THURSDAY, SEPTEMBER 13, 1883

SCIENTIFIC ASPECTS OF THE JAVA CATASTROPHE

CAUTION and scepticism, which are necessary to the student of every branch of natural science, ought to be the especial attributes of the vulcanologist and seismologist. No other natural phenomena so strikingly affect the imagination or so powerfully excite the fancy as do the volcanic outburst and the earthquake. These catastrophes usually occur too with such startling suddenness and with such an entire absence of warning that the witnesses are not unnaturally paralysed by fear and terror. Under such circumstances the wildest and most improbable stories are received and circulated with easy credence, and no attempt is made to separate the real from the imaginary.

Illustrations of these remarks might be adduced in connection with each of the great subterranean disturbances which have taken place during recent years. Thus the accounts received of the earthquake of Agram stated that fissures had opened in the ground from which smoke and flames issued and along which volcanic cones were thrown up. The report of the Commission appointed by the Hungarian Government to investigate the facts of the case upon the spot proves conclusively that these stories had no other foundation than the emission of small jets of water and the formation by them of sand-cones, a phenomenon frequently witnessed during earthquake shocks. An article in the last number of this journal upon the recent earthquake in Ischia shows that similar discrepancies exist between the first hastily-published accounts and the soberer testimony of careful observers.

In the case of the Java catastrophe, however, there appears to have been at least one attempt to hoax the newspaper-reading public by deliberately manufactured accounts of the event. A detailed statement purporting to come from an eye-witness, and telegraphed by way of America, was published in many of the daily papers. The circumstances recorded in this statement would have been startling indeed had they been true, but, as a writer in the *Scotsman* has already pointed out, the account bears too manifestly on its face abundant proofs of its want of genuineness.

Setting aside these fictitious accounts, and making every allowance for the exaggeration naturally resulting from terror, and the difficulty which under the circumstances of the case there must be of obtaining reliable information, sufficient remains to prove that the recent catastrophe resulted from one of the grandest and most destructive volcanic outbursts which have occurred in modern times.

The scene of this outburst was at what is at the present day probably the focus of the most intense volcanic activity upon the face of the globe. The Island of Java contains no less than forty-six great volcanic mountains, nearly one half of which have been in activity during historical times. This chain of volcanoes is continued in the southern part of Sumatra. Since the colonisation of these islands several volcanic eruptions on the very grandest scale have taken place, at points not very distant from the scene of the recent catastrophe. In 1772 occurred the great eruption of Papandayang, when the whole upper part of the mountain was blown away, leaving a vast crater fifteen miles long by six miles broad. The quantity of material ejected during this eruption was so great that, according to Dr. Junghuhn, an area of seven miles radius around the mountain was in a single night covered with scoriæ and ashes to the depth of nearly fifty feet. Forty native villages were overwhelmed, and 3000 persons perished. In 1822 the neighbouring volcano of Galunggong was in eruption, and 114 villages were buried beneath the scoriæ and ashes, while the destruction of human life was so great that more than 4000 killed were recorded in the official reports.

According to the most reliable accounts received up to the present time, the recent outbreak would appear to have been far more fatal to human life than either of its predecessors, and the most potent agent of destruction in this, as in so many other cases, would seem to have been the great sea-wave produced by the earthquake-shock, rather than the showers of materials ejected from the volcanoes.

Divested of their marvellous accompaniments, and read by the light of modern vulcanologic science, the accounts already received of this great catastrophe seem to prove the occurrence of the following events:—First, the ejection of enormous quantities of fragmentary materials; secondly, the production of great changes in the form and outlines of the volcanic Island of Krakatoa; thirdly, the throwing up of a line of new volcanic cones on a fissure opened in the sea-bed between Java and Sumatra; and, fourthly, the occurrence of one or more earthquake shocks, giving rise to forced sea-waves of great destructiveness.

The quantity of materials ejected during these eruptions is proved by two facts recorded in the accounts already published: firstly, the widespread and long-continued darkness, doubtless produced by the clouds of finely comminuted dust carried away from the volcano by the wind; and, secondly, by the vast mass of scoriæ which seems to have accumulated upon and floated over many portions of the surface of the surrounding seas.

Concerning the extent and nature of the changes of the features of Krakatoa we must await further and reliable evidence. As in the case of Papandayang, the destruction of the volcano was doubtless primarily due to the eruptive action, which truncated the cone and formed a gigantic crater, and whether or not this action was accompanied by subsidence, whereby the disappearance of the island was consummated, it remains for further investigations to determine. It is well, however, to bear in mind that many reputed cases of the submergence of islands have on further examination resolved themselves into the removal of materials by explosive action, just as most instances of the elevation of volcanic islands above the sea-level have been doubtless due to the piling up of the materials above the level of the waves.

The position and relations of the new line of volcanic cones must be determined by the surveying vessels which will doubtless be sent to the spot so soon as it is considered safe to do so. Fortunately a number of admirable charts of these seas have been constructed by the hydrographers of this and other countries, and by a comparison of these with the new charts which will now have to be made we shall be able to judge of the actual changes in the features of this part of the globe which have been wrought by this great outburst. It appears to have been the first belief of the naval authorities upon the spot that these changes were of such magnitude as to render it unsafe for vessels to attempt to pass the Straits of Sunda until new surveys had been made. Later accounts, however, prove that the principal channel by which vessels traverse the straits has remained unaffected by the eruptions.

We may confidently hope that a comparison of the times at which the great sea-wave, produced by the earthquake, reached various ports will enable us to correct and extend our knowledge concerning the depth of certain portions of the Pacific and Indian Oceans. For this, as for many other details of great importance to science, we must await the careful collection and sifting of evidence which will doubtless be undertaken by a Commission appointed by the Dutch Government.

The portion of the Island of Java visited by this terrible calamity is exceedingly fertile, rich, and populous, and if the present estimate of the loss of life be not excessive, this catastrophe must probably rank as the greatest which has occurred in modern times, so far as the destruction of human life is concerned.

The repeated eruptions of Vesuvius and Etna have failed to drive away the vine-dressers from the fertile slopes of those mountains, and in the same way the forces of destruction which evidently lie dormant beneath Java only produce temporary interruptions in its story of plenty and prosperity. As it is now, so was it in past geological times. The districts of Hungary, Auvergne, and the Inner Hebrides, which in former geological periods were subjected to subterranean disturbances similar in character and violence to those which now affect Java, were, in the intervals between the volcanic outbursts, rich and fertile, a fact which is testified to by the remains of forests and of the wild animals which roamed through them, found in the deposits lying between successive lava-flows. Volcanic eruptions are frequently very destructive; earthquake shocks are often still more fatal to man and his works; but fortunately successive catastrophes of both kinds are usually separated by long intervals of time, and it is the recognition of this fact which leads men to brave alike both kinds of danger.

AUTUMN SANITATION

IT is not only the steady decline of cholera in Egypt that gives substantial assurance that we shall now escape any epidemic in this country, but it is also the advancing season. There are, however, few subjects concerning which less is known than the influence of climate and season on the progress of the infectious diseases. But, as regards cholera, we know from experience that it is not very likely to make its appearance in this country when once the colder weather has set in. It has generally first shown itself with us during the hot summer months, and it is probable that a foul, damp air, together with a certain degree of warmth, are most favourable to its prevalence. It is not that we have never suffered from it during the colder months, for it was somewhat widely prevalent in October and November of 1853, the year which preceded the great epidemic of 1854, when so many cities, both in the Old and New World, were devastated. And even though actual winter has, even on such an occasion as that referred to, for a time completely checked the further progress of cholera, yet there is no reason to believe that any cold which the human frame can bear has the power of destroying the infection. At Moscow and at Orenburg in 1830 cholera prevailed in spite of a temperature of $-4°$ F. And judging from analogy it would appear that much lower degrees of temperature than these fail to destroy infections such as that of cholera. Thus, tubes containing the characteristic spores of the bacillus anthracis have been exposed to a temperature of $-32°$ F.; and yet on being thawed they have remained potent for harm as before. Indeed, we may infer that, provided other conditions necessary for the life of the contagion are present, warmth is not essential, and that no amount of cold is absolutely incompatible with the development either of the cholera poison or of the infection of many other contagia. Still, cholera has been with us essentially a summer epidemic, and as each week of the present month passes away without its being imported into the country we may feel more and more assured that we have succeeded in escaping the danger of an outbreak.

There is also another disease that with the commencement of autumn rapidly subsides. We refer to that form of diarrhœa known as infantile, a specific disease that causes year by year a large fatality, especially in certain of our manufacturing towns. This disease, too, is, to a certain extent, one of season. At Leicester, Preston, and Nottingham, the death-rate from this cause is always exceptionally high during the third quarter of the year, its main incidence being on the first two months of the quarter. Thus, taking the year 1881, it appears that, whereas the mortality from this cause in the twenty large towns and cities of England was 409 and 593 respectively, it rose to 4390 in the third quarter. But temperature alone does not account for this large mortality. Oldham, Rochdale, and Halifax resemble the three towns abovenamed in many important social and other respects; they do not materially differ from them as regards climate, and yet the infantile diarrhœa death-rate is with them always exceptionally low. Indeed the difficult problems connected with the etiology of this disease are such that the Government have commissioned Dr. Ed. Ballard to make a comprehensive inquiry into its causes, and it is hoped that his investigations, which have now been in progress for more than two years, will throw important light on the whole subject.

But as the diseases of one season subside those of another make their appearance. Many of the public are under the vague impression that cold weather and a good sharp frost have some effect in "clearing the air" and in getting rid of infection. But, as regards some diseases, this is altogether a mistake. Thus, typhus fever and small-pox, which are at their lowest ebb, or altogether disappear, during the hot summer months, tend to reappear as the autumn sets in, and they assume their greatest force at the depth of winter. But this again is probably not all due to seasonal causes. The cold with which these diseases are so specially related forces those who are poor and ill-clad to remain huddled together indoors; the greater the cold, the worse the overcrowding in the densely-peopled portions of our cities, and hence opportunities for personal infection, which are at their minimum in the hot summer months when doors and windows are open, reach their maximum in the coldest months.

Some diseases find the autumn months especially congenial to their development and spread, and of these the one that merits most attention as the present season advances is enteric fever, or typhoid fever as it is more commonly, but less appropriately, named. So peculiarly is this affection identified with the autumn months that amongst its best-known synonyms the terms autumnal fever or fall fever are well known; and under ordinary circumstances the largest number of attacks occurs in the month of October; November follows next, and then come September and August. Fortunately, as regards enteric fever also, something more than season is needed to favour its appearance and spread. The infection of enteric fever is of all others the one that in our climate can most easily be rendered harmless. For its development it needs that special form of filth which is associated with human excreta, and whether these foul the air of our dwellings by reason of defective means of drainage, or whether they pollute the soil on which we live or from whence we derive our water supplies, it matters little. Wherever the contamination is there is a soil adapted to the reception and cultivation of the infection. In this respect enteric fever resembles cholera, and, if the warnings which have been so widely circulated throughout the country during the past few months with regard to the measures that should be taken with a view to the prevention of the latter disease have not been unheeded by the public and by our sanitary authorities, we should this autumn feel more satisfied than we ever have done that the conditions necessary to the spread of this autumnal fever do not prevail amongst us as they have done heretofore. Scarlet fever, again, often reaches its widest prevalence towards the commencement of the fourth quarter of the year; and respiratory diseases, including pneumonia, which has now come to be regarded as much more frequently a specific pulmonary affection associated with defective local sanitary circumstances than a mere result of cold, as a rule rise steadily in prevalence until about the middle of November, when they again tend to subside.

Seasons and their predisposing influences must necessarily go and come, but they alone do not suffice for the production of the specific infections. As the science of preventive medicine progresses, we may hope that other conditions, as necessary to the development of infection as are the climatic ones, will steadily be removed, and that our sense of security against preventable disease may not be troubled by mere considerations of season. For the moment the indications are to secure that the air in our dwellings, as also our water, milk, and other food supplies, shall be as far as practicable free from the risk of all contaminating influences; to maintain, as regards our homes and our bodies, the utmost procurable cleanliness; and so to clothe ourselves that we shall be able to resist the depressing effects of the damp and cold which are sure to alternate with the finest weather an autumn season can produce.

TROPICAL AGRICULTURE

The Tropical Agriculturist: a Monthly Record of Information for Planters of Coffee, Tea, Cocoa, Cinchona, Indiarubber, Sugar, Tobacco, Cardamoms, Palms, Rice, and other Products suited for Cultivation in the Tropics. Compiled by A. M. and J. Ferguson, of the *Ceylon Observer.* (London: J. Haddon and Co., 3, Bouverie Street, 1882.)

A BULKY volume containing thirteen monthly numbers and occupying more than a thousand pages can hardly fail to contain a large amount of varied and useful information, especially when it deals with such a subject as tropical agriculture. Not only tropical but subtropical regions are laid under tribute, the latter being represented chiefly by Southern Australia, New Zealand, and China, while Ceylon and the various provinces of India receive, as might be expected, the greatest share of attention. There are, moreover, abundant references to several oceanic islands which have within recent years been invested with more or less political interest. Thus of Fiji it is stated that the planters are chiefly concerned in growing sugar-cane, coffee, and cotton, and though it is claimed that the first-named is indigenous, the best kinds of cane grown in the plantations have been introduced. The Sea Island cotton is easily cultivated, but the production has lately fallen off, the quotations being too low to tempt the planter. Tobacco answers well, and it is believed that cocoa, tapioca, ginger, pepper, and all sorts of spices, camphor, and vanilla, might also be profitably grown. Madagascar appears to have bright agricultural prospects before it, as it is admirably adapted to the cultivation of sugar and coffee, and indeed as a sugar-growing country it seems likely that it will before many years leave Mauritius in the background. The small islands between Madagascar and the mainland are enthusiastically spoken of as a new planting region: "situated in a most salubrious climate, between the southern tropic and the line, they are admirably adapted for the cultivation of sugar, coffee, vanilla, cocoa, spices, cloves, and other products, many of which are pure articles of luxury, and will always command a high price in the European market."

Judging from the space allotted to them and the amount of interest that appears to centre round them, the staple crops of tropical agriculture are tea, coffee, cocoa, and sugar; cinchona and tobacco; indiarubber, cotton, and gums, to say nothing of rice. Of the first group, tropical countries may rest fairly securely in the cultivation of tea, coffee, and cocoa, and although the sugar-cane is largely planted in the southern United States and the sugar-beet is so extensively grown in Europe, yet we gather that sugar cultivation is a thriving industry in India, Java, Mauritius, the Malay peninsula, Queensland, Fiji, Brazil, Jamaica, and Trinidad. Cinchona is of course a highly popular subject, and from this volume alone a very large amount of useful information may be gleaned. On account of the rapid development of the electrical industries and of the increasing use of elastic tires for wheels, the demand for indiarubber and guttapercha is continually increasing, and this will no doubt be met by the extended cultivation of these products. The official papers relating to the introduction of the Para and Ceara rubber plants into

India are reproduced; the original seeds which were obtained in South America were sown at Kew, and the young plants sent thence to the East, but the precarious nature of the undertaking may be inferred from the fact that only about three per cent. of the seeds germinated. It is pleasant to read here and there spontaneous testimony to the value of the Royal Gardens at Kew and of the Indian Botanic Gardens.

Of controversial subjects the coffee leaf disease attracted most notice, considerable space being devoted to the reports and letters of Mr. Marshall Ward, and to the discussions arising therefrom. On p. 15 is a complacent suggestion that as crops cannot always be got from the branches of the coffee tree they might be got in another form from the roots by grinding up the cockchafers that there abound and selling the beetle powder, mixed with a little coffee, as real coffee, carrying on the entire manufacture in Ceylon to prevent any tampering on the part of dishonest middlemen in London! This pleasant notion is based on the assumption that "the British public will consume anything not absolutely dirt that is sufficiently adulterated to suit their palates."

The marked contrast between our home agriculture and that of the tropics is afforded in the very few and scanty references to live stock of any kind. English agriculturists are continually relying more and more on their flocks and herds and less on their corn crops for remunerative returns. There is, indeed, a solitary reference to Aden cattle, which are bred inland, and derive their name only from the port whence they are shipped. They have a high reputation as dairy stock, and have been used with success for crossing with some of the Indian herds on the Government farm at Saidapet, Madras. The only allusion to sheep-farming is to that of Australia.

