THURSDAY, DECEMBER 26, 1878

THE ASSOCIATION OF LOCAL SOCIETIES

LOCAL Societies: What is their aim and what purpose do they serve? How may this aim be most surely gained? How can this purpose be most effectively carried out? These are questions which naturally arise when considering the subject of local societies.

The *aim* of every local society should be to raise the intellectual status of the locality. The *purpose* to do so in that way most generally useful. It is the mind of the community which has to be raised by affecting the minds of the individuals. Individual minds are to be affected by contact with material surroundings. These surroundings influence us through the powers of observation, hence *careful and accurate observation* must exist among the members of a society fulfilling its proper functions. The greater the number of members exercising such observation the greater the usefulness of the society. It is almost needless to instance other mental qualities as necessary for success, because experience shows that when once the observing faculty has received its due share of attention, the power of using the observations made follows in due course. The faculty of observation must be drawn out and cultivated by contact with matter in relation to man, and by contact with matter considered apart from man as existing in a state of nature. And just as it is important that in the culture of the individual a one-sidedness should be specially avoided, so in raising the culture of a community it is equally important that opportunities or suggestions for mental improvement *all round* should be afforded. Hence we are inclined to think it advisable that especially in the case of small country towns scientific studies, or suggestions for such, should proceed from the same platform as those studies which are often spoken of as more purely literary. Of course *literature* includes the records of science, but still for general purposes the meaning is clear when a literary institute or society is spoken of as distinguished from a scientific. Among the lower types of animals there is a want of specialisation of parts; very different functions may be performed by the same part or the whole of the body; in the higher, specialisation prevails, each function has its own organ, and the function is performed more efficiently. In large towns science may be pursued apart from general literature, and even each special science may stand on its own platform, but in small towns this is out of the question, and I believe unadvisable, for the over-performance of one function in the lowly organised society s checked by the claim of the general body. Moreover, the tastes of a community being naturally various, it becomes essential to present intellectual food of various kinds. Hence we cannot but think that small local societies should be both literary and scientific. The two aspects of culture will support and strengthen each other, and the introduction of a new clique, or party, or sect be avoided. For it must be remembered that one of the distinct collateral advantages of such societies is that a common platform is provided upon which men of all political or religious beliefs can stand and work together. No one who is acquainted with the social conditions of our small towns can underrate the importance of this.

But how are such societies to work? I would reply, from within, outwards. Not, in the first place, by calling in extraneous help, by engaging eminent men to give courses of lectures, but by arousing the spirit of inquiry and observation amongst the townsfolk. Let but a few natives come forward with short papers on any subjects with which they may be especially acquainted, the subjects being treated in such a way as to elicit a discussion or inquiries, a spirit of interest will soon be aroused, and minds put into a proper attitude for the reception of truths before quite unknown to them, and for the prosecution of some special subject as a study. In practice I would strongly advise the following course to be pursued by any embryo literary and scientific society. Have two classes of meetings: one the *ordinary meeting*, at which members alone (and therefore townsfolk) should read short papers, upon which a discussion should afterwards be encouraged; and *public lectures*, given mainly by non-residents, and to which the general public should be admitted on the payment of a small fee. At the ordinary meeting the local talent and observation is drawn out, and at the public lecture new subjects are introduced to the notice of members. At the former, notices of local phenomena and history, or the occasional original investigations of members, are recorded; at the latter, new lines of thought are often indicated, or systematic instruction given in some one subject.

A society established on some such basis is then in a position to encourage the collection of objects of *local* natural history, to establish a *local* museum, and carry out field excursions during the summer months. Moreover, the experience of many years past has shown me that the life—and therefore the growth of culture—in such a society is far greater than in those cases where only a yearly course of *lectures* is organised, the greater part of them being given by strangers. Next comes the oft-repeated question, But how long will such a society last? Many are ready to say, We have tried some such plan, and success has attended our efforts for one or two years, and then the society has died out. On this part of the subject a few words will now be said, and the remarks made are founded upon experience gleaned amidst the practical working of local societies in Cumberland during the past nine years.

How, then, can permanence be ensured? In a small town or district local resources and talent are apt to become exhausted or unavailable. A time will surely come when the intellectual movement will wane and the society be on the brink of non-existence. But the very usefulness of such a movement must consist in its stability; there should be a growth, not a bare existence. To insure this stability I suggested some years ago that the four societies then existing in the Lake District and West Cumberland should be united for general purposes, while each society should retain its individuality. After many preliminary difficulties were overcome, the union was effected, and since that time each society has grown stronger, four new societies have been formed, and the total number of members increased from a few hundred to nearly 1,200.

The objects to be attained by this association of societies are as follows:—1. Increased strength to be derived from mutual help, encouragement, and a spirit of honest emulation. 2. The union affords greater facilities towards publishing transactions and securing the services of eminent lecturers. 3. An annual meeting of the associated societies affords an opportunity for the discussion of principles of working and promotes the general life. 4. The annual meeting being held in a fresh town each year helps to keep the country alive to the Association work, and encourages the formation of new societies.

The constitution of the Cumberland Association is as follows:—The president to be a man of local note and high culture, and to serve for a period not greater than two years.[1] The Presidents of individual societies to be vice-presidents of the Association. The council of the Association to consist of two delegates from each society, chosen annually. The treasurer and secretary (honorary) to be one and the same person, and fully acquainted with the county in all its aspects.

The working of the Association is carried on thus: The Association secretary keeps a record of all papers and lectures brought before the individual societies. Before the commencement of each winter session he communicates with all the local secretaries, and from his knowledge of available intellectual stores in the county, helps each in the drawing up of the winter programme in whatever direction help may be specially needed. It is his duty also to help forward the establishment of local classes where such are possible. At a council meeting held in the autumn some public lecturer is decided upon who shall go the round of the associated societies during the winter, and a grant is made towards his expenses from the Association funds (of which more anon), the rest being made up by each society served.

The annual meeting takes place at Easter or in May, and lasts two or three days. The Association President delivers his annual address, reports from the several societies are read and discussed, original papers are read, lectures given by one or more eminent men, and field excursions made.

At the close of each winter session the local secretaries send into the Association secretary any papers which have been selected by the local committees as worthy of publication. If the Association council approve these papers they are published in the *Transactions* at the Association expense. The funds of the Association are gathered thus: Each society pays an annual capitation grant of 6*d.* per head on all its members. There is also a class of Association members, residing at a distance from, and not belonging to, any local society, who pay an annual subscription of 5*s.*, and are virtually considered members of all the societies, and have the privileges of such. The *Transactions* are sold to the societies and Association members at the price of 1*s.*, the public being charged 2*s.* 6*d.* Some of the societies purchase copies to the full number of their members, and present them, others take only a limited number of copies (determined by the local society committee) and re-sell to those of their members who care to possess them. In this way the greater part of an edition of 800 copies of the *Annual Transactions* is disposed of. Authors are allowed extra copies of their own papers at a moderate charge, and when all expenses are met, a fair balance is left to carry on to the next year.

It should be noted that of the eight societies in Cumberland, now associated, the local annual subscriptions of members in each society is generally 5*s.*; in one case, however, it is 3*s.* 6*d.*, and in another 2*s.* 6*d.* It is a rule of the Association that members going from one society to another to afford help in the carrying out of the various programmes, should have their expenses paid by the society helped. Such is the general constitution and mode of working of the Cumberland Association, which has undoubtedly succeeded in its aim, so far as the keeping up of existing societies and the formation of new ones is concerned. The *Annual Transactions*, too, include many papers of local value, and some of general interest, while among the eminent men who have kindly come forward to lend their services at the Annual Meetings, are the Astronomer-Royal, the Bishop of Carlisle, Prof. Shairp, Prof. Wm. Knight, and I. Fletcher, M.P., F.R.S. At present, however, the Association is but in its infancy, and may be considered more or less of an experiment, yet that some such method of union is desirable amongst local societies in the various counties or districts of England few will deny. Time will show how the system may be improved and varied to suit special circumstances, but I cannot but think that the plan of association to carry out the larger objects of societies, and the annual meeting of the associated societies in successive towns of a county, must economise labour and promote the healthy culture of the county in which the work is carried on.

Amongst the difficulties presenting themselves in the early days of the association, the following occurred. For several previous years a Cumberland and Westmoreland Antiquarian and Archæological Society had flourished, and it was feared that the new County Association would clash with its existence. The Antiquarians thought it best not to amalgamate with the associated society, its constitution being in many points different from theirs, but it was resolved that whenever papers, bearing on local antiquarian or archæological subjects were read before any of the associated societies, these papers should be offered by the Association council to the Antiquarian and Archæological Society for publication in their *Transactions* if deemed worthy. Moreover, some of the officers of the Association are active members of the Archæological Society, and so far from their being any antagonism, the two decidedly help one another forward in the general work of gleaning local knowledge and diffusing culture.

As hon. secretary of the Cumberland Association, I should feel very grateful for any hints or suggestions from the readers of NATURE. What is wanted in every county is more culture, and that carried on in a *natural* way, and with a true love of nature in all her aspects.

J. CLIFTON WARD

[1] The Lord Bishop of Carlisle acted as president for two years, and I. Fletcher, M.P., F.R.S., is now in his second year of presidency.

NEWCOMB'S LUNAR RESEARCHES

Researches on the Motion of the Moon, made at the United States Naval Observatory, Washington. By Simon Newcomb, Professor U.S. Navy. Part I. (Washington, 1878.)

THE author prefaces his work with the remark that for several years after the publication of Hansen's Tables of the Moon, there was a very general belief that the motion of our satellite could be followed by their means with the same accuracy as that of the other heavenly bodies, after having been made the subject of astronomical and mathematical research for two thousand years. This expectation was soon proved to be far from borne out. Prof. Newcomb showed in 1870 that the accuracy of the Tables since 1750 had been secured only by sacrificing the agreement with observations previous to that epoch, and that about 1700 the Tables deviated more widely from the observations than the previous ones; and it may be added that those who had been engaged in examining the old eclipses were aware that Hansen's Tables did not represent the phenomena only as far back as the commencement of the eighteenth century, so well as Burckhardt's or Damoiseau's, which had been used for our ephemerides up to the date of their publication.

A new investigation of the subject for the purpose, if possible, of ascertaining the cause of these unexpected deviations was entered upon at the Naval Observatory, Washington, and was made a part of our author's official duties in that establishment. In the present volume we have the results of researches on the discordances in question, based upon observations before the year 1750. This portion of the work was originally intended to follow the study of the mathematical theory of the inequalities of long period in the moon's mean motion, but for reasons explained in the Introduction, that part of the inquiry is still incomplete. The year 1750 was fixed upon as the terminal point in the investigation in this first part, as it is the epoch when exact meridian observations commenced, and that which separates the period within which we are in possession of observations reduced on modern data, from the period during which neither really accessible observations nor tables of reduction are available.

Prof. Newcomb supplies an historical introduction in which the discovery of the secular acceleration, and the examinations of ancient eclipses with the view to fix its correct amount by observation are briefly noticed, as also Ferrel's paper, published in 1853, containing "the first known attempt to calculate from theory the retardation produced by the action of the moon upon the tidal wave," and the researches of Adams and Delaunay. He proceeds to give a summary of the data now at our disposal for determining the apparent secular acceleration of the moon from observation alone. These include the statements of ancient authors from which it has been inferred that total solar eclipses have been witnessed at certain points of the earth's surface at dates approximately indicated, and the author points out the uncertainty attending our interpretation of such records. He considers that the circumstance which we should regard as most unequivocally marking the totality of an eclipse is the visibility of stars, though he thinks that even this criterion is hardly to be admitted as conclusive, because Venus may be seen during a considerable partial or an annular eclipse, and at certain times when there is no eclipse at all, there is also another difficulty in some cases, in determining the precise localities where the phenomena were observed. We have also the series of lunar eclipses upon which Ptolemy founded his theory and which are recorded by him in the Almagest. These are followed by the observations of the Arabian astronomers, chiefly contained in an Arabic manuscript belonging to the University of Leyden, a translation of which was made by Caussin and published by the French Government in 1804, under the title "Le Livre de la Grande Table Hakémite." Prof. Newcomb remarks that this work contains what are entitled to be considered the earliest astronomical observations of eclipses which have reached us, for although some of the data furnished by Ptolemy, Theon, Albategnius, and others, may have been the results of astronomical observations, in no case have the quantities actually observed been handed down to us. The entire number of eclipses in this collection is twenty-eight, and the times of concluded beginning and ending were usually determined by noting the altitudes, which were recorded sometimes in whole degrees only, at others "in coarse fractions of a degree." There must remain a doubt how nearly such times apply to those of actual contact, but Prof. Newcomb suggests in this part of his work that by the mean of all the observed times the error in the moon's mean longitude can be reduced to not more than a minute of arc. The Arabian observations are followed by those of European observers prior to the invention of the telescope, including Regiomontanus, Bernard Walther, and Tycho Brahe: of the latter, the author remarks, "It is wonderful if so indefatigable an observer never observed an occultation of a star or planet by the moon, yet I have never succeeded in finding any such;" he made a careful examination of Tycho's observations during periods in which the bright star Aldebaran must have been occulted, to no purpose. The observation of eclipses and occultations with the aid of a telescope, Prof. Newcomb remarks, may be considered as commencing with Bullialdus and Gassendus, but they had no clock, and only fixed the time by noting the altitude of the sun or a star. The application of the clock commences with Hevelius, and in the scarce volume of his "Machina Cœlestis" are found a number of occultations thus observed. Then follow the observations of Flamsteed and the astronomers at the observatory of Paris, the Cassinis, La Hire, and Delisle, the latter of whom also observed at St. Petersburg. Prof. Newcomb, during a visit to Paris, while Delaunay was in charge of the Observatory, was fortunate in having all the archives of that establishment unreservedly placed at his disposal. He found amongst them most of the original note-books of the French observers since the year 1675, in which were contained a great number of occultations that had been quite forgotten, those which had appeared in the *Memoirs* of the Academy, forming but a small fraction of the whole. Again, on visiting Pulkowa, M. Struve gave him access to the records of Delisle's observations, 1727 to 1747, forming a useful supplement to those of Paris, which had diminished in number after 1720.

Prof. Newcomb concludes this historical notice with remarks on observations since the time of Bradley, and granting certain fundamental premises, suggests that the secular acceleration of the moon may admit of nearly as accurate determination from the modern observations, as from their combination with the ancient ones.

Having thus briefly recapitulated the data available for investigation, the author adverts to ancient eclipses, presumed to have been total from the narrative of the historians, at certain points of the earth's surface. The eclipses he admits in this category are eight in number, from that of Thales, B.C. 585, to the eclipse of A.D. 364, described as total at Eoos. Looking for mention of distinct indications of totality, he has not included such an eclipse as that of B.C. 763, the record of which was discovered by Sir Henry Rawlinson on one of the Nineveh Tablets in the British Museum, where importance appears to be attached to it by the description being underlined; hence its presumed totality. The eclipses of the moon recorded by Ptolemy in the Almagest are thoroughly examined, with satisfactory results, except in the case of the eclipse of B.C. 383, December 22, where there appears to be a mistake as to its having been really observed at Babylon; and he concludes that during the eight centuries preceding the Christian era the mean longitude of the moon in Hansen's Tables requires a correction of about 18′. The Arabian eclipses, solar and lunar, twenty-five in number, between A.D. 829 and 1004 are then compared with the Tables.

In the next two sections Prof. Newcomb supplies a full description of the method adopted for deducing the errors of the lunar elements from the eclipses and occultations, and for determining the effect of changes in the elements upon the path of the central line of an eclipse, adopting, in the latter case, formulæ originally given by Bessel.

In a following portion of the volume, occupying about a hundred pages, are given, mostly as originally recorded, the observations of occultations and eclipses by Bullialdus and Gassendus, Hevelius, and, as the author terms them, the astronomers of the French School, between 1670 and 1750, preserved in the archives at the observatories of Paris and Pulkowa; it is needless to say that this section possesses a great value, supplying as it does the particulars of so many observations hitherto unpublished. We have then the positions of the moon from Hansen's Tables, used in the comparison of the preceding observations with theory, in the calculation of which, and for the numerical work generally, Prof. Newcomb was assisted by a grant from Congress, sufficient to enable him to employ two computers. Certain modifications of the strict form of application of the Tables were considered allowable for the older observations, their degree of accuracy rendering an exact computation of Hansen s Fundamental Argument of no advantage, and he considers such modified plan of employing Hansen's Tables, preferable to the use of the older Tables, which might be adopted for the sake of saving labour. A "Tabular exhibit of Reduction of the Occultations," 286 counting immersions and emersions separately, is then given. The equations of condition for the occultations, and a provisional solution follow.

In the next section the author presents an elaborate discussion of eclipses from 1620 to 1715, which it may be remembered is the last total eclipse that was methodically observed in this country. In this series is included the eclipse of 1639, June 1, observed by Gascoigne and Horrox amongst others, and 1706, May 11, which was total in the south of France, but of which Prof. Newcomb takes no other observations into account than those of La Hire, of Paris: the fact, indeed, is that the observations along the belt of totality appear to be strangely and unaccountably discordant, so far at least as regards the beginning and ending of the total phase. The eclipse of 1715 is very fully discussed, and remarking that by Halley's organisation of a numerous body of observers throughout the path of the moon's shadow across England, valuable observations for determining its limits were procured, the author deduces from them a correction to the motion of the moon's node, which he finds to be $10'' \times T$ (T being counted in centuries from 1850), the argument of latitude being diminished by this amount; this result he considers nearly certain with respect to its algebraical sign, but observes that it must be affected by any corrections of Hansen's value of the moon's parallax.

