the general formula $C_{10}H_{16}$, certain terpenes of the formula $C_{15}H_{24}$, and cymene, $C_{10}H_{14}$. The terpenes experimented upon were hesperidine, myristicene (obtained in three different ways from oil of nutmeg), wormwood, all of which gave on atmospheric oxidation, peroxide of hydrogen and acetic acid. Citronella and Ylang Ylang, clove-terpene ($C_{15}H_{24}$), were found to develop no peroxide of hydrogen. Cymene obtained from three sources and exposed to atmospheric oxidation was also found to develop peroxide of hydrogen. These researches prove that in terpenes of the formula $C_{15}H_{24}$ the carbon exists in an allotropic form.

SECTION D—BIOLOGY

Department of Anthropology.

One day was chiefly occupied by a valuable series of papers on the population of the Indian region. A combined discussion on the three papers now to be noticed followed their reading. The first paper was by Sir Walter Elliot, *On the original localities of races forming the present population of India.* After some preliminary remarks, he said that the circumstance of colour was one of the most observable signs of difference of race, and the very word for the Aryan institution of caste was *varanum*, or colour, they having doubtless introduced it to distinguish themselves from the Dasyns or alien peoples with whom they came in contact on crossing the Indus. The author detailed the different colours or races now inhabiting India, and went on to remark that it is now generally admitted that the centre of dispersion from which all the peoples of the earth had migrated was Central Asia. The first great wave that surmounted the Himalayan barrier, at a time when the earth's surface was in a different condition from what it is now, could no longer be traced as a separate and distinct people. Remnants of the primeval movement were now only to be found amongst the most degraded denizens of the hills and forests, and probably in the despised slave population. The great Dravidian migration must have been made much later in time. It was probably not a simultaneous movement, but consisted of successive swarms, which would account for the existence of well-defined groups among them, which had preserved their characteristics unchanged to the present day.

But the normal representatives of the race were to be found in the mountaineers of Central India, where, protected by regions of deadly malaria encircling their highland territory, they have for ages bid defiance to hostile aggression, and preserved their habits and independence unchanged. The ground on which so many at first sight heterogeneous races were united under the title of Dravidian was mainly community of language, but that test was not infallible. A better link was furnished by similarity of form, features, colour, and structural coincidence. He maintained that the characters of Prof. Huxley's Australioid type could be traced among the classes of Dravidians, modified as was to be expected, among those most exposed to external influences, but still always apparent to a practised eye. There was nothing to show by what routes the first settlers arrived. Their advance was probably a slow and gradual percolation from different parts of the north through the mountain barrier that cuts off India from the rest of Asia. The migratory instincts or necessities of the people of Central Asia exerted themselves in all directions. Of the exact seat of the brown-skinned, wavy-haired Australioids, they had no definite knowledge. But the Mongols and Mandchurians sent off successive hordes to the south-east, whence in time the teeming population of China sought an eastern direction. Those people were thus brought into contact with tribes already settled there from a more westerly quarter. Thus the inhabitants of Siam, Burmah, and the Malayan Peninsula, spoke a monosyllabic language, but wrote it in a Dravidian character, and Mr. Hodgson found the scattered tribes around Nepaul partaking of the same mixed characters, both with regard to race and language.

Mr. Hyde Clarke's paper *On the Himalayan Origin of the Magyar and Fin Languages*, attempted to prove his theory by facts of analogy in the languages themselves, and by inferences from facts of history. He found that the affinities of Magyar and Fin were strongest for the languages of East Nepaul.