Of course, in such a volume as the one before us, the matter is necessarily of a very heterogeneous character, but it is all concerned more or less directly either with agriculture itself, or with the economic and industrial aspects of the art as pursued in the hotter regions of the globe. As a record of the experience of tropical planters, of the difficulties and drawbacks of climate and of soil they have to contend with, of the good or indifferent results which have attended their efforts at acclimatisation, of the measures they have adopted to minimise the evil effects of insect or fungal attacks, and not less as an interesting historical summary of the progress of tropical agriculture, such a work as this carried out on the lines on which it has been begun cannot fail to possess a permanent value. Young men especially, who, having learnt something of the art of agriculture in the stern school of British farming, would fain try their skill under a tropical sun, will find collected here a large mass of useful information such as perhaps it would hardly be possible to obtain elsewhere.

W. FREAM

OUR BOOK SHELF

Vorlesungen über Pflanzen-physiologie. Von Julius Sachs. (Leipzig: Wilhelm Engelmann, 1882-83.)

THE fourth edition of Prof. Sachs's well-known text-book of botany being nearly exhausted, his friends and publishers urged him to set about the preparation of a new edition; but the revisal necessary for the publication of the fourth edition had been so irksome that nothing would induce the author to attempt the task again. Moreover his views on many important questions concerning the physiology of plants had changed: points once considered all-important had lost much of their importance, and expanded views acquired in the progress of time could not be made to fit into the framework of the old work.

Prof. Sachs for years has been a most successful teacher of botany. His text-book, large and technical though it was, has had a most successful career in German-speaking countries; translated into French by an eminent French botanist, and into English under the auspices of the Delegates of the Clarendon Press at Oxford, there needed no higher testimonies to its worth; still, instead of being content with the success of his volume, he now refuses to look at it, and utterly casts it from him. "As long as the artist is pleased with his work, he can add a touch here and there, or can even go in for greater changes; but this is not sufficient when the work has ceased to be the expression of his idea, and this is the attitude I stand in with regard to my text-book." This state of mind has resulted in the publication of the fine volume which we now notice; in size and general appearance it differs very little from the author's text-book, but under the style of lectures it appeals to a wider circle of readers than mere college students. Ardently anxious that the very important modern views on plant physiology should be known to all fairly educated people, these lectures, without sacrificing scientific accuracy, are written in a style as free as possible from the fatiguing use of long and purely technical words; they are purposely written too in a slightly dogmatic style, for it is clearly a lecturer's duty to put before his audience his own individual views upon even debated questions; his hearers have a perfect right to know what impresion the general aggregate of scientific facts has made upon his mind, and while this would be out of place in a technical text-book of the science, it harmonises well with a course of lectures.

At the end of each lecture some—we could have wished for more—bibliographical notes are added for the benefit of those readers who wish to plunge deeper into the subject.

The publishers wished that a new revised edition of the systematic part of the text-book should have been "tagged" on to these lectures, but Prof. Sachs declared that he had neither time nor inclination for the task, which he commits to the care of Prof. Gœbel, whose separate treatise on this part of the subject has lately made its appearance. We hope the day may not be far off when these charming lectures on plant physiology will be read in English by a large number of our cultivated public.

E. P. W.

Accented Five-figure Logarithms of Numbers from 1 *to* 99999 *without Differences.* Arranged and Accented by L. D'A. Jackson. (London: W. H. Allen, 1883.)

IN this work are comprised two sets of tables. The first set (pp. 1-221) is indicated by the above title-page; the second is entitled "Accented Five-figure Logarithms of Sines, Tangents, Cotangents, and Cosines of Angles from 0° to 90° to every Hundredth of a Degree" (pp. 224-270). There is, further, a one-page "Comparison of French and English Decimal Scientific Systems at 32° and 39° Fahrenheit in vacuo." The possessors of the same author's "Accented Four-figure Logarithms" are already acquainted with his principles of accentuation; to those who have not this work we need only say that *excess* and *defect* are clearly indicated in the printing, and that the degree of accuracy attainable in any piece of calculation is very rarely inferior to that reached by the longer calculations with the ordinary seven-figure tables. The logarithm of any number is seen at a glance, so that there is no using of differences, proportional parts, or antilogarithms. In his introduction the author works out some examples with ordinary unaccented five and seven-figure tables, and with these accented tables. On the hypothesis that the tables are correctly printed—we have detected no error—we commend this book as being one that will save much time in calculation without entailing a loss of accuracy. The figures are very clearly printed.

LETTERS TO THE EDITOR

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

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

The Earthquake of Ischia

In NATURE, August 30, p. 414, a correspondent remarks: "The recent catastrophe in the Island of Ischia has called the attention of those who make a study of such disturbances of the earth's surface to the simultaneous occurrence of earthquakes in various parts of the world"; while in NATURE of August 16, p. 368, Mr. Milne, in his article on "Earth Pulsations," says: "The directions in which these tips of the soil take place, which phenomena are noticeable in seismic as well as microseismic motions, Rossi states are related to the directions of certain lines of faulting."

With a view to call attention to the connection between earthquakes occurring in different parts of the globe, either simultaneously or successively, I submitted to the British Association at Swansea a paper on the relation between coast-line directions and local ties in Europe marked by frequency of earthquakes, as also a map illustrative thereof. In this paper, and in a previous one published in the *Transactions of the Royal Irish Academy*, I started with, and endeavoured to prove, the principle laid down by Rossi as to the connection between lines of faulting and earthquake movements, and the map submitted tended to show this relation as being very marked for certain lines of direction.

I now beg leave to call attention to the following lines of action shown thereon in relation with the Ischia earthquake. Amongst the lines cited were those of the *east coast of Sweden* and *east coast of Sardinia*, both nearly parallel, and thereon marked. As regards the first, I state in the memoir (p. 508 of the *Proceedings of the Royal Irish Academy*, 2nd series, vol. iii.; *Science*, No. 8, May, 1882):—

"The section of this line between Rome and Rimini is one of the best marked earthquake lines in Italy, whilst the section between Pola and Brück is also well defined as a direction by a series of points along which shocks have been continually occurring." I further add: "Between Palermo and Naples a parallel to this coast-line seems to be marked by earthquake movements cited as having extended from one point to the other (April 16, 1817)." Now this line passes precisely at Ischia, and, being extended, passes at or near the following places noticeable for earthquakes:—

Corleone, Palermo, Ischia, Teano, Isernia, Lanciana, Grossa Island, Neustadt (Carmola), Marburg, Semering Pass, Neuburg (near Vienna), Znaïm, Glatz, Breslau. Its prolongation represents the axis of the Baltic and the coast-line of Finland from Nystad to Biorniborg.

The west coast of Sicily furnishes a parallel to this direction, and on it are the following earthquake points:—

Frosinone, Aquila, Aseoli, Laybach, Hirschberg, the Riesengebirge, Glogau, and quite recently (May) the points in Finland—Nykerleby, Wasa, and Itterseppo.

Although unacquainted with the geology of the countries traversed by these directions, I am convinced that there occurs marked jointing both in Sicily and in Italy corresponding to the direction of these lines, about N. 10° 30′ E.

It seems to me that they furnish a means of connecting these phenomena, and that they allow of some approach being made to the determination of the laws which govern the occurrence of earthquakes in these parts of Europe. J. P. O'REILLY

Mr. Romanes and Modern Philosophy

As an adherent of the school of thought which Mr. G. J. Romanes (NATURE, vol. xxviii. p. 387) has subjected to such a spirited criticism, I may perhaps be allowed to make a few comments upon his position.

If all science, at least all that has outgrown the mere registration of facts, consists in the application to the latter of certain necessary principles of thinking, it is at least possible that philosophy—an important part of whose function is the systematic elucidation of these principles—may be of some use in scientific procedure.

Mr. Romanes, if he accepts the fact that nature is an object of knowledge, cannot deny that it is governed by the essential conditions of knowing. It is usual, but hardly fair, to confound such obvious statements with the subjective idealism which "makes the universe revolve round the philosopher." A necessary principle is one that evidently does not apply simply to the experience of a single man, a corollary that ought to save much confusion concerning the relations between mind and matter. If, too, it had been recognised that such principles admit of no ulterior possibilities, we would have been spared the controversy anent non-Euclidean space.

With regard to their application to facts of experience, or conceptions derived from these, it may be remembered that the sciences in whose results we place the most confidence—*e.g.* mathematics, mathematical physics, and astronomy, are chiefly deductive. With regard to the portions of these which apparently consist of empirical generalisations, it should not be impossible to show that they, in common with the whole of mathematical science, really flow from the constitution of our experience of nature.

In dealing with the question of mechanism and teleology it is a common error to think that in using the higher categories there is any supersession of such principles as those of cause and effect. As objects of outer experience, organisms are certainly conditioned by the latter; but when they are regarded as subjects, as well as objects, we are compelled to recognise the one-sidedness of such categories—to read into them, as it were, our own active subjectivity. Even the most dogmatic materialist might suspect that the conception of cause and effect is not adequate to a complete solution of the problems presented by living beings. In accepting causality, then, as a truth of universal application, it is not in any sense unscientific to regard it as merely one among the principles which regulate nature.

With regard to the special applications of teleology (in the philosophical sense), it is easy to find instances of incorrect deduction, because of the undeveloped condition of this portion of the subject. Treatises upon it can therefore only be considered as suggestive. Mr. Romanes seems to fear that such efforts will create a dogmatism fatal to scientific progress, although he is aware that the tendency of the times is in exactly the reverse direction. The *a priori* of modern philosophy is of a far different nature from that of scholasticism, and may be in many cases quite as scientific as that which determines the impossibility of perpetual motion, or prophesies a transit of Venus.

Crewe, August 30 ALFRED STAPLEY

Animal Intelligence

THE columns of NATURE have sometimes been open to statements illustrating the practical sagacity of animals of the lower classes. Allow me to place before you the history of an occurrence which appears to prove the power of organisation in the common house-mouse.

The room to which I shall refer is one of several which were built as additions to the original house; it was used solely as a bedroom. I think it very probable that the old and the new apartments were so united that there was no clear mouse-way between them.

I had been sleeping alone in the room, I believe for several weeks, without any disturbance. One night I was woke up, I believe some hours after midnight, by such a grinding under the floor as I never heard before. It was evidently useless for me to attempt to interrupt it, and indeed I was rather curious to observe what would ultimately happen, and I lay quiet in bed. Daylight approached, and still the grinding continued. At last the noise suddenly ceased, and in a minute the room seemed to be filled with mice, running about in every direction. I did not, however, perceive that they mounted the bed or the bed-furniture. At last I perceived a mouse ascending the wall. In my full front view was a long bell-pull, hanging nearly from the ceiling to the floor. A mouse (I fancied, larger than the other mice) deliberately climbed to the top, turned himself round, and for some minutes quietly surveyed the room; then deliberately descended; and, in two or three minutes, not a mouse was left in the room. I slept in the same room many weeks after this occurrence, but I never again perceived the sign of a mouse.

I imagine that the mice inhabiting the house had perceived that this room was now partially inhabited, and that they suspected that it would probably contain something interesting to them; that, acting under a general, or chief engineer, they directed the whole strength of their tribe to work an entrance into the room; that their chief engineer, as soon as an entrance was gained, proceeded to examine the contents of the captured fortress; and that, thoroughly disappointed, he gave the signal for retreat, which the whole body of mice instantly obeyed.

September 10 A. B. G.

"Cholera and Copper"

WITH reference to the letter on the above subject in this week's NATURE, it is quite true that the last visitation of cholera was especially severe here, yet in no single instance was a worker in the copper works of the neighbourhood attacked. It is the common boast of the copper-men that, although they lost many members of their family, living in the same house, by the dread disease, yet neither in the last visitation nor the previous ones was there a copper-man, *i.e.* a man working at a furnace, attacked.

There is no doubt that these men take large quantities of copper sulphate into their systems, for not only do they breathe the fine dust of regulus always floating about, but they handle their food with unwashed hands, or, if washed, not washed clean, and that their hands are covered with soluble copper salts is evidenced by their action on the iron tools which they handle, these quickly receiving a deposit of metallic copper.

This seems to be pretty fair evidence that copper is a preventive for cholera.

I may point out that it is not copper or any of its compounds which injures the vegetation, but sulphur dioxide, the principal gas evolved in smelting copper ores, and which goes by the name of "copper smoke." W. TERRILL

Ffynone Club, Swansea, September 8

Antiquities saved by Protective Resemblance

A LARGE number of pillar stones marked with crosses, early Christian inscriptions and oghams, have been destroyed in Britain by farmers during the present century; a still greater number must have been destroyed before these objects began to attract special attention. A great number of the still remaining examples have been utilised as gate-posts and rubbing-stones for cattle, *i.e.* upright stones set up in fields by Welsh farmers for cattle to rub their itching skins against. This fortuitous resemblance of the slightly squared inscribed stones has protected them from destruction. A few of the flatter examples have been utilised as bridges over narrow streams. Nearly all the examples which have not resembled the above-mentioned objects have met with destruction. It is a sort of survival of the fittest.

In Wales there are many ruined churches and monastic establishments with interiors gutted. Most of the old stone altar-slabs have so closely resembled doorsteps, that they have been saved. It is no uncommon thing to see an altar-slab with its five little crosses utilised as a doorstep to a cottage near a deserted church.

The bowl of a font often bears a sufficiently strong resemblance to a pig-trough to insure its preservation, and if the font is not visible in a ruined church the strong probability is it will be found utilised as a pig-trough in some neighbouring farmyard. Fonts with shallow bowls are specially preserved.

Stone coffins sometimes owe their preservation to their resemblance to and suitability for horse-troughs.

In some instances old churches are now used as barns, and in others as residences for farmers or farmers' men; sometimes a wooden floor has been erected across an old church and the upper part used as a store for hay, and the altar end as a pantry. I have seen the recess of the piscina furnished with a wooden door and the interior used as a cool receptacle for butter and lard. A fortuitous resemblance has protected it.

I could write out a large number of examples of the above and other curious instances of "protective resemblance" in antiquities. Indeed the above facts are so well known to antiqaries that, unless very inconvenient, no "rubbing-stone" or stone gate post is left unexamined in a strange district. Doorsteps, flat stones across streams, and stone hog-troughs are always carefully scrutinised by experienced archæologists.

WORTHINGTON G. SMITH

Meteor

A METEOR of surpassing brilliancy made its appearance here at about 4.46 p.m. on July 12. Its form might be described as somewhat rocket-like. It was observed streaming slowly from the west in an easterly direction, at an apparent altitude of about 45 degrees. Some idea of the brilliancy of this phenomenon may be formed when it is mentioned that it was seen in broad daylight, the sun setting on that day at 4.35 p.m. I notice the meteor was observed over a wide extent of country on the Canterbury Plains; it was noticed from Christchurch, and also at Rangiora, to the north. THOMAS H. POTTS

Ohinitahi, New Zealand, July 14

The Meteor of August 19

THE meteor described in your issue of August 23 (p. 389) was well seen here (lat. 1° west, long. 54° 15′ north) and formed a splendid object. It bore a little east of south, and its apparent path was nearly horizontal from west to east, towards and at about the same declination as the full moon. It would be interesting if its height above the earth were approximately ascertained and stated from the various observations made.

The Grange, Nawton, Yorkshire C. D.

