THURSDAY, JANUARY 14, 1886

## THE VEGETABLE GARDEN

*The Vegetable Garden. Illustrations, Descriptions, and Culture of the Garden Vegetables of Cold and Temperate Climates.* By MM. Vilmorin-Andrieux, of Paris. English Edition. Published under the direction of W. Robinson, Editor of the *Garden.* 8vo, pp. 601. (London: Murray, 1886.)

THIS is an English edition of a book which under its original title of "Les Plantes Potagères" has been received with encomiums alike by gardeners and by men of science. Professedly addressed solely to practical gardeners, it is so conscientiously elaborated that it has become, and will remain, a standard book of reference for the naturalist. This is a great triumph for the author, M. Henry Vilmorin. The botanist would at first glance naturally be disposed to consider such a book as at best merely a descriptive trade catalogue of an eminent seed firm, and the biologist might perhaps look askance at the notion of deriving any information of value for purposes of pure science from its pages. But on further examination, it will be found that the merely trade element is ignored, and that the descriptions of the several plants treated of are so carefully drawn up that they will as far as they go meet the requirements of the naturalist.

These descriptions are founded, as we have personal reasons for knowing, not only on the observation of plants growing in the author's seed grounds near Paris, but also on the inspection and comparison of the same or allied forms in the market gardens of Europe generally. The experimental garden of our own Royal Horticultural Society at Chiswick has been utilised in this way, while even the smaller market gardens in the vicinity of Continental towns have not been left unvisited.

Those who have not previously attended to the matter will possibly experience a feeling of surprise at the large number of varieties here enumerated. They are familiar, perhaps, with broccolis and cauliflowers, though they would find it hard to distinguish between them. They might pick out savoys from cabbages, but for the most part they would roughly class such things as "greens," and not pursue the subject further.

While all vegetables are grown for food, subdivisions may be created among them in accordance with special requirements. There are, for instance, the supply of the markets, the exigencies of the exhibition table, the demands of private establishments, and, we might add, the demands resulting from the eager competition of commercial men. The rule of the survival of the fittest has to be applied with special modifications to each of these cases.

Quality is not so much an object of solicitude to the market grower as early or quick growth and abundant produce. The man who can send his produce to market earlier than his fellows has a distinct advantage, and thus it is that foreign competition is so serious an affair for the English grower. Not only from the Channel Islands but from the South of France and Algeria come immense quantities of salads, vegetables, and fruits to our markets. The smaller cost of labour and the high prices obtained balance the expense of the long transit. The home-grower, on the other hand, can continue the supply after the Continental sources have become exhausted, and thus in some cases late varieties are preferred by our growers. But in any case for market purposes on a large scale—for the supply of the general public—the crops must be large, and hence it is that a market gardener will grow what he knows to be "a good cropper" rather than a plant of better quality but which is less productive.

The exhibition tables at our flower and fruit shows, although they foster a good deal of fantastic extravagance, nevertheless effect much good by allowing of the exhibition of numerous varieties, so that the grower may see which is good and which less good, or which is specially suited to his conditions and requirements. They also stimulate the zeal of the growers and powerfully promote good cultivation. This must have been remarked by many at the exhibitions held by the Royal Horticultural Society for the last two years in connection with the "Health" and with the "Inventions" Exhibitions. Such displays render great services to cottagers and others by bringing under their notice new and improved varieties often just as easy to cultivate as those of inferior quality. On the other hand, the "prize system" too often leads to the devotion of an inordinate amount of attention to mere size and external appearance. The bigger and the more symmetrical the exhibit the better the chance of a prize. The huge roots seen at the cattle shows, for instance, are in their way marvels of culture, but their feeding value is considerably less in proportion to others of more moderate size and perhaps less shapely appearance. Water, rather than starch and sugar or nitrogenous compounds, is the predominant element in these overgrown products. When such vegetables as these get prizes the judges are to blame and the societies are doing their work badly.

In private establishments the case is different. While the supply must of course be adequate, the quality of the vegetables is a matter of greater consideration than the mere quantity.