Mr. Bertram Hartshorne, of the Ceylon Civil Service, read a paper on the interesting *Weddas of Ceylon*, who still depend for their means of subsistence upon their bows and arrows, and pass their lives in the vast forests of Ceylon without any dwelling-houses or system of cultivation. There is an entire absence of any flint or stone implements among them, and their state of barbarism is indicated by the practice of producing fire by means of rubbing two sticks together, as well as by their habitual disregard of any sort of ablution. Their intellectual capacity is very slight; they are quite unable to count, or to discriminate between the colours; but while their moral notions lead them to regard theft or lying as an inconceivable wrong, they are devoid of any sentiment of religion except in so far as that may be inferred from their practice of offering a sacrifice to the spirit of one of their fellows immediately after his decease, their idea of a future state being that they become devils after death. They never laugh, and they are very noteworthy as being the only savage race in existence speaking an Aryan language. Their vocabulary consists largely of words derived directly from the Singhalese; others indicated an affinity with Pali or Sanskrit, whilst there remained a considerable residue of doubtful origin. There was an absence of any distinctly Dravidian element.

In the discussion on these papers, Prof. Rolleston said that the ethnology and languages of Hindostan were now in pretty much the same state of fusion as those of Great Britain. Since the writings of Sir George Campbell and others, and the excellent publications of the Indian Government, he had arrived at the conclusion that the Australioid and not the Mongolian type was that which formed the substratum all through the outcast tribes of India: this accorded also with the probabilities of evolution. He believed that the earliest races of mankind were eminently Australioid, with long and narrow heads. With regard to the Weddas, it was a most interesting question whether they were really a degraded outcast Sanskrit population. Max Müller was of that opinion; and their possession of the bow and arrow, which no Australioid ever had, tended in that direction. Their skulls were not Australioid.—Sir George Campbell did not know that there was any authentic case of degradation of a race. In this instance the *primâ facie* inference seemed to him to be that the Weddas were an aboriginal race. Very small tribes which had been reduced in numbers easily changed their language under the influence of a more powerful surrounding people. From the photographs of the Weddas he pronounced their absolute identity in feature with many of the barbarous aboriginal tribes of India which he had seen, and which were distinctly non-Aryan. The use of the bow was universally known among the aboriginal races of India, which had the same notions about witchcraft, &c. as the Weddas. He asked for information as to their strength in the left arm, which Mr. Hartshorne had mentioned, for he had always supposed that the use of the bow called forth strength in the right arm.—Mr. Hartshorne said that in his experience of shooting with the bow, he had found that the great tension in pulling the bow was on the muscles of the left fore-arm. He was therefore prepared to find that the Weddas were stronger in the left arm, and it was so.—Sir Walter Elliot agreed with Sir G. Campbell as to the aboriginal character of the Weddas, but believed in the possibility of great degradation.—Mr. Hyde Clarke said they had all the appearance of being an aboriginal people. Their speaking an Aryan language was no decisive reason for calling them Aryans.

Dr. Leitner gave a graphic summary of the results of his travels and researches in the Central Asian region to which he has given the name Dardistan. He gave the following as the chief results of his investigations:—"First, we have ascertained the existence of a number of languages—one of which, Chilasi, the object of my mission, is a mere rude dialect—which were spoken at or before the time that Sanskrit became the 'perfect' language, for no one who can speak any of the derivative languages of India can class the bulk of the Dard languages among them. Secondly, the legends and traditions of the Dards show a more European tone and form than anything we find in India. Thirdly, by the adoption of the term Dardistan for the countries between Kabul, Kashmir, and Badakshan, we are driven to compare a number of races which offer certain analogies, and which may have a certain history in common since the time of Alexander the Great's invasion of India. Fourthly, our Government now know accurately what they certainly did not know before 1866, the modern history of the countries bordering on Kashmir." He found that the dialects in this district, which were in a highly inflexional state, had been preserved from deterioration by isolation and other causes. He had very little doubt that Dardistan was the first halting-place of the Aryan migration to India; the second being Kashmir. There was as great a difference among some of their dialects as between French and Italian. They had songs, legends, and fables of superior character, which he had carefully taken down and would publish. Among the evidences of their high state of civilisation were the respect shown to the female sex, and the liberty and responsibility accorded to them; their love and charity to animals; and the charm and beauty of their legends. They called themselves the brethren of the Europeans. Associated with them was a race of predatory kidnappers, very similar to them, but speaking a somewhat different language. He had found a great quantity of art products, especially sculptures, which clearly indicated a great influence of Greece upon them in very early times, probably through the existence of the Bactrian kingdom. There was no trace of the later and more extravagant influences of Buddhism, but scenes essentially Buddhistic and Asian were treated after the Greek manner, and very much with the Greek success. Expression attained a high level in these works.