HERMANN MÜLLER

THE news of the death of Hermann Müller of Lippstadt will come with a sense of personal loss to many of our readers, who have looked with interest for his frequent contributions to the columns of NATURE on the branch of natural history which he has made specially his own—the mutual relations to one another of insects and flowers in promoting cross-fertilisation. Much as we owe on this subject to some of our own naturalists, especially Darwin and Lubbock, the chief authority in it is, and probably always will be, Hermann Müller. Any future inquirer will necessarily turn, for the main part of his information, to his two great works, "Die Befruchtung der Blumen durch Insekten," published in 1873, and "Alpenblumen, ihre Befruchtung durch Insekten," published in 1881. The mass of information contained in these volumes is simply marvellous. In the first place the author has worked out with the greatest care the structure of those classes of insects which play the greatest part in the fertilisation of flowers with regard to their capacity for collecting nectar or pollen, and for carrying pollen from flower to flower. A very large proportion, including all the commoner ones, of the species which make up the phanerogamic flora of Central Europe are then taken up *seriatim*, the structure of the male and female organs described, illustrated often with very careful drawings, and always with reference to any special contrivances connected with the mode in which insects obtain the honey; and then a list is given of all the insects which he has observed visiting the flower. No one who has worked in the same field will fail to recognise the unfailing trustworthiness and accuracy of his observations. The "Befruchtung der Blumen" has only during the present year been presented to English readers in Mr. D'Arcy Thompson's translation, with an appreciative preface by the late Mr. C. Darwin, a notice of which will shortly appear in our columns. But these two works by no means exhaust Prof. Müller's labours in his favourite subject, as his numerous contributions to our columns show. He was also a frequent contributor to the German periodical *Kosmos*, discussing, with great wealth of knowledge and acute reasoning, the origin of species, the genesis of the colours of flowers, the laws of variation, and other similar subjects. Dr. Müller's contribution, "Blumen und Insekten," to Schenk's "Handbuch der Botanik," which forms a part of the "Encyklopædie der Naturwissenschaften," now in course of publication, is an admirable *résumé* of the whole subject.

Dr. Müller died in harness, having fallen a victim to an attack of inflammation of the lungs at Prad, in Tyrol, on August 25. A. W. B.

SECOND NOTE ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY

THE fact that the note on this subject inserted in NATURE, June 14, p. 151, was copied *in extenso* by the *Electrical Review*, by the *New York Electrical World*, and I believe by some other papers, as well as the fragmentary way in which these observations must of necessity be obtained, encourages me to ask for a little further space. This is the more pardonable as the writer in the former paper, in two editorial articles which he founds on my observations, shows ignorance and misconception of certain physiological facts involved in them—a misconception the correction of which by myself he does not think fit to publish.

On August 23, during my visit to the ward, it became obvious that a hopeless and incurable case of renal disease was rapidly sinking. It occurred to me that the patient, being in a state of uræmic drowsiness almost amounting to coma, there would be no inhumanity in adding small electrical currents to the other stimulants which as a last chance we were sedulously administering. I accordingly immersed his feet, which were rapidly getting cold, in hot baths of salt and water connected with Wheatstone's bridge. This and the brandy caused a decided rally, and the temperature became normal, viz. 98° F. The resistance then taken was 1100 ohms from one foot to the other. At 3 p.m., however, he rather suddenly relapsed, his hands and nose becoming cold. The following series of observations was taken :—

2.55	p.m.—temperature 98°	1100
3.0	,, ...	900
3.5	,, ...	870
3.7	,, ...	850
3.12	,, ...	840
3.13	,, ...	820
3.22	,, temperature 95°	800

We concluded that death was imminent, and I ceased the experiment, intending to renew it after the event. But on returning to the ward at 4.36 I found him somewhat better and warmer. I applied the large leaden poles, to which I will refer presently, to both feet, so as to reduce the resistance to a minimum. The following remarkable series of resistances was obtained. The thermometer,[1] being found too slow in its action to follow the flickerings of the expiring lamp of life, was not used, the hand applied to the skin being quite competent to detect the great changes of surface heat :—

4.36	p.m.	640
4.40	,, ...	600
4.45	,, ...	570
4.50	,, (rally)	750
4.55	,, (relapse)	700
5.0	,, (great rally)	770

He was still very cold, but began to ramble in his usual incoherent way (having been slightly deranged for several years), and I therefore left him for the night. On returning next morning early I found he had died an hour and a half later. Had I not been greatly fatigued myself, I should have stopped to secure an observation during post-mortem refrigeration, and before the access of rigor mortis. As it was I found the corpse in full state of rigidity. We managed to have the testing apparatus set up by 12.30, and without any great disturbance of the body I applied the leaden poles.

After some preliminary tests I obtained two excellent observations with reversed currents, and found them both exactly alike at 1150 ohms.

Then came the last experiment with which I now have to trouble you, namely, the question of skin resistance. A tremendous hubbub has been made about this since the time of Duchenne. I believe it has been enormously exaggerated. My anonymous critic of the *Electrical Review* quaintly says : "We *most of us*" (*sic*) "know the effect of keeping the feet in salt and water, or water alone" (he does not name soap and water !) "for any length of time. The skin turns white and swells, *enlarging the pores* (*sic*) ; indeed nearly the whole of the outside skin is of a spongy nature." I need not prolong the quotation, because I simply deny his facts, except where foot-washings have been "like angels' visits," &c., &c. The carefully-prepared epidermis of my patients is entirely free from this hypothetical and inaccurately stated cause of error. So I hope is mine ; indeed I feel the full value of the implied limitation of the cautious phrase "most of us." Seriously speaking, it is too bad that an observer of average capacity, and I hope moderate honesty, should be accused of such elementary blunders on mere *a priori* grounds. Now for fact : Before going to the deadhouse I had provided myself with two silver needles, used for the electro-puncture of aneurisms, and intended to convey a very strong coagulating current from a powerful battery. I inserted one of these to the depth of three inches into the plantar muscles of each foot of the corpse, leaving everything else untouched. I expected the enormous reduction of resistance above named. To my surprise the Wheatstone bridge gave 1200 ohms in either direction of current, or 50 more than with large lead and salt-water electrodes. This alleged skin resistance is then only true in the dry state, and is easily conquered by very simple means. Cases of diabetes have been cited in confirmation of this supposed resistance, and it has been explained by the peculiar dryness of the skin in this complaint. A patient now in my ward, though a tall emaciated man with long spindle shanks, only gives 1340 ohms from foot to foot, with either salt-water baths, or with the lead electrodes as here described. This is rather under than over the average.

One word as to the lead electrodes themselves, and the manner of using them. The intelligent and kind lady nurses of our hospital, whom I like to call by their grand old name of "sisters," and who throughout this inquiry have seconded me in the most self-sacrificing way, are instructed to get ready certain patients for me each morning. The process consists in wrapping both hands and feet in coarse flannel saturated with strong warm brine for an hour before the experiment. Sometimes the process so graphically described by my commentator occurs, and is dealt with accordingly. I then proceed to wrap the members one by one in a surgical covering of flannel soaked in the same conducting solution. Over this I fold, also in surgical fashion, a strip of thin sheet lead about eighteen inches long, and one and a half inches broad. On the top of all is an ordinary spiral bandage, which moulds the whole to the shape of the limb, and squeezes out superfluous fluid. An indiarubber covered wire leads to my testing table. I may add that each hand or foot is separately deposited on one of the vulcanised rubber waterproof sheets commonly used in the wards, and which I find to be excellent insulators. The first few observations are commonly rejected ; always if they show any suspicion of diminishing. But after even half an hour's maceration this is rarely the case. Between every two observations I put the patient himself on short circuit, to discharge any currents of polarisation

[1] In the axilla. I hope to use surface thermometers on a future occasion. In my former paper the axillary temperature obviously lags behind that of the extremities.

Every measurement is at least double, and made with currents in opposite directions.

In conclusion I may remark first, that this, like my former note, only deals with part of a larger inquiry; and secondly, that the results above stated were open to all comers, and were carefully watched by Dr. Percy Smith, Dr. Shepherd, and others of my colleagues and pupils.

W. H. STONE

THE INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES[1]

AS the result of an International Convention held on the 20th May, 1875, an International Bureau of Weights and Measures has been created at Paris with the object of securing an international metric system, and which should take account (1) of all comparisons and verifications of the new prototypes of the metre and the kilogramme; (2) of the conservation of the international prototypes; (3) of periodical comparisons of national standards with the international prototypes, as also of comparisons of thermometric standards; (4) of the comparisons of the new prototypes with the fundamental standards of non-metric weights and measures employed in different countries and in science; (5) of the marking and comparison of geodetical measures; (6) of the comparison of standards and scales of precision, the verification of which may be sought by governments, by learned societies, or even by mechanists and students.

An international committee of weights and measures composed of fourteen members, comprising physicists, mathematicians, surveyors, and astronomers, all of different nationalities, has been intrusted with the supreme direction of the bureau. The president of this committee is General Ibañez, director-general of the Geographical and Statistical Institute of Spain, and its secretary Dr. Hirsch, director of the Observatory of Neuchâtel. It meets once a year for the discharge of its functions at Paris.

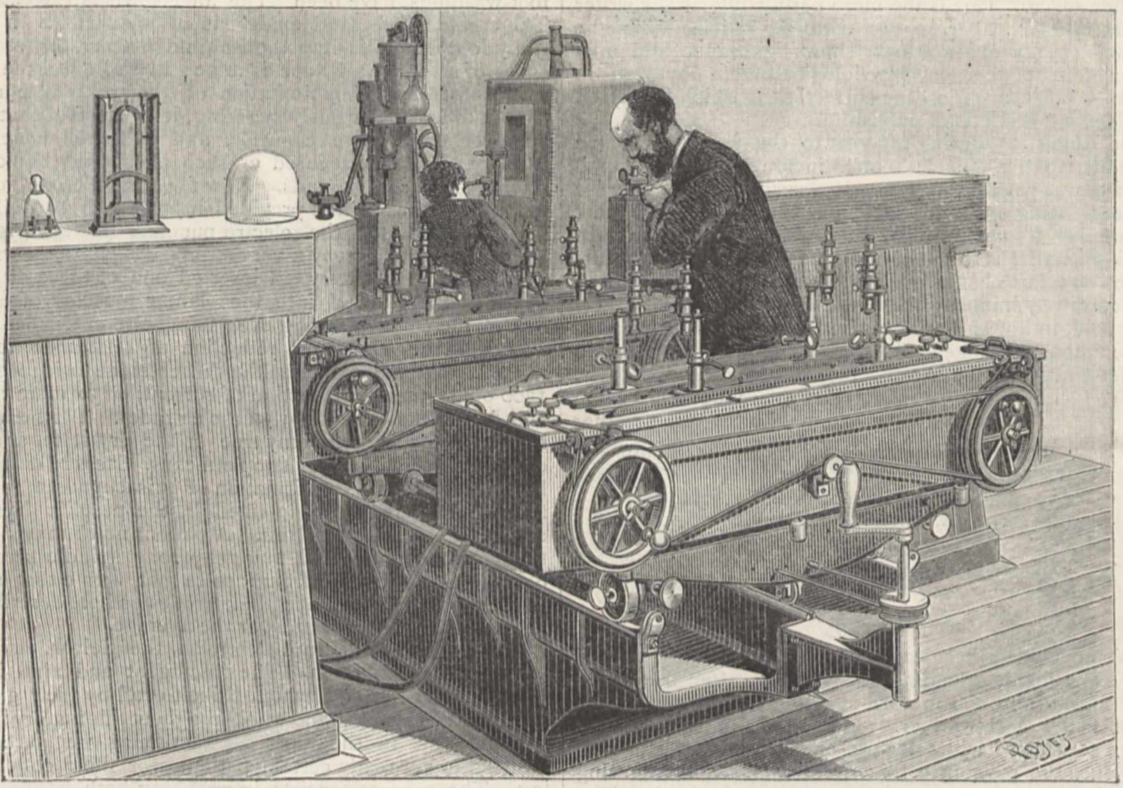


FIG. 1.—"Comparateur" for Measuring Absolute and Relative Dilatations.

Twenty States were represented at the preliminary diplomatic conference of 1875. Of these, seventeen (or nineteen) have signed the international convention which was the result of its deliberations. One alone of these States has not ratified the convention; and consequently the expenses of the foundation and maintenance of the International Bureau of Weights and Measures have hitherto been defrayed by the following sixteen States: Germany, Austria-Hungary, Belgium, the Argentine Confederation, Denmark, Spain, the United States of America, France, Italy, Peru, Portugal, Russia, Sweden, Norway, Switzerland, Turkey, Venezuela. These, representing about 351,000,000 of people, have already contributed over 1,000,000 francs towards the foundation of the International Bureau. The Government of Servia has since given in its adhesion to the convention.

In order to provide for the erection of the necessary structures for the observatory of the International Bureau of Weights and Measures, France made a grant of ground in the park of St. Cloud formerly occupied by the Pavillon de Breteuil, safely removed from all disturbances and surface tremors such as would have been experienced in the centre of a large city amidst the whirl of carriages and the working of machines.

In front of the observatory of the International Bureau are offices set apart for mechanical laboratories. Behind are spacious chambers in which are distributed the various instruments of precision employed in metrological work. These chambers are surrounded by walls of great thickness; they are lighted by skylights so arranged as to prevent solar rays from penetrating: and are environed by a lobby isolating them from the exterior. The object

[1] From *La Nature*.

aimed at by these arrangements is to secure to the utmost degree possible the continuance of a uniform temperature which in certain operations is a condition of success.

The labours of the bureau are naturally divided into two sections; one dealing with standards of length, the other with standards of mass or weight. The first comprises principally the settlement of equations of the various standards, that is of their lengths compared with the prototype which forms the universal basis of departure; the ratio of their expansions; the study of their subdivisions or of their multiples; and particularly of the great rules (*règles*) which serve geodesists for the measurement of terrestrial bases. Similarly, the section of weights is busied with determining the relations of several primary kilogrammes to the kilogramme prototype, with the adjustments of their subdivisions, with the computation of their specific weights, &c. These various labours are divided among a certain number of experts constituting the scientific staff of the bureau. We shall hastily indicate the principal instruments used in both sections, instruments which are the workmanship of the ablest mechanists in the whole of Europe, and which in general show the last limit of perfection attainable in this precise branch of mechanics.

The instruments belonging to the section of linear standards or metres, are called "comparateurs." A comparateur for metres *à traits* is essentially composed of two microscopes solidly fixed and immovable, provided with micrometers under which, by a peculiar mechanism, the two rules it is desired to compare with each other can be successively introduced. The bureau possesses several of these instruments, each adapted to a special purpose, and consequently distinguished by peculiarities of construction.

The first is Brunner's comparateur, designed for comparisons in air of metres *à traits*. The two microscopes are by means of cantilevers riveted on large stone pillars or monoliths resting on massive masonry. The micrometers with which they are furnished follow the arrangements usually observed in the case of astronomical instruments. Each is composed of a kind of little rectangular box, lengthened and flattened, fixed on the body of the microscope below the eyepiece. In this box is a frame capable of being displaced from right to left. On the frame are stretched two spider threads, very fine, parallel and very close to each other, which constitute the parallel spider threads or reticule. The displacement of the frame is effected very slowly by means of a micrometer screw of perfect workmanship, which is worked from the outside by means of an enlarged micrometer-head or drum, the circumference of which is divided into a hundred equal parts. By turning this round the experimenter moves the screw, which in turn moves the frame and displaces the spider threads visible in the field of the microscope. The image of the lines traced on the rule as given by the objective then lies in the plane of these threads. To bisect a line is to make the parallel threads coincide with the image of this line, that is by turning round the drum so as to bring the threads into such a position that the line may appear exactly in the middle of them; the position occupied by the parallel threads will in that case be indicated by the reading of the micrometer-head or drum. Should a second line appear under this microscope in a different position, it will be necessary, in order to bisect it in turn, to displace the threads, that is to make the drum revolve a certain number of divisions. The distance corresponding with one displacement of a division being known, the distance between the first and second lines can then be calculated.