We now reach the main conclusions to which Prof. Newcomb is led, by the laborious and masterly discussion of observations prior to 1750, of which necessarily little beyond an outline has been given here. The theoretical value of the secular acceleration of the moon's mean motion due to the cause discovered by Laplace, has been fixed with accuracy by Prof. Adams and Delaunay: the latter geometer, carrying his approximation to a greater number of terms than had been included by Prof. Adams, assigned $6''\cdot18 . T^2$. But this value, as is well known, has not been found to accord with the older observations, and the difference between the theoretical value and that which observation seemed to require, has been generally attributed to a retardation of the earth's axial rotation; thus, as Prof. Newcomb remarks, "the apparent secular acceleration will be made up of two parts—the one a real acceleration, the other an apparent one, due to the change in our measure of time. But further, he says it will be found that the hypothesis of a constant tidal retardation does not account for the observed mean motion of the moon, and either the retardation must be supposed variable, even to becoming at times an acceleration, or it must be admitted that her mean motion is affected by changes not hitherto explained. He then proceeds to inquire what deviations of the moon's mean motion remain unaccounted for, and with this object he first collects into tabular form the individual corrections to Hansen's mean longitudes, derived from the discussion of eclipses and occultations from 1621 to 1728, with their probable errors, the latter being necessarily somewhat arbitrary, for want of data for their rigorous computation. The older results do not exhibit larger discordances than might be expected. In order to complete the investigation of anomalies in the moon's mean motion unexplained, it was necessary to have the errors of Hansen's Tables from 1750 to the present epoch, or to 1875, and these were partly obtained from Hansen's paper in the *Monthly Notices* of the Royal Astronomical Society, and, since 1850, from Part III. of the publications of the American Transit of Venus Commission, where it was shown by Prof. Newcomb that at the epoch 1875·0 the meridian observations at Greenwich and Washington agreed in indicating a correction to the tabular mean longitude of $-9''\cdot7$. The occultations about the same time giving a correction nearly two seconds less, it is assumed that the true correction at this epoch was $-8''$. Hansen introduced in his tables a term depending on the argument, 8 times the mean motion of Venus *minus* 13 times the mean motion of the earth, which has not been theoretically explained, and is to be regarded as empirical. This term is therefore removed from the theory, before examining, as Prof. Newcomb proceeds to do, how nearly theory alone, without any empirical correction, will represent the observations. It is then at once apparent that the residuals cannot be represented by corrections to the epoch of mean longitude, mean motion, and secular acceleration, and any approximation to a mean value of the latter would have different values, according to the mode of using the data. To obtain the best result from the ancient and modern observations combined, it was deemed advisable to assign a minimum probable error of 4″ or 5″ to each residual for the modern observations. Equations of condition for correction of epoch, mean motion and acceleration are formed, and extend from B.C. 688 to A.D. 1875, or over a period of 2,500 years, and the resulting corrections to Hansen's values for 1800, are, for mean longitude, + 3″·90, mean motion – 19″·03, and for secular acceleration – 3″·36; Hansen's adopted value of the latter being 12″·17, the value which best satisfies the observations discussed by Prof. Newcomb is found to be 8″·8. Though he considers this correction to the tabular acceleration to be clearly indicated, the residuals for the modern observations are yet of such magnitude as to be wholly inadmissible, and therefore the theory in its present state will not represent observations with any value of the secular acceleration, and respecting the cause of the magnitude of these remaining differences, he makes two hypotheses: (1) that they are only apparent deviations caused by inequalities in the earth's axial rotation, (2) that they arise from one or more inequalities of long period in the actual mean motion of the moon. Examining the effect of the first hypothesis, he arrives at the conclusion that if it be correct "the problem of predicting the moon's motion with accuracy through long intervals of time must be regarded as hopeless since it cannot be expected that variations in the earth's axial rotation will conform to any determinable law," and, he adds, "success in tracing the deviations in question to the moon itself and to the theory of gravitation is therefore a consummation to be hoped for." With regard to the second hypothesis, it is seen that the residuals of the equations of condition indicate that the modern observations may be nearly represented by a term having a period of between 250 and 300 years, and hence Prof. Newcomb inquires how closely an empirical correction to Hansen's first term depending upon the action of Venus, the period of which is 273 years, will accord with the modern observations, and he finds a very satisfactory agreement. An additional diminution of 10″ in the secular mean motion of the moon results, which at the present epoch involves a further diminution in the secular acceleration of 0″·5, that the ancient observations may be well represented; thus the acceleration becomes 8″·3 . T^2. A table is given exhibiting the corrections to Hansen's mean longitude from 1620 to 1900 for every tenth year; in 1880 it is – 11″·2, and in 1900 – 24″·6.

This important volume concludes with some remarks upon the bearing of the value of the moon's secular acceleration deduced from the investigations, of which we have endeavoured to give a general outline here. Prof. Newcomb thinks it is apparent that one of two propositions must be accepted: "Either the recently accepted value of the acceleration and the usual interpretation of the ancient solar eclipses are to be radically altered, the eclipse of – 556 not having been total at Larissa, and that of – 584 not having been total in Asia Minor; or the mean motion of the moon is, in the course of centuries, subjected to changes so wide that it is not possible to assign a definite value to the secular acceleration." It is certain that there will be a difference of opinion upon his main conclusions, and for this he expresses himself fully prepared. If a definite theory of the apparent inequalities of long period in the moon's motion cannot be formed, or if the moon's mean motion is subject to such changes from age to age that no invariable and well-defined value of the secular acceleration can be deduced, then he urges it is not certain that the question whether Hansen's tabular mean longitude during centuries preceding the Christian era does or does not require a considerable negative correction can ever be conclusively settled, since no conclusions can be drawn except from observations made near the period in question, and he advocates the necessity of a further investigation into the eclipses and other data on the two hypotheses, first that Hansen is correct during the period named above, and second, that a correction of – 16′ is required, and suggests that the question should be examined in this manner by some independent authority. If, on the other hand, it is not possible to form a perfect theory of all the inequalities in the moon's mean motion independently of observations, he thinks it will be practicable to arrive at a value of the secular acceleration from the modern observations, reliable within 0″·5.

ROSCOE AND SCHORLEMMER'S CHEMISTRY

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in Owens College, Manchester. Vols. i. and ii. (Macmillan and Co.)

THIS work is a most valuable contribution to the literature of chemistry. Its aim, as stated in the Preface, is to place before the reader a fairly complete, and yet a clear and succinct statement of the facts of modern chemistry, whilst at the same time entering so far into a discussion of chemical theory as the size of the work and the present transition state of the science permit, special attention being also paid to the accurate description of the more important processes in technical chemistry, and to the careful representation of the most approved forms of apparatus employed.

The manner in which this design has been carried out is such as might have been expected from the high reputation of the authors. The work commences with a very interesting historical introduction, in which the progress of chemistry is traced from the early times, in which it was merely an art subservient to alchemy, medicine, and a few branches of manufacture, to the time when, by the gradual accumulation of observations, and the discussion of them by men of philosophic mind, it rose to the rank of a science. A clear and impartial discussion is given of the relative merits of the various workers by whose labours the system of chemical philosophy now accepted was developed, showing how the phlogistic theory of Becher and Stahl first established a common point of view from which all chemical changes could be regarded, and enabled chemists to introduce something like a system by which analogous phenomena could be classified and referred to a common cause; further, how the experiments of Black, Cavendish, and Lavoisier first showed the importance of attending, in the study of chemical changes, to the alteration in weight of the substances concerned; and how Lavoisier was ultimately led to the true theory of chemical combination, which regards it as consisting simply in the addition of one element to another, the weight of the product being exactly equal to the sum of the weights of the combining bodies.

Next follows a sketch of the labours of Bergmann, Richter, Cavendish, and others, which led up to the establishment by Dalton of the great doctrine of combination in multiple proportions, on which he founded the "atomic theory." The early experiments of Dalton are briefly described; his table of the relative weights of the atoms of certain elementary and compound bodies is given; and the Introduction ends with an account of the manner in which the exact values of the atomic weights were determined by Thomas Thomson, Wollaston, and more especially by Berzelius; of the discovery of the compound nature of the alkalis by Davy, and of a number of new elements by various chemists; and lastly, of the development of Organic Chemistry and its true relations to the chemistry of inorganic bodies; and the final establishment—chiefly by the researches of Liebig—of the fact that the science of Physiology consists simply in the physics and chemistry of the living body.

The Historical Introduction is followed by a chapter on the General Principles of Chemical Science, in which the methods by which the laws of chemical combination have been established are more fully described, especially that by which Lavoisier demonstrated the nature of combustion and the indestructibility of matter. This part of the subject is well illustrated by diagrams of the apparatus used in these important investigations. A list of the elements with their combining weights is then given, and a table exhibiting the arrangement of the elements in groups, chiefly, but not entirely, according to their combining capacity or quantivalence. Next follows a section on the laws of chemical combination, the methods of analysis and synthesis, the manner in which the law of equivalents and the law of multiple proportions were established, and the explanation of these laws by Dalton's atomic theory. This theory is adopted by the authors as the basis of all their explanations of chemical phenomena, and in this we think they are right: for without insisting on this theory as a matter of absolute certainty, we cannot but regard it as the only theory yet proposed which gives any rational and connected view of the laws of chemical action as established by experiment. There are, indeed, chemists of great eminence, who do not admit it, but hold out expectations of much more satisfactory explanations founded on dynamical views of chemical action. But these views have not yet been sufficiently developed to form a connected theory, and meanwhile we must make what we can of the theory of atoms, which, after all, is not necessarily inconsistent with any dynamical laws, or in other words, with any relations of matter to heat and electricity, that future experiment and observation may develop. There are, indeed, some philosophers who would have us believe in motion without matter, or in other words, in the movement of nothing at all; but this is high transcendental ground, on which we must humbly confess our inability to tread.

The consideration of the volume-relations of gases in combination, as established by Gay-Lussac, leads to the statement of Avogadro's law, according to which *equal volumes of all gases contain the same number of molecules*. This the authors rightly put forward as a hypothesis, the truth of which—like that of the law of gravitation—must be established by its accordance with the whole range of observed phenomena: for as such it must be received by the ordinary student, who is scarcely prepared to understand the manner in which it may be shown to follow as a necessary consequence of the kinetic theory of gases. An exposition is then given of the physical properties of gases, the continuity of the liquid and gaseous states, as demonstrated by Andrews, also a sketch of the kinetic theory of gases; and the chapter concludes with an explanation of the principles of Chemical Nomenclature and Notation.

The remainder of vol. i. treats of the Non-metallic elements. The preparation and properties of these bodies and of their compounds with one another, together with their industrial applications, are carefully described, and excellent figures are given of the apparatus employed for investigation and lecture illustration, also of manufacturing "plant." Especially worthy of notice are the illustrations connected with the manufacture of bleaching powder, sulphuric acid, and coal-gas. The volume concludes with a chapter on Crystallography, copiously illustrated with diagrams. The notation used is that of Naumann, which, for descriptive purposes, is perhaps the clearest and most graphic yet devised.

Vol. ii., part 1 is devoted to the general properties and classification of the Metals, and to the special description of those belonging to seven out of the twelve groups in which they are arranged by the authors. In this part of the work we find the same clearness and accuracy of description and explanation which are conspicuous in the first volume, both in the purely scientific portions and in those which relate to industrial applications. Excellent descriptions and figures are given of the manufacture of alkali and of glass, and of the metallurgy of zinc, copper, lead, silver, and mercury.

The book is well printed, and remarkably free from typographical errors. The few that we have noticed are not likely to mislead, and it is therefore not worth while to specify them, with the exception, perhaps, of one, occurring on p. 38 of vol. i., where it is said that the specific heats of the several elements are "universally" (instead of "inversely") proportional to their atomic weights.

The work, when finished, will afford the most complete systematic exposition of the existing state of chemical science that has yet appeared in the English language; and chemists will look forward with pleasure to the appearance of the second part of vol. ii., which will contain a description of the Iron manufacture, and to that of vol. iii., which will be devoted to the ever-growing subject of ORGANIC CHEMISTRY.

OUR BOOK SHELF

Manuals of Elementary Science—Crystallography. By H. P. Gurney, M.A. (Society for Promoting Christian Knowledge, 1878.)

THIS excellent little manual satisfies a want long felt, for, up to the present time, there was no book in which a general knowledge of the system of crystallography, first developed by Prof. Miller in his "Treatise on Crystallography," 1839, could be obtained. Prof. Miller's treatise and Tract are mainly occupied with the methods of calculation, and require a considerable knowledge of trigonometry. The manual before us aims at doing for these books what the crystyllographic introduction to Naumann's "Mineralogie" does for his "Lehrbuch der Krystallographie." It therefore avoids all the analysis used in the calculation of crystals, and limits itself to explaining the elementary geometrical principles involved in the representation of the faces by indices.

The method of development of systems of symmetry, rendered so familiar to us by Prof. Maskelyne, has been almost necessarily followed, and the author has consequently inverted the usual order of discussion of the different systems, beginning with the Anorthic, that of simplest symmetry, and proceeding through the different types of symmetry up to the cubic system, that of most complex symmetry. In the different systems the characteristic forms are shown to flow so simply from the conditions of symmetry that a moderately bright student ought to be able to deduce them himself after following Mr. Gurney's exposition in the first two or three systems. In his discussion of the rhombohedral system the author follows Prof. Miller. The hexagonal system, of which the rhombohedral is a hemisymmetrical development, is so imperfectly manifested by crystals that its discussion is only of theoretic interest and is unsuited to an elementary manual. In his discussion of merohedrism the author has not attended to the limiting condition, pointed out by von Lang, that the merohedral form should not be identical with the characteristic form of a system of lower symmetry, although here, likewise, he has the sanction of Prof. Miller's authority. The condition, however, is justified by the most recent observations, which have placed most of the minerals displaying such merohedrism in the systems of lower holohedral symmetry. We can heartily recommend the book to students even if they be able to study the more advanced text-books.

LETTERS TO THE EDITOR

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

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

On the Ancient Pitch of Organs

As I am obliged to intermit my researches on organ pitch for a few months, owing to pressure of other work, I wish to make a note of the point to which I have advanced, after consulting many old books, and actually measuring pitch and length of many old organ-pipes, for which I am obliged to the kind politeness of organ-builders, organists, and friends. When my researches are complete, they will appear with details in a paper to be read before the Society of Arts on the History of Musical Pitch, about a year hence. The delay arises from the difficulty of getting information from the Continent.

In England we have no organs older than the Restoration, 1660, as the Puritans smashed all church-organs in 1644-46.

The principle used by organ-builders was to make a certain pipe of the length of some multiple or easy sub-multiple of the standard length of measurement in their own country, and determine the other notes from its tone, according to the mean-tone or unequal temperament in universal use for organs everywhere till 1830, but beginning to be disused in France in 1834 and in England in 1854. There is an apparent exception in St. Jacobi Kirche at Hamburg, where equal temperament is claimed for 1720, when J. S. Bach played on that organ, and possibly in other old German organs. In England I have found the old unequal temperament still existing at St. George's Chapel, Windsor Castle, Kew Parish Church, St. Katherine's, Regent's Park, All Hallows the Great, Upper Thames Street, Maidstone Parish Church, St. Mary's, Shrewsbury, and several other organs which have been very recently re-tuned. The first equally tempered organ by Messrs. Gray and Davison was sent out in 1854.

The pitch note used from 1500 to 1650, at least in Germany, seems to have been F, for which a 13-foot pipe was employed for our F in the 16-foot octave. But the foot varied so much in Germany, being 3 per cent. longer than the English on the Rhine, and in Austria, and much shorter than the English in Central Germany, that the pitch thus determined varies by one to two equal semitones. The Brunswick foot, in 1620, where we have Prætorius's reference, possibly gave a tone of 35 vib. for the 13-foot pipe, an octave below the ordinary violoncello C.

In England Tomkyns (before the Commonwealth) fixes the F as 5 feet, which gives the A as 4 feet, and the double octave of this as 1 foot, and hence comes under the next category. The 13-foot F gives a 13-inch treble C, which, for Rhenish feet, would have a pitch of 425, whereas Handel's A was 423, having a pitch a minor third higher. This minor third constantly recurs. In Hamburg the St. Jacobi organ is a minor third sharper than the St. Michaelis organ, the first being a tone sharper and the latter a semitone flatter than French pitch. And strangest of all, the St. Jacobi organ had formerly one of its stops tuned to the low St. Michaelis pitch.

The old reason for fixing the pitch seems to have been to put the ecclesiastical tones within easy fingering for the organist, without using the chromatic notes (which *Arnold Schlick*, 1512, naïvely says is not convenient for most players), at the same time that they were within easy reach of a baritone voice. This is a point I have not yet worked out completely.

In England the foot-rule seems to have been generally adopted in early organs as the means of giving a standard, and it is not till about Green's time—a century ago—that I find it varied from this by a small fraction of an inch—not exceeding two-fifths.

The pitch of an open metal cylindrical flue-pipe used for the open diapason stop (but not "a show-pipe"), measuring 12 inches from the lower lip to the open end, varies from 472 to 475 vibrations in a second at 60° F. The variations are due to the size of the diameter, the force of wind, the opening at the foot, and the method of voicing. I have known such a pipe raised two vibrations in a moment by a slight alteration in voicing. This is the old standard pitch in England. Varieties depend upon the name of the note which it represents, and the classes of organs which I have met with in books or in reality, have hence been called by me the A foot, B flat foot, B foot and C foot organs.

1. The A foot organ has A 472 to 475. This was *Tomkyns's* pitch, as shown by Sir F. Gore Ouseley in his edition of "Orlando Gibbons," and seems to have been the pitch for which that composer wrote his Church music. It gives the mean tone C 565 to 570. As the French diapason normal is really A 435·875 (as determined originally by M. Cavaillé-Colt, and verified this year by Mr. Hipkins in Paris, by means of a Scheibler 440), this makes Tomkyns's pitch about three-quarters of a tone sharper than French pitch. This is the present existing pitch of St. Katherinen Kirche at Hamburg. The St. Jacobi organ, and also that in the Cathedral of St. Marie, at Lübeck, is a whole tone higher than French pitch. The great Franciscan organ at Vienna, 240 years old and untouched, gives A 460, which is only a semitone sharper than French pitch. These are the sharpest existing organs I have met with. The Franciscan organ is only used for the old ecclesiastical tone singing of the monks. This was also possibly the pitch recommended by *Prætorius* for church organs, the drawing in his book (1618) giving the B pipe one Saxon foot in length, with strong pressure of wind, and the Saxon foot being 7 per cent. shorter than the English.

It is as well to mention in passing that the tones and semitones here spoken of for measuring purposes, if not otherwise qualified, are *equal* semitones, and that, near enough for such purposes, an equal semitone and tone higher have 6 and $12\frac{1}{4}$ per cent. more vibrations, and thus a quarter and three-quarters of a tone higher have 3 and 9 vibrations more per cent. For unequal or mean tone temperament, a *small* semitone has $4\frac{1}{2}$, a *large* one 7, and a tone 12 per cent. more vibrations. These numbers are very convenient for rough estimations.

The old French foot is 6 per cent. longer than the English, hence the one-foot pipe will be a semitone lower than the English, or about 443 to 446 vibrations. I have not met with a case of a French organ with A 443, or the one-foot pipe on A. But *Mersenne*, 1636, places the one-foot pipe on G, and this gives mean-tone A 496 and C 593. Now the St. Jacobi organ had actually A 491 and C 584 (equal temperament, making the C lower), as determined by forks tuned to the pitch and then measured. Hence, Mersenne's pitch, which even M. Cavaillé-Coll considered must be a mistake, actually exists at the present day.