Prof. Rolleston read a paper *On the Applicability of Historical Evidence to Ethnological Inquiries*, in which he showed the danger of drawing conclusions from isolated expressions of historians unless they were of the first class, such as Cæsar and Tacitus. He quoted modern examples of carelessness and inaccuracy in this respect. He referred especially to the Cimbri, who were dealt with in the next paper, and expressed his inability, from any historical investigation, to come to a satisfactory conclusion as to who they were.

Prof. Rawlinson's paper *On the Ethnography of the Cimbri* was in favour of the Celtic theory of their ethnological character. He said that in favour of the theory that they were Germans the following considerations were urged:—The supposed etymology of their names; their geographic position before they began their wanderings in Jutland and between the Rhine and the Elbe; their close alliance with the Teutons, whom all allowed to be Germans; their physical characteristics, blue eyes and flaxen hair; some points of their manners and customs, especially the fact that their armies were accompanied to battle and directed by priestesses rather than priests; and, lastly, the statements of Julius Cæsar, Strabo, Pliny the elder, and Tacitus, who include the Cimbri in their lists of German nations. The advocates of the Celtic theory relied chiefly on five arguments: (1) the name Cimbri, which they identified with the term Cymry or Cymraeg, which was still the native name of the Welsh; (2) the almost unanimous authority of the Greek and Roman writers, excepting Julius Cæsar; (3) the individual names of Cimbri, which were Celtic; (4) the fact that the Romans employed Celts as spies to bring them intelligence of the designs of the enemy during the Cimbric war; (5) the manners and customs of the people, which were held to be far more Celtic than German. They also joined issue on the argument from the physical characteristics of the race, which they held to be, according to the description given, at least as near the Celtic as the German type. Prof. Rawlinson then proceeded to examine the various arguments, holding that the balance was in favour of the Celtic origin, though it was a point open to dispute, and unless fresh data should be obtained, which seemed very unlikely, would always remain among the vexed questions which would divide ethnologists.—Dr. E. A. Freeman dissented from Prof. Rawlinson's conclusions, holding strongly to the opposite theory. He especially censured his rejection of the evidence of Julius Cæsar and Tacitus.

The ethnology of New Zealand and Polynesia received much attention owing to the presence of two distinguished authorities, the Rev. Wyatt Gill, from the Hervey Islands, and Dr. Hector, of the New Zealand Geological Survey. The connection between the origin of the Maories and the Polynesians was brought out in a series of papers followed by a valuable discussion. Mr. W. S. Vaux, in a paper *On the probable origin of the Maori race*, concluded that the Maories were the descendants of the great colonising race of yellow men who originally migrated from Central Asia. The Rev. W. Gill then read a paper *On the origin of the South Sea Islanders*. Mr. Gill said that Mr. A. R. Wallace, in his "Malay Archipelago," has advanced the theory that the Polynesians are descended from a race which once overspread a vast submerged southern continent. As the land gradually sank, a few of the aborigines may have escaped to the tops of the loftiest mountains, around which subsequently coral reefs were found. Admitting that Polynesia is pre-eminently an area of subsidence, and its great widespread groups of coral reefs may mark out the positions of former continents, Mr. Gill believed that Mr. Wallace's reference was unwarranted. (1) Supposing that human beings inhabited this great southern continent at the period of the subsidence, and that a remnant escaped, it is not probable human life could have been sustained on the tops of these mountains for any considerable time, owing to the want of food and water. (2) The theory is utterly opposed to the native accounts of their own origin, which all point to the north-west. (3) The spread of the race can easily be accounted for on the basis of historical facts. In 1862 he saw on Manuâ, the easternmost island of the Samoan group, a small boat which had accidentally drifted from Moorea, a distance of 1,250 miles, and no life was lost. A few months later on in the same year Elikana and his friends drifted in a canoe from Manihiki to Nukurairae, in the Ellice group, lying N. W. of