Under the microscopes is the body itself of the comparateur, composed primarily of a strong framework of brass, exceedingly massive and steady, forming on its upper borders a kind of railway on which rolls a heavy carriage, movable at pleasure by a handle which controls a system of cog-wheels. Surmounting this carriage is a long box or metal trough with double walls formed of two cases inclosed one within the other. This box is designed to receive the two rules that are to be compared. These are placed beside each other in the middle of the box on supports of suitable form. The box is provided with various contrivances by means of which the observer, while observing the microscopes, is able to adjust the rules; to lower them or to raise them; to bring them into focus at the two extremities, or to displace them longitudinally or transversely, as may be required. The box is able, moreover, to receive a sufficient number of thermometers, which are observed with the aid of special lenses fitted in the lid which covers the whole and which

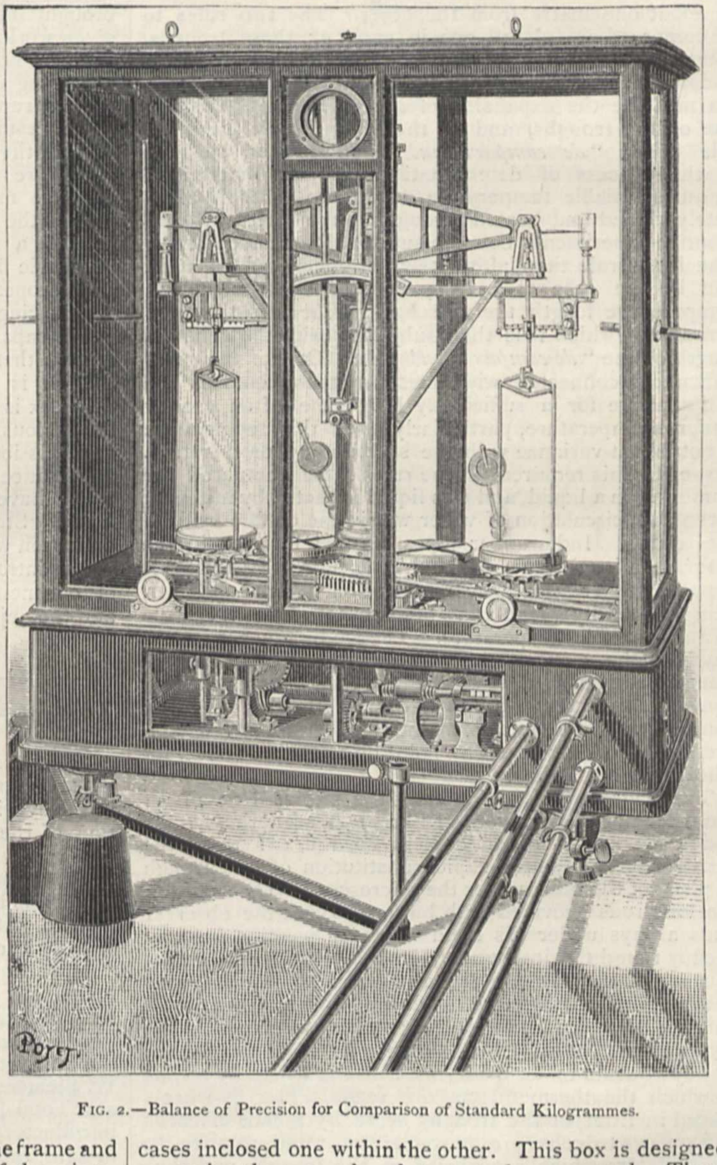


Fig. 2.—Balance of Precision for Comparison of Standard Kilogrammes.

protects the interior of the apparatus from every rapid variation of temperature. By the movement of the carriage the observer brings successively under the microscopes, the two metres the difference between which he wants to ascertain; he bisects the lines of both, and this operation made at the two extremities furnishes the equation desired between the two rules.

Another of the comparateurs represented in the accompanying diagram (Fig. 1) is designed for the measurement of expansions. As in the preceding comparateur we here also find two microscopes with fixed micrometers and a carriage moving on rails. Here, however, the carriage contains two distinct boxes or troughs; each at a distance of about one metre from the other. The two rules to be compared are placed one in each of these troughs. They are thus to some extent independent of each other, and may therefore be introduced at different temperatures. To measure the expansion of one rule, you place it in one of the troughs, and in the other trough the other rule called "*de comparaison*." This latter, so long as the process of determination lasts, is maintained at an invariable temperature, while the other is alternately cooled and heated throughout the series of consecutive experiments between sufficiently extended limits. The latter rule then alternately contracts and expands, and in the case of each particular experiment you compare the length the rule has reached at the temperature to which it is then subjected, with the constant length of the "*de comparaison*" rule. One of the great difficulties connected with these measurements is the maintenance for a sufficiently long time of an exactly uniform temperature, particularly when this temperature is notably at variance with the surrounding temperature. To secure this requirement the rules to be compared are immersed in a liquid, and this liquid is heated by means of a constant circulation of water within the double walls of the trough. Indiarubber pipes, as may be seen in the diagram (Fig. 1), are used for this purpose. The water is supplied from a large metal reservoir outside the chamber, being heated by a regulating system that causes it to issue at an invariable temperature. Thence by pipes it is conveyed to the comparateur, traverses the trough in a continuous stream, and is then carried off by waste pipes, conveniently arranged, into a drain. By this means a constant thermal state is maintained, within a few hundredths of a degree, at any point up to forty degrees, for whole hours at a time.

The diagram (Fig. 1) indicates the principal details of the mechanism employed. In front is seen the handle which by means of an endless cord draws away the carriage and allows the rapid substitution of one trough in place of the other under the microscopes. On the sides, the long rods provided with buttons, which the observer finds always under his hand whatever position he may occupy round the instrument, have the power of acting equally on the carriage by means of a cog-wheel placed under it, and of moving it backwards and forwards by a uniform slow movement. On the lids are perceived the heads of the different keys which enable rectifications of all adjustments to be made, as also the lenses by means of which the thermometers are read. The fly-wheels placed in front of the troughs serve by means of cords and pulleys to convey a movement of rapid rotation to the agitators which are placed in the trough, and thus vigorously intermingle the strata of the liquid, and secure uniformity of temperature in all parts of the bath.

With these apparatuses the difference can be determined between two metres at a given temperature with an exactness reaching to some ten-thousandths of a millimetre. In order to obtain such nicety, it is of course necessary that the lines of the metres be traced with sufficient fineness and distinctness to fit them for the magnifying power employed.

The two instruments just mentioned are fitted for the comparison of metres alone. The *comparateur universel*, on the other hand, allows comparisons to be made of any lengths whatever from below a metre up to two metres. The aspect of this new comparateur is entirely different from that of the two preceding. The microscopes which in all cases constitute the essential parts, instead of being fixed, are here mounted on carriages, which can be moved over a kind of bridge placed horizontally between two stone pillars. This bridge is formed by a large block of brass furnished with steel surfaces on its upper edges, which serve as a support and guide to the microscopes in their movements. It is perfectly rectilineal and horizontal When, by moving the carriages, the microscopes have been brought into the position they require to occupy for a given work, they are fixed by tightening a lever with the aid of a knob which controls a screw. Below, as in the preceding comparateurs, is a massive carriage likewise bearing supports on which are arranged the rules needing to be examined. These supports are equally furnished with all the necessary means of adjustment. These latter, again, are worked by a mechanism too complicated to allow so much as an idea of it to be communicated without the help of diagrams. The comparateur contains, besides, a standard rule of two metres, divided along its whole length into centimetres, two supplementary microscopes mounted on a special carriage and designed for marking the subdivision of a metre, various accessory pieces capable of serving for comparison of measures *à bouts*, either one with another or with measures *à traits*,[1] &c. It is entirely inclosed in a large mahogany box. This box is furnished with windows necessary for lighting the various parts, and with the orifices required for the transmission of movements to the interior, &c., and has the appearance of an imposing and elegant piece of furniture.

We have still to mention a comparateur for metres *à bouts* by Steinheil's method; and to add that this beautiful collection will in the course of a few months be completed by the introduction of a geodetical comparateur for rules of four metres, which is actually in process of construction, and the object of which is indicated by its name.

(*To be continued.*)

THE VIENNA INTERNATIONAL ELECTRIC EXHIBITION

FOR two weeks, the arrangement of the machinery being nearly complete, the Exhibition has been open in the evening from 7 till 11. The effect of the illumination of the immense interior of the Rotunda and its annexes by the various incandescent and arc lamps, and of the surrounding places which are lighted by large reflectors, is brilliant. The electric railway of Siemens and Halske between the Rotunda and the Praterstern is already in operation. The theatrical performances at the "Asphaleia" Theatre, which is lighted by 1500 Swan lamps fed by a large Zipernowsky alternating current machine, have also begun this week.

The series of lectures to be held at the theatre during the Exhibition was inaugurated on August 27 by Sir C. W. Siemens, with a lecture "On the Temperature, Light, and Total Radiating Power of the Sun." After a short introductory sketch of the nature of the terrestrial sources of light, the lecturer gave an account of the ratio of the three forms of radiant energy, viz. heat, light, and actinism, as produced by the sun and terrestrial light sources. Then referring to the difference between the statements of various astronomers and physicists relating to solar temperature, he expressed his opinion that the temperature of the sun could not exceed 3000° C., and explained the experimental methods he used for measuring the solar temperature. The second lecture was delivered on Sep-

[1] A metre or other measure *à bouts* is one whose ends exactly coincide with the ends of the material of which it is made; a measure *à traits* is bounded by lines within the margins of the material on which it is traced.

tember 1, by Dr. Aron of Berlin, "On the Telephone and Microphone." In this lecture the principles were explained on which the construction of the different telephones and microphones is based. There were also mentioned the variations of timbre as produced by these instruments; according to the experiments of Helmholtz the higher tones are transmitted better by the telephone, and therefore the timbre becomes clearer, while by the simple microphone, as Dr. Aron had found, the deeper tones are better transmitted, causing a duller timbre, but this failure is avoidable by using microphones with two coils. The lecturer explained also the principle of a new instrument, invented by himself, called the semaphone. In this instrument the variations of the current in a coil of insulated wire are transmitted by induction to another coil joined to a telephone or microphone. Dr. Aron has made experiments with his semaphone at Berlin, and was able to hear signals, the distance between the two coils being 70 feet. A similar experiment was carried out by Dr. Aron in the course of his lecture, and we could hear the noise made by a Neef's interrupter far from the lecture room, using a Siemens' telephone; the distance between the two coils being 3 feet.

Electric lighting is very well represented at the Exhibition, and a variety of new incandescent and arc lamps is to be seen there. As to the number of lamps exhibited, the first place is taken by the Swan lamps. Nearly 2000 Swan lamps are distributed at the theatre, the splendidly furnished interiors, and other parts of the building, fed by dynamo machines or by Faure-Sellon-Volckmar accumulators. The durability of these lamps is tested by a collection of lamps exhibited by Ganz and Co., used 1720 to 2330 hours. The carbon filaments do not show any damage, only the glass bulb being darkened by a carbon deposit. The exhibition of Edison lamps is not so extensive as it was at previous exhibitions. The Maxim lamps are used for lighting the Oriental pavilion and some of the interiors. The Lane-Fox lamps are also lighting some furnished apartments, and show the applicability of incandescent lamps for street-lighting by lighting the "Ausstellungsstrasse." The lamp of C. H. R. Müller has a screw-like curled carbon filament to make the emission of rays uniform in all directions. The U-shaped carbon strap of the Greiner and Friedrich's incandescent lamp is prepared from lamp-black and graphite, coal-tar being used as cement. The coal-tar, at first treated with sulphuric acid, is heated till it becomes an asphalte-like mass, to which lamp-black and graphite are then added, so that a stout paste is formed. By pressing this paste through a little fine hole a thin thread is obtained, which is cut in pieces and dried. If dried, the U-shaped pieces are burned. The carbonised fibres of *Musa textilis* are used for the incandescent lamps of Dr. Puluj. Very interesting is the Bernstein lamp, exhibited by the Bernstein Electric Light Manufacturing Company of Boston. It is claimed by the inventor to have many advantages over the other incandescent lamps. With an electromotive force of 23 volts and a current of 7 amperes, it has an illuminating power of 65 candles; it is stated to be more durable than the other lamps, and more economical, by rendering the light-giving carbon able to expand and contract without being liable to injury and breakage, and therefore capable of withstanding the action of strong currents, so as to avoid the disintegration which takes place in carbon filaments of high resistance. A large number of lamps can be used in series, and long distances can be lighted by means of a thin wire; the lamp is very appropriate for street-lighting. A hollow U-shaped carbon cylinder as big as a lucifer match is used as the light-giving part, having a comparatively large illuminating surface. This carbon cylinder is quite elastic, and its surface resembles knitwork. Though the manufacturing process of the carbon is not yet published, it seems to be very probable that the carbon cylinder is prepared by carbonising a hollow knitted or woven string, a metallic wire being put through during the burning process to support it. The ends of the U-shaped cylinder are connected with pear-like socket pieces of carbon, to which the two conducting wires are attached, entering the thin end of the carbon blocks, secured by means of a reddish cement. Such a lamp, fed by sixteen Faure-Volckmar accumulators, gave, as could be seen at the lecture delivered by Sir William Siemens, a white, dazzling light resembling an arc lamp.

Vienna, September 10

THE EDINBURGH BIOLOGICAL STATION

THE proposal to form a biological station at Granton, which was some time ago brought before the Royal Society in a paper by Mr. Murray of the *Challenger* Commission, has now taken definite shape. A lease of Granton Quarry for fifteen years has just been granted by the Duke of Buccleuch at a nominal rent, and Mr. Alexander Turbyne, salmon fisher, has been appointed keeper of the station, and will enter on his duties next week. Meantime some preliminary experiments have been made, and cages have been put down at the station, and structural work has been commenced in the way of fencing, building of walls, and putting the banks into proper order for further operations.

The proposal for the formation of the station, which it is meant to call "The Edinburgh Marine Station for Scientific Research," had its origin in the resolution of the Committee of the late Fisheries Exhibition in Edinburgh to hand over the surplus funds derived from the Exhibition to the Meteorological Society, to be applied to the purpose of carrying on investigations with respect to fish, with a recommendation to establish a zoological station, and to apply to Government for assistance in the work. The Meteorological Society appointed a subcommittee to consider the best means of applying this money to the purposes for which it was granted. This Committee had many consultations, and set afoot investigations at various ports as to the temperature of the water, habits and food of the fish, &c. They also had their attention carefully directed to the advisability of establishing a zoological station; and the suitableness of the old quarry at Granton for the purpose has been in various ways brought before the public, both at the Royal Society and at the meetings of the Meteorological Society. The scheme for founding a station there first took definite shape on the offer of a gentleman interested in research to build a floating laboratory at the quarry for the purpose of making experiments and investigations. Recently this gentleman was again communicated with, in respect that, after full consideration, it was thought that a floating laboratory, although an essential part of the scheme, was not, perhaps, the first that should be undertaken. In reply to a representation to this effect, the gentleman has written to Mr. Murray, the convener of the Station Committee, expressing his readiness to adopt the alterations proposed, and to give the 1000*l*. for the purpose of founding a zoological station for scientific research at Edinburgh, instead of building a floating laboratory, as originally suggested. He was not surprised to hear, he adds in his letter, that it would cost more than that to carry out the whole of the scheme. It seemed to him that they would require at least 1500*l*., in addition to his 1000*l*., to carry out all their proposals, and they should consider if this additional sum should not be raised before they commenced operations. However, he left the matter in the convener's hands to apply the money as he thought best, inclosing 100*l*. to cover preliminary expenses, and repeating the two conditions of his donation, viz. (1) that the convener should take the general direction of the station for at least three or four years; and (2) that his name was not in the meantime to be made public. The resolution which has now been come to is to go on with the undertaking, and the scheme is of a twofold character: (1) to undertake a scientific exploration and description of the Firth of Forth and the adjacent parts of the North Sea; and (2) to establish a marine station for biological investigation and research, where competent scientific men may find laboratories and all the appliances for research free of charge. With respect to the first branch of the scheme, it is meant to take the temperature of the water at fixed points of the Firth, extending from the fresh water of the Firth out to points beyond the Isle of May. The temperatures of the surface water and of the bottom and intermediate waters are to be taken at stated intervals throughout the year. It is part of the same scheme to note the character of the surface fauna and flora regularly throughout the year at these points, and the changes in the specific gravity of the water at the different times of the year and at different parts of the Firth. Observations will also be carefully laid down on the Admiralty charts of the nature of the bottom, and of the deposits, throughout the whole region, and a record of the animals living upon these is also to be attempted, so as to arrive at a complete scientific description of the bottom and its deposits. To this will be added a record of the effects upon the fauna, &c., of the admixture of river and ocean water at different parts, and of circumstances favourable or inimical to life and growth. Under the second branch of the scheme the proposal is to establish at Granton Quarry, and at various places in the Firth, investigations as to the hatching, breeding, and growth of various kinds of fish and marine invertebrates in inclosed spaces, or in cages moored at various points. The central station will be situated at the quarry. Here it is proposed to build, on a high part of the banks surrounding the quarry, a substantial cottage, from which a beautiful view of the whole Firth will be had. The cottage is to be fitted up with laboratories, and will consist of about six rooms, and cost from 400*l*. to 500*l*. On a level piece of ground adjoining the quarry there will be erected an iron cottage and shed for the keeper of the station, and for housing the trawls, dredges, nets, and other instruments required for the proposed investigations. This will cost from 150*l*. to 200*l*. Also, as part of the scheme, there is to be built a floating laboratory—that is to say, a laboratory built on a barge of the description mentioned to the Royal Society, and supplied with all the materials and apparatus requisite for biological investigation. This structure, it is interesting to note, will be so fashioned that it may be taken to any part of the Firth of Forth, and moored in sheltered spots during the summer wherever it may be thought desirable that investigations shall be carried on at any particular spot. This laboratory, it is intended, will give accommodation for three naturalists, with workrooms, and will cost about 800*l*. The station, furthermore, is to be provided with a steam launch fitted for dredging purposes and the making of hydrographic observations. The launch, according to the design, is to be built upon the plan of the steam pinnace that accompanied the *Challenger* during her cruise, but much larger, and will be provided with a separate engine for rolling in the dredges. This again will cost about 800*l*. In addition to these things there will be a small portable house belonging to the station, which may be put up on Inchkeith, Inchmickery, Inchcolm, or the Isle of May, should it be desirable to carry on any observations at these places. This, together with the cages formerly described for inclosing portions of the ocean and water of the quarry, will cost, it is estimated, about 300*l*. more. The fund which was granted by the Committee of the Fisheries Exhibition is to be applied, at the rate of 300*l*. a year for three or four years, to the keeping up of the station and the payment of the annual working expenses, including the salaries of a resident naturalist, an engineer, and a keeper. So that what is now wanted in order to the full equipment of the station is about 1500*l*. to pay for the permanent works which are required before the station can be in complete working order. In the event of this sum being forthcoming at an early date, it is thought that the whole institution would be in working order next spring—probably by March or April.