2. The B flat foot organ, or B flat 472 to 475. This gives A 442, C 528, on the mean-tone temperament, that is, actually the pitch desired by the Society of Arts and not attained. This pitch was used by *Thomas Harris* in the Worcester Cathedral organ of 1666, by *Berhard Schmidt* (or Father Smith, as he has been called), in Durham Cathedral, 1683, Hampton Court, 1690, St. Paul's Cathedral, 1694-7, Trinity College, Cambridge, 1708, as I have ascertained, and probably in all his organs. It seems to have been occasionally used by the *Jordans*, who seem also to have built an A foot organ; but my inquiries are not yet complete. It is the favourite pitch of modern English organ builders, as I have ascertained by measuring the pitch-pipes of seven of the principal builders in London, which vary from C 524 to 528, at 60° F., to which all pitches are reduced.

3. The B foot organ, or B 472 to 475. This gives in England A 422 to 425, and C 506 to 512. This pitch was in general use, from at least 1700 to 1820, over England and over Germany. I found it in *Renatus Harris's*, All Hallows, Barking, 1675-7; St. Andrew Undershaft, 1696; and St. John's, Clerkenwell (date unknown); in *Harris and Byfield's*, St. Mary's, Shrewsbury; in *Byfield, Jordan, and Bridge's* two Great Yarmouth organs, 1733-40; in *Byfield and Green's*, St. Lawrence, Reading, 1771, and St. Mary's, Islington, 1772; in *Glyn and Parker's*, All Hallows the Great, Thames Street, 1749; in *Schnetzler's*, German Chapel Royal, St. James's Palace (date uncertain); in *Green's*, St. George's Chapel, Windsor, 1790; Winchester College Chapel, 1780; St. Katherine's, Regent's Park, 1778; and Kew Parish Church (date unknown). Glyn and Parker built the organ which Handel gave to the Foundling Hospital, 1750, and Handel, after conducting a performance of the "Messiah" there, in 1751, left his tuning-fork behind him. This fork is now in the possession of Rev. G. T. Driffield, Rector of Bow, and shows A 423, which is presumably the pitch of that organ. Mozart's clavier-maker, Stein, at Vienna, 1780-90, used a fork one vibration lower, A 422, which was undoubtedly the pitch of Haydn and Beethoven, and hence of Church music generally. It is a quarter of a tone flatter than French pitch. This was the pitch used when the Philharmonic Society was started in London, 1813, and was retained to 1826. Silbermann's organ at the Roman Catholic Church, Dresden, was about a comma flatter, or A 415.

4. The C foot-organ, or C 472 to 475 and A 495. The only instance known to me in England is Trinity College, Cambridge, as recorded in 1759 by the celebrated Dr. Robert Smith, its master, in his "Harmonics." But this was after its pitch (which was originally that of a B flat foot-organ) had been lowered a mean tone, by shifting the pipes, which, as he tells us, made it agree with the Roman pitch-pipes of 1702. But the French foot being a semitone flatter than the English, the Versailles B foot-organ (1786) had a pitch of A 396, C 474, as shown by the fork preserved in the Conservatoire in Paris, and hence precisely agreed with the altered Trinity College organ and the Roman pitch-pipe. Delezenne, in 1854, was fortunate enough to find an old dilapidated organ at the Hospice Comtesse, near Lille, which gave C 448, as near as he could measure, agreeing well with C 443, the calculated pitch of the French C foot organ.

This seems to be the first attempt at systematically finding the pitch of organs. The pitch of the pipes was in all cases found, when they could be actually heard, by beats with tuning-forks made for me, to the extent of an octave, on the basis of Scheibler's 256, 435, 440 (which I have reason to believe perfectly accurate), by Valantine and Carr, 76, Milton Street, Sheffield, and I have also reason to believe that these latter forks are not more than half a vibration wrong with Scheibler in any case. But before my complete paper is ready I shall have verified them by eighteen other forks of Scheibler now being very carefully copied at Crefeld. To hear the beats I stand thirty or forty feet away from the organ, and hold the fork over a resonance jar tuned to its pitch by pouring in water. The bellows is first filled, and no pumping is allowed during the ten seconds that I count. The beats are beautifully distinct, and I consider the result to be correct within one-fifth of a vibration.

The correction for temperature, which is most important (as at C 500 it is more than half a vibration per degree Fahr., to be added for higher and subtracted for lower temperature), is found by the following rule:—Add four per cent. to the number of vibrations observed, divide result by 1,000, and multiply by the number of degrees required. I have thus harmonised measurements made between 73° and 45° F.

The rule for finding pitch from measurement was given by M. Cavaillé-Coll (*Comptes Rendus*, 1860, p. 176), and, reduced to English measures, is as follows:—

Let L be the length, in English inches, of an open flue cylindrical metal diapason from the lower lip to the open end, and D its internal diameter, also in inches. The latter measure is frequently difficult to make, on account of the jagged, or "coned," or compressed, extremity. Then use the outer circumference, by wrapping a piece of paper round the pipe where it is truly circular; calculate the diameter as $\frac{7}{22}$ circumference, and throw off $\frac{1}{20}$ inch for the thickness of the pipe, to find D, which has to be known with considerable accuracy.

Let V be the number of double vibrations in the pipe, at 60° F., then

$$V = \frac{20080}{3L + 5V}.$$

I tried this formula with a whole octave of pipes at Green's St. Katherine's organ, and found that the error rarely reached one comma (or 1 in 80), which many persons can't hear, and never reached two commas (or 1 in 40). Since a quarter of a tone is 3 per cent. (or 1 in $33\frac{1}{3}$), and a semitone is 6 per cent. (or 1 in $16\frac{2}{3}$), this gives a far better knowledge than we can obtain by ordinary estimation of ear, without counting beats by measured forks.

It would confer a great favour on me if any one could give me these dimensions of old, unaltered organ pipes for the pipe which is nearest to twelve English inches in length, anywhere, especially abroad, naming the place and the note, and, if possible, date and builder, or would point out any existing unaltered old organs. ALEXANDER J. ELLIS

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The Formation of Mountains

MR. ALFRED R. WALLACE asks one of our "great" physicists to enlighten us about the possibility of the interior of the globe "cooling more rapidly than the crust." If he will turn to a chapter on Conduction in such a work as Maxwell's "Theory of Heat," he will find an explanation of the principle. At p. 247 is a passage especially relating to the loss of heat by the earth.

But perhaps even a little physicist may help our great naturalist as the mouse did the lion.

In the first place it is of course understood that whenever it is said that "the interior of the globe cools more than the crust," it is not meant that it ever becomes cooler than the crust, but only that the interior, from age to age, goes on getting cooler than it was before, whilst the crust keeps at nearly a constant temperature.

An illustration, which I think gives a good idea of this process, may be taken from the dispersion of a crowd of persons in the street. Suppose each person to represent a certain quantity of heat. Then the number of persons in any space may be considered to represent its temperature, so that the crowded part will represent a very hot space. As the people disperse they move off the more quickly the further they get from the dense mass.

Now draw two lines near together across the street at some small distance from the densest part of the crowd, and let the space between these two lines represent the crust of the earth, while the space occupied by the crowd represents the earth's interior, and all beyond the outer line represents infinite space. Then the number of people passing outwards between the two lines at any particular moment will represent the quantity of heat in, and so the temperature of, the crust. At the same time the number of persons remaining in the crowd will represent the quantity of heat in, and so the temperature of, the interior. Then it will be obvious that as the crowd disperses the number of persons at any one time between the lines may continue about the same (although the individuals will be changed), whilst those in the central crowd become fewer and fewer. This illustrates how the temperature of the crust may continue nearly uniform in spite of the continued loss of heat from, and cooling of, the interior.

I believe that I have long ago proved that the mere cooling of a solid earth would not give the amount of contraction needed to account for the observed inequalities of the surface, and I surmise that a diminution of the earth's volume has been caused by the escape of steam and gases from volcanic vents during past ages. This view has, however, attracted more attention in America than at home. O. FISHER

Harlton, Cambridge, December 13

Magnetic Storm, May 14, 1878

I AM inclined to think that Mr. Mance's observations (vol. xix. p. 148) upon the earth currents observed at Kurrachee must be incorrectly reported. To agree with the observations in China, Stonyhurst, Greenwich, and Haverfordwest, they should have commenced at 4 A.M. on May 15, and terminated at 5 P.M. on the same day (Kurrachee time).

It is a pity that electricians do not record these currents in absolute units. To say that the current was equal to fourteen Daniell cells means nothing unless the resistances present are also given. If an earth current is observed upon a cable it is easy to reproduce this current upon the same galvanometer with a known resistance and a known electromotive force, and then to express its value in webers or milliwebers. Thus if at Kurrachee 50° were noted on a galvanometer, and one Daniell cell reproduced this deflection through a total resistance of 125 ohms, then the current would be equal to $\frac{1}{125}$, or ·008 weber or 8 milliwebers, a magnitude which every electrician would understand. Moreover, if the length, resistance, and general direction of the cable or wire were given, as well as the direction of the current itself, the difference of potential of the earth at the two ends would be known. This if the cable were 246 miles long, and lay due east and west, and its resistance were 5ω per mile, then in the above case

$$\frac{E}{1230} = \cdot 008$$

$$E = 9\cdot 84 \text{ volts},$$

which is the difference of potential of the two ends.

If simultaneous observations were made in this way at numerous stations on the earth's surface, we should be able to plot out the distribution of potential on the globe, and arrive at some better knowledge of the cause of earth-currents than we have at present. W. H. PREECE

December 20

The Derivation of Life from the North

ATTENTION has been called by the President of the Royal Society to the labours of Mr. Dyer, as pointing in the case of plants to the conclusion that their various forms have been developed and dispersed from the north. I presume it is recognised that similar conclusions have been arrived at by Mr. A. R. Wallace in the case of animals. Mr. Wallace points to the palæarctic region as the great centre of their development or creation. On reading "The Geographical Distribution of Animals" when it first appeared, I was so much struck with the evidence adduced, that I was tempted to write and ask him if his work might not be said to occupy the following position in the history of unravelling what was formerly the mystery of geographical distribution. Mr. Darwin and others, including Mr. Wallace himself, had found a causal nexus in the case of islands, had shown that the faunas of islands had been derived from that of the nearest mainland, and in a character and degree varying concomitantly with the degree of their present disconnection therewith. They had thus completed the necessity for "centres of creation." Did not "The Geographical Distribution of Animals" afford the requisite evidence for carrying this commencement to its logical conclusion: for showing that in their turn the great continents themselves, or, more precisely, those which are outlying to the central mass (which is in the north, around the Pole), have a similar dependence, and have borrowed their own faunas from that northern mass, in a character and degree proportional to the dates and degree of their connection or separation from it, the islands might then be said to be the satellites, and the great zoological regions the planets of this system, all having borrowed their life directly or indirectly from a *single* "centre of creation."

To render this still clearer to my own mind I had a map of the world designed on a polar projection, the northern hemisphere being projected to somewhat beyond the southern tropic. By this means the manner in which the land surface of the globe is built around the pole is clearly seen, and the extremities of America, Africa, and Australia, extending into the great oceans of the world, are embraced, or nearly so. When the subdivisional regions (zoologically) of each of these great projections, and of the whole, are marked in colours, a succession of zoological strata, to speak rather inaccurately, appears. By carrying an ideal section from the supposed centre of creation in the north through either of these three great extremities, and from thence to the nearer, and afterwards the more remote, dependencies of those extremities (remote not in point of actual distance, as in degree of connection), we pass in each case through zoological strata of different types, until we arrive at those where no land-mammals are to be found at all. And this succession in space, as evidenced by geography, corresponds in a rough way with the succession in time, as revealed by geology. 1. As we recede in distance we meet with increased dissimilarity. 2. This dissimilarity partakes of a recession in type. 3. Some of these geographical districts seem to have their counterparts in geological periods. The Ethiopian region, as Mr. Wallace shows, presents us with the exiled miocene fauna of Europe in the most striking manner. Eocene forms may be seen in its dependency of Madagascar, or in the West Indies. Highly isolated Australia with its marsupials, &c., appears as if it were still in the secondary age. Oceanic islands, such as New Zealand, with a more beautiful climate, and more extensive surface than Great Britain, give us no land mammals at all. In others the reptiles "possess the land."

Mr. Wallace's plan is an excellent illustration of the comparative method, and shows how a careful classification leads to the solution of historical questions connected with the causes of that classification. Those causes are in this case comprised in the inference that a succession of waves of life has been propagated from the north, not all of which have had an equal extension, nor all encountered similar modifying circumstances.

If these inferences are not correct, perhaps Mr. Wallace would kindly set me right. J. W. BARRY

1, Duncombe Place, York

Glaciation of the Italian Lakes

HAVING spent some time lately on the border-land between Switzerland and Italy, it has occurred to me that a note on some glacial features of that region may prove of interest to the readers of NATURE.

The Lake of Lugano is a rock-basin. I believe it to have been scooped out by the glaciers which have formerly descended from the Alps. Of this there is abundant proof. The crystalline rocks in their lower reaches possess the easily recognisable outlines of *roches moutonneés*, but the stratified mesozoic rocks have lost these characters. Above Lugano and Agno these features are very well marked, and in these localities striation is tolerably frequent, the direction of the striæ being southerly. Along both sides of the southern extension of Mount St. Salvatore to Moreate, striæ can be seen in a few places near the lake-level, and the same is the case on both the Pianbello and Generoso shores. At the southern extremities of the lake are abundant moraine-mounds. Erratics are also present, most being gneissose or granitic, but a few have fallen upon the moving ice from nearer localities, as they are of dolomite. The moraine masses are cut through by the northerly flowing streams, but, after passing the parting between the waters flowing towards Lake Lugano and those running into Lake Como, there is the appearance of great destruction of the moraines. Unfortunately I had a mere cursory glance down the Val della Tresa, through which the drainage of the lake flows to Lago Maggiore. It has often been remarked that in this South Alpine lake district, the *débris* left by the glaciers is exceedingly small when contrasted with similar regions north of that mountain chain. To every observer, indeed, this must be obvious. Two reasons have been adduced in explanation—1. That the glaciers have been smaller from the cold of the glacial period not having extended over Italy, as indicated by the absence of the reindeer and other sub-arctic species from the drift; 2. That denudation has been enormous. In spite of the southern exposure it hardly appears probable that there could have been sufficient difference in the size of the northern and southern glaciers to cause this contrast, as long as the glacial period lasted, for the vapour-laden winds from the sea must have deposited much of their moisture on the southern slopes as snow. But, as the ice-age passed away, the southern aspect of the Alps would doubtless be freed from its influence sooner than the northern, and hence, while snow and ice reigned in the latter area, the regions south of the mountains underwent denudation, probably, for a vast period. I would therefore suggest that the comparatively small amount of glacial *débris* on the southern side of the Alps may arise from that region having been exposed for a much longer time to sub-aërial waste, and in particular to the floods caused by the more rapid melting of the snows on the southern slopes of the mountains. GEO. A. GIBSON

10, Old Square, Birmingham, December 16

Electric Lighting

IN the *Proceedings* of the Philosophical Society of Manchester Mr. Wilde has described a new electric lamp, in which the carbons are placed parallel to each other, as in a Jablochkoff candle, but without any insulating material. It may not be apparent to all why the arc always locates itself at the extremity of the pair of carbons, and this, as Mr. Wilde observes, whether the lamp be erect or inverted. The explanation is that the current in each carbon repels the electric arc exactly as the current in the mercury-troughs repels the connecting cross-wire in a well-known experiment of Ampère. In Wilde's candle we have two fixed conductors, the carbons, and a movable conductor, the arc. Since the current in the arc and the current in either carbon are one from the other towards the point of junction of that carbon with the arc, the arc must be repelled by the carbon. One cannot but be surprised that Ampère's experiment did not suggest the discovery made by Mr. Wilde, that the insulating material might with advantage be omitted in Jablochkoff's candle. J. HOPKINSON

4, Westminster Chambers, December 12

The Reproduction of the Eel

THE time of spawning and the differences between the sexes of the common eel has been hitherto unknown. Last year for the first time in this country eels (*Anguilla bostoniensis*) containing eggs were found, December 31, by Mr. Edwards, at New Bedford, Mass., as stated by Mr. Putnam in the *Proceedings* of the Boston Society of Natural History. From observations on eels brought me at Providence, R.I. it appears that eels spawn here in salt or brackish water from October to the end of November, as I have found several eels containing eggs which seemed nearly ripe, the ovary being full and large. To-day on examining the testis of a male eel 17 inches long, the mother-cells and sperm-cells were found, the latter numerous and lively, from $\frac{1}{3000}$ to $\frac{1}{20000}$ inch in diameter. The eggs were white, just large enough to be distinguished by the naked eye, measuring a little less than $\frac{1}{3}$ mm. in diameter. The females are larger than the males, with the belly white, while the males are easily distinguishable by the dark bellies and a narrow silvery or golden median stripe. A. S. PACKARD, Jun.

Laboratory of Brown University, U.S., November 27

AERATED BREAD

SOME remarks upon aerated bread which were made by Dr. B. W. Richardson at the recent general meeting of the company working Dr. Dauglish's patents require examination. Now it is noteworthy how imperfect our knowledge of the chemistry of the mill-products from the cereal grains still remains. Without such knowledge we are not in a position to dogmatise as to the exact nutritive values of different kinds of bread. As I pointed out in a previous article on "Real Brown Bread,"[1] the statement that whole wheaten meal, bran, pollard, &c., contain more nitrogen, and therefore more flesh-formers than fine flour, rests upon no certain basis of analytical fact. And if it were proved that all the nitrogen of the most nitrogenous of mill-products does really exist in what are called albuminoids or flesh-formers, we cannot ignore the presence of much indigestible fibrous material in bran and pollard, material which is not only non-nutritive itself, but which locks up in an inaccessible form much of the real nutrient substances associated with it. Thus a sample of wheaten bran, or rather, fine pollard, may refuse to give up to the boiling dilute acid and alkali used in fibre-determinations more than six-sevenths of its nitrogenous matter; and it can hardly be expected that the secretions of the alimentary canal will prove successful in withdrawing a larger proportion. Indeed, the analysis of the residues of such foods after having been submitted to the digestive process, has confirmed this expectation in the case of the human subject. Moreover, while a not inconsiderable part of the albuminoid matters of the outer coverings of the grain thus escapes digestion from its mechanical condition, there is good reason to believe that a further portion remains unabsorbed, by reason of the rather hurried passage of the branny particles through the digestive tract. And the same causes which operate to prevent a part of such flesh-formers as exist in the bran from being utilised, affect also and in a similar way the useful mineral substances which abound in the coarser mill-products, as well as the oil or fat which they contain.