Samoa, a distance of 1,360 miles. Half of the party on board perished from want of food and water. In both these instances the drifting was from east to west, before the trade winds. A far more remarkable event occurred in Jan. 1858, during the prevalence of the violent easterly winds, when a numerous family of adult natives drifted from Fakaofo, in the Union group, north of Samoa, to an uninhabited spot known as Nassau Island; thence to Palmerston's Island; and finally to Maugaia, where Mr. Gill lived; altogether a distance of more than 1,200 miles in a south-easterly direction. (4) The colour, hair, general physiognomy, habits, character, and especially the language, of the Polynesians clearly indicate a Malay origin. This could not be accidental. Mr. Gill's impression was that long ages ago the progenitors of the present race entered the Pacific from the S.E. fork of New Guinea, but were driven eastward by the fierce Negrito race. The greatest distance from land to land, as they pressed eastward, would be from Samoa to the Hervey group, about 700 miles, which had been successfully performed by natives in their fragile barks under Mr. Gill's own observation.

In the subsequent discussion Prof. Rolleston expressed his opinion that there was little difference between Papuans and Australoids; the superficial differences were outweighed by great radical points of resemblance. He referred to the Rev. S. J. Whitmee's paper in the *Contemporary Review* for February 1873 as of the highest value on this question of the origin of the races of the Polynesian islands. This opinion was diametrically opposed to Mr. Wallace's.—Dr. Hector described the three chief race-types among the Maories. The first was rarely met with except in the extreme south; it was of the same type as the aborigines of the Chatham Islands, with a distinct dialect, only comprehensible by old Maories. They had a sloping forehead and strong muscular ridges on their skulls, which were very distinct from the great majority of Maori skulls. The other two types were now pretty well intermixed. One was more common in the northern extremity of the Northern Island, having yellow shock hair and high cheek-bones. The third was the ordinary Maori. He mentioned the fact that the Maories had a much better knowledge of the natural history of their country than any people he had ever heard of. The older Maories had noticed and had distinct names for nearly all their plants, not merely those that were of use; and the same names, with slight modifications, were universally in use throughout a country a thousand miles in length. They had generic names by which they grouped plants according to their affinities in a way impossible to most people who were not educated botanists. The Veronicas of New Zealand appeared under a very great variety of external forms, yet they were all identified by one name.—The Rev. W. Gill, in closing the discussion, said that difference in shade of colour was not to be relied upon as a test of difference of race; for he had seen the most intense blackness produced in Polynesia in those of the poorer classes who habitually spent much time in salt water, while the wealthier classes remained of a much lighter hue.

General H. B. Carrington, of the United States army, read a very interesting paper *On the Indians of the North-Western States.*

The Anthropological Department has been one of the best sustained this year, a result attained by its inclusiveness of a wide range of subjects relating to the history of mankind, and by reason of the high authority of many who addressed the department on their respective studies. The President showed himself a worthy leader, illuminating most of the subjects discussed and fostering discussions which were interesting alike to students and to the general public.