It is believed by a number of our scientific men that an undertaking of this kind, which will afford the means of making continuous observations into the circumstances which affect marine animals and plants—their food and their enemies—is the true method of getting the information necessary to settle many of the vexed questions with respect to the life histories of our food fishes, both of the salmon and sea fishes. The Firth of Forth yields special facilities for work of this kind. Thus, almost all our food fishes are frequenters of the Firth, and it is known to have a rich fauna, which has at various times been investigated by distinguished naturalists, as by Johnston of Berwick; Parnell, Allman, Forbes, Herdman, and others. A thorough investigation of the kind proposed will lead to great additions to knowledge, and will probably give the information that was wanted as to the evil effects or otherwise of trawling, which is one of the vexed questions at the present day. By directing their efforts to the thorough working out of a somewhat limited area like the Firth of Forth, in its meteorological, hydrographical, and biological aspects, the Committee believe that more rapid progress will be made than by intermittent observations at widely separate points. Such a station will also be a great boon to naturalists who desire to work at any special subject. Naturalists are often deterred from undertaking investigations because of the difficulty of providing themselves with dredges, steam-launch assistance, &c. Here they will have these ready at hand whenever they choose to visit the station. So that, from this point of view, in addition to the purely scientific aspect of an undertaking of this kind, it probably will be found to have a very wide economic bearing. The plans of the floating laboratory and of the other structures to which reference has been made are in the hands of Mr. Murray, from whom persons interested or desirous to aid in the carrying out of the scheme will receive every information they may wish to have.

NOTES

THE Directors of the Ben Nevis Observatory met on Thursday, 6th inst., and out of a list of nineteen applicants elected Mr. R. T. Omond, Edinburgh, Superintendent of the Observatory. Mr. Omond was a distinguished student of Edinburgh University, and for the past six or seven years has been chief assistant of Prof. Tait in conducting an extended series of physical experiments on the influence of pressure on deep-sea thermometers, the maximum density of water under different pressures, and cognate subjects of inquiry. The results of his work have been communicated in the form of papers to the Royal Society of Edinburgh. Mr. Omond's duties began from the above date; and shortly two assistants will be appointed, so that in October next a staff of three observers will have taken up their station at the Observatory, prepared to enter upon the work of the coming winter. The highest section of the bridle-road to the summit of the Ben was finished on Thursday at noon, and the first pair of horses which ever ascended the mountain made the ascent in the afternoon, carrying 2 cwt. each of building material. The building of the permanent Observatory commenced on the following day. A number of horses are employed carrying up material, and the Observatory is expected to be finished early next month. Arrangements are also being made for laying a telegraphic cable from Fort William to the Observatory, and it is fully expected that the work will be finished by the time the observers take up their residence on the Ben. We understand that the directors have asked Mr. Buchan, secretary of the Scottish Meteorological Society, to visit several of the more important meteorological observatories on the Continent, beginning with that of Hamburg, and including some of the more notable high-level stations, and report on the automatic and other instruments in use there, with a view to a full and satisfactory equipment of the Ben Nevis Observatory next summer. During the coming winter the work will be mostly restricted to eye observations, with the object of collecting information regarding the climate of the Ben, so as to form some guide to the directors in determining the nature of the automatic and other instruments that will be required for making the various observations and conducting the important physical researches which it is proposed to carry out.

THE Lords of the Committee of Council on Education have been informed by the Secretary of State for Foreign Affairs that a note has been received at the Foreign Office from the French Chargé d'Affaires in England stating that the meeting of the Electrical Units Conference at Paris has been postponed till April 2, 1884.

THE president of the American Association at the Minneapolis meeting was Prof. C. A. Young, and as retiring president at the Philadelphia meeting next year it will fall to his lot to give the presidential address. We have already given Principal Dawson's presidential address, and this week we give the address in Section A of Prof. W. A. Rogers on the German survey of the northern heavens. Other important addresses are those of Prof. Rowland, who spoke eloquently on behalf of pure science, Prof. Cope on the evidence of evolution in the history of the extinct mammalia, and Prof. Hitchcock on the early history of the North American Continent. Dr. Folwell, president of the University of Minnesota, pointed out in his address of welcome some of the great triumphs of science in its application to practical purposes: "the further extension of scientific method," he said, "till it shall become the guide of conduct in the everyday life of all men, is now the chief problem in education." So far as reports have reached us, no paper of striking importance was read at the Minneapolis meeting.

THE members of the Swedish Meteorological Expedition at Spitzbergen arrived in Gothenburg on the 6th inst.

AT the general meeting of Tweed Commissioners, last week, it was agreed that 20*l.* should be voted for recommencing investigations regarding the life-history of the various Salmonidæ which frequent the River Tweed. Were similar investigations carried on by the River Conservators in England and by Fishery Boards in Scotland and Ireland, there can be no doubt that information would soon be obtained on many points which are now obscure.

IN the *Comptes Rendus* for September 3 M. Milne-Edwards announces the return to France of the *Talisman*, which had sailed last June to explore the waters of the Atlantic. The expedition has examined the marine fauna along the seaboard of Morocco and the Western Sahara, as well as the waters of the Cape Verde, Canary, and Azores Archipelagoes.

MR. A. HASTINGS WHITE sends us a letter from an Australian correspondent, deploring the wholesale destruction of forests, especially in New South Wales. The correspondent writes:—"I do not know if I have ever mentioned anything about the more than wholesale destruction of the timber going on out here at the present time; but the facts are these. It is a common belief that killing off the timber improves the pastures, and so it does no doubt for a time, but at what a terrible cost. Thousands of acres are killed every year, not even a bush or seedling of timber being left to grow, by cutting a ring round the trees, either into the wood or else by taking a ring of the bark off. The destruction of timber in this way on Crown lands is something terrible; in parts of the country one may travel for miles at a stretch and see nothing but bleached and dead trees, as if a blight had come over the land."

THE Danish ship *Ceres*, having just arrived in Copenhagen from Julianhaab in Greenland, reports that the *Sophia*, with Nordenskjöld's expedition on board, arrived at that place on June 17, having encountered no ice between Iceland and Greenland. After two days' stay there the *Sophia* proceeded to the cryolite quarries at Ivigtuk, where she took in coals. On June 26, the journey was continued to North Greenland. All was well on board.

ADMIRAL MOUCHEZ has asked for the credit required for the publication of the catalogue of stars established by the Paris Observatory for the last twenty-seven years. The number of stars tabulated amounts to 40,000, but the expenses are so heavy that it is doubted whether the required credit will be granted by the Government.

AN International Forestry Exhibition is to be held next year in Edinburgh.

THE International Medical Congress met last week in Amsterdam. The attendance was very large, delegates having arrived from almost every civilised country on the globe. Amongst the representatives of England were Sir Joseph Fayrer and Professors Lewis and De Chaumont of Netley. The Congress was opened by Prof. Stockvis of Amsterdam University, and the Burgomaster of Amsterdam, who welcomed the Congress on its assembly in the Dutch capital. Amongst the honorary presidents of the Congress are Sir Joseph Fayrer, Professors Lewis and De Chaumont, and Dr. Sydney Jones of New South Wales. The inaugural address was delivered by Prof. Stockvis, after which the Congress proceeded to its more special work under different sections.

AN International Society of Electricians has been formed in Paris under the presidency of the Minister of Posts and Telegraphs, its main object being to centralise all information bearing on the progress of electricity, and to promote its spread and development. Information as to the society may be obtained by writing to M. Georges Berger, 99, rue de Grenelle, Paris.

ON Wednesday last week an electric tramcar trial was successfully accomplished in Paris by the French Electrical Power Storage Company. At three o'clock p.m. the vehicle, an ordinary three-horse tramcar, left the Place de la Nation in the far east, and, after traversing the capital through several important thoroughfares, reached the starting point soon after six o'clock. A distance of thirty English miles was thus made in about three hours. There was not the slightest accident. The ease with which the car was turned off one set of tram lines and got on to another across several yards of unmetaled ground is stated to have been admirable. The locomotion is effected by means of Faure accumulators, weighing some fifty hundredweight, which are fixed under the tramcar seats and connected with a Siemens' machine placed under the floor. The machine, which makes twelve hundred revolutions a minute, sets in movement, by means of a pulley, an axle to which are connected the chains which give impulse to the wheels. These wheels revolve sixty times to twelve thousand revolutions of the machine. The speed of the electric tramcar is nine and a third miles an hour on level ground, and five and a half miles on an ascent. The present tram lines are not well adapted for the new locomotion. On the newer lines the movement was sufficiently smooth, but on those that have been laid for some time there was a marked difference, and the actual working force was considerably lower than the indicated horse-power. The estimated cost is one-half that of horse trams.

It is proposed to establish a permanent meteorological observatory for the Bristol Channel. Mr. E. J. Lowe, who for the last forty years has carried on a regular series of meteorological observations at Highfield, near Nottingham, has recently purchased the Shirenewton estate, near Chepstow; and, being convinced of the real importance of establishing an observatory which may be carried on through future years without interruption, he has generously offered to present the whole of his valuable collection of meteorological instruments, together with his books and papers, towards the establishment of such a permanent observatory, for which he also offers to give the site, together with such stone and lime as may be required for the erection of the necessary buildings, provided a sufficient sum can be raised in the district to build the same, and to provide a small endowment towards the maintenance of a limited staff of assistants, who would, in the first instance, be under his gratuitous guidance and supervision. Previous to making this offer publicly known, Mr. Lowe conferred with the Meteorological Department of the Treasury, by whom Mr. Scott, F.R.S. (the director of the Department), was sent down, and his report was in every way most favourable, both as to the great utility and importance of the scheme, and also as to the admirable site which Mr. Lowe proposed to offer.

Prof. Brown Goode, the Commissioner of the United States to the International Fisheries Exhibition, has just received a telegram from Prof. Baird, the United States Commissioner of Fish and Fisheries, to the effect that Mr. Ryder, the embryologist of the Fish Commission, has finally solved the problem of the culture of oysters from artificially impregnated eggs, and that on the 4th inst., at the Government station at Stockton, Maryland, there were many millions of young oysters three-quarters of an inch in diameter which had been hatched from eggs artificially impregnated forty-six days before. It may be added that oysters were artificially impregnated in America by Dr. Brooks, of Baltimore, in 1879, but the difficulty hitherto met with in hatching them has been to prevent the young oysters from escaping and being lost immediately after they are hatched, since the spat passes through the meshes of most finely-woven fabrics, such as flannel.

We have before us No. 15, Part II., of the "Encyclopædia of Physical Sciences" (from the publishing house of Eduard, Trewendt, Breslau), which closes Wittstein's Alphabetical Manual of the Pharmaceutical Technology of Botany. As a conclusion to the work are appended three tables: (1) of the German and other popular names of drugs; (2) of the officinal Latin names; (3) of the systematic Latin names of mother-plants. The 16th number contains the continuation of the Alphabetical Manual of Chemistry published by Ladenburg, and among other things gives a very comprehensive and concise work by Tollens on "Analysis," and an important monograph by Weddige on "Aniline." The last number which has reached us of the "Encyclopædia of Physical Sciences" is the 34th, Part I., being at the same time the 50th of the whole series. It brings the Alphabetical Manual of Zoology, Anthropology, and Ethnology a considerable stage forward. The editing of this work from the letter F onwards has been committed to Reichenow in Berlin, who, with the old contributors and a large number of newly added cooperators, such as Sussdorf, Vetter, E. Taschenbergs and Georg Pfeffer, is pushing the work rapidly forwards.

On September 3 the steamer *Nordenskjöld* arrived at Hammerfest with the Dutch Meteorological Expedition saved from the *Varna* on board. The party states that the *Varna* was crushed in the ice on Christmas Eve last, but did not founder until July 24, after which date they were lodged on board the *Dijmphna*. One of the crew died during the winter. The scientific staff are exceedingly well satisfied with the result of their labours, with the exception naturally of the magnetic researches. Although Hovgaard was confident of getting into open water in August, he had decided that if not free by August 15 half the crew, under Lieut. Olsen, should leave the ship and attempt to reach the coast of Siberia at Yálmal, while he, with the other half, would winter on board. All was well on board when the Dutch departed.

The Norwegian geologist, Amund Helland, states that, having measured the following Iceland glaciers, he finds their area in Norwegian square miles to be: Vatnajökull, 150; Langjökull, 26; Hofsjökull, 25; Myrdalsjökull, 18; Drangajökull, 15; Glámujökull, 8; Forfajökull, 2; and Eyriksjökull, 2. By way of comparison he mentions that the Norwegian glacier, the Justedalsbræ, is only 14½ miles. It will thus be seen that the Iceland glaciers are larger than any others in the world, as those of the Alps and the Pyrenees are even smaller than the Norwegian.

The Norwegian zoologist, Prof. Robert Collett, a member of the Norwegian North-Sea Expedition, has written an interesting paper on the beaver in Norway. Formerly, he states, this interesting animal was found in many parts of the country, but now only in two rivers in the south. In 1876 a colony of them appeared near Porsgrund, which, however, disappeared again in 1880. Although he estimates the total number of animals at present in Norway at only about 100, he does not believe they are decreasing.

University College, Bristol, is showing considerable enterprise in extending its curriculum and improving the efficiency of its teaching. In the curriculum of work for the coming session there is an increased extension of laboratory instruction; this is a very pleasing feature. During the past session the chemical laboratory was very largely attended. The physical and electrical laboratory is now in full operation, and valuable apparatus has already been procured, though more is wanted when the funds can be obtained. A biological laboratory has also been commenced. In the ensuing session we see that a geological laboratory will be provided. Special arrangements are also made for the systematic use of the engineering workshops. The success of the engineering department hitherto has been most encouraging; and we are glad to see that the Council have now provided several facilities for the study of architectural drawing, and special arrangements for the practical work of students in this department have been made with various engineers, surveyors, and architects in and near Bristol. The medical school is rapidly growing, and already the necessity for further accommodation has become apparent. Want of funds seems to be the only check to the fuller growth and increased prosperity of the College. We believe, however, that the citizens of Bristol will not allow an important institution which is doing so much good work to feel the need of liberal support.