The different requirements we have alluded to entail a corresponding variation in the amount and character of the supply. In addition differences of soil, climate, and other local conditions necessitate other variations. What is suitable for one place is not so for another. When these facts are considered, the wonder that there should be so many varieties will pass off. In most instances these plants have been in cultivation for centuries. They exhibit, some more, some less, the tendency to vary which is the common attribute of all creatures. Having secured a variation suitable for his purpose, whatever it may be, the object of the grower is to fix it and perpetuate it, and only those who have visited our great seed-farms know with what jealous care and with what labour this is effected. When, as is the case with the Brassicas, the facilities for intercrossing are great, the difficulty of preserving a pure stock is intensified.

For scientific purposes, for the purpose of ascertaining the nature and possible range of variation within limited periods, and either under the same or under different conditions, a visit to an experimental garden like that at Chiswick, or to one or other of the great commercial seed-farms, is most instructive.

This aspect of the matter is treated of in the original preface, but we regret to see that it has been omitted from the English version. This is an injury to the book from the point of view of science, and an injustice to the reader, who would value the opinion of so careful an observer as M. Henry Vilmorin. It must suffice here to say that although he recognises the "unstable and perpetually changing characters" of plants, especially when submitted to cultivation, this variation, vast as it is, does not, in his opinion, affect either the number or the position of "legitimate species." M. Vilmorin's natural qualifications and his exceptional opportunities, of course, entitle him to be heard with very great respect, but we suspect most English botanists and cultivators would arrive at different conclusions from the same premises.

Of the value of this book to practical men it is not necessary to speak here: it is because it offers so valuable a storehouse of carefully observed facts of value to the biologist that we have alluded at such length to a volume which might at first be thought to be of interest to gardeners only, but which, we may repeat, is eminently worthy the attention of all those interested in the vast questions connected with variation and inheritance.

There are many points on which we should have liked to have commented, but the exigencies of space forbid. We have only to add that the translation has been well done by Mr. W. Miller, that some practical details have been added to adapt the book for English use, and that a very copious index is provided.

MAXWELL T. MASTERS

## *PROFESSOR MARSHALL ON THE FROG*

*The Frog; an Introduction to Anatomy and Histology.* By Prof. A. Milnes Marshall, M.D., D.Sc., M.A., F.R.S., Beyer Professor of Zoology in Owens College, Manchester. Second Edition. (Manchester: J. E. Cornish, 1885.)

THE teaching of biological science never received a greater impetus than that which took its origin in the establishment, fourteen years ago, of the laboratory at South Kensington, now associated with the Normal School of Science and Royal School of Mines; and the publication, some three years later, in connection therewith, of Huxley and Martin's "Elementary Biology," marks an epoch no less definite or important. The large number of teaching laboratories which have since been founded, wherever the English tongue is spoken and even on the Continent, have almost without exception been modelled directly or indirectly upon the Kensington plan. Practical directions for working have been issued in connection with most of them, compiled along the lines of Huxley and Martin, but specially adapted to the requirements of the individual schools. Of these, most are still alone used in the dissecting rooms for which they were written; some few have, however, been published separately, the volume before us being one of their number.

The author's name is a guarantee of the thoroughness of the work, and he has done well in taking such a prototype for a guide. We read in the preface that the book is a first instalment, to be followed by a second dealing with "types of the principal zoological groups;" as the ultimate success of the project will depend upon the selection of these types, and especially upon the evenness of balance maintained in dealing with them, we reserve full criticism until the completed work is before us.

The present edition is mainly noteworthy for the introduction of illustrations—fifteen in number. The original ones are for the most part somewhat rough, though accurate in detail, and they have the merit of representing the structures as they will meet the eye of the student. Fig. 10, however, would bear recutting, for if the bones are "represented black" why not the columella-auris; and continuity should certainly be shown between the brain and very sketchy labyrinth. The maxillo-palatine commissure—described (p. 84) in its proper place—should also find a representative in Fig. 13, and in connection with the renal-portal vein of Fig. 4 afferent renal branches might be advantageously introduced. The relation of the mesentery to the kidneys in Fig. 2 also needs looking into.