SCIENTIFIC SERIALS

American Journal of Science and Art, September.—The original articles are: On the formation of hail in the spray of the Yosemite Fall, by W. H. Brewer. The paper describes a visit paid to the fall in April last. The amount of water passing over the fall was estimated at 250 or 350 cubic feet a second, and the height is 1550 feet. In the spray, which stung the hands and faces of the visitors, hail or ice-pellets were found. "It will be noticed that at the time when this hail was observed, the sheet was in the full blaze of the sun from top to bottom. . . . The air near was of a temperature of 70°. Prof. Le Conte has suggested that perhaps the cooled air within the sheet is somewhat compressed and condensed in the base of the fall, and when liberated just outside by its expansion, freezes a part of the spray."—On Southern New England during the melting of the great glacier, by J. D. Dana: Part I. (we reserve our notice till the completion of the article).—On the mechanical work done by a muscle before exhaustion, and on the "law of fatigue," by the Rev. S. Haughton, M.D. Dr. Haughton announces his aim is be to show (1) That both series of experiments made by Prof. Nipher (given in the February number) are a valuable contribution to the facts of animal mechanics; (2) That they are not only consistent with "the law of fatigue" proposed by Dr. Haughton, but illustrate both that law and his "Coefficient of Refreshment;" (3) That Prof. Nipher's discussion of his own valuable experiments is worthless, as it is based on an empirical formula, which has no meaning and leads to no further consequences: (4) That the law of fatigue, which explains not only Prof. Nipher's experiments, but so many other experiments also, is entitled to be received provisionally as a law of animal mechanics, and followed up by deduction to its legitimate conclusions.—Earthquake of December 1874, by Prof. D. S. Martin. "The general phenomena presented nothing peculiar."—On some interesting equine calculi, by R. H. Chittenden.—Results of dredging experiments off New England coast, by A. E. Verrill. Four pages of tables are given, and a note is added on methods of preserving specimens. Picric acid was found to be valuable.—On the passage of two bolides in 1872 and 1874 over Middle Kentucky, by J. Lawrence Smith.—Notes on the gases accompanying meteorites, by Prof. J. W. Mallett. The purpose is to question whether Prof. Wright has sufficient evidence for his conclusion, "the stony meteorites are distinguished from the iron ones by having the oxides of carbon, chiefly the dioxide, as their characteristic gases instead of hydrogen."—On a new vertical lantern galvanometer, by Prof. G. F. Barker. The arrangement is for demonstration to a large audience, deflections obtained by induction currents, thermo-currents, voltaic currents, &c.—On another gigantic Cephalopod (*Architeuthis*) on the coast of Newfoundland, December 1874, by A. E. Verrill. The total length is estimated at forty feet.

THE *Journal of the Chemical Society* (June 1875) contains in detail Prof. Clerk-Maxwell's paper On the dynamical evidence of the molecular constitution of matter, which was duly published in NATURE. The other papers in this part are:—Researches on the action of the copper-zinc couple on organic bodies, by Dr. J. H. Gladstone and A. Tribe. The authors in this (eighth) paper treat of chloroform, bromoform, and iodoform.—On the action of nitrosyl chloride on organic bodies (second paper), by W. A. Tilden; the action on turpentine oil is considered.—A note by Prof. Story Maskelyne on the crystallographic characters of nitrosoterpene is given as appendix to the last paper.—Dr. H. Armstrong contributes a note on isomeric change in the phenol. series, which gives new proof of the energy and unceasing attention this gentleman bestows upon his interesting researches.—The last paper is a note on the effect of passing the mixed vapours of carbon disulphide and alcohol over red-hot copper, by Th. Carnelley. It was found that the following bodies were formed: $CH_3.COH$, COS (carbon oxysulphide!) C_2H_4, C_2H_2, CH_4, and H, and neither H_2S nor SO_2. The copper is superficially converted into sulphide, and amorphous carbon is deposited.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, Aug. 1.—This number contains the concluding part of Herr Wilczek's paper on the calculation of the arithmetical mean of constant quantities. Also an account, by Herr von Jedina, of a cyclone encountered by the corvette *Helgoland* in the North Atlantic, remarkable for the steadiness with which the wind blew from east at its commencement, the great expansion of the front in comparison with the rear, and the slow rise of the barometer after passing the centre.—Among the Kleinere Mittheilungen is a notice of the late Dr. Theorell, and a paper by Herr C. Braun, on the theory of storms.

Rendiconto delle Sessioni dell' accademia delle scienze dell' istituto di Bologna.—The longer papers read at the Academy during the academical year 1874–5 were twenty-nine in number, besides numerous notes and memoirs of smaller interest. We note the following, as of special interest to our readers:—On some phenomena consequent upon contusions of the abdomen and of the spine, by Dr. P. Loreta.—On some argillaceous slate of Miocene origin, by G. A. Bianconi.—Several papers by Prof. F. Selmi, on researches made on poisonous alkaloids, their differences in properties, their determination when mixed with others in organic matter, and with innocuous alkaloids,