M. Bertrand read, at the sitting of September 10 of the Paris Academy of Sciences, a report drawn up by the Mayor of Grenoble, assisted by a commission of engineers, contradicting the rumour that the experiments on the transmission of power to a distance by the Marcel-Deprez system had failed. On the contrary, the success was complete. A power of eight horses was conveyed to Grenoble, and the original motive power underwent only a loss of 40 per cent. The force conveyed to Grenoble was utilised not only in pumping water, but in sewing, in moving machinery of every description, &c. The experiments lasted during a lengthened period, and are being continued. We must state that the distance is 14 kilometres, and the wire of copper instead of iron.

Fresh shocks of earthquake were felt at Casamicciola on the 9th and 10th inst.

M. L'HOSTE, a French aëronaut, crossed the Channel in a balloon on Sunday; he left the French coast at 5 p.m. on Sunday, and landed at Smeeth, near Ashford, at 11.

THE additions to the Zoological Society's Gardens during the past week include a White-fronted Capuchin (*Cebus albifrons*) from South America, presented by Miss A. Tanner; two Common Marmosets (*Hapale jacchus*) from Brazil, presented by Mr. H. H. Forbes Eden; three Mexican Deer (*Cervus mexicanus* ♂ ♀ ♀) from the Island of Santa Cruz, presented by Capt. Edwin Cole; a Getulian Ground Squirrel (*Xerus getulus*) from Morocco, presented by Mr. Geo. D. Cowan; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Capt. W. F. Small; a Common Squirrel (*Sciurus vulgaris*), British, presented by Master C. B. Webster; two Stink-pot Terrapins (*Aromochelys odorata*), a Pennsylvanian Mud Terrapin (*Cinosternon pennsylvanicum*), a Mississippi Alligator (*Alligator mississippiensis*), a Sharp-nosed Crocodile (*Crocodilus acutus*) from Florida, presented by Capt. E. Cole; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. F. L. B. Payne; a White-fronted Capuchin (*Cebus albifrons*), a Black-faced Spider Monkey (*Ateles ater*), a Pileated Jay (*Cyanocorax pileatus*), a Spotted Tinamou (*Nothura maculosa*) from South America, two Ruddy Finches (*Carpodacus erythrinus*) from Siberia, a Jackdaw (*Corvus monedula*), British, four Eyed Lizards (*Lacerta ocellata*), South European, purchased.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF MAY 6.—The *Comptes Rendus* of the sitting of the Paris Academy of Sciences on the 3rd inst. contain the reports from the observers sent by the French Government to Caroline Island in the Pacific for the observation of the recent total eclipse of the sun. The party was composed of M. Janssen, M. Trouvelot of the Observatory of Meudon, M. Pasteur, photographer, and an assistant, who were accompanied by Prof. Tacchini, director of the Observatory of the Collegio Romano, and Herr Palisa of the Observatory of Vienna, the discoverer of a large number of minor planets. One of the main objects of the expedition was a search for so-called intra-Mercurial planets, and it is to the observations made in this direction that we shall refer here. Herr Palisa and M. Trouvelot were especially occupied with this work. The former had a telescope of 6 inches aperture, with short focus and large field, equatorially mounted. M. Trouvelot had two telescopes, one of 3 inches aperture, with large field, reticule, and interior circle of position, and one of 6 inches aperture giving a high magnifying power. The 3-inch telescope formed a sweeping instrument with a field of about 4½ degrees diameter, for the exploration of the circumsolar region. Both telescopes were on a parallactic mounting, and in order to secure rapid record of positions and dispense with the readings, which cause the loss of valuable time, M. Janssen had caused what he terms "tracelets de microscope" to be applied to the circles of right ascension and declination. Each of these, placed in the hands of an assistant, allowed of there being made, on the direction of the observer, a fine stroke across the divided circle and its vernier, so that subsequently, with the aid of this very precise indication, the instrument could be replaced in the position of the observation and the necessary readings made at leisure. It was arranged that MM. Palisa and Trouvelot should divide the work, each attending specially to one side of the sun. The Vienna astronomer's instrument, properly, as it seems, a comet-seeker, by Merz, had a magnifying power of 13, giving a field of 3°. With this, on totality taking place, he commenced his search, starting from the sun towards Saturn, at first on the south, and when he did not thus find stars he returned to the sun, and swept more to the north. In this way he recognised *nine* stars, all which are identified in the Bonn *Durchmusterung*. We give the list of stars, correcting two misprints in the *Comptes Rendus* (14°,355 should be 16°,355, and for 20°,542 we should read 20°,543), and appending the positions of the stars for the Bonn epoch 1855·0: thus, with the sun's place reduced to the same epoch, the relative positions of the stars with respect to his centre will be readily seen:—

Bonn Zone and number of star.	Magnitude.	Right Ascension. h. m. s.	Declination. ° ′
16,355	5·7	2 41 13·1	+16 51·5
16,484	6·0	2 31 15·2	16 4·1
19,477	4·2	3 3 20·8	19 10·7
19,578	5·5	3 33 57·8	19 13·9
19,582	6·0	3 35 24·5	19 12·5
20,527	4·5	3 6 34·5	20 30·5
20,543	5·0	3 12 51.0	20 37·3
20,551	5·0	3 14 24·8	20 13·4
20,556	5·8	3 16 4·2	+20 17·4

The result of his search Herr Palisa states to be that, between the limits (1855·0) 2h. 52m. from +14° to +19°, to 3h. 40m. from +16° to +22°, there was no star of the fifth magnitude unmarked in his chart, this, it should be mentioned, being a lithographic chart supplied to him by Prof. Holden, one of the American observing party.

M. Trouvelot's attention was first directed to the study and figure of the corona, but, after the totality had lasted two minutes, he applied himself to explore the region west of the sun. He moved his telescope 10° in declination to the north of the sun's centre, and swept slowly from that point from east to west, to a distance of 15° in right ascension. The first sweep brought out a small whitish star; two other sweeps were made without any result; but in the fourth he saw a bright star of a decided red colour, which he estimated at 4 or 4½ magnitude. Its approximate position was a little to the north, and a little to the west of the sun, but the cause of a more exact determination of position not being made will be best given in M. Trouvelot's own words:—"En voulant amener cet astre dans le champ très restreint de l'oculaire du 6 pouces (0·16m.), afin de chercher à constater s'il montrait traces soit d'un disque, soit d'une phase, il se produisit une certaine confusion parmi les deux aides que j'avais placés aux cercles horaire et de déclinaison pour guider la course des balayages, et bien que l'étoile traversât le champ visuel, il me fut impossible de retenir en place la lunette, et dès lors de reconnaître son caractère et sa position." In the abstract of results of observations appended to the reports of the observers, after reference to Herr Palisa's experiences, we read in the *Comptes Rendus*: "M. Trouvelot arrive à un résultat moins net pour le côté ouest, mais nous savons que cet observateur distingué désire revoir la région où se trouvait le soleil au moment de l'éclipse avant de ce prononcer." It is stated that the photographs, though not yet examined in a complete manner, appear to support the negative result obtained by Herr Palisa as to the existence of an intra-Mercurial planet.

A NEW COMET.—A Dun Echt circular (No. 78) notifies the discovery of a comet by Mr. Brooks on September 2, which was thus observed by Mr. Wendell at the Harvard College Observatory on the following night:—

Greenwich M.T. h. m. s.	R.A. h. m s.	Decl. ° ′ ″
September 3 at 16 9 24·5	16 35 15·6	+ 64 49 33

Daily motions in R.A. − 36s., in declination − 12′. It is described as circular; less than 1′ diameter; tenth magnitude; well defined nucleus, and no tail.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS[1]

THE illustrious Argelander was accustomed to say in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the *Astronomische Gesellschaft*, at its formation in 1865, was the proposition to determine accurately the coordinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organisation) belongs the credit

[1] An address delivered by Prof. William A. Rogers before the American Association for the Advancement of Science at Minneapolis on August 15, 1883.

of arranging a scheme of observations by which, through the co-operation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labour were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definitive results which the discussion of all the observations is expected to show, we may with profit consider the object sought in the undertaking, the general plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty,—a problem of which what we call universal gravitation is only one element, if, indeed, it be an element,—a problem which reaches farther than all others into the mysteries of the universe,—the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the *Gesellschaft* in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar coordinates as a base; but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1830 were referred to the origin from which stellar coordinates are reckoned by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose coordinates were supposed to be well known. The unequalled Pulkowa observations for the epoch 1845, form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of all the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed: viz. Bessel's Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, Gould's d'Agelet for 1783, Piazzi for 1800, Auwer's Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1825, Bessel for 1827, Struve for 1830, Argelander for 1830, and Pulkowa for 1845.

An analysis of these catalogues reveals four important facts:—

First, that, a large share of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

Second, that in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

Third, that, with the exception of the Polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

Fourth, that each catalogue involves a system of errors peculiar to the observers, to the character of the instruments employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension *inter se* were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the *Monthly Notices of the Royal Astronomical Society* in 1861 (xxi. 245), on the positions of the Radcliffe catalogue. I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time. It is as follows:—

Diff. of R.A. (Greenw. 12 Year Cat.—Rad.) = $-$ 0·38s. + 0·32s. sin (a + 5h. 32m.). Extending this expression to terms of the second order, it may be put under the form $\Delta = a$ constant + ($m \sin a + n \cos a$) + ($m' \sin 2a + n' \cos 2a$) +, &c.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which would give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all is the more remarkable since the primary stars upon which the Radcliffe positions depend are nearly the same as those employed at Greenwich. In reality the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference. The speaker has shown (*Proc. Amer. Acad.* 1874, 182) that the weight of the errors of the provisional catalogue assumed fell between the first and the third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors in either one or in both, it is clearly impossible either to determine the nature of the errors or to assign their true cause until a fundamental system has been established which is free both from accidental and from periodic errors,—from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar coordinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits $-30°$ and $+46°$ declination. A comparison of the places of these stars for a given epoch with the same stars in any catalogue for the same epoch enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons—from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed:—

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Åbo catalogue for 1830 and the Pulkowa catalogue for 1845. One is reminded in this connection of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal Society I have endeavoured to show that with us the experiment *entirely* fails."

b. During this interval the constant differences between the earlier catalogues and Newcomb's system vary between +0·17s. for Pond, 1820, and $-$0·19s. for Pond, 1830; and for later catalogues between +0·07s. for Cambridge, 1860, and +0·02s. for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British *Nautical Almanac*, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralise the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0·10s.,—a quantity which can be readily detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors which follow nearly the same law as those which characterise English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neutralise the errors inherent in distinctively English and French observations.

d. In the case of several catalogues residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instrument with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law and have nearly the same magnitude as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British *Nautical Almanac* and the *Connaissance des Temps* are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860 the ephemerides of the *Nautical Almanac* were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon *Nautical Almanac* clock stars. At English observatories it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the *American Ephemeris* follow nearly the same law as the errors of the *Nautical Almanac*, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfer's *Tab. Reg.*, upon which the *Berliner Jahrbuch* is based, has no well-defined systematic errors, and the correction for equinox is nearly the same in amount as in the *American Ephemeris*, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the *American Ephemeris*.

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox:—

		Average error for each star.
Argelander	1830	1·1
Pulkowa	1845	1·1
Greenwich	1845	2·0
Greenwich	1860	2·0
D'Agelet (Gould)	1783	2 2
Cape of Good Hope (Henderson)	1833	2·2
Greenwich	1850	2·2
Greenwich	1871	2·2
Paris	1867	2·4
Washington	1846–52	2·5
Struve	1830	2·5
Cape of Good Hope	1856	2·8
Radcliffe	1860	3·1
Greenwich	1840	3·1
Bessel	1825	3·2
Pond	1830	3·7
Gillis	1840	3·8
Madras (Taylor)	1830	3·9
Cape of Good Hope (Fallows)	1830	3·9
Radcliffe	1845	4·5
Armagh	1840	5·0
Piazzi	1800	5·3
Bessel's Bradley	1755	7·9
Lalande	1800	13·2
Lacaille	1750	24·9

It is obvious from these relations that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevent any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which is now well nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different instruments, and by different observers. He says: "With all these precautions, we do not find by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. Nachr.*, Nos. 1532-1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in Publication xiv. of the *Gesellschaft*, is no less than 1·2s. The present difference outstanding between the Pulkowa and the Greenwich systems at 10° south declination is 1·7s.

Within the past five years the labours of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in the case of a large number of stars; but the different systems still differ systematically *inter se* by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the *Gesellschaft* at its session in Leipzig in 1865, Argelander, who was then president of the Society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. Nachr.*, i. 257). It was in pursuance of this plan that the zones between − 15° and + 15° in declination were observed. These zones were to form the groundwork of the Berlin charts; and Argelander, in the execution of the Bonner *Durchmusterung*, simply carried out the second part of Bessel's recommendation.

With the exception of the observations of Cooper at Markree Observatory, and the charts of Chacornac, these two great works—the second being a continuation of the first, under a better and more feasible plan—are the only ones in existence which give us any knowledge of the general structure of the stellar system.

The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the groundwork of these charts. The coordinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The *Durchmusterung*, undertaken by Argelander at Bonn, was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the coordinates of 324,198 stars situated between − 2° and + 90° declination. It includes stars to the 9·5 magnitude, the coordinates being given to tenths of seconds of time, and the declinations to tenths of minutes of arc.

The first definite proposal of this work undertaken by the *Gesellschaft*, however, appears to have been made by Bruhns. In the course of a report upon the operations of the Leipzig Observatory, he stated that in his view the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles; but he proposed at the same time that the positions of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution:—

"The *Astronomische Gesellschaft* regards it as needful that all the stars to the ninth magnitude occurring in the *Durchmusterung* should be observed with meridian circles, and commissions the Council to arrange for the execution of the work."

This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realisation of a plan of organisation which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published, and presented to the Society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the Society he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve, as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged, but he felt obliged to ask the Society not to require from him the immediate execution of the plan, but to intrust the serious consideration of it and the preparation for it to his zealous friends in the Council.

Upon the motion of Struve, the Society, by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867 Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the *Proceedings of the Gesellschaft*, but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session, and their report, which is published in the place of the paper presented by Argelander, is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give anything more than a general abstract of it. Since it differs in a few minor points from the first report of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows:—

a. The limits in declination of the proposed series of observations are $-2°$ and $+80°$. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit $+80°$ was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the *Durchmusterung* to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Königsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact coordinates of these stars are to be determined at Pulkowa, thus securing the unity necessary in order to connect in one system the observations of different zones.

d. Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work it will be desirable to use only three or four transit threads and only one or two microscopes. In order to facilitate the reductions to apparent place the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient if the change in position during the observations can be interpolated to 0·2s.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and a half or at the most two hours are not advisable, first, because the zero points will be too far apart, and, second, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report all the astronomers present who were willing to take part in this work were requested to communicate with the Council, stating the regions of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipzig, and Mannheim signified their intention to share in the work. Leyden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the *Durchmusterung*, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations. Preparations for the work of observation were immediately commenced, and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of *Haupt-sterne* consists of 336 stars to the fourth magnitude, observed at Pulkowa by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of *zu za t-sterne* consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkowa observatory by the Zone Commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted each star was observed eight times—four times in each position of the instrument. The observations were differential with respect to the *Haupt-sterne*.

The results were published by Struve in 1876, and the places there given were used in the first reduction of the Harvard College observations for 1874-75, and perhaps in some other cases.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkowa. The Zone Commission established by the *Gesellschaft*, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and the most perfect catalogue of fundamental stars yet published. The Pulkowa system for 1865 was adopted as the basis; but, in order to obtain greater freedom from accidental errors for individual stars, the final catalogue was obtained by combining with the Pulkowa series the Greenwich observations from 1836 to 1876, the Harvard College observations for 1871–72, the Leipzig observations in declination only, between 1866 and 1870, and the Leyden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkowa system.