The text bears the mark of a writer in full sympathy with the difficulties which beset a beginner; more importance might, however, well be given to a consideration of those matters of symmetry, locality, and general utility, which must be mastered before studying anatomy proper. The customary restrictions put upon at least the terms *anterior*, *posterior*, *lateral*, should be clearly set down in addition to those given on p. 13 and elsewhere, and the positions of the organs should be described accordingly. If this were so, the description of the liver given on p. 17 would, for example, be more accurately rendered than it is.

It would facilitate the demonstration of the bile ducts in so small an animal, if the student were directed to simply squeeze the gall-bladder after having opened up the intestine, instead of risking the insertion of a destructive bristle as advised on p. 20.

A special feature has been made of the histological section of the work, but, granting its thoroughness, we would fain see some of the frog's tissues retained for those supplemented from other animals, especially in the case of bone, where so highly instructive an example as that of the long bone is to hand. In describing the nerve fibre the nodes of Schmidt have been overlooked, but here our author is not alone; considering the years that have elapsed since their discovery, it is strange that they should only recently have found mention in our English text-books. Reflecting on the doubtful nature of the so-called non-medullated nerve-fibres, it is a pity that the ultimate ramifications of a medullated fibre in so out-of-the-way a place as the cornea should be made (p. 89) to do duty for them.

The book is neatly and carefully got up, but a future edition should not be published without an index. The description of the mesentery in relation (p. 18) to the alimentary canal, and those of the attachments of the corpus adiposum and testes (p. 17) might well be much modified; and the like is true of the statement that the skeleton gives (p. 45) "precision to the movements" of the body. The heading (p. 48), "peculiar vertebræ," is bad, and olfactory *sacs* reads on p. 49 "olfactory nerves," in error. A somewhat remarkable typographical error is the "mpanic cavity" of p. 98.

In conclusion, we would take exception to the references (pp. 28 and 74) to the suppressed aortic arches of the embryo and to the mode of development of the nervous axis, unless their introduction bears upon the lecture scheme adopted at the Victoria University. If so, well and good, but if not, we are of opinion that such supplemental statements should be inserted, in a book of this kind, as footnotes or their equivalents. It is sufficient that the beginner should realise that three pairs of aortic arches exist in the adult, alone under consideration.

## OUR BOOK SHELF

*Methods of Research in Microscopical Anatomy and Embryology.* By C. O. Whitman, M.A., Ph.D. (Boston: S. E. Cassino and Co.; London: Trübner and Co. 1885.)

WITHIN the last few years a number of new methods have been suggested for use in microscopical, and more particularly embryological, research, and a glance at almost any one of the recent memoirs on these subjects will serve to show how much is due to the employment of new methods. It is, however, extremely important not to lose sight of the fact that complicated methods are exceedingly likely to produce false or misleading appearances. To carry on successfully any microscopical research it will probably be necessary to invent new methods or at any rate modify old ones to suit the exigencies of the case. To do this an acquaintance with the methods which have been used by previous observers is necessary, and in addition a clear idea of such general principles as it is possible to formulate with regard to the action of various classes of reagents upon various tissues.

A great number of the new methods have been described, and this often in a few words only, in special memoirs, so that they are often overlooked.

"Hitherto," says the author of the work before us, "most of our standard books of reference on methods have been rather complex in character, dealing with the microscope and technical methods as subordinate and introductory to the main subject of histology."

With regard to certain special methods there appears unfortunately to be a reluctance on the part of their inventors to reveal what they thus make a sort of trade secret, "withholding it on the ground that others are not entitled to the advantages of your experience." Dr. Whitman in his present work has sifted the numerous methods which have been suggested, and has given histologists the benefit of his great practical experience in rejecting some while recommending others; he has also endeavoured to formulate as many general principles as possible, though of course there is more to be done in this respect, our knowledge being at present insufficient to generalise to any great extent.

We notice with regret a slight tendency in the work before us as well as certain histological schools to neglect almost entirely the older and simpler methods of cutting sections. Serial section-cutting is now such an important item in all morphological work that it is apt to be used to the exclusion of older methods, which give in many cases undoubtedly better histological results.