&c.—Helminthological observations by Dr. Ercolani, on dimorphisms, on *Filaria immitis*, and on a new species of dog Distoma.—Anatomical description of the eye of the European mole, by Dr. Ciaccio.—On the organisation of the brain of Eolidida, by Dr. Trinchese.—On the changes of form of *Amœba limax*, by the same.—On a non-microscopic new and rare parasitic fungus, which is developed on the larva of a living cricket, by G. Bertoloni.—Analytical remarks on some theorems of Feuerbach and Steiner, by Prof. E. Beltrami.—On the continuity of feeling, by Dr. D. C. Biagi.—On the reasons of the low statures which were generally observed amongst the conscripts of the last decennium in some communities in the neighbourhood of Bologna and other districts of Italy, by Dr. P. C. Predieri.—New observations on the minute structure of muscular fibre, by Dr. Ercolani.—Proofs for the contemporariness of the glacial epoch with the Pliocene period at Balerna and at Monte Mario, on the Rhine, by G. A. Bianconi.—On the effects of electric sparks on phosphorus in hydrogen, in nitrogen, in ammonia, and in muriatic acid; and on the effects of electric currents on water, on sulphuric acid, on alcohol, and on bisulphide of carbon, by Dr. Santagata.—Researches on capillary tubes, by Prof. Villari.

Sitzungsberichte der naturwissenschaftlichen Gesellschaft Isis in Dresden, October to December, 1874.—The meetings of this society are divided into five classes, besides general meetings, viz., one for mineralogy and geology, one for prehistoric archæology, one for chemistry, physics, and mathematics, and one each for botany and zoology.—The more important papers read in the different sections during the last three months of 1874 were:—In the mineralogy and geology class: On a peat-like formation occurring at Lindenau, near Leipzig, containing a great number of beetles, one or two species of which are now extinct, by Von Kiesenwetter. On a number of minerals collected during a tour in Saxony, by E. Zschau. On the occurrence of calc-sinter near Quedlinburg, by Herr Ackermann.—In the botany class: On hedge plantation in Australia, by W. Ferguson. On the culture of plants in rooms, particularly of Palmæ, by Adolph Petzold. Report of the results of botanical excursions made during 1874, by A. Voigt.—In the zoology class: Remarks by Th. Kirsch, on "Darwinism and the Researches of Cuvier and Newton," a work lately published by Herr Wiegand.—On Haeckel's calcareous sponges and his Gastræa theory, by Herr Ebert.—In the archæology class: Report on the Archæological Congress at Stockholm, by Dr. Mehwald. On some flint implements from the cave near Rochefort, by Dr. Geinitz. On a piece of reindeer horn upon which rough drawings of horses are visible, and which was found near Thayingen, in Switzerland, by the same.—In the physico-chemical class:—On ozone, by Dr. Schürmann, a highly interesting and elaborate paper; the author gives a detailed account of the history of ozone, and then speaks of its properties, preparation, reactions, presence in the atmosphere, action on the animal organism, and thoroughly exhausts the subject. On tables for barometrical measurements of heights, by Prof. Neubert. Meteorological phenomena observed at Dresden during 1874, by Herr Fischer.—At the general meetings, a paper on the earthquakes of the sixteenth and seventeenth centuries was read; the others being all of minor interest.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 13.—M. Frémy in the chair.—The following papers were read:—A note by M. Faye relating to the approaching eclipse of the sun.—M. Bertrand then made some remarks on the paper read at the last meeting by M. Bienaymé.—Report on a memoir by M. Lefort, entitled "Critical examination of the basis of calculation usually adopted to appreciate the stability of metal bridges with straight prismatic beams, and propositions for the adoption of a new basis."—Report on M. Boussinesq's paper on the theory of flowing waters.—Memoir on the observations made at Peking of the Transit of Venus, by Mr. J. C. Watson, chief of the American expedition.—A note on the greasy matter in the grain of the oil-tree of China, by M. S. Cloez.—On the development of Heteropoda, by M. H. Fol.—On the migrations and metamorphoses of marine endoparasitic Trematoda, by M. A. Villot.—On some reactions of hæmoglobine and its derivatives, by M. C. Husson.—On the probable origin of the two hailstorms observed on July 7 and 8, in some parts of Switzerland and the South of France, by M. D. Colladon.—On the non-regeneration of the crystalline lens in man and in rabbits, by M. J. Gayat.