The following observatories have taken part in the zone observations:—

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
	° °		° °
Nicolaieff ...	− 2 to + 1	Lund	+35 to +40
Albany	+ 1 ,, + 5	Bonn	+40 ,, +50
Leipzig	+ 4 ,, +10	Harvard College	+50 ,, +55
Leipzig	+10 ,, +15	Helsingfors ...	+55 ,, +60
Berlin	+15 ,, +25	Christiania ...	+65 ,, +70
Cambridge (Eng.)	+25 ,, +30	Dorpat	+70 ,, +75
Leyden	+30 ,, +35	Kasan	+75 ,, +80

The zone between −2° and +1° was originally undertaken at Palermo, that between +1° and +4° at Neuchâtel, that between +4° and +10° at Mannheim, and that between +35° and +40° at Chicago.

In the latter case the great fire at Chicago crippled the resources of the observatory to such an extent that Safford was compelled to relinquish the work, which was at that time quite far advanced.

Attention was called at an early date to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the *Durchmusterung* to the tropic of Capricorn. This was undertaken by Schönfeld at Leipzig.

In the report to the *Gesellschaft* at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hindrance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkowa system by a connecting link (*Mittelnlied*), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuysen, of Leyden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the cooperation of Melbourne, and Peters suggested the advantage of securing Washington as an additional "mean term" (V.J.S. 1877, p. 265).

The next reference to this work is contained in the *Gesellschaft* for 1881 (V.J.S. xv. p. 270). A list of 303 southern stars is here given whose exact places were at that time being determined at Leyden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuysen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of eighty-three southern fundamental stars by Auwers appears in this number of the *Gesellschaft*. The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard College (Safford) for 1864. For stars not observed at Pulkowa, the general catalogue of Yarnall (1858-1861), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkowa system for 1865. It is understood that the coordinates of the list of 303 stars are to depend upon this extension of the general system of Publication xiv. to the limits required by the southern *Durchmusterung* of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude and involving the difficulties of the scheme of observations undertaken under the auspices of the *Gesellschaft*. The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind have been made to determine the systematic relations existing between the coordinates of each zone with those of its neighbour.

It is probable, however, that the experience of Gill will be repeated on a larger scale. In 1871 he solicited the cooperation of astronomers in the determination of the coordinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix. p. 99, of the *Monthly Notices of the Royal Astronomical Society*. Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0·24s. in right ascension, and 2·3″ in declination. In four cases the difference in right ascension exceeds 30s., and in four cases the difference in declination exceeds 3·0″.

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found:—

Authority.	Δ α	Δ δ	Authority.	Δ α	Δ δ
	s.	″		s.	″
Königsberg	+·005	−0·71	Leyden ...	−·053	−0·19
Melbourne	+·026	−0·49	Paris	+·055	+0·01
Pulkowa ...	+·005	+0·36	Washington	−·120	+0·78
Leipzig ...	+·049	+0·40	Harvard Coll.	−·072	+0·09
Greenwich ...	+·009	−0·56	Cordova ...	−·032	−0·20
Berlin ...	+·044	+0·67	Oxford ...	+·076	+0·21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations: viz. the difference between the right ascensions determined by the eye-and-ear method and those determined with the aid of the chronograph.

The programme of the *Gesellschaft* makes no provision for the elimination of errors which depend upon the magnitude of the stars observed; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard College Observatory, the direct effect of a reduction of the magnitude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Besides this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leyden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the writer two other sources of error have been detected. It has been found that there is a well defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavourable atmospheric conditions differ systematically from those made under favourable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about ·08s. too great, and that the observed declinations are about 0·8″ too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed or may yet reveal. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of sidereal motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis of universal gravitation. Do the same laws pervade the interstellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion, or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system, or has Kepler indeed pierced the depths of the universe in the discovery of the laws which gave him immortality? Are we to take the alternative stated by Ball,—either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal intepretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and α Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for anything we can tell, may be practically regarded as infinite?" Can we assert with Newcomb, that in all probability the stars do not form a stable system in the sense in which we say that the solar system is stable,—that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear, or curved, to say nothing of the laws which govern that motion. How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Mädler that the Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the *Gesellschaft*, and in the survey of the southern heavens at Cordova by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the sidereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to effect an elimination of the accidental errors of observation without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First, the proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted the derived proper motions are functions of this element.

Second, the general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motion at any point, the effect of these errors upon the values of the coordinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the fundamental stellar systems independently determined by Auwers, Safford, Boss, and Newcomb, we find the following deviations *inter se* at the end of a century:—

	Maximum mean deviation in a century. $\Delta\alpha$	$\Delta\delta$		Maximum systematic deviation in a century.	
Auwers *minus* Safford ...	−0·22s.	+0·2″	...	0·23s.	1·1′
Auwers *minus* Boss ...	—	+0·8	...	—	2·1
Auwers *minus* Newcomb	−0·09	+0·8	...	0·06	2·2

It is the common impression that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situated on the right line which joins the two stars measured from π and ω Herculis. The velocity of this motion is such that the sun, with the whole *cortège* of bodies depending on him, advances annually in the direction indicated, through a space equal to 154,000,000 miles.

It must be admitted that there is a general agreement in the assignment by different investigators of the coordinates of the solar apex. This will be seen from the following tabular values:—

Authorities.	Right Ascension.		Declination.
	° ′		° ′
Herschel, 1783	257 00	...	+25 00
Prevost, 1783	230 00	...	+25 00
Klugel, 1789	260 00	...	+27 00
Herschel, 1805	245 52	...	+49 38
Argelander, 1837	257 49	...	+28 50
Lundahl, 1837	252 24	...	+14 26
Struve, 1837	261 22	...	+37 36
Galloway, 1837	260 01	...	+34 23
Mädler, 1837	261 38	...	+39 54
Airy, 1837	256 54	...	+34 29
	261 29	...	+26 44
Dunkin	261 14	...	+32 55
	263 44	..	+25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz. upon Bradley.

(*b*) The deviations *inter se* probably result in a large measure from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Biot in 1812, Bessel in 1818, and Airy in 1860, reached the conclusion that the *certainty* of the movement of the solar system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a member of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of coordinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motions were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condition are formed from which a correction is found to the assumed position of the apex, by the methods of least squares.

It must always be kept in mind that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume 0·20″ as the limit of precision in the absolute determination of the coordinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present state of instrumental astronomy.

It is safe to say that there is not a single star in the heavens whose coordinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which the error will at some future time be found to fall within this limit. The law of probabilities requires this, if the maximum limit falls within 1″. But who is prepared to select a particular star and say that the absolute position of this star in space cannot be more than 0·2″ in error?

e. At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford assumed that the distances are, at least approximately, in inverse proportion to the magnitude of the proper motions. The general result of his investigations up to this point is that there is some hope of using the solar motion as a base to advance our knowledge of stellar distances. Later investigations have been made by De Ball, but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

It is clear from this brief review that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the *Gesellschaft*. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and prepare the way for a more perfect scheme of observation in the next century.

INDIAN METEOROLOGY[1]

III.

THE next paper we shall notice is No. IX., by Fred. Chambers, on "The Winds of Kurrachee." The station dealt with is not only a representative one of the Arabian sea current, but is remarkable for exhibiting the highest average monthly wind velocity of any place in India. The observations used were furnished by a Beckley's anemograph for 1873, 1874, and 1875.[2]

In discussing the annual variation, Mr. Chambers adopts a plan which has been followed out with much success by his brother in his great work on the meteorology of the Bombay Presidency, viz. its separation into *normal* and *abnormal* north and east components.

It is thence found that the former are closely related to the corresponding barometric variations, and represent that part of the grand monsoon system which affects Kurrachee, while the latter are found to be connected with a system of local convection currents, due to (relatively) local temperature variations. These latter, though subordinate to the former i point of magnitude, are still sufficiently large to mask the true nature of the regular monsoon currents which obey the barometric law. This is more especially the case in Bengal, where, as it appears both from evidence furnished in this paper and elsewhere, the activity of the monsoon currents is far less than on the west coast of India, while the absolute efficiency of the *local* variations is about the same.[3]

Another important result deduced, is that the causes which produce the abnormal variations in the wind and pressure components, are similar to those which produce the annual variations. Thus, when the barometer rises abnormally a tenth of an inch, it is accompanied by an abnormal wind of 4·4 miles per hour from N. 55° E., while a similar rise in the barometer from summer to winter gives rise to a wind of 4·7 miles per hour from N. 57° E.

This principle, which, though *a priori* probable, has not hitherto been supported by direct evidence, is without doubt destined to play an important part in the meteorology of the future, and to form one of the few channels by which we may hope to arrive at a correct knowledge of the effects of the suspected intrinsic variation of solar radiation on terrestrial meteorology. Thus Mr. Chambers says: "If the sun's heat is itself subject to fluctuations, either periodical or irregular, corresponding meteorological effects similar to those which are produced by the sun's change of position must result;" and he adds: "The relation at Kurrachee appears to be one of the kind that would be anticipated on the supposition of the sun's heat being variable, and in itself affords a reason for suspecting, if it does not tend to prove, such variability."

In discussing the diurnal variations, Mr. Chambers divides the winds into two great classes, *convection* or ordinary currents, in which the air moves from relatively cool to relatively warm regions, and *anti-convection* currents, or "winds of elastic expansion" as Blanford calls them, which blow outwards from regions of high temperature. Each of these classes is again divisible into two sub-classes, (1) general and (2) local.[1]

If each of these systems is possible, as Mr. Chambers infers, the resultant variation is evidently a very complex one, and the main difficulty in discussing it, evidently consists in being able to adequately separate each component in turn from the rest. For this purpose Mr. Chambers employs Bessel's formula, and though he admits that the components derived by this method, do not necessarily represent physically distinct variations, its use in this case, as well as in others throughout this work, is attended with such favourable results, as to constitute a plea in favour of its more general adoption by English meteorologists.

To follow all the details of the investigation would be beyond our scope. It may therefore be briefly noted that the greater part of the variation of the north component, is due to the alternate land and sea breeze (convection currents), while a portion at any rate of the variation of the east component, is due to local anti-convection currents which prevail only in the drier months. Further, the direction of the local anti-convection currents varies with the varying position of the centre of maximum temperature range in the peninsula, while that of the coast convection currents is nearly constant.

By an ingenious plan for eliminating the variations due to coast convection currents, and by choosing the months so as to reduce the local anti-convection currents to a minimum, the existence is further proved of a system of general anti-convection currents, which, it may be remarked, were first noticed by Mr. Laughton in 1871, consisting of a double diurnal oscillation of the east component, which in the case of Kurrachee reaches its maxima at 10 a.m. and 9 p.m. and its minima at 4 p.m. and 2 a.m. respectively. These general anti-convection currents have been likewise proved by Mr. Chambers to exist at Calcutta, Belgaum, Bermuda, and Falmouth, *i.e.* in places where the ordinary convection currents differ completely both in character and intensity.

A comparison of the rainfall with the wind at the end of this paper leads to a conclusion similar to that drawn by Mr. Blanford, viz., that rain seldom falls as long as the summer monsoon continues to blow steadily, and Mr. Chambers hence infers, that a strong, damp wind from the seaward, is not the only condition required to produce rain. If this rule is only meant to apply to the place where the wind prevails, it is doubtless correct; but it seems open to misinterpretation if taken in a more general sense, since the laws of cyclonic systems and experience, both tell us that the reason why there is little rain on the coast when the sea wind is blowing strongly, is because the area of lowest pressure towards which the wind is spirally blowing is situated in the interior of the country, and that when there is *least* rain on the coast there is probably *most* inland.

Paper X. "Some Results of the Meteorological Observations taken at Allahabad during the Ten Years 1870-79," by S. A. Hill.—This paper, which represents the most complete discussion of the climatic elements at a single station in the interior of India that has ever been published, contains much that is valuable and highly suggestive to the physical meteorologist. To the climatologist it is especially interesting, owing to the inland as well as tropical position of the station. In May and June, Allahabad is one of the hottest places in India, the maximum temperature in the shade often rising above 115° Fahr., while in that terribly hot year, 1878, the temperature actually rose up to 119°·8 on June 19.

Nearly all the elements are discussed by the aid of Bessel's formula, and as it is a paper which cannot readily be reviewed in detail, we propose merely noticing one or two of the most salient conclusions deduced by the author.

One remarkable feature that comes out from the discussion of the diurnal barometric oscillation, is its "continental" character. Like Yarkand and other typically continental stations, the fall of the night tide is very small, and the ratio of the amplitude of the semi-diurnal to the diurnal component, is not only smaller than

[1] Continued from p. 430.

[2] The small elevation of the anemograph (only 15·6 feet above the ground) is open to some objection, but this is a good deal compensated for by its unusually free exposure.

[3] The resultant ranges of the wind variations obeying the barometric law are as follows:—

Kurrachee	26·6
Bombay	20·5
Calcutta	6·2

[1] These latter are dealt with in detail in those papers of Mr. Chambers's which have already been alluded to.

that at marine stations like Bombay, but reaches its minimum value in the hottest part of the year, when the ratio at the latter stations is rising towards its maximum.

When discussing the vapour tension, Prof. Hill remarks that, "while the diurnal variations of vapour tension and atmospheric pressure are connected with each other in so far as they are both effects of the diurnal inequality of temperature, it is doubtful whether there is any other connection between them except in an indirect way. At a dry station like Allahabad, where the range of the inequality of vapour tension is less than one-fourth of the range of pressure, it could never be supposed that the observed variation of the barometer is *caused* by the variation of the quantity of aqueous vapour in the air."

In explaining the afternoon minimum of vapour tension which is so distinctly marked at Allahabad during the dry hot months, a suggestion of Mr. Blanford's is noticed referring it to the semi-diurnal interchange between the lower and upper currents (which is supposed by Dr. Köppen to account for the diurnal increase in the velocity of the wind), supplemented perhaps by diffusion. The occurrence of a maximum of cloud nearly simultaneously, lends countenance to this view.

The clouds and rain are found to manifest similar diurnal variations, reaching their maxima nearly (1) when the temperature is lowest; and (2) when the vapour in the air reaches a maximum, either by diffusion from below or intermixture with the lower strata.

The heaviest fall recorded in one day during the ten years of observation was 15·48 inches between July 29 and 30, 1875, which has only been approached in the plains by the rainfall at Purneah in Bengal on September 13, 1879, and the rainfalls on September 17 and 18, 1880, when the disastrous land-lip at Naini Tal took place. These abnormal falls are found to be due to the passage of small cyclones (secondaries, as they are generally termed in European weather bureaux) which strike the land on the coast of Orissa, and move northwards along a line separating the westerly winds of Southern India from the easterly winds of the northern plain—the axis as it were of the entire monsoon system.[1] The occurrence of the fall so far west as Naini Tal, together with its exceptional character in 1880, appears to the writer to have been due to the preponderance in that year of the eastern over the western monsoon system.

In regard to wind, Allahabad conforms to the general rule deduced by Dr. Hann for stations near sea-level in every part of the world, viz. that the velocity of the wind in every season is greatest about the hottest hour of the day.

The double diurnal rotation of the wind, exhibits a peculiarity which is of considerable interest in relation to Mr. Chambers's hypothesis of the connection between it and the diurnal variation of the barometric pressure. It is that in the dry hot months, the loop in the diagram representing the nocturnal variation is almost invisible, while in the rainy season it is much more pronounced, in correspondence with the nocturnal barometric tide which undergoes similar changes. In the marine climate of Bermuda, as Mr. Chambers has shown, both the nocturnal wind and barometer variations are nearly equal to those which occur during the day, and, in proportion as the climate of Allahabad becomes moister and therefore more maritime, so the variations in these elements appear to approximate in character to those at marine stations.

Paper XII. "The Meteorology of the North-West Himalaya," by S. A. Hill.—In this paper, which was originally compiled for a gazeteer and afterwards expanded, the author gives one of the most lucid and exhaustive accounts of the meteorology of a single district that we have ever had the good fortune to meet with. Not only is the region, one of peculiar interest, owing to the extraordinary facilities it presents for the observation of atmospheric changes, in vertical as well as horizontal range, but the manner in which the data are discussed is so eminently exhaustive, and withal attractive, that it virtually forms an almost complete epitome in miniature of meteorological science. In a preliminary description of the climate, and while noticing the great heat of the Punjab and North-West Provinces as compared with regions further south, Prof. Hill alludes to the investigations of Poisson, Meech, and Wiener, as showing that the total heating effect of the sun is a function of *the time during which he is above the horizon of a place*, as well as his altitude. The region where, according to their calculations, most heat falls from May 7 to August 7, lies about latitude 41°, and it is to this circumstance, together with the dryness of the air and absence of cloud, that Prof. Hill ascribes the excessively high temperature of June and July in the extreme north of the Punjab, and in the plains of Yarkand and Kashgar still farther north.