Now let us see what are the distinctive advantages claimed for Dr. Dauglish's bread-making process by Dr. Richardson. It yields a bread which is said to be "perfectly clean, perfectly wholesome, and completely nutritious." As to the perfect cleanliness of this mechanical process for making bread there can be no question; it is immeasurably superior to the barbarous and old, but, as Dr. Richardson remarked, *not* "time-honoured system of kneading dough by the hands and feet of the workman." And we may agree, with almost equal confidence, in the statement that aerated bread is perfectly wholesome. The stream of pure water charged with carbonic acid gas vesiculates the dough, which has required neither alum, nor blue vitriol, nor lime-water, to check the irregular fermentation, and neutralise the sourness of mouldy or otherwise damaged or inferior flour. But, on the other hand, the adoption of the aerating process does not of itself necessarily exclude *all* adulterations of the bread: materials to whiten the loaf and to cause the retention of a larger percentage of water may still be used. As to the small loss of nutrient materials involved in the ordinary fermentation of dough, it hardly merits consideration. Perhaps Dr. Richardson alluded to it because it gave him an opportunity of having a fling at his old enemy, alcohol, of which it has been found that a newly-baked loaf, made by means of yeast, contains about 0·25 per cent. So that a man who eats twenty quartern loaves has therein consumed an amount of alcohol which is commonly contained in one bottle of port! But if there be no really serious loss of starch by conversion, first into sugar, and then into carbonic acid gas and alcohol, there can be no doubt that a number of altered products are present in a fermented loaf, and that these are less abundant and less variable in nature in aerated bread. But the presence in fermented bread of larger quantities of sugar, gum, and soluble starch than are found in aerated bread is not usually a disadvantage so far as the digestibility of the loaf is concerned. It is rather to the production of lactic acid and of nitrogenous ferments by the use of yeast or leaven that we should attribute the uncertain value of ordinary bread. The quality of the yeast, too, must not be left out of consideration, as some of our home and of our imported supplies are by no means of a satisfactory character.

[1] See NATURE, vol. xviii, p 229.

I may now discuss the third meritorious feature which Dr. Dauglish regarded as a conspicuous advantage possessed by his process—a view which is now endorsed by Dr. Richardson. We are told that "in brown bread, which contains the envelopes or coverings of the whole grain, the flesh-formers amount to 10 per cent., and that, while it is possible to live upon brown bread, without any other food, the health suffers, and death finally ensues, on white bread alone." "The flesh-formers in white bread amount," we are told, "to 7 or 8 per cent." Now by the use of the aerating process, a light and palatable loaf may undoubtedly be readily made, either from the entire meal of crushed wheat, or from such meal from which nothing but the long or coarse bran has been excluded. Such a result cannot be secured with any degree of certainty when yeast is used. This is quite true. But it is not by any means an ascertained fact that brown bread or whole meal bread contains a larger proportion of flesh-formers than white bread from the same wheat. If we make no deduction on account of insoluble and inaccessible flesh-formers in the coverings of the grain, we shall have to make a very considerable deduction on account of nitrogenous matters which are not really albuminoid or flesh-forming, and which are extensively present in the coats which form the main substance of pollard and sharps. We can hardly attribute flesh-forming properties to the "diastase," "cerealin," and other obscure nitrogenous ferments which we know to abound in the coarser mill products. Now these are the very bodies which the aerating process enables us to include in our bread without making it heavy, the very bodies whose presence contributes most largely to raise the percentage of assumed flesh-formers, from the 7 or 8 per cent. in white bread to the 10 per cent. in real brown bread. Now, although we have no absolute method of distinguishing between the true flesh-forming nitrogenous bodies in food-stuffs and those whose value is at the best problematical, yet the "carbolic acid process" which I devised in 1873, and which has latterly been attracting a good deal of attention, furnishes some instructive results when applied to the various mill products of the cereals. For instance, I found (in 1875) that pearl barley contains at least 92 per cent. of its nitrogen in the form of genuine coagulable albuminoids, but that "fine dust," consisting of the richest parts of the barley grains, though it contains three times as much nitrogen as pearl barley, does not contain twice as much unmistakable flesh-forming substance. A similar observation was made on comparing the coverings of the wheat grain with the flour—93 parts out of every 100 of nitrogen in ordinary wheaten flour being certainly in the form of flesh-producers, while the proportion in the entire bran sinks to an average of 72, and sometimes touches a much lower figure. The same result has been obtained with a large number of food materials, in some succulent vegetables the albuminoid nitrogen not exceeding one-fifth of the whole.

It is evident, from the preceding considerations, that whatever be the nutritive or medicinal merits of whole-meal bread, it cannot be definitely stated to be a much richer food so far as flesh-formers are concerned, than white bread made from the flour of the same sample of wheat grain. If we deduct from the supposed 10 per cent. of flesh-formers in whole meal bread $\frac{1}{5}$ or 2 per cent. because of the existence of non-albuminoid nitrogen in the branny particles, and because of the indigestible condition of a small part of the true albuminoids, we leave but 8 per cent. a proportion which does not greatly exceed the 7 or $7\frac{1}{2}$ per cent. usually attributed to white wheaten bread.

On some future occasion I may have a few words to say, should the Editor of NATURE accord his permission, as to the bread question treated of in the "Dietaries of Prisons' Report," lately presented to Parliament.

A. H. CHURCH

ON THE COMBUSTION OF DIFFERENT KINDS OF FUEL

OUR attention has been called to this subject on perusing a paper read before the American Institute of Mining Engineers. In this communication it is attempted to be proved, that the manner in which charcoal unites with oxygen in iron furnaces, explains its alleged superiority over mineral coal.

The question appears to us to involve considerations outside the mere technical limits of iron smelting. We therefore submit the following remarks, which have been prepared at our request, by a gentleman whose important researches on the economic application of fuel in the smelting of iron are known to all the world.—ED. NATURE.

Prof. John A. Church, of Columbus, states, in a recent publication, "that it is a well-known fact that under similar conditions a ton of pig-iron can be made from any ore with less fuel when charcoal is used than when coke or anthracite is employed for heating." He then discards, as untenable, all former explanations of this doctrine proceeding either from scientific or practical men, and maintains that "the highest carbon duty is given by the fuel which withdraws the most oxygen from the blast in a given time."

The professor afterwards proceeds to explain that charcoal, being highly porous, presents a more extended surface to the action of the oxygen than the more compact forms of carbon as it exists in coke or anthracite. He then considers the effect of what is designated in the paper as diluted oxygen on its way to the upper regions of the blast furnace.

In this latter statement with regard to free oxygen, it seems to be overlooked that whatever difficulty this gas may have in attacking dense carbon, there can be no reason why carbonic oxide, generated from coke or anthracite, should not be as easily burnt to the state of carbonic acid as the same carbonic oxide is burnt when formed by the combustion of charcoal.

If it could be shown that all the carbon consumed in the hearth of the blast furnace were in the condition of carbonic acid, then, no doubt, free oxygen might be expected there, but in the presence of so vast a volume of inflammable carbonic oxide in that region such a condition of things is scarcely possible.

It is unnecessary, however, to occupy space with merely speculative matter when actual analyses inform us that in a furnace using coke, not only is there no free oxygen at a very short distance from the tuyeres, but that there is no carbonic acid. In point of fact the whole of the gaseous carbon exists there in the form of carbonic oxide.

Admitting, then, the highest carbon duty to be dependent on the law laid down in the paper, it is clear that at a distance of two or three feet from the point where the blast enters, the conditions in this respect of a furnace burning charcoal or coke are the same.

Prof. Church quotes, in the course of his observations, the results obtained from two furnaces, one using charcoal the other anthracite, and from these he infers that there is an "inherent difference" between the two kinds of fuel of 900 lbs. per ton of iron in favour of charcoal. This figure is a mighty one when it is considered that the charcoal actually consumed for each ton of pig is under 1,800 lbs.

Somewhat inconsistent with this conclusion is the instance quoted of a Lake Champlain furnace, where the consumption of anthracite has been reduced nearly 25 per cent. below that quantity which formed the basis upon which the "inherent difference" was estimated.

The paper further recommends as a reasonable consequence of the superiority of charcoal over coke, that iron manufacturers should, as far as possible, imitate the physical structure of the former in the produce of their coke ovens. Our "perversity" in preferring hard to soft coke is condemned, and we are invited to put reason in the room of traditions which have not "one recorded fact to sustain them."

My attention, as an iron-smelter, was drawn to the very small consumption of charcoal in a certain instance quoted by Prof. Tunner, of Leoben, and being unable to reconcile his experience with my own observations, I obtained from this respected authority further confirmation of the exactness of his figures. Along with this he kindly repeated his analyses, including one not given in his original paper, viz., that of the gases taken on leaving the furnace, in other words, after they had *completed* their work.

Other investigations prevented my examining the question at the time with that attention it deserved, and I concluded, upon other grounds than those advanced by Prof. Church, that there was some actual difference in the mode in which charcoal effected the reduction of at least *some* ores.

In this I now think that I was mistaken, and I will endeavour to prove that the author of the paper commits a grave error in comparing two blast-furnaces working upon different kinds and quantities of ores and flux used in the production of pig-iron. I will also attempt to demonstrate that the use of hard coke, held to with such "singular perversity," can be maintained by "reason" as well as by "tradition."

Carbonic oxide, as is well understood, is the chief reducing agent in the smelting of iron, and for our present purpose we may regard it as the only substance which deprives the metal, as it exists in ores, of its oxygen. The carbonic acid which is formed by this deoxidising action possesses a tendency, at certain temperatures, diametrically opposed to that of the lower oxide of carbon—it reoxidises the iron.

This conflicting property of the carbonic acid is kept in check, in iron smelting, by the presence of carbon in the form of its lower oxide, being about double that which is present in its highest form of oxidation.

When I came to examine the ratio in which the carbon in these two conditions existed in the escaping gases, as described privately by Prof. Tunner, I observed that they agreed very nearly with that which I imagine constitutes, in furnaces using coke, a state of equilibrium between the gases and the ore in the process of reduction.

Now no one will dispute that when two pounds of carbon are burnt to the state of carbonic oxide and one pound to the form of carbonic acid the quantity of heat is the same, from whatever source the carbon itself may be derived.

If, after having determined a perfect resemblance between the extent to which the fuel has been oxidized, in the furnace, when using charcoal and using coke, we find that there is no difference in the amount of heat wasted, it is time to consider whether the pig-iron itself, obtained by means of these two kinds of fuel, may not require very different quantities of heat for its production.

It may be here stated that the Cleveland furnaces in England, with which I compared those of Carinthia, were consuming 21 to 22 cwts. of coke against 14 or 15 cwts. of charcoal used in the latter per ton of iron. The weight of the slag produced in the former was very much the larger of the two. The additional heat absorbed in fusing this substance was further augmented by that required for the decomposition of silica and phosphoric acid, the bases of which are found more largely in Cleveland than in Carinthian pig. The diminished quantity of fuel burnt in the charcoal furnaces reduces very greatly the weight of gases, and this consequently lessens the amount of heat carried away through their instrumentality.

The following figures exhibit the number of centigrade heat units required, according to my calculations, per unit of each kind of pig-iron referred to.

	Cleveland iron.	Carinthian iron.
Evaporation of water in fuel	15	15
Reduction of oxide of iron and dissociation of carbonic oxide	1730	1730
Expulsion of carbonic acid from carbonate of lime	250	25
Decomposition of this carbonic acid	265	25
,, water in blast	130	75
,, silica and phosphoric acid	200	50
Fusion of iron	330	330
,, slag	830	330
Loss by radiation	270	200
Carried off in gases	440	210
	4460	2990

Now when the proportions between the heat requirements and fuel-consumption come to be examined, we find that the two sets of numbers agree almost exactly; for—

$$4460 : 2990 :: 21 : 14{\cdot}08$$

Since writing on this question, nearly seven years ago, I have had frequent opportunities of examining the performance of charcoal-furnaces in Sweden, Italy, and Spain, as well as in Virginia, Kentucky, Ohio, Missouri, and Michigan, in the United States. I soon became aware that different kinds of ores demanded very different quantities of charcoal for their treatment. In some cases the weight was nearly as low as that consumed in Carinthia, in others it was quite as much as if coke had been the fuel employed. I failed on every occasion to detect any circumstance beyond the change in the ore itself, or in the nature of the iron it afforded, to which I could ascribe the difference in question. All doubt, however, on the supposed difference of value between the vegetable and mineral fuel was dispelled on receiving from the owners of works in the North of Spain and in Virginia, statements from their books exhibiting the smelting of the same ores with the two kinds of fuel. The difference in these was often in favour of the coke, but such difference as existed could easily be accounted for by fluctuations in the amount of foreign matter in the fuel, or in small changes in the quality of the ores.

It only remains for me briefly to mention the grounds upon which, for chemical reasons, hard coke may be superior to that softer variety recommended by Prof. Church.

Carbonic acid, as is pointed out in the paper by this author, possesses the power of dissolving carbon, and every pound so dissolved involves a double loss. There is a reduction of temperature where the action takes place, and there is a pound less of fuel remaining to be burnt at the tuyeres. I ascertained, however, by direct experiment, that all forms of carbon are not equally easily affected by carbonic acid, that in hard coke being capable of resisting the solvent action much better than the same carbon as it occurs in soft coke. The importance, therefore, of employing our fuel in that condition where it is least susceptible of being oxidized in the region of the furnace where its combustion is useless, need not be further insisted on.

I am ready to admit with Prof. Church, that the traditional opinions of practical men have often required correction from scientific investigation, but upon the present occasion, so far as I have been able to judge, the ironmaker and the chemist agree in the relative value of hard and soft coke. I. LOWTHIAN BELL

PAPUAN HERPETOLOGY[1]

IN this memoir, which forms a portion of the thirteenth volume of the *Annals* of the Civic Museum of Genoa, we have by far the best account of the herpetology of

[1] "Catalogo dei Rettili e dei Batraci raccolti da O. Beccari, L. M. D'Albertis e A. A. Bruijn, nella sotto-regione Austro-malese." Por W. Peters e G. Doria. *Ann. del Mus. Civico*, Genova, vol. xiii., 1878.

the great Papuan subregion, that has hitherto appeared. At the same time the authors have not attempted to make a perfect faunistic work of it. Only such species as are represented in the collections made by the Italian travellers, Beccari and D'Albertis, on their various joint and several expeditions, and in the collections sent to Genoa, by Herr Bruijn of Ternate are enumerated in the list. On the other hand the mass of these collections is so great—consisting of 3,000 examples from 44 different localities, and the series thus brought together is so much more nearly perfect than that which any other authors have had before them—that the result has been to give us an excellent idea of the general character of the reptiles and batrachians of this division of the earth's surface. Let us, therefore, look through the pages of Messrs. Peters and Doria's excellent memoir, and see what general views we can obtain from it as to the peculiarities of this little-known branch of the Papuan fauna.

In the first place, land-tortoises are few in the Papuan sub-region. Our list contains but two species, one from Amboina and Celebes, the other from the southern extremity of New Guinea, and very doubtfully distinguishable from an Australian species.

Crocodilians are also scarce. The series contained examples but of one species—*Crocodilus porosus*—which extends from India into Northern Australia.

Of lizards, besides the monitors, of which genus not less than 16 species are contained in the list, the great mass are Skinks, Geekos and Agamids. As in Australia the Skinks are especially numerous, upwards of 30 species being represented in the series, of which 8 are now described for the first time.

The ophidians are also well represented in the Papuan fauna, the series examined by our authors affording examples of 54 species. Amongst the Boas *Liasis* and *Chondropython* are characteristic Papuan forms, whilst the Australian *Morelia* also extends into New Guinea. But no greater sign of the essential unity of the Australian and Papuan faunas can be shown than the presence in New Guinea of the three Elapine genera, *Diemenia, Pseudechis* and *Acanthophis*, all characteristic forms of the Australian continent. At the same time the presence of an *Ophiophagus* in New Guinea, proves that an intruding element from the north has reached thus far.

About 20 batrachians close the list, amongst which *Limnodytes* and *Hyla* seem to be the most prevalent genera. The Australian *Pelodryas cærulea* is abundant all through the sub-region. A new and very singular form obtained by Beccari in the interior of New Guinea, near the river Wa-Sampson is characterized as *Sphenophryne cornuta*, and a second, obtained, by the same explorer on Mount Corfak as *Xenobatrachus ophiodon*. The latter shows some points of affinity to the Australian *Myobatrachus*.

Seven excellent lithographic plates illustrate some of the rarities described in this memoir, and two maps are added to show the exact localities where the collections were found. The maps we see with great pleasure, as in our opinion no zoological work of a faunistic character can in these days be considered complete without such an appendage.

THE METEOROLOGICAL SOCIETY LECTURES

A COURSE of six lectures on meteorology was recently delivered under the auspices of the Meteorological Society at the Institution of Civil Engineers. The purpose of these lectures, we believe, is to spread a knowledge of the known facts and principles of meteorology, and in this useful mission we cannot but wish the Society every success. We here give abstracts of two of these lectures, by Mr. J. K. Laughton and the Rev. W. Clement Ley. The subject of Mr. Laughton's lecture was—

Air Temperature, its Distribution and Range

After calling attention to the importance of climatic knowledge the lecturer dwelt on the fact that though all heat as affecting climate emanates directly or indirectly from the sun, temperatures have but little relation to latitude except when the distances are very great. He illustrated this by reference to isothermal and isabnormal maps, and went on to speak in some detail of the several causes of the disagreement between isotherms and parallels of latitude. Locally there is a very great difference between the temperatures of adjacent localities on account of the sunny aspect or sheltered situation of some as compared to others, as is found in an extreme degree in such places as the Undercliff of the Isle of Wight; but geographically, a cause of very considerable importance is the nature of the soil. The air over sandy or sterile ground is heated by direct contact and by radiation, to a degree far in excess of what happens to air resting on grass-grown or verdant plains; heat proceeding from an obscure source is unable to escape through the air, just as obscure heat-rays may be caught and accumulated in a closed conservatory or in a glass-covered box, so that the air may be raised to a very high temperature: several instances are on record of a temperature of 130° F. being observed under such circumstances. On the other hand, when the solar heat falls on ground, whether grassy or snow-covered, that will not easily part with it, the air may remain cool, or even cold; as is found in our every-day experience in summer of the pleasantness of a field path as compared with a high road; and as is shown more markedly by the great power of the direct rays of the sun in the Arctic, or at elevated stations in the Alps or Himalayas, whilst the snow is lying all around, and the temperature of the air is far below freezing-point.