VIENNA

K.K. Geologische Reichsanstalt, April 6.—On Miocene chestnut trees, by O. Heer.—Diallogite after manganese blende and barytes; pseudomorphs after fahl-ores of Przibram, by Ed. Döll.—On the occurrence of native gold in the mineral shells of Verespatak, by F. Posepny.—On the Culm flora of the Moravian-Silesian roofing slates, by D. Stur.

April 20.—On remains of *Ursus spelæus* from the cave of Igritz, by F. von Hochstetter.—On the meteorite of Lancé, by R. von Drasche.—On a geological detailed map of the surroundings of the Seisser Alp and of St. Cassian, by E. von Mojsisovics.—On a map of the upper Vilnöss and the lower Enneberg valleys, by R. Hörnes.—Geological report from the investigation district of the Oetz-valley group, by G. A. Koch.

May 4.—Presentation of a new special map of the Austro-Hungarian Monarchy, F. v. Hauer.—Characteristics of some minerals occurring on the Przibram ore deposits, by F. Babánek.—Report by Dr. E. Tietze from his travels in Persia.—On a new fossil resin from the Bukowina, by J. von Schröckinger.—On *Cervus megaceros* from Nussdorf, by Dr. F. von Hochstetter.—On a human cranium found in the diluvial Loess of Manners-Porf, by Dr. J. Woldrich.—On Noric formations in Transylvania, by E. von Mojsisovics.—On the phosphorites of the Lavant valley, by H. Wolf.

GÖTTINGEN

Royal Society of Sciences, July 10.—At this meeting of the Society the following papers were read:—On the electric elementary laws, by Herr Riecke.—A note on the toxicological action of phenols, in particular of thymol, by Th. Husemann.—On Rötteken's eye of Actinia, by Dr. Hub. Ludwig.—A note by Herr Fromme on the maximum of temporary magnetism in soft iron.—On the potential function in space extended in several directions, by M. Jouelli.

BOOKS AND PAMPHLETS RECEIVED

BRITISH.—The Royal Tiger of Bengal: Dr. J. Fayrer, M.D., F.Z.S. (Churchill).—Jummoo and Kashmir Territories. A Geographical Account, by Frederic Drew, F.R.G.S., F.G.S. (E. Stanford).—Proceedings of the Berwickshire Naturalists' Club.—Brande and Cox's Dictionary of Science, Literature, and Art. 3 vols., new edit. (Longmans).—Further Researches in the Mathematical Science, by the author of "The Two Discoveries" (Bridgewater, Pine).—Bristol and its Environs (Bristol: Wright and Co.)—The Geology of British Guiana: C. S. Brown, F.G.S., and J. G. Sawkins, F.G.S, (Longmans).—A Manual of Mollusca: S. P. Woodward, A.L.S., F.G.S. (Lockwood).—The Native Races. Vol. iii.: Hubert Howe Bancroft (Longmans).—Tapeworms: T. Spencer Cobbold, M.D., F.R.S., F.L.S. (Longmans).—An Introduction to Animal Physiology: E. Tully Newton, F.G.S. (Murby).—The Abode of Snow: Andrew Wilson (Blackwood).—Quarterly Journal of the Geographical Society (Longmans).—Journal of the Scottish Meteorological Society (Blackwood).

COLONIAL.—Centrifugal Force and Gravitation. Six parts: John Harris (Montreal).—The Immortality of the Universe: J. A. Wilson (Melbourne, G. Robertson).—Report of the Meteorological Reporter of Bengal.—Report of the Nidnapore and Burdwan Cyclone.—Magnetical and Meteorological Observations at the Magnetic Observatory, Toronto, Canada, 1841 to 1871 (Toronto: Copp, Clark, and Co.)

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