In fact (and this is a point which we think has been a good deal overlooked by climatologists) the annual range of temperature, is not merely dependent on the sea distance of a place, but also on its latitude; the further it is from the equator, *cæteris paribus*, the greater the amount of heat that falls in the summer months. On the other hand, since the summer season diminishes in length as the latitude increases, the region where the effect upon the temperature reaches its *maximum for the longest period* is probably about lat. 25½°, where the greatest amount of heat falls from equinox to equinox.

With respect to the other factor, humidity, the North-Western Himalaya are found to differ very markedly from what may be termed the South-Eastern Himalaya. Thus Darjeeling, though higher, has very nearly the same temperature in January as Simla, Chakrata, or Mussoorie, where the winter rains are more prevalent; while in May and June, owing to its coming in for a much more copious share of the summer monsoon, it is seven or eight degrees cooler.[1]

The vertical variation in the annual and diurnal ranges of temperature is found to be dependent chiefly on differences in the relative humidity of the air, the ranges being greater at the surface than at 6000 feet, where the lower cloud strata prevail, and greater again at the most elevated stations, where the radiation is excessive.

Another important element—the vertical decrement of temperature—is found to vary considerably in amount and rate at different seasons, being on the whole greater in the summer than in the winter up to 6000 feet. Above this height, especially in the inner ranges, the temperature diminishes very slowly, partly owing to the greater latitude, and partly to the absence of *cloud*, and it is to these circumstances, quite as much as to the small amount of precipitation, that the well-known fact of the snow-line being higher on the northern than on the southern side of the Himalaya, must be attributed. After working out the decrements in detail by the help of the method of least squares, Prof. Hill finds that on the mean of the year the temperature diminishes on the mean latitude 32°, at the rate of 2°·8 per 1000 feet, or 1° in every 357 feet of ascent. In the Eastern Himalaya, it is more rapid, being 1° for 320 feet.

It is interesting to observe, as Prof. Hill says, that, assuming the rate of decrement to be uniform over the southern slope of the North-West Himalaya, "a mean temperature of 50° Fahr., equal to that of London, would be attained at a height of 9600 feet, and the annual range of temperature would probably differ little from that observed in England. The hill sanitaria, at heights of 6000 to 7000 feet, possess climates comparable as regards temperature to those of Nice, Mentone, and other health resorts on the Riviera," only they appear to be somewhat superior to these in having a much smaller annual temperature range.

Prof. Hill calculates the height of the perpetual snow-line on the south slope of the North-West Himalaya to be 17,800 feet, which is a good deal higher than the measurements given hitherto. It seems probable, however, that a good many of these, by mistaking glaciers for snow, erred in making it too low. On the inner ranges bordering on Thibet, for reasons already noticed, the snow-line is about 2000 feet higher.

The diurnal variation of pressure—that hitherto unsolved problem for meteorologists—is discussed, though briefly, yet in a masterly manner, and the analogy of the mountain type by vertical exchange of air between their summits and the valleys, to the coast type caused by lateral exchange of air between the sea and the land, is noticed in connection with the corresponding system of mountain winds.

It is evident, from a perusal of this as well as other facts in connection with the diurnal range, that a complete explanation of it can only be attained by discussing data embracing a wide area, in order to eliminate all such local variations of the normal type.

The annual variation of pressure, is also very well described and explained, and we cordially commend it to the perusal of teachers of physical geography out of text-books, in which the old

[1] Mascart, in his "Météorologie appliquée à la Prévision du Temps," has noticed a similar tendency in European storms to move "vers la région des vents faibles."

[1] This explains why Darjeeling is such a healthy place for children, to whom a high summer temperature is so fatal.

stock notions of how the monsoons are caused in India are so prevalent. The explanations of many of the text-books are in fact a libel on the intelligence of both teacher and pupil.

It is usually said that the air is heated over the land expands, and rises (presumably in a courant ascendant). The air from the sea then rushes in to supply the tendency to vacuum, and this constitutes the monsoon.

The true state of the case is, however, quite different. The way in which the air is removed from North India is not by ascending, but by lateral currents which constitute the "hot winds." The vertical expansion by which a larger portion of the whole atmosphere is lifted above the level of the hill-stations would indeed rather tend to *raise* than *lower* the isobaric planes towards the north, and as no true "courant ascendant" can exist until the air is rendered moist by the monsoon rains, the lateral winds are the only means by which the isobaric planes are caused to slope northwards prior to their arrival.

The annual variation of pressure at the level of Leh, which is 11,538 feet above sea-level, shows us that at a still higher elevation the phases of annual barometric variation are exactly contrary to those which occur on the plains, the minimum occurring in mid-winter, and the maximum in midsummer.

It is further shown by Prof. Hill that the peculiar double oscillation at the hill-stations, which in correspondence with their position is intermediate in character to those in the two extreme cases, is due to exactly the same causes as the single oscillation on the plains—a fact which will prove of much utility in further research on this complicated question.

The winds for the most part correspond to the barometric variations. The constant south-westerly direction of the wind at the elevation of the hill-stations is, the result of two independent circumstances, viz. (1) the small depth of the winter (north-east) monsoon, above which the south-west anti-monsoon blows, and (2) the great height to which the summer (south-west) monsoon reaches. Above the latter monsoon it is not known how the wind blows, but in accordance with cyclonic laws it should be *north-west*. Perhaps future research will verify this inference.

The discussion of the humidity observations, leads to results which corroborate some previously obtained from somewhat meagre data by General Strachey. On the assumption that Hann's empirical formula with the value of the constant as given by Hill is correct, viz.—

$$\log p = \log P - \frac{h}{23058},^{1}$$

it is found that "at an elevation of 23,000 feet, or about the average height of the snowy peaks, the quantity of vapour in the air is only one-tenth of that at sea-level. *The extreme dryness of Thibet and Ladakh is thus easily accounted for.*"

The relative humidity depending on the temperature, obeys quite different laws, and undergoes variations very similar to those in the amount of cloud.

The average height at which cloud would be formed in the rainy season, is calculated by Prof. Hill to be about 4000 feet, and it is interesting to note that this elevation agrees with that of the zone on which the greatest amount of rain falls in the Himalaya, the exact height of which is found to be 4240 feet above sea-level. Above this height the rainfall decreases rapidly owing to the exhaustion of vapour, but in the case of the Himalaya this decrease is rendered more prominent owing to the outer ranges cutting off the supply of vapour to those more in the interior by promoting abnormal precipitation in their own vicinity.

E. DOUGLAS ARCHIBALD

[1] Where p P are the vapour tensions at the given elevation and sea level respectively, and h is the height in feet.

MULTIPLEX CAMERA BACK

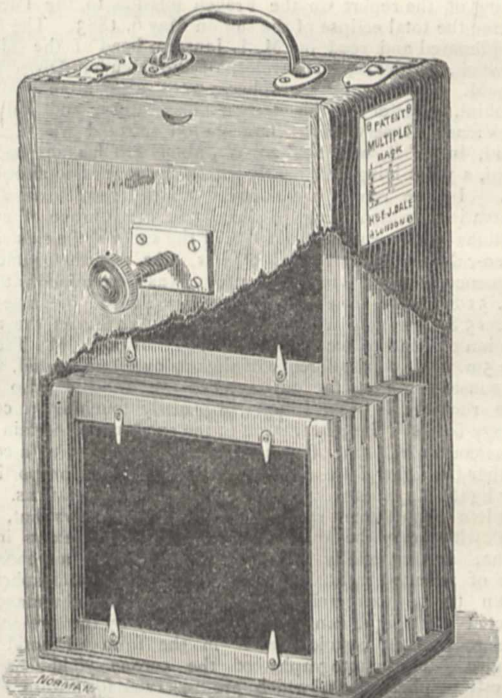
THE great advance in tourist photography by reason of the production of the more sensitive and rapid gelatine dry plates now used in such large numbers has led to continued improvements in the construction of portable photographic apparatus.

Considerable difficulty has always been experienced in carrying a sufficient supply of sensitive plates for a day's tour.

To meet this want not only are large numbers of double backs carried but the changing box has also been devised. The latter consists of a cabinet arranged to carry twelve sensitive plates and a specially constructed dark back for the camera. When a plate is required to be changed, the dark back is attached to the changing cabinet, and by the action of springs and shutters a sensitive plate is transferred from the cabinet to the dark back, which is then removed and exposed in the camera as desired. The changing box is complicated and expensive, besides adding another piece of apparatus to the tourist's luggage. The greatest difficulty, however, arises from the very merits of the gelatine plates themselves.

They are so sensitive that the utmost care is required to keep every trace of light from the plate, and double backs that appear perfect to the eye, yet by the action of the sensitive plates themselves are found to be imperfect. It is obvious that the multiplication of double backs and the shutters forming part of them, adds to the liability of access of light and consequent fogging of plates. With the use of a changing box the same trouble is experienced, with occasionally further difficulties, caused by variations in thickness or sizes of sensitive plates, the latter sometimes refusing to pass from the changing box to the back or *vice versâ*, very often causing loss of time, temper, and plate as well.

We give illustration and description of an improved apparatus that, by its simplicity of action, appears to obviate the



difficulties before mentioned, and to possess merits of its own that will insure the success desired by the inventors. The apparatus combines in one cabinet the dark back and the changing box, and is the invention of Messrs. J. H. Hare and H. J. Dale.

The woodcut (which shows part of the outer cabinet cut away to give a view of the interior construction) will immediately explain its action.

The cabinet or multiplex back is made large enough to contain thirteen plates in two tiers, the lower tier containing seven and the upper tier six plates. The plates are secured in holders or carriers, with a thin metal back to each to prevent the light passing through the plate which may be exposed to those behind it. In the front of the cabinet is the usual sliding shutter, which draws up half way for exposure of the front plate of the lower tier.

At the back of the cabinet is a shutter which can be entirely removed when required to refill the back with plates. In the front shutter a small window of non-actinic glass is provided, through which the number of the sensitive plate ready to be exposed can be seen. In the back shutter two quick-running three-thread screws are provided, the lower one to bring the plates of the lower tier up to focus, and the upper screw to tighten the upper tier of plates to prevent damage during travelling. The multiplex back fits into the camera in the usual manner. When the plate has been exposed, the shutter is closed, back removed from camera, and both screws at back unscrewed. Then the back is gently turned over; the first half-turn causes the front plate of lower tier (just exposed) to pass into the upper tier, and then the second half-turn causes the back plate of upper tier to pass to the back of lower tier, while the second plate of lower tier has now come to the front, and is ready for exposure.

If any particular plate is required to be exposed, repeat the operation of revolving the box until the number of that particular plate is seen through the window in the shutter. An ivory tablet is provided on the side of the cabinet to register the numbers of the plates as exposed.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 3.—M. Blanchard, president, in the chair.—The sitting was chiefly occupied with the reading of the report on the French mission to the Pacific to observe the total eclipse of the sun on May 6, 1883. The report was prepared and read by M. J. Janssen, head of the mission, to which, besides M. Trouvelot, of the Meudon Observatory, and M. Pasteur, photographer, were also attached MM. Tacchini, director of the Roman Observatory, and Palisa, of the Vienna Observatory. The station selected was Caroline Island, in 10° S. and 152° 20′ W., about 200 leagues north of Tahiti, a point lying very nearly within the zone of total obscuration. In the report are summed up the results of all the observations, which aimed especially at the solution of certain questions touching the constitution of the sun and the existence of the so-called intra-Mercurial planets. As regards contact the commencement of total obscuration was determined at 23h. 31m. 51·8s. mean time at Caroline Island; end of same 23h. 37m. 15·8s., leaving a difference of 5m. 24·1s. as the actual duration of totality according to M. Trouvelot. M. Tacchini gave 5m. 23s., or a difference of slightly over one second, which was considered as so far satisfactory. M. Tacchini also made some remarkable observations, especially touching a certain analogy between the constitution of the spectrum of certain parts of the corona and that of comets. In his attempt to ascertain whether the light of the corona contains any large proportion of solar light, M. Janssen succeeded beyond his expectations. The complete Frauenhofer spectrum seen by him shows that, apart from what may be due to diffraction, there exists in the corona, and especially in certain parts of it, an enormous mass of reflected light. And as the coronal atmosphere is known to be extremely attenuated, such an abundance of reflected solar light can be explained only by the presence in these regions of cosmic matter in the form of solid corpuscules. The photographs of the corona yielded several interesting phenomena, which are reserved for future study. For the present it will suffice to remark that these photographs show a more extended corona than that obtained from telescopic observation. The phenomenon also appeared limited and fixed during the period of total obscuration. A photometric measurement of the luminous intensity of the corona, which M. Janssen had prepared by means of photography, showed that in Caroline the luminosity of the corona was greater than that of the full moon. This is the first time that a precise calculation has been made of this phenomenon. On May 13 the mission re-embarked on board the *Éclaireur*, and on the home voyage visited Hawaii during the volcanic disturbances in the crater of Kilauea. M. Janssen took this opportunity of making a spectrum analysis of the flames emitted by the molten lavas, and was able to determine the presence of sodium, hydrogen, and carburetted combinations.—On the antiseptic frigidity of sores, by M. Gosselin.—Note by M. J. Delauney on the indications some years ago formulated by him on the probable epochs of great earthquakes. In a note inserted in the *Comptes Rendus* for November 17, 1879, the author considered it probable that the influence of Jupiter and Saturn on seismic disturbances is due to the passage of these planets through meteoric bodies situated in the mean longitudes of 135° and 265°. In the approximate table of future earthquakes accompanying the note, the year 1883 was not mentioned. But in another note inserted in *La Nature* for October 23, 1880, a fresh calculation of probable epochs of seismic agitation, brought down to the year 1920, mention is made of the date 1883-85, when disturbances might be expected owing to the transit of Jupiter through the August meteors.—Observations of the new planet (234) made at the Paris Observatory (equatorial of the west tower), by M. G. Bigourdan.—On the affinities of the eocene floras of England and the west of France, by M. L. Crié.—Fresh remarks on the *Phylloglossum Drummondii* (Kunze), by M. C. Eg. Bertrand.—On a process for extracting alcohol by means of lemon juice, by M. Levat.—On the fermentation of bread-stuffs, by M. G. Chicandard.

Vienna

Imperial Academy of Sciences, July 12.—D. Stur, on the morphology and systematics of culmian and carbon fauna.—H. Jahn, electrolytic studies (preliminary note).—A. Adamkiewicz, on the theory of brain pressure and on the pathology of brain compression (part ii.)—Th. von Oppolzer, communication on a series of observations (just completed) for the absolute determination of gravity at Vienna.—W. Fossek, on a derivative of isobutyraldehyde analogous to hydrobenzoin.—On the preparation of isobutyraldehyde free from acetone, by the same.—H. Molisch, researches on hydrotropism.

July 19.—C. von Ettingshausen, on the Tertiary flora of Japan.—F. Brauer, on two parasites of the June beetle (*Rhizostrogus solstitialis*): 1, *Hirmoneura obscura*, Mg.; 2, *Phorostoma lata*, Egg.—B. Mandelstamm, studies on innervation and atrophy of the laryngeal muscles.—T. Korteweg, on the question whether the variations in the length and height of the singular periods of frequency of sun-spots were produced by the interference of two periods of unequal but invariable length and height.—V. Hausmanninger, new observations on the impact of cylindrical caoutchouc rods.—M. Loewit, on the formation of white and red blood-corpuscles.—L. von Barth and H. Weidel, on the oxidation of morphine.—G. Goldschmidt, on papaverine.—J. Habermann, on some basic sulphates.—On arbutin, by the same.—M. Hoenig and E. Zatzek, on the direct estimation of carbonic acid in presence of sulphides, sulphites, and thiosulphates.—On the action of permanganate of potassium on some sulphur compounds, by the same.—A. Waage, on the action of ammonia on propionaldehyde.—E. Lippmann and F. Fleissner, contribution to a knowledge of azylines.

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