But greater far than the effects of differences of soil are the effects of ocean currents, which warm the air to an almost incredible degree. Mr. Croll has calculated that the surface-water of the North Atlantic, if deprived of the Gulf Stream, would be reduced to a temperature very far below freezing-point; that the heat which the Gulf Stream disperses into the superincumbent air would, if converted into power, be equal to the united force of some 400 millions of ships such as our largest ironclads. This heat, thrown into the air, is wafted by the south-westerly winds over the north-west of Europe, and very largely over our own country. It is this that makes the extreme difference between the climate on this side the Atlantic and that on the other, that gives us green fields and open harbours during the winter, whilst in Labrador or Newfoundland they are buried in snow or choked with ice.

The carrying power of water is so great as compared with that of air that the climatic effect of winds heated by contact with hot earth is relatively small. The scirocco of the Mediterranean, a wind heated over the great African desert, has often been referred to as the "snow eater" of Switzerland. This has been proved to be a mistake. The snow-eating wind of Switzerland is a wind from the Atlantic, warmed by the Gulf Stream, and rendered dry and hot by the condensation of its vapour as it passes over the mountains. Similar winds have been observed in many different parts of the world—in New Zealand, in Norway, in Greenland, and in North America, where their peculiar dryness, carrying off all moisture, renders the grass so inflammable that the smallest accidental spark lights up a fire which may spread over a country, and is thus the true cause of those immense prairies which are a distinctive feature of North American geography. But such hot winds are quite distinct from such winds as blow from the Sahara, or the Stony Desert of Australia, or from many other sterile tracts of country; winds which are merely the escape of air heated to an extreme degree by contact with the burning soil. These hot winds are for the most part merely disagreeable; but cold winds are very often dangerous; in the North-Western States of America a cold wind, ushering in a violent snow-storm, caused the death of more than 300 people in January, 1873; and in many other localities, a cold wind bringing in a sudden fall of temperature through 40 or 50 degrees, is always a cause of grave anxiety. Our English "Blackthorn Winter" in April or May is only one, and a subdued instance of the ill-effects of such cold spells.

The presence of moisture in the air, by checking radiation from the ground by night, or during the winter, softens the rigour of the seasons, makes the summers less hot, the winters less cold. It is this that constitutes the difference between "insular" and "continental" climates; it is the want of the vapour-screen which causes "excessive" climates such as we read of in the far east, where, as near Khiva, a summer of more than tropical heat is succeeded by a winter of Arctic rigour. In a very extreme degree the climate of Astrakhan contrasts with that of Fuegia, and yet the mean temperature of the two is about the same; but in the one the seasons are excessive, in the other the difference is but small. The difference in the products of the two countries is thus very great: on the one hand, plants requiring great heat, but able to withstand the cold, on the other plants of a more tender nature which can flourish with a very moderate amount of warmth; in the one grapes and corn, in the other fuchsias and veronicas.

In studying climate it is therefore necessary to observe not only the greatest heat and the greatest cold, but also the mean temperature. These can only be observed by means of thermometers, for personal feelings may be the effects of many other causes—of wind or evaporation, or state of health, or peculiarity of constitution, and are absolutely no index to the state of the air temperature.

The lecturer then proceeded to speak of the different kinds of thermometers, several of which were exhibited, and of the several stands for sheltering them. The Meteorological Society has decided positively in favour of the Stevenson stand, and directs its observers to record the temperature at 9 A.M. and 9 P.M., as well as the highest and lowest, as registered by the maximum and minimum thermometers. He then described some novel and ingenious contrivances for automatic registering, such as the "turn over" of Messrs. Negretti and Zambra, and the "chronothermometer" of Mr. Stanley, and concluded by pointing out that these instruments were but a means to an end, and that the study of climate was the study of nature in one of her most beautiful and most varied aspects.

Mr. Ley's lecture was on

Clouds and Weather Signs

The lecturer dwelt, in the first place, on the unsatisfactory condition of this portion of meteorology, as contrasted with those branches of the science in which instruments are employed. The great impediment to our progress arises from the fact that cloud-observation is, in large measure, an incommunicable art, requiring a special training of the eye. Specimens of the objects of study cannot be exhibited, neither is it possible, in illustration of the subject, to refer to types of cloud depicted in the well-known paintings of the best artists, because the latter, aiming at the production of atmospheric effects, employ the materials most easy to handle, which are commonly the least typical cloud-forms. The old classification and nomenclature of clouds is highly unsatisfactory, having been framed at a time when the relation of wind and weather to the distribution of barometric pressures was unknown; and with this relation the forms and movements of the clouds are intimately connected.

As regards configuration, clouds seem naturally divisible into two groups, those which arrange themselves in layers, whose vertical diameter is small as compared with its horizontal, and those which assume spherical or hemispherical shapes; and this division is related to certain physical conditions of the atmosphere and of the earth's surface beneath the cloud. It is, however, essential that we should possess some name or system of names to distinguish those clouds which are conveyed by the upper currents, and the term cirrus, with its compounds, must be more closely restricted to this class of clouds than has yet been done. From the use of weather-maps a new science of the winds has originated, on which all attempts at weather forecasting must be based. The movements of the upper clouds afford most valuable information concerning the distribution and movement of the areas of high and low barometric pressure. Rules by which this information may be interpreted, based in great measure on a former investigation by the lecturer,[1] are somewhat complex, and cannot well be given in a brief *résumé* like the present.[2] It may be sufficient to explain that in the front of an advancing barometric depression there usually extends a bank of the halo-producing cirro-stratus, the exterior edge of which is, roughly speaking, a parabola, the focus of which lies in the line about to be traversed by the centre of the depression. On the right-hand of the centre this bank or sheet is abruptly broken and is succeeded in the rear by local shower-clouds. On the left-hand the sky commonly continues overcast, but the cloud-plane gradually descends until little is to be seen but low stratus. A backing of the upper current takes place until after the centre of the depression has passed. In whatever direction a depression is advancing the same characteristic phenomena are observed. Thus changes in the clouds indicate to us probable alterations of wind and weather.

While the nimbus, which exists in the front of a depression, first makes its approach evident by changes in the higher cloud-strata, the process of nubification is the converse of this in those local showers which commonly occur on the right-hand and in the rear of a centre of depression, and therefore when the barometer is rising or just about to rise. These latter are developed in an upward direction through the formation of cumulus. The precipitation which occurs in them—always preceded by a change of appearance in the domes of cloud, which assume a soft and cirriform aspect—is attributed to the neutralisation of electricities as the summit of the cloud passes into a higher region; but there are important differences of appearance between those cumuli which are likely, and those which are unlikely, to undergo this transformation.

A physical explanation is given of the ordinary weather signs derived from the colours of the sky, from "visibility," and from unusual refraction. Attention is invited to some peculiar types of cloud, especially to a very elevated turreted stratus, often erroneously termed cirro-cumulus, occurring with high temperature on the south-western borders of anti-cyclones, a forerunner of thunderstorms. The formation of the low-level stratus and of the fog which usually prevails in our winter anti-cyclones, seems to be due to a downward movement of the air at a time when the earth's surface is colder than the atmosphere at a slight elevation above it.

In the course of this lecture sketches of some of the more definite varieties of cloud, with arrows indicating the direction of the currents, and a diagram showing the movements of the upper currents, and the prevailing cloud-types around areas of depression, were exhibited by the aid of the oxy-hydrogen lime-light.

[1] "Relation between the Upper and Under Currents of the Atmosphere around Areas of Barometric Depression." *Quart. Journ.* of Met. Soc. vol. iii. p. 437.

[2] The lectures will shortly be published.

THE MOTION OF A LUMINOUS SOURCE AS A TEST OF THE UNDULATORY THEORY OF LIGHT

1. ALTHOUGH the undulatory theory of light may now be considered as completely established, still any confirming test of a physical theory is in itself interesting as a fresh illustration of a natural truth. Considering how at one time crucial tests of this theory were sought after, it would appear perhaps rather an anomaly that attention should apparently not have been given to the effect attendant on the motion of a luminous source as a test between the two rival (undulatory and corpuscular) theories, and that more especially as the test would appear to be in principle a simple and decisive one. I should have considerable diffidence in directing attention to this point, but no record apparently exists of experiments proposed or attempted with this view. It might be said that the possible existence of practical difficulties in the way of carrying out the test may account for this; but then practical difficulties are seldom allowed to stand in the way, if a theoretic *principle* be correct; and, unless a thing were seriously proposed and discussed, no attempt would be made to surmount the difficulties that might exist in the way of carrying it out. Sir John Herschel, as far as he touches on this point, would appear to have had the idea that such a test between the two theories could not, *in principle*, be applicable; for he says ("Outlines of Astronomy," p. 214), speaking of the effect attendant on the motion of a luminous body, "The effect in question, which is *independent* of any theoretical views regarding the nature of light, ... &c." It is true he mentions afterwards, in a foot-note, a difference in the effect in the case of the two theories as regards the *velocity* of light, in the case of a luminous source moving directly towards or from the observer; but the following fundamental difference in the case of the two theories appears not to have been taken into account.

2. For the sake of precision we will take a special example. Suppose a luminous source with such an adjustment as to emit a parallel beam of light,[1] and let the luminous source be supposed put in motion in a direction at right angles to the path of the beam. Then on the basis of the corpuscular theory (according to which light consists of projected particles) since the particles or corpuscles, according to the laws of motion, partake necessarily of the motion of the body emitting them; so, therefore, if the corpuscular theory were true, the path of the emitted beam of light would be exactly the same in direction as if the luminous body were at rest. So, therefore, if we imagine a screen of the same breadth as the luminous beam to be placed at a distance, so that this screen is *exactly* illuminated when the luminous source is at rest, then (according to the corpuscular theory) this screen would also be *exactly* illuminated when both it and the luminous source were put in motion with equal velocities (in parallel paths) in the same direction.

3. This, however, will not be the case if the undulatory theory be true, for it is a known fact that waves emitted in a medium do not partake of the motion of the body emitting them. For when once a wave has left the body, the wave is dependent solely on the medium for its propagation and is not influenced by the motion of the body one way or the other. It follows, therefore, that in the case of a moving luminous source emitting waves transversely to its path, the waves forming the parallel beam will be left behind, or will not partake of the motion of the luminous source. The waves will form a slanting track of light which will no longer strike *exactly* the opposed distant screen, but will fall somewhat to the rear of it. The luminous beam which, when the screen and source were at rest, was exactly eclipsed or intercepted by the screen, will (when the screen and source are in motion) commence to escape behind the edge of the screen, or the eclipse will no longer be total.

4. Here, therefore, we should have in principle a simple and decisive test between the two theories, provided insuperable practical difficulties do not stand in the way of carrying it out (for which object probably various methods would suggest themselves).

In order to contrast further the different effects that would be produced in the case of the two theories (corpuscular and undulatory), we may consider various possible cases of relative motion, also the effect when the beam is received directly in a telescope, or in the eye. We have already considered the case where the beam is observed objectively (by the use of a screen), which we may call *Case I.*

5. *Case II.*—We may now consider the case when a telescope[2] is used. We will take the above example of a luminous source in motion, emitting a parallel beam of light at right angles to its path, and we will imagine that this beam is received in a distant *stationary* telescope, placed normal to the path of the moving luminous source, so that the beam flashes down the axis of the telescope at the instant of the passage of the luminous source. Then we have to compare the effects produced in the case of the undulatory and corpuscular theories. On the undulatory theory, waves emitted by a luminous source do not partake of the motion of the source, so that at the instant when the wave of the beam (singling out a particular wave) flashes down the axis of the telescope at the moment of passage of the luminous source, the source will have already moved on a distance from the point where the wave left it, this distance representing that traversed by it during the time the light took to pass from the source to the telescope; and the source is therefore seen out of its true position by precisely that amount.

[1] We consider a *parallel* beam of light for simplicity, though it is not necessary to the principle.

[2] Of course the effect produced by aberration is the same with the eye alone as with a telescope, but we prefer to consider the latter, as its larger scale enables the effect to be visualised better.

This is perfectly evident, and the correction for this error in the estimate of position (of the value above indicated) constitutes the well-known "equation of light."

6. We have now to consider what takes place on the corpuscular theory. Here the projected corpuscles will partake of the motion of the source. Singling out, therefore, one of the corpuscles that flashes down the axis of the telescope at the instant of the passage of the luminous source; this corpuscle will possess the transverse velocity of the source that emitted it, and therefore the corpuscle will not pass straight down the axis of the stationary telescope, but in its passage will deviate laterally from that axis. The telescope would accordingly have to be inclined in order that the corpuscle might pass along the axis. This deviation of the corpuscle from the axis of the telescope will cause the luminous source to be viewed out of its true position, and it is easily seen that this visual error in the estimate of the position of the source on the corpuscular theory is precisely the same in amount as the previous error (due to a different cause) on the undulatory theory. Indeed, the error on the corpuscular theory is simply a case of "aberration" due to the relative motion of the telescope and light), and the correction for it, according to known principles, is the same as the other correction on the undulatory theory, termed "equation of light.'" It is a remarkable fact, therefore, that though the path of the light in its transit is very different in the case of the two theories, the visual error in the estimate of the position of the object is the *same*, so that this error cannot itself serve as a test between the two theories. There is, however, one marked distinction between the two theories; for while on the undulatory theory a position of the telescope *normal* to the path of the moving luminous source, causes the flash of the beam to pass down the axis of the telescope; on the corpuscular theory, on the other hand, the telescope has to be *inclined* in order that the flash of the beam may pass down its axis. Here, therefore, we have a definite physical effect serving as a point of distinction between the two theories.

7. *Case III.*—We will now take the case when *both* the luminous source and the distant observer are in motion, moving with equal velocities in parallel paths alongside each other in the same direction. Here on the corpuscular theory, in which case the corpuscles partake of the motion of the source, since the whole system therefore moves with equal velocity, the whole system will therefore be *relatively* at rest; so that the light will pass across and enter the telescope just as it would have done if everything were at rest, or there will be no peculiarity in the passage of the light whatever on the corpuscular theory. It is widely different on the undulatory theory, for here the beam of light passing between the source and the telescope will be left behind in the medium, and therefore in the first place, the moving telescope, in order to catch the parallel beam, will have to be placed back a certain distance in the rear; for since the light takes a slanting track between the source and telescope (the degree of slant depending on their common velocity), the telescope to intercept the light, can no longer be placed exactly *opposite* the luminous source. The distance the telescope will require to be placed back evidently must be equal to that traversed by it during the time the light takes to pass from the source to the telescope. Secondly, the light on the undulatory theory will suffer aberration in passing along the tube of the telescope, owing to the latter being in motion relatively to the light; no such aberration taking place on the corpuscular theory, since the corpuscles are moving at the same velocity as the telescope. Thirdly, on the undulatory theory, there will be a correction necessary, due to the motion of the luminous source ("equation of light"); such correction not being required on the corpuscular theory, since on that theory the light emitted partakes of the motion of

the source. But it is a curious fact that the two errors occurring on the undulatory theory ("aberration" and "equation of light") happen precisely to counteract each other; so that therefore the luminous source is seen in its *true* position, *i.e.*, in the *same* position as by the corpuscular theory. The fact of the two errors compensating each other therefore prevents this occurrence of error from serving as a visual test between the two theories. There is, however, a distinct objective difference between the two theories in this case, as regards the *position* of the telescope (previously referred to); but before recurring to this, we may consider more closely the mode in which the above compensation of the two errors is brought about.

8. It is well known that the effect of the error termed "aberration" is to make the luminous source appear *forwards* of the position it occupied when the wave left it, and this by an absolute amount equal to that traversed by the telescope during the time the light took to pass from the source to the telescope. But from the fact that the luminous source is in motion, the latter *is* actually at the time of observation situated *forwards* of the position it occupied when the wave left it, and by precisely the above amount (since the source is moving at the same velocity as the telescope). Hence the two errors will precisely compensate each other, and the luminous source will be seen through the telescope in its true position, *i.e.*, in the *same* position as by the corpuscular theory. But it is important to note that on the undulatory theory, the telescope, in order to receive the parallel beam emitted by the luminous source, must be placed *not* opposite the source, but somewhat backwards, to make up for the slanting track of the beam attendant on the motion of the luminous source. On the corpuscular theory, on the other hand (where the track of the beam of light is *normal* to the line of motion of the source), the telescope must be placed *opposite* the source in order to catch the beam. The *position* of the telescope in the two cases therefore constitutes a distinct physical difference between the two theories, which might serve as a test.[1]

9. The above considerations may suffice to show in a simple manner that important differences exist in the effects attendant on the motion of a luminous source on the corpuscular and undulatory theories of light, and that these differences would be *in principle* capable (supposing practical difficulties surmounted) of constituting a simple and decisive test between the two theories.

S. TOLVER PRESTON

[1] The fact pointed out by Sir John Herschel in regard to the difference in the *velocity* of light on the undulatory and corpuscular theories, in the case of a luminous body moving directly towards or from the observer, would appear also to be worthy of remark. It is evident, for example, that in the case of a luminous body moving towards the observer, the velocity of light would *add* itself to that of the body on the corpuscular theory; but the velocity would be unaffected on the undulatory theory.

GEOGRAPHICAL NOTES

THE Paris Geographical Society held last Friday its annual December meeting, at its hotel, under the presidency of Admiral La Roncière le Nourry. The report of the progress of geography was read by M. Maunoir, the secretary. M. de Ujfalvy delivered an address on the region of Central Asia which he visited, and which may be termed the extreme frontiers of the Russian empire, and which are just now attracting so much notice. This traveller will soon come to London to give the same address before the Royal Geographical Society. His exploration was made at the expense of the French Government. On the following Saturday a banquet took place at the Continental Hotel. The usual toasts were given. After dinner M. de Lesseps, who is very likely to be nominated president of the Society for 1879, at the April meeting, gave some account of his visit to Tunis with Capt. Roudaire, in order to investigate the conditions of the creation of the new Saharan Sea. M. de Lesseps described the whole scheme as being easily practicable for a sum of not more than 60,000,000 of francs. He said that the French extension telegraph system was extending all over Tunis and Tripoli, and that Arabs were accustomed to follow the telegraphic line as their camels travelled at a quicker rate when following its track. He intimated that the Egyptian telegraphic system was extending to the equator, and that he advised M. Cochery, the Director of French Postal Telegraphy, to extend it all over Sahara up to Senegal.

PETERMANN'S *Mittheilungen* (for so it will continue to be named) for December contains a long and careful account of Chartography at the Paris Exhibition; Dr. Carl Martin contributes a paper, based on Chilian sources, on the Chonos Archipelago, which is accompanied by a map. An excellent paper on the present condition of Afghanistan (with map), is contributed by Herr F. von Stein. This number, besides the usual table of contents to the volume, contains a complete alphabetically arranged index to Dr. Behm's useful monthly summaries.

WE have received a specimen of the first number of one of those stupendous geographical works of which the French are so fond, and the best example of which is Elisée Reclus' "Géographie Universelle." Indeed, the new work announced seems to have pretty much the same object as that of Reclus, though the method is different. The new work is to be issued by the Librarie des Connaissances Utiles, and the author is M. Charles Hertz. Its title is "La Géographie Contemporaine d'après les Voyageurs, les Émigrants, les Colons, les Commerçants." It will consist of ten series of from three to five volumes each. We trust the publishers will find subscribers patient enough to wait for the end. There will be from 600 to 800 maps and hundreds of illustrations, and the work will be issued in weekly parts. We calculate it will take fifteen years to complete. Judging from the specimen, a good deal of narrative and adventure will be introduced into the work, and that it will be at least as entertaining as instructive. The first series will deal with polar and maritime expeditions, the second with Africa, the third with Asia, the fourth with Australia, and the fifth with means of communication, geographical societies, &c. The other five series will be devoted to a description of the nations of European origin and their enterprises over the globe. It is a grand scheme.

DR. THOLOZAN, physician to the Shah of Persia, is organising a scientific exploration of the province of Khuzistan, the southern province of Persia. The expedition will start from Bassorah on February 1 next.

NOTES

THE Corporation of the City of London having determined to identify themselves with the movement by the Livery Companies of London for the advancement of technical education, on Thursday last elected the following to serve on the Board of Governors of "The City and Guilds of London Institute for the Advancement of Technical Education":—The Lord Mayor *ex officio*; the Recorder, *ex officio*; Aldermen: Sir Thomas Dakin, Knt.; Sir Robert W. Carden, Knt.; Mr. William Lawrence; Sir Francis W. Truscott, Knt.; Sir Wm. A. Rose, Knt., F.R.S.L., F.R.G.S.; Simeon C. Hadley; Common Council: Mr. Joseph Beck, F.R.A.S.; Mr. W. Bassingham; Mr. J. L. Shuter, F.R.A.S.; Sir C. Reed, Knt., LL.D., F.S.A., Dep.; Mr. George Shaw; Mr. J. Edmeston; H. Lowman Taylor, J.P., Dep.; S. E. Atkins, Dep.; Sir Jno. Bennett, Knt.; Mr. Henry Greene; Mr. John Faulkner; Mr. Thomas Waller.

LAST week Dr. Gladstone read an important paper at the Society of Arts on science teaching in elementary schools. He assumed, first, that it is not good that poor children should go forth into the world in gross ignorance of the material objects among which they must always live and work; secondly, that it is far from desirable to try to make scientific men and women of boys and girls of twelve and thirteen years of age. "This earth," Dr. Gladstone said, "is our dwelling-place, from the cradle to the grave; our bodies are the complicated machines, so wonderfully made, by which every action of ours is performed; the sun, clouds, and atmosphere influence us every day; the animal, vegetable, and mineral kingdoms are ready to yield us their supplies; and the great mechanical and chemical forces, with heat, light, and electricity, are ready to be our servants if we do not allow them to become our masters. Every man, also, in his handicraft or trade, as well as every woman in her domestic duties, has to deal with some facts and objects of nature specially connected with them." Dr. Gladstone then proceeded to point out the present state of the question, showing that very much yet remains to be done ere science takes the place it ought to occupy in our elementary schools, though the energetic London School Board is doing much to give science-teaching an established place in the schools under their charge. He referred to the universality of science instruction in Germany, and expressed a hope that a "knowledge of common things" would soon take its place alongside of the older subjects in all our elementary schools.

WE are glad to learn that the health of Prof. Hoffmann, the well-known chemist, is now completely restored, and that he is again among his pupils.

M. FLAMMARION writes to us in reference to a note from a correspondent last week, that the subscription he is organising is mainly for the purpose of founding at Paris a free observatory created by private means, on the model of those which exist in England. It is desired to establish in the observatory the most powerful instrument which the funds will enable to be constructed. This instrument is intended above all for the physical investigation of the planets, and particularly of the moon, "the vital state of which," M. Flammarion writes, "may thus be definitely settled. It is not proved," he says, "that the moon is a dead planet, but the progress of optics appears to me to be now such as to justify a serious investigation for traces of life upon it; in fine, to settle what opinion ought to be held on the question of the habitability of the moon."

THE September number of the *Mineralogical Magazine and Journal of the Mineralogical Society of Great Britain and Ireland* contains three original papers. Mr. Sorby gives a few test-experiments of his new method of determining the refractive indices of mineral plates, which will hardly be new to readers of NATURE. Prof. Heddle continues his papers on the geognosy of Scotland. The present one is almost entirely occupied with the geology of the Island of Fetlar, and gives several analyses of the minerals found in its different rocks. Prof. How's contribution to the mineralogy of Nova Scotia is mainly taken up with mordenite and some altered nodules found at Cape Split; the rest of the paper is not much more than a list of localities. Ten pages are taken up by very poor abstracts from the *Zeitschrift für Krystallographie* and other periodicals, which are far inferior to those published in some of the weekly journals. Most of these are by a gentleman who may be a mineralogist, but is scarcely a crystallographer. He is grandly impartial as to notation, apparently following the authors in using either the Millerian or Naumannian. When using the former he often forgets to state the system in which the mineral crystallises. When the latter is used in an English mineralogical journal, the least we can expect is to have the Millerian equivalents given side by side with the Naumannian symbols. The conversion is not difficult, and tables for the purpose are given in all the text-books. Near the end is an abstract by the editor of a flattering notice of his own book, the good taste of which is obvious. We find it difficult to think that a magazine so indifferently conducted can prove either a commercial or a scientific success.

CAPTAIN HOWGATE has sent us some of the results of the preliminary United States Arctic Expedition in the *Florence*, in the shape of reproductions of photographs and of drawings by the Eskimo. The latter are rude, but vigorous and amusing, and show the well-known talent of the natives for drawing.

EXPERIMENTS on the electric light, with Jablochkoff candles, have been tried at Havre, and, in consequence of the frequency of maritime collisions, are attracting much notice from seamen. The *Breeze*, one of the British mail steamships, was stranded a few days ago, when trying to enter the Calais port, owing to the prevalence of fogs. This accident would not have taken place if an electric light had been placed at the end of the jetty, as had been proposed. It is said the matter has been reported to the French Minister of Public Works. The electric light experiments in the Avenue de l'Opera and in front of the Palais Legislatif are to be continued up to January 15, but at the expense of the company, the Municipal Council having refused to pay more than the sum which would have been spent by them if the streets had been lighted with ordinary gas.

THE question of "reserves" for the aborigines having been recently raised in the Queensland Legislature, it has been recommended that the system of the Durundur reserve should be extended, as there are many other places where it might be advantageously tried.

WE have received from Messrs. De la Rue several specimens of their diaries, which are marked by all their usual elegance and usefulness. Much valuable information is prefixed to several of the diaries, though we still regret the absence of some of the astronomical information which used to give them a distinctive value. Messrs. Letts have also sent us a number of their various forms of diaries, the marked feature of which is their utility. They are of all sizes and prices, and no one need have any difficulty in providing himself with this useful help to order and memory. Messrs. Letts have also published a handy weather table by Mr. Saxby, containing a great deal of useful and well-arranged information.

THE *Madras Mail* states that great progress is being made in the cultivation of chinchona in the Wynaad, and that nearly a million plants have been taken there this year from the Neddiwuttum estates, and this is in addition to what is obtained from the extensive chinchona nurseries on all the coffee estates. All the poorer parts of these are being planted with chinchona which is found to thrive well where coffee will not grow.

A LETTER from Peking states that the Viceroy Li Hungchang has entered into a contract with Mr. Arnold Hague, of New York, an able geologist and mining expert, for the purpose of prospecting the gold, silver, and other mines in the north of China. Mr. Hague is expected to start shortly from Tientsin for the mining regions. The just published Consular Report from Canton also states that General Fang, a well-known military officer, has been instructed to arrange for an immediate supply of European machinery to be used in local mines. It appears to be thought that there is great likelihood of the early working of coal and other mines in the provinces of Chihli, Kiangsi, Kiangsu, and Szechuen.

FOR some time past there has been so little water in portions of the Grand Canal of China, that it has only been navigable by small boats, and for this reason, among others, it has been found necessary to send the grain tribute up to Peking by sea. The last mail from China, however, brings news of an unusual rise in its waters. A correspondent of the *North China Herald* left Tientsin on October 10 and observed no especial increase in the water until he reached the town of Hsingchi, about seventy miles to the south. There he met with a south-west wind, and noticed the water up to the top of the banks. Further on the people were seen raising narrow ridges to prevent the water from overflowing into the fields. Beyond the city of Tsingchow, and as far as the Narpi district, large tracts of land were under water. The general level of the country east of the canal is about six or eight feet below the artificial embankment, and in not a few places this is very weak from the constant wash, especially at the bends, so that the danger is serious. The writer had not been able to learn the cause of this unusual and rapid rise in the Grand Canal, but the Chinese attribute it to heavy rains in the mountainous region to the south-west, though it has never been known to happen before so late in the year.

THE Board of Trade having received, through the Foreign Office, reports from Her Majesty's Consul at Taganrog as to the recent destruction of corn crops in the neighbourhood of that place by a beetle described as the *Anisoplia austriaca*, have communicated with the Entomological Society of London, and have been favoured by that Society with a report upon the subject. The Report is signed by Messrs. McLachlan and Waterhouse, who state that the insect *Anisoplia austriaca* has nothing whatever to do with the "Colorado beetle." It belongs to a group of beetles (Rutelidæ) allied to our common cockchafer, but is of very much smaller size. There can be no doubt, they state, that the eggs are deposited in the earth at the roots of corn and grasses, that they soon hatch, and that the larva feeds upon the roots. How long a period elapses before the pupal state is assumed they know not, but they think that two years may be the outside limit, and that in the autumn of the second year of its existence, the larva either forms a cocoon in which it remains quiescent until the following spring, when it assumes the pupal state, or, as is more probable, it assumes that state in the autumn, and the perfect insect may be developed soon afterwards, but remain in the cocoon until the following summer. All accounts, however, they have been able to refer to concerning this and congeneric species, agree (as does the information furnished by Mr. Carruthers) in attributing the chief damage to the perfect insect, which feeds upon the green corn in the ear. After referring to the abundance of the beetle in some parts of Germany, they commend, as a preventive of its ravishes, rotation of crop and encouragement of insectivorous birds, and state their opinion that there is no reason to apprehend the recurrence year after year of such multitudes of the beetles as have occasionally appeared. "In the present state of entomological science," they conclude, "it is impossible to accurately account for visitations like this, which occur with many insects, injurious or otherwise. It may be that the pupal condition is prolonged indefinitely, or until circumstances favour its determination; by this reasoning (which is warranted by what we know to be the case in some other insects) the pupæ might be accumulated from year to year, and the perfect insects from these accumulations burst forth simultaneously."

AT the second monthly meeting of the Statistical Society, held on the 17th inst., Dr. Mouat, Foreign Secretary of the Society, who was deputed to represent it at the meetings in Paris of the Demographic Congress, and of the Permanent Commission of the International Statistical Congress, and, in Stockholm, of the International Penitentiary Congress, read reports of the proceedings of those bodies, so far as they were of interest to the Statistical Society. The chief work of the Permanent Commission was its own reorganisation as the executive of the Statistical Congress, and the adoption of a scientific scheme of classification of statistics for international purposes. Statistical annuals for 1877 have been published in France, Italy, Prussia, Austria, Hungary, and Belgium, all differently arranged. With a view to the adoption of uniformity of system on strictly scientific principles, M. Deloche, the chief of the General Statistical Department in Paris, suggested such a system, and it was adopted. Taking territory and population as its basis, the subsequent grouping of the many and varied facts by which human activity is manifested was in the order of the faculties to which they naturally attach themselves, viz., the moral, the intellectual, and the physical. Reference was made by Dr. Mouat to the large amount of valuable statistical information collected and published annually in the form of the statistical abstracts of the British Empire; and to the mass of miscellaneous statistics furnished from time to time to Parliament. Dr. Mouat was of opinion that the time had not yet arrived for the scientific classification of statistics, and he suggested for consideration a more simple form under four heads, viz., (1) Territory and Population; (2) Revenue and Commerce; (3) Laws and Government; and (4) Miscellaneous Statistics, containing all that cannot be grouped under any of those heads. The Demographic Congress was entirely devoted to the consideration of questions of population in the aggregate, or the natural history of society, as distinguished from physiology—births, deaths, marriages, migrations, &c.

THE *Times* a few weeks ago gave prominence to some facts showing the highly electrical condition of the atmosphere in Canada during the fine dry winter there. A letter from Mr. A. H. McNab, of Teignmouth, in the *Western Times* of December 17, will show that similar conditions exist in England during similar weather. He says:—"About 7.45 P.M. on Friday I was crossing Shaldon Bridge from the Teignmouth side, and immediately after passing the 'drawbridge' I was much surprised to find both sides of the bridge illuminated at certain regular distances with pale blueish lights resembling flickering lamps. My first impression was that, owing to the dense fog that prevailed, some sort of lamps had been placed on the bridge as a warning for the boats and barges passing up and down the river, but on coming to the first iron upright in the bridge railing I at once came to the conclusion that the light was electrical, for each point of iron had a flame of about two inches in height, composed of electric sparks issuing from it, accompanied by a hissing sound resembling that caused by the escape of gas from unlighted burners, and all the iron points along the bridge were similarly illuminated, producing a most charming effect. The point of the umbrella which I carried was also emitting a light, and when moved about produced sparks and sound. When I was midway across the bridge a passenger suddenly appeared in the fog and called out to me, 'Sir, your umbrella has a light on the end of it.' His surprise, however, was not lessened when I informed him that his own umbrella had the same. So far as I observed the phenomenon was entirely confined to the bridge, and on my return some time after it had entirely disappeared. Perhaps the large amount of iron in the bridge may have something to do with it."

THE mission of Sergeant Jennings, of the United States Signal Corps, has not been quite fruitless in Paris. A sort of weather indicator has been placed, by order of the Prefect of the Seine, at the doors of the Luxembourg Palace; others will, it is said, be very shortly installed in several places of Paris.

As No. 1 of "Dimmock's Special Bibliography" (Cambridge, Mass.) we have received a descriptive list, arranged according to date, of the Entomological Writings of Prof. John L. Le Conte, compiled by Mr. S. Henshaw. There are 150 entries, and the list seems compiled with great care and must prove useful to entomologists.

ABSORPTION OF WATER BY THE LEAVES OF PLANTS

THE experiments of Boussingault, referred to in NATURE, vol. xviii. p. 672, find a fitting sequel in those of the Rev. G. Henslow, detailed in a paper read before the Linnean Society on November 7. Although gardeners universally maintain that growing plants have the power of absorbing water through their leaves, both in the liquid and the gaseous form, in addition to the power of suction through the roots, yet the contrary theory has been in favour during recent years among vegetable physiologists. The first recorded experiments of any value on the subject were about the year 1727, by Hales,[1] as described in his "Statical Essays;" the conclusion to which he came being that "it is very probable that rain and dew are imbibed by vegetables, especially in dry seasons." This result was confirmed by Bonnet in 1753. A century later, however, in 1857, Duchartre, experimenting on the absorptive power of plants, came, after considerable wavering, to the conclusion that rain and dew are not absorbed by the leaves of plants. This opinion has been, with but little exception, held by all physiologists during the last twenty years, notably by De Candolle and Sachs; the explanation offered of the fact that withered plants revive when placed in moist air or when the leaves are moistened, being that transpiration is thus stopped, or is more than counterbalanced by the root-absorption. In his "Text-book of Botany" (English edition, p. 613), Sachs says:—"When land-plants wither on a hot day, and revive again in the evening, this is the result of diminished transpiration with the decrease of temperature and increase of the moisture in the air in the evening, the activity of the roots continuing; not of any absorption of aqueous vapour or dew through the leaves. Rain again revives withered plants, not by penetrating the leaves, but by moistening them, and thus hindering further transpiration, and conveying water to the roots, which they then conduct to the leaves." McNab has, however, proved that leaves do transpire, even in a moist atmosphere, provided they are exposed to the action of light.

The results of Mr. Henslow's experiments, extending over several years, are altogether in accordance with those of Boussingault, and may be considered to set the question of the absorbent power of the leaves of plants completely at rest. The following are the chief conclusions arrived at:—1. The absorption of water by internodes. The experiment consisted of wrapping up one or more internodes of herbaceous plants in saturated blotting-paper and in noticing the effects. As a rule the leaves on the shoots rapidly perished, showing that transpiration was too great for the supply. The stems, however, kept fresh for different periods up to six weeks. 2. Absorption by leaves, to see how far they could balance transpiration in others on the same shoot. The general result is that as long as the leaves remain green and fresh in or on water, they act as absorbents, but that the leaves in air keep fresh or wither according as the supply equals or falls short of the demand. 3. To test how far leaves on a shoot can nourish lower ones on the same shoot. It appears that it is quite immaterial to plants whether they be supplied from water by the absorbing leaves being above or below those transpiring. Water flows in either direction equally well. 4. Leaves floating on water. It was found that one part of a leaf can nourish another part for various periods, though the edges out of water died first. 5. Absorption of dew. A long series of cut leaves and shoots were gathered at 4 P.M., then exposed to sun and wind for three hours, then carefully weighed and exposed all night to dew. At 7.30 A.M., after having been dried, they were weighed again, and all had gained weight and quite recovered their freshness, proving that slightly wetted detached portions do absorb dew. 6. Imitation dew. Like results followed from using the "spray," by which dew could be exactly imitated. 7. Plants growing in pots and of which the earth was not watered, were kept alive by the ends of one or more shoots being placed in water; *e.g.*, *Mimulus moschatus* not only grew vigorously and developed axillary buds into shoots, but even blossomed.

By these interesting experiments the physiological botanist is again placed in harmony with the gardener who syringes his plants, not merely for the purpose of washing off dust and insects, but in order to facilitate the actual absorption of water by the surface; and with the field botanist, who sprinkles the plants in his vasculum with water to keep them fresh till he reaches home. The fact which now seems established beyond all doubt by the observations of Darwin and others, that certain plants have the power of absorbing through their leaves and digesting the remains of animal substances, also implies, as a necessary corollary, the absorbent power of leaves for certain liquid or gaseous substances. In connection with this subject sufficient attention has perhaps not been attracted to the observations of Prof. Calderon, as detailed in a paper printed at Madrid, in English (1877), entitled, "Considerations on Vegetable Nutrition."[1] Calderon's statements—which, however, require at present to be confirmed by other observers—are to the effect that plants have the power of absorbing the nitrogenous organic matter which is constantly floating in the air, and that, if air be deprived of all organic matter, it is unable to sustain vegetable life, all the physiological functions of plants being then suspended.

A. W. B.

[1] In his "Geschichte der Botanik," pp. 515-521, Sachs gives an admirable epitome of the great service rendered to the progress of botanical science by the researches of this eminent botanist and physicist.

UNDERTONES

THESE formed the subject of a lecture delivered by Herr Auerbach before the meeting of Naturalists at Cassel this year.

The term "undertones," he pointed out, is an extension of the nomenclature which denotes certain accompanying tones of a given note "overtones." Undertones may be observed in the following way:—If a struck tuning-fork be set on a sound-board, a tone is heard sounding strongly, which before was little perceptible. The stem of the fork makes longitudinal vibrations, which, by action on the sounding-board (a very thin one), generate transverse vibrations, and these spread over the large surface of the plate. Should the tone of the board only differ in intensity from that of the fork, the vibrations executed by the stem of the tuning-fork must be small; it is otherwise, however, when the vibrations exceed a certain amount.

Herr Auerbach demonstrated his observation with a tuning-fork giving the A of a violin, and so 435 vibrations per second. When he placed the vibrating fork firmly on a sonud-board, the tone was heard distinctly at a distance. When, however, he brought the tuning-fork, struck very vigorously, into very light contact with the plate, there was heard the lower octave of the fork's tone. With other materials, which were not then at his command, he could produce also the lower fifth of the lower octave, and the lower fourth of this tone, *i.e.*, the double octave of the fork's tone. The vibration numbers of these resonance tones are $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., of that of the tuning-fork's tone, *i.e.*, the resonance-tones form the series of the *harmonic undertones*.

With regard to the mode of occurrence of these tones, Herr Auerbach said this: "To prove to you, first of all, that the strength of the vibrations is the fundamental condition of the phenomenon, I will once more make the experiment, and continue it longer. You heard first, again, the lower octave; but then the tone sprang over into the higher, so that it became identical with the proper tone of the tuning-fork. Consider this result along with the fact that the vibrations of the fork rapidly diminish; remember, too, that it is only when the fork is vigorously struck and lightly placed on the plate that the undertones occur, and you will see that the cause of the phenomenon lies in the amplitude of the vibrations."

Herr Auerbach further supposes that the resonance-surface of the plate, being imperfectly elastic, follows the movements of the stem of the fork immediately downwards indeed, but not upwards; an interval then occurs which only disappears on the next passage downwards of the stem. If the retardation be a small one, the plate, at the moment of meeting the stem a second time, has nearly completed a vibration. If, however, the retardation be great, the undertones arise (as the lecturer showed graphically) from the combination of the vibrations of the stem and the plate. The stem-end, *i.e.*, in this latter case, is no longer an unconditionally free end, but its freedom is a periodic function of the time, and this period is twice as great as that of the tuning-fork vibrations. "That the undertones arise in consequence of internal friction, was easy to see *à priori*; what the experiments have shown and explained is the interesting fact, that precisely the *harmonic undertones* are produced; that is a consequence of the fact that the resonance is the action of a periodic force, and so, in a certain sense, a discontinuous phenomenon, otherwise the undertones must form a continuous series, which is not the case."

[1] See NATURE, vol. xv. p. 108.

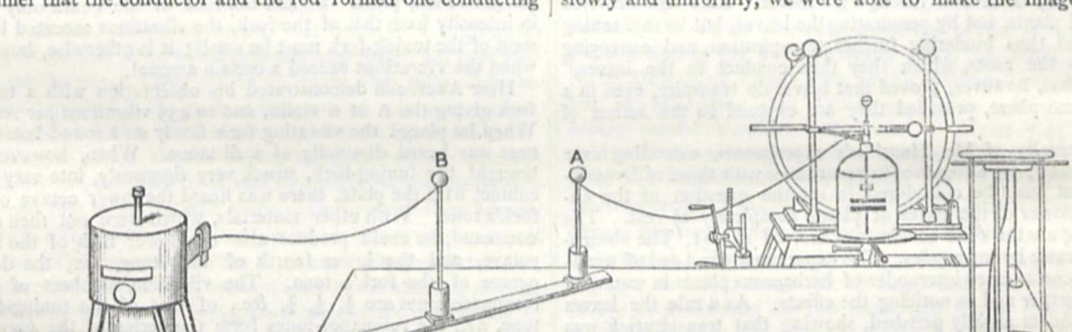
"It is obvious," he continues, "that the description given of the phenomenon is incomplete, for ductility, elasticity, variation of the resonance-plate, &c., co-operate to produce a more complicated phenomenon. I have tried a great many materials for undertones, and found that they fall, in this respect, into three groups. In the middle stand those materials which furnish undertones, that is, the great majority of all substances in general. On the one side are those substances which, as soon as the vibrations are pretty strong, give no resonance-tones, but merely an indeterminate noise; to this group belong rolled plate metal and most kinds of glass. On the other side are those substances which, however strong the vibrations, always give the tone of the tuning-fork. I have found only one example of this, viz., the wood of mountain fir, in thin polished plates. It was natural to try the belly of a violin, which is mostly made of fir-wood, for undertones, and in this way form an idea as to the elasticity of the wood, on which the excellence of the instrument greatly depends. From the German violins I have examined, I have always obtained undertones; from the few authentic Italian violins accessible to me I obtained, on the other hand, always the original tone. But I acknowledge that more abundant material is necessary for a decision of this question."

THE DISRUPTIVE DISCHARGE OF ELECTRICITY[1]

By means of the following method we have been able to investigate the laws of the disruptive discharge of electricity of high potential—a subject of investigation which is the complement of that in which Drs. Warren de la Rue and Müller have been simultaneously engaged. In making these experiments I have had the able co-operation in succession of Messrs. Salvesen, Connor, Stewart, Simpson, and Playfair.

The method essentially consists in connecting the prime conductor of the Holtz machine, not with the electrometer directly, but with an insulated spherical ball placed at some distance from an equal spherical ball, the latter being connected with the electrometer. The woodcut represents, *in situ*, the apparatus which was used in the case of the gases. The receiver of the air-pump, which has a rod capable of moving air-tight, was attached to one of the conductors of the Holtz machine in such a manner that the conductor and the rod formed one conducting system. Projecting from the plate of the air-pump was a short metal rod, which formed one conductor with the metallic parts of the air-pump, and, by means of a wire, with the uninsulated conductor of the Holtz machine. Electrodes of various forms were made to screw on to the ends of the rods. Of the two insulated brass balls one, A, was fixed; the other, B, could be moved along the connecting board. The wire joining A to the collar of the receiver is insulated with gutta-percha. The electrometer in connection with B is one of Sir W. Thomson's divided ring reflecting electrometers.

When the potential of A is raised by driving the machine, the potential of B is also raised, and this goes on until a discharge takes place between the electrodes inside the receiver. Hence the maximum deflection of the spot of light from zero is an indication of the difference of potential of the two surfaces between which the spark passed immediately before the discharge. By breaking the contact between the conductors of the Holtz machine before beginning to turn the wheel, and, by turning slowly and uniformly, we were able to make the image of the



wire move up continuously, and to be at rest at the instant of discharge. After the discharge took place the image fell back to zero, or a point near zero. We always noted the position taken up by the image when the conductor of the machine was completely discharged.

The force resisting the deflection of the mirror is the action of two external magnets upon several small magnets fixed to the back of the mirror.[2]

One great merit of our method is the rapidity with which observations can be made. Three readings were in general taken for each entry. The mean of these is very probably free from any error due to accidental variations in the passage of the spark. An extensive series of observations have been printed in full in the *Transactions* of the Royal Society of Edinburgh. The following are the more important results:—

A spark was taken through air between plates at a constant distance, and the distance between the balls A and B varied. Let V denote the induced potential, and r the distance between the centres of A and B; then the experimental curve obtained satisfies the equation—

$$V = 6081 r^{-1} - 42{\cdot}26$$

for values of r greater than 24 centimetres; but for less values of r the function requires to be corrected by being multiplied by—

$$\cdot 524 + \cdot 02 r.$$

Sparks were taken through air at the atmospheric pressure between parallel metal disks of 4 inches diameter for distances up to 1·2 centimetres. The function for V, the difference of potential in terms of s, the length of the spark is—

$$V = 66{\cdot}94 \sqrt{\{ s^2 + \cdot 205 s \}},$$

[1] Abstract by the author of thesis for D.Sc. and other papers printed in the recently issued part of the *Transactions* of the Royal Society of Edinburgh. By Alexander Macfarlane, M.A., D.Sc.

[2] Our results were reduced to absolute measure by means of the absolute electrometer represented on the table.

the equation of a hyperbola whose semi-transverse axis is ·1025 centimetres, and semi-conjugate axis 6·8623 C.G.S. units. From the above equation we infer that—

$$R = 66{\cdot}94 \sqrt{\left\{1 + {\cdot}205 \frac{1}{s}\right\}}$$

where R denotes the electrostatic force; from which it is evident that as s becomes smaller R becomes greater. A similar curve was obtained when hydrogen was substituted for air.

When the disks were heated before taking the sparks, the curve obtained satisfies the equation—

$$V = 87{\cdot}04s - 19{\cdot}56s^2,$$

a parabola, from which we deduce—

$$R = 87{\cdot}04 - 19{\cdot}56s.$$

It was found that when the capacity of the charged conductor was changed, the difference of potential required to produce a spark remained constant.

When the discharge was continued so as to keep the spot of light at a fixed deflection, the reading was always less than for the corresponding single discharge, but the curves were similar.

Readings were taken of the difference of potential required to produce a ·5 centimetre spark through air at different pressures from the atmospheric to 20 mm. They give—

$$V = {\cdot}0458 \sqrt{\{p^2 + 203p\}}$$

where p denotes the pressure in millimetres of mercury.

The electric strengths of several gases were determined by comparing the differences of potential required to pass a ·5 centimetre spark through the gas at the atmospheric pressure.

Dielectric.	Electric Strength.		
	Macfarlane.	De la Rue and Müller.	Faraday.
Air	1	1	1
Carbonic Acid ...	·95	1·06	·91
Oxygen	·93	1	·71
Hydrogen	·63	·54	·53
Coal Gas	·93	—	·71

"Electric strength" is the term used by Prof. Clerk-Maxwell to denote the physical constant in question. I have added, for the sake of comparison, values deduced from the results of De la Rue and Müller and of Faraday, but the ratios given do not strictly give the relative electric strength, but the ratio of the lengths of spark when the difference of potential is kept constant.

The difference of potential required to produce a spark between two spherical balls is approximately proportional to the square root of the length of the spark. This we have verified up to 15 cm.

On proceeding to investigate the discharge through insulating liquids, we first took up oil of turpentine. The liquid was placed in a glass jar of 7 inches diameter and 5 inches height. A screw passing through the bottom of the jar served to fix the lower electrode, and also to afford conducting connection with the earth. We observed four modes of discharge: by means of threads of solid particles, by motion of the liquid, by a disruptive discharge, and by motion of gas bubbles. When a chain was formed the index of the electrometer behaved as if a current were passing. The discharge, when sufficiently great, broke the thread and turned into a spark. The liquid was more easily set in motion when its surface was not much higher than the upper plate. The bubbles of gas appeared to be formed by the passing of the spark. They were always attracted to the negative electrode. When the electrification was neutralised they of course adhered to the under surface of the upper disk; when the disk was electrified negatively they still adhered; when positively they were repelled so as to remain suspended in the liquid or to adhere to the lower electrode, according to the greater or less distance between the electrodes. At a diminished pressure the bubbles produced at the upper surface were observed to effect the discharge by carrying the electricity with them to the negative electrode. The fact that it is possible to cause a shower of electrified bubbles to descend and produce a flash and sound on impinging on the lower surface appears to throw some light upon the nature of lightning balls.

Similar phenomena were observed in paraffin oil, excepting that the gas bubbles produced were generally attracted to the positive surface.

We observed the differences of potential required to pass a spark through paraffin oil and oil of turpentine between plates for distances up to ·5 cm. It was impossible to observe for greater distances, as our insulated wire allowed the charge to escape. For paraffin oil,

$$V = 750\,s - 15;$$

therefore,

$$R = 750.$$

The above has not been reduced to absolute measure. Thus R is constant in the case of the liquids, but variable in the case of the gases.

Electric Strength of Liquid Dielectrics

Air	1
Paraffin Oil (kind used for burning)	4
Oil of Turpentine	3·7

Sparks were taken between two platinum wires placed at right angles to one another. When one of the wires was heated by a voltaic current the electrometer deflection was diminished by about one-fourth of its amount.

We have also investigated the effect upon the electric spark of heating the air round the disks, the pressure being kept constant. The deflections of the electrometer for a constant spark for temperatures from 20° C. to 280° C. indicate a curve which slopes down gradually as the temperature is increased, while the deflections during cooling give a curve which is somewhat lower at the lower temperatures.

These experiments were made in Prof. Tait's laboratory, to whom we are indebted, not only for the use of apparatus, but also for ever ready advice.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 10.—The loss of electricity by an insulated charged body in rarefied gas in an envelope that has conductive connection with the earth is here stated by Herr Narr to be due to two processes distinct in time and intensity, the first, one of outflow, rapid and intense, the other, one of dispersion, slow and weak. The intensity of the former increases with decreasing density of each of the gases used (CO_2 air and H), and also on substituting one gas for the other in the order just given, the density remaining constant. These differences between the gases decrease with the density, and in vacuum fal within the limits of errors of observation. In discussing these results, Herr Narr is led to regard the condensed layer of gas on the conducting system as an insulator, not as a conductor.—Dr. Holz finds that the specific magnetism of magnetic ironstone is the greatest of all magnetic substances hitherto examined. Its maximum permanent magnetism is nearly as great, and partly greater than that of steel as hard as glass. Its permanent magnetism is sooner removed in demagnetisation with the same external forces than that of steel, &c.—Dr. Strouhal enunciates the laws of a mode of sound-production not much studied hitherto, that, viz., of rapid swinging of a rod, a blade, or the like, in air, or the passage of air-currents over strong wires or sharp edges, &c. —Herr Braun contributes a long and interesting paper on the development of electricity as equivalent of chemical processes.—Herr Koch demonstrates the applicability of the method of determining coefficients of elasticity from the bending of short bars supported at the two ends, the sinking in the middle being measured by means of Newton's interference-bands, and he suggests a more thorough investigation of the elasticity of crystals, by the improved means he describes.—Some remarks on the atomic weight of antimony, with reference to Cooke's recent research, are communicated by Herr Schneider.

American Journal of Science and Arts, November.—In the opening paper Prof. Dana considers the value of some distinctive characters generally accepted in defining certain kinds of rocks, as, "older and younger," foliated or not, and porphyritic structure; showing them to be often trivial and inapplicable. —With regard to the relative agency of glaciers and sub-glacial streams in the erosion of valleys, Prof. Miles considers that the streams are of primary importance in working in advance of the ice in deepening and enlarging these valleys, and that the glaciers abrade, modify, and reduce the prominent portions left by the streams, and give them the well-known glaciated surfaces.—Prof. Holden describes certain cloud-shaped forms (obscuring the smaller forms of Janssen) observed on the sun's disc on September 16, and cites a like observation made by Prof. Langley, in 1873, who thinks the effect chiefly due to our own atmosphere, while disposed to admit the possibility of some obscuration in the sun itself.—A pseudomorph after anorthite, from Franklin, New Jersey, is described by Prof. Roepper; and Prof. Verrill gives an account of recent additions to marine fauna of the east coast of North America.—There is also a notice of Edison's sonorous voltameter.—Prof. Marsh's important contribution on the principal characters of American Jurassic dinosaurs has been previously referred to in these columns.

Morphologisches Jahrbuch, vol. iv., Part 3.—Studies on the innervation of the hair-bulbs of domestic animals, by R. Bormel, 70 pages, 3 plates.—On *Gloidium quadrifidum*, a new genus of Protista, by N. Sorokin.—The development of the knee-joint in man, with remarks on the joints in general, and the knee-joints of vertebrates, by A. Bernays.—The skeleton of the Alcyonaria, by G. von Koch, including a minute description of the skeleton in each genus, a general account of it, and a new systematic arrangement, 33 pages, with 2 plates.—C. Hasse continues his studies on fossil vertebræ; this part is devoted to their histology, and is illustrated by 4 plates.

Zeitschrift für wissenschaftliche Zoologie, vol. xxxi. Part 2.—Contribution on the Julidæ, by E. Voges, dealing very considerably with the tracheal system and its development. There are descriptions of many new species of Julus, Spirostreptus, and Spirobolus; 68 pages, 3 plates.—On the development of the blastoderm and the germinal layers in insects, by N. Bobretzky, with figures chiefly of *Porthesia chrysorrhœa*.—On the genus Brisinga, by H. Ludwig.—On Aspidura, a mesozoic genus of ophiurid, by Hans Pohlig.—On the structure and development of sponges, Part 5, by F. E. Schulze; another most valuable contribution, the author having now completely followed the development of *Sycandra raphanus*, 34 pages, with 2 beautiful plates.

Parts 3 and 4 in one.—On the cerebral sulci in Ungulata, by Julius Krueg; the paper deals very largely with the fœtal development of the convolutions, 50 pages, 4 plates.—Contributions to the anatomy of Ophiurans, by Hubert Ludwig, treating especially on the skeleton of arm and mouth, and the sexual organs, 50 pages, 4 plates.—On the generative organs of *Asterina gibbosa*, by Hubert Ludwig, 1 plate.—An account of the anatomy of Magelona, an interesting form, by Dr. W. C. McIntosh, of St. Andrews; translated from English for the journal, 72 pages, 10 plates.—On some cases of parasitism among Infusoria, by J. van Rees.—Brief notes on the development of Anodon, by C. Schierholz.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 12.—"The Magic Mirror of Japan," Part 1, by Professors W. E. Ayrton and John Perry, the Imperial College of Engineering, Japan. Communicated by William Spottiswoode, M.A., Treas. R.S., &c.

The President stated that Prof. Ayrton had agreed to give, in the Friday evening discourse on January 24, at the Royal Institution, a full account of Japanese mirrors, so that on the present occasion he understood the authors of the paper merely proposed to enter very shortly into the subject.

Prof. Ayrton commenced by remarking that mirrors in Japan held a very high position, and constituted the most prominent feature in the Japanese temples, taking the place of the cross in Roman Catholic countries, and that the principal mirror in the Imperial Palace ranked higher than even the Emperor himself. He referred to the important place the mirror held in the very limited furniture of a Japanese household; to the respect attached to it by the women, and to the fact that while the sword was considered as "the soul of the samurai" (or two-sworded class) the mirror was looked on as "the soul of the woman." He next showed experimentally the so-called magic property possessed by certain rare bronze mirrors, sold by the Chinese at about twenty times the cost of the ordinary mirrors of that country, and which consisted in these mirrors being able to reflect from their smooth polished faces the raised patterns of birds, flowers, dragons, or Chinese letters with which their backs were adorned. He stated that he had found this property to be possessed by a very small percentage of the Japanese mirrors which he had experimented on, but that its existence was quite unknown to the people of that country. The phenomenon had been known in China for centuries, and that, therefore, while he showed it experimentally to the Fellows, he did so in case there might be some there who had never seen it, in consequence of these magic mirrors being rare; but he desired it to be remembered that it was not the phenomenon itself but the explanation of it which he had the honour of bringing before them as new.

After citing all the possible ways in which this curious reflecting power could be accounted for, and referring to a number of printed notices that had at various times appeared of the magic mirror, the majority of which were accompanied with a theoretical explanation, he remarked that as the authors had apparently not made direct experiments with the mirror itself to elucidate the cause of the phenomenon, but rather to have satisfied themselves with endeavouring to find out how it could be reproduced in Europe, it was not to be wondered at that many of the suggested possible explanations were very far from the truth. Up to the present time, he believed, the idea of inequality of density of the surface of the metal mirror produced naturally in cooling, or in the supposed process of stamping, seemed to have found most favour in the West, while the belief that this variation in density arose from trickery on the part of the maker was the view entertained in China. Sir David Brewster and Sir Charles Wheatstone, on the other hand, who also thought that trickery was the explanation, believed the artifice to consist in the maker skilfully scratching on the face of the mirror, before polishing, lines exactly corresponding with the pattern on the back.

Prof. Ayrton next described what was the explanation of the phenomenon his experiments, made during the winter of 1877-78, had led him to, viz., that there existed extremely slight irregularities in the curvature of the polished surface (quite invisible to direct vision), of such a nature that the thicker parts, corresponding, of course, with the raised patterns on the back, were flatter than the remaining convex surface, so that there was less dispersion of light from the thick portion than from the thinner. He then described one of a series of diagrams illustrating various experimental arrangements of convergent and divergent beams of light which the authors had availed themselves of, and the use of which constituted, he said, the essence of the system of investigation employed by Prof. Perry and himself, and he explained that if his theory of the phenomenon was correct, then placing the screen, on which the reflection of the light from the Japanese mirror was cast, in a certain position, the phenomenon ought to *disappear*, and again putting the screen in another position, the phenomenon ought to be *inverted*; that is, instead of a bright image on a dark ground, which hitherto had alone been what has been observed by previous investigators, a dark image of the pattern on a bright ground ought to appear. This disappearance and absolute inversion of the phenomenon he said he had found to actually take place, but that he was compelled from want of time to leave the experimental exhibition of it for the Royal Institution. Various other facts, such as the necessity of holding the screen rather near, but not very near, the mirror when ordinary sunlight without lenses was employed, was, like the inversion phenomenon just referred to, shown to be explainable only on the inequality of curvature theory, and not on the inequality of density theory.

The next question that arose was how was this inequality of curvature produced? This was explained to be due to the method employed by the Japanese for making the face of the mirrors convex, which method had hitherto been quite unknown to foreigners, but which Prof. Ayrton had, after much trouble, found to consist in scratching the face while cold with a *megebo*, or "distorting rod." During the operation the mirrors became visibly concave, but, receiving a "buckle," sprung back again so as to become convex when the pressure of the rod was removed. The thicker parts of these magic mirrors yielded less under the pressure, were made therefore less concave when under the rod, and sprung back less, or became less convex, when the pressure of the rod had been removed. He then showed how this explained the fact discovered by Prof. Atkinson, of the Imperial University, Japan, in 1877, that a small scratch made on the back of a mirror with a blunt nail, although producing apparently no effect on the other side, became nevertheless visible as a bright line on the screen when a light was reflected from the mirror.

Prof. Ayrton concluded by remarking that while the Japanese knew nothing of the so-called magic phenomenon that formed the subject of the paper that evening, he had ascertained that they had used another property for their priestcraft, but the account of this he would reserve for the lecture at the Royal Institution.

"On the Torsional Strain which remains in a Glass Fibre after Release from Twisting Stress," by J. Hopkinson, D.Sc., F.R.S. It has long been known that if a wire of metal or fibre of glass be for a time twisted, and be then released, it will not at once return to its initial position, but will exhibit a gradually decreasing torsion in the direction of the impressed twist. The best method of approximating to an expression of the facts has been given by Boltzmann ("Akad. der Wissensch. zu Wien," 1874). He rests his theory upon the assumption that a stress acting for a short time will leave after it has ceased a strain which decreases in amount as time elapses, and that the principle of superposition is applicable to these strains, that is to say, that we may add the after-effects of stresses, whether simultaneous or successive. Boltzmann also finds that, if $\phi(t)\,\tau$ be the strain at time t resulting from a twist lasting a very short time τ, at time $t = 0$, $\phi(t) = \frac{A}{t}$, where A is constant for moderate values of t, but decreases when t is very large or very small. A year ago I made a few experiments on a glass fibre, which showed a deviation from Boltzmann's law.

The glass fibre examined was about 20 inches in length. The glass from which it was drawn was composed of silica, soda, and lime—in fact, was glass No. 1 of my paper on "Residual Change of the Leyden Jar" (*Phil. Trans.*, 1877). In all cases the twist given was one complete revolution. The deflection at any time was determined by the position on a scale of the image of a wire before a lamp, formed by reflection from a light concave mirror, as in Sir W. Thomson's galvanometers and quadrant electrometer. The extremities of the fibre were held in clamps of cork; in the first attempts the upper clamp was not disturbed during the experiment, and the upper extremity of the fibre was assumed to be fixed; the mirror also was attached to the lower clamp. This arrangement was unsatisfactory, as one could not be certain that a part of the observed after-effect was not due to the fibre twisting within the clamps and then sticking. The difficulty was easily avoided by employing two mirrors, each cemented at a single point to the glass fibre itself, one just below the upper clamp, the other just above the lower clamp. The upper mirror merely served, by means of a subsidiary lamp and scale, to bring back the part of the fibre to which it was attached to its initial position. The motion of the lower clamp was damped by attaching to it a vane dipping into a vessel of oil. The temperature of the room when the experiments were tried ranged from 13° C. to 13·8° C., and for the present purpose may be regarded as constant. The lower or reading scale had forty divisions to the inch, and was distant from the glass fibre and mirror 38⅝ inches. Sufficient time elapsed between the experiments to allow all sign of change due to after-effect of torsion to disappear. In all cases the first line of the table gives the time in minutes from release from torsion, the second the deflection of the image from its initial position in scale divisions.

Experiment VI.—Twisted for 121 minutes.

t	½	1	2	3	4	5	7
Scale divisions	191	170	148	136	126½	119¼	108½

t	10	15	30	65	90	120	589
Scale divisions	97	84½	63½	41½	34	28	3½

The time was taken by ear from a clock beating seconds very distinctly.

The first point to be ascertained from these results is whether or not the principle of superposition, assumed by Boltzmann, holds for torsions of the magnitude here used.

If the fibre be twisted for time T through angle X, then the torsion at time t after release will be $X\{\psi(T+t) - \psi(t)\}$ where

$$\psi(t) = \int \phi(t)\, dt.$$

If now $T = t_1 + t_2 + t_3 + \ldots$ we may express the effect of one long twist in terms of several shorter twists by simply noticing that

$$X\{\psi(t) - \psi(t+T)\} = X[\{\psi(t) - \psi(t+t_1)\} + \{\psi(t+t_1) - \psi(t+t_1+t_2)\} + \{\psi(t+t_1+t_2) - \psi(t+t_1+t_2+t_3)\} +, \&c.]$$

Apply this to the preceding results, calculating each experiment from results for shorter time. Let x_t be the value of $\psi(T+t) - \psi(t)$, that is, the torsion at time t, when free, divided by the impressed twist measured in same unit; we obtain the following tables of comparison:—

Results for T = 121 compared with those from T = 20.

t		½	1	2	3	4	5	7
x_t observed	...	0·00979	871	758	697	648	612	556
x_t calculated	...	—	1070	950	880	830	780	730

t		10	15	30	65	90	120	589
x_t observed	...	497	433	325	212	174	144	18
x_t calculated	...	670	600	500	380	350		

The three last tables agree in indicating a large deviation from the principle of superposition, the actual effect being *less* than the sum of the separate effects of the periods of stress into which the actual period may be broken up. Kohlrausch finds the same to be the case for india-rubber, either greater torsions or longer durations give less after-effects than would be expected from smaller torsions and shorter periods.

The deviation from formula $\phi(t) = \frac{A}{t}$ appears to indicate the form $\phi(t) = \frac{A}{t^{\alpha}}$, α being less than, but near to, unity. If $\alpha = 0·95$ we have a fairly satisfactory formula.

$$x_t = A'\left(\overline{T+t}^{\frac{1}{20}} - t^{\frac{1}{20}}\right), \text{ where } A' = \frac{A}{t^{a}} \text{ when } T = 121.$$

In the following table the observed and calculated values of x_t when T = 121 are compared, A′ being taken as 0·032.

t		½	1	2	3	4	5	7
x_t observed	...	0·00979	871	758	697	648	612	556
x_t calculated	...	0·00976	870	755	691	643	600	550

t		10	15	30	65	90	120	589
x_t observed	...	497	433	325	212	174	144	18
x_t calculated	...	493	429	320	218	176	147	42

Taking the formula $\phi(t) = \frac{A}{t}$ these experiments give values of A ranging from 0·0017 to 0·0022. Boltzmann for a fibre, probably of a quite different composition, gives numbers from which it follows that A = 0·0036.

In my paper on "Residual Change of the Leyden Jar," that subject is discussed in the same manner as Boltzmann discusses the after-effect of torsion on a fibre, and it is worth remarking that the results of my experiments can be roughly expressed by a formula in which $\phi(t) = \frac{A}{t^{\alpha}}$. For glass No. 5 (soft crown) $\alpha = 0·65$, whilst for No. 7 (light flint) it is greater; but in the electrical experiment no sign of a definite deviation from the law of superposition was detected.

Geological Society, November 20.—R. Etheridge, F.R.S., vice-president, in the chair.—Rev. James Crompton and John Dennis Paul were elected Fellows.—The following communications were read:—On the upper greensand coral fauna of Haldon, Devonshire, by Prof. P. Martin Duncan, F.R.S. The author in this paper stated that since the publication of his supplement to the British fossil corals, published by the Palæontographical Society, several new corals have been obtained at Haldon by Mr. Vicary, of Exeter. Twelve additional species were noticed, of which ten were new. This brings the total number of species in the Haldon Greensand up to twenty-one. The new species are thus distributed:—*Aporosa*: Oculinidæ (1), Astræidæ (3), Fungidæ (5); *Perforata*: Turbinariæ (2); *Tabulata* (1). The paper concluded with remarks on the genera and species represented, from which it appeared that the coral fauna of Haldon is the northern expression of that of the French and Central European deposits, which are the equivalents of the British upper greensand. The Haldon deposit was formed in shallow water, and the corals grew upon the rolled *débris* of the age.—Notes on *Pleurodon affinis*, sp. ined., Agassiz, and description of three spines of cestracionts from the lower coal-measures, by J. W. Davis, F.G.S.—On the distribution of boulders by other agencies than that of icebergs, by C. E. Austin, C.E., F.G.S.

December 4.—Henry Clifton Sorby, F.R.S., president, in the chair.—Rev. William H. Allen, George Grey Butler, John Dixon, Rev. William Downes, B.A., Joseph Drew, M.D., Arthur Tom Metcalfe, E. P. Monckton, M.A., Albert J. Mott, Philip Lutley Sclater, Ph.D., F.R.S., William Hobbs Shrubsole, and Alexander Thuey, were elected

Fellows of the Society.—The following communications were read:—On some mica-traps from the Kendal and Sedbergh districts, by Prof. T. G. Bonney, F.R.S., and F. T. S. Houghton, B.A. The rocks described by the authors are mapped by the Geological Survey on quarter sheets 98 N.E., 98 S.E., and 97 N.W., and in parts briefly mentioned in the accompanying memoirs, under the generic name mica-trap. Seventeen examples are described macroscopically and microscopically, and of eight chemical analyses are given. It appears better to call one a porphyrite and two diorites (micaceous varieties). The remainder are all characterised by abundance of mica (biotite). Augite also appears to have been generally a constituent; but it has almost invariably been replaced by secondary products, calcite, dolomite, viridite, &c. Three are crystalline in structure; one of these is named minette, the others kersantite. The remaining eleven show a microcrystalline or cryptocrystalline base. It is proposed to call eight of them minette-felsite, the rest kersantite-porphyrite. These rocks commonly occur in rather narrow dykes; they are intrusive in Silurian strata, and, in the authors' opinion, are undoubtedly true igneous rocks.—Pleistocene notes on the Cornish coast near Padstow, by W. A. E. Ussher, F.G.S. In this paper the author described certain deposits seen in a small bay near St. Enodock's chapel, and known as Daymer Bay, and in section at Greenway cliffs.—The pleistocene history of Cornwall, by W. A. E. Ussher, F.G.S. In the first part of this paper the author, from his own observations and the writings of other geologists, gave detailed descriptions of the various superficial deposits of Cornwall, as exposed in numerous coast-sections. In the second part he discussed the relative ages of these deposits, and proposed a classification.

Physical Society, December 14.—Prof. W. G. Adams, president, in the chair.—Mr. W. Gleed and Mr. J. G. McGregor were elected members of the Society.—Prof. Guthrie read a note by Mr. C. Boys on a condenser of variable capacity. This condenser was designed for use in connection with the Holtz electrical machine to show the effect of condensation on the length of the spark. It consists of a test-tube coated externally with tinfoil to form the inner armature and a glass tube inclosing the test-tube, and having its outer surface covered with tinfoil for the outer armature. The inner tube can be slid out or in along the length of the external tube, and the capacity thereby varied. Prof. Guthrie showed that a spark from the Holtz machine could by its means be gradually reduced. Prof. Macdonnel stated that he had for some years used a similar apparatus, the inner coating, however, being strong sulphuric acid.—Dr. O. J. Lodge exhibited a differential thermometer in which saturated water vapour takes the place of air or other gas. This application is based on the fact that the pressure of a saturated vapour in contact with its liquid depends only on the temperature. An ordinary cryophorous answers the purpose when held so that the water occupies part of one bulb and a part of the stem next it; the greater the length of the water column in the latter, that is, the more horizontal the cryophorous is held, the greater the sensitiveness of the instrument. If, now, there be a difference of temperature between the two bulbs there will be a difference of pressure in the vapour in their interiors, and the level of the water will change until the pressure is equilibriated. When both bulbs are at one temperature the water in tube and bulb is on a level. Unlike air thermometers, the sensitiveness does not depend on the size of the bulbs or tube, and there is no increase of volume of the vapour. Another form consists of a U-tube, with bulbs at the end of each arm, each bulb having some liquid and the bend of the tube containing a short column of it, or, for greater sensitiveness, a series of films across the tube like diaphragms. This thermometer is found to be correct for temperatures below that of the ordinary temperature of the water and vapour, but inexact for high temperatures. With these latter the vapour tension is not the same throughout the tube, and distillation is set up. The instrument is a much more sensitive thermoscope than the air thermometer, and there is almost no limit to its sensitiveness to low temperatures. The radiation from the hand, held six inches from it, sensibly affects it, as also does the radiation from a piece of ice. For class purposes it is likely to be useful, from its simplicity and range of delicacy.—Mr. W. Clarke, Cooper's Hill College, from a series of experiments which he is making on the surface-tension of liquid gases, by means of their capillarity, gave the surface-tension of sulphurous anhydride as 2·3 milligrammes per square millimetre at -15° C.

Royal Microscopical Society, November 13.—Mr. H. J. Slack, president, in the chair.—A paper was read by Dr. Royston Pigott on some further inquiry into the limits of microscopic vision and the delusive application of Fraunhofer's optical law of vision, in the course of which he described numerous experiments to show that this well-known formula depends upon the laws of diffraction from rays reflected by bright discs or objects, but that it failed when applied to dark lines which were capable of being rendered visible far beyond the limits therein laid down. The subject was illustrated by numerous diagrams and by objects and apparatus exhibited in the room.—The President detailed the result of some recent experiments to determine the distance at which a human hair could be seen under various conditions by ordinary unassisted vision.—Mr. F. H. Wenham read a paper on the measurement of the angle of aperture of objectives, in which he described the method of measuring the true angle of aperture as distinguished from that of the angle of field with which it was commonly confused.—Mr. Henry Davis read a paper on the pygidium of insects, showing the organ commonly known by this name had its representatives in the Neuroptera, Gryllidæ, and other groups of insects, as well as in the flea and lacewing fly. He gave reasons for regarding it as a special organ of sensation conveying to the insect an intimation of the presence of dangerous enemies. Some discussion upon the subject took place between Mr. C. Stewart, Mr. Beck, Mr. Slack, and the author of the paper.—Some further communications, arising out of correspondence with Mr. Bedwell, were laid before the meeting by the Secretaries.—Three new Fellows were elected.

Institution of Civil Engineers, December 3.—Mr. Abernethy, vice-president, in the chair.—A paper was read on the heating and ventilating apparatus of the Glasgow University, by Mr. Wilson W. Phipson, M. Inst. C.E.

VIENNA

Imperial Academy of Sciences, October 17.—The following among other papers were read:—Remarks on Stephan's fundamental formulæ of electrodynamics, by Dr. Margules.—A hypothesis on the physical state of the sun, by Prof. Puschl.—Light as a reagent, by Herr Bohatta.—On the *Tonsilla* and *Bursa pharyngea*, by Dr. Ganghofner.

October 24.—On a simple apparatus for obtaining a constant gas pressure, by Dr. Handl.—A contribution to the doctrine of conic sections in descriptive geometry, by Prof. Miksic.—On meteorology of Alpine heights, by Dr. Hann.—On the formation of a space-curve of the fourth order, with a double point on a conic section.

November 7.—On the Venus transit of December 6, 1882, by Dr. Friesach.—Contribution to knowledge of internal friction in iron, by Herr Klemencic.—On the pitch of a tuning-fork in an incompressible liquid, by Prof. Kolacek.—Determination of orbit of the sixth comet of 1874.

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