Dr. Whitman has also collected a large number of most important observations with regard to the best method, time, and place of obtaining material; these are of course very incomplete, but it is to be hoped that he will see his way towards continuing them, and that others will follow his excellent example.

*Alternating Currents of Electricity.* By Thomas H. Blakesley, M.A. "*Electrician* Series." (London: Published at the Office of the *Electrician*, 1885.)

THIS is a very unsatisfactory little book; indeed it is difficult to find anything favourable to say of it, except that it is concerned with a subject which is of considerable importance, and which might be treated in an interesting and instructive manner. It is a reprint of papers, originally published in the *Electrician*, on Alternating Currents of Electricity, and professes to deal with various problems connected with them by geometrical methods. But the methods are long and intricate, and the work is not well done;—carelessly written and printed in the beginning, the style remains unchanged. The errors in form are numerous, the figures are not good, and geometry and algebra are mixed up in formulas in the most puzzling and irritating way. We find commas between the factors of products (all through pp. 11, 12, 13), and diagrams in which the letters are illegible in several places. In one investigation covering three or four pages, we have the letter *C* used for capacity of a condenser, for electric current, for the sum of a series of cosines, and for designating points in the diagrams. In fact the whole book is full of confusion, and is a model of what mathematical writing ought not to be; while we cannot imagine that it will prove useful or even intelligible to the telegraph engineers for whose benefit we may suppose it was put together. J. T. B.

*Third Annual Report of the New York Agricultural Experiment Station, for the Year* 1884. (Albany, N.Y.: Weed, Parsons, and Co.)

THIS Experiment Station was established by an Act of Legislature passed in 1880, and amended in 1881. The management is intrusted to a Board of Trustees, who appoint a director, horticulturist, botanist, chemist, stenographer, farmer, and assistants. Such an organisation must be considered as a step in advance beyond anything yet done in this country, being a direct action on the part of the Government to promote the exact knowledge of agriculture. This is the main point we desire to bring before the readers of NATURE. Among the many voices raised on behalf of technical instruction of artisans and others engaged in industrial pursuits, or of musicians and artists, few are to be heard in favour of the promotion of exact agricultural knowledge. The Americans are wiser, and are establishing what they call "experiment stations" in various parts of their wide territory. A few of the objects of investigation at present occupying the attention of the staff of the New York Station may be enumerated as follows:—(1) Fertiliser analysis; (2) sample orchards containing single trees of each known variety; (3) soil temperatures at various depths; (4) digestibility of various foods; (5) germination of commercial seed; (6) a study of maize; (7) root-distribution by root-washings; (8) milk; (9) diseases of plants. These sections furnish material for 418 pages, abounding in tables of results of great practical value. The pains taken in thoroughly working out the conditions of milk-production in the case of two cows, "Meg" and "Gem," are evidence of great activity and zeal. The weight of the cows was taken daily from September 17 to November 12. The weight of food consumed, the accurate analysis of the food, the *daily* weight of solid and liquid excrements, the *daily* yield of milk, the *daily* analysis of the milk,—all this carefully and punctually recorded, and fixed in tables, is a work of great importance, not only as bearing directly upon dairying, but having likewise a physiological value. Such constant daily observations are not only essential, if the experiment is to be of any practical value, but must be beyond the efforts of practical farmers, who really ought not to undertake such investigations. But the value to the community at large when such experiments are conducted quietly and regularly by persons specially set apart and paid to carry them out cannot be overrated. They must not be attempted by ordinary dairymen in ordinary stalls, and with ordinary business appliances, but can only be carried out by trained hands, in specially constructed stalls and with special arrangements, all of which must be carried out at a *loss*, which loss is the reason for an endowment. It is hard to say whether the perusal of such a Report as now lies before us impresses most with admiration for American activity or regret for English supineness. JOHN WRIGHTSON

## *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.*]

### Major Greely on Ice, &c.

IN the long and interesting address of Major Greely at the special meeting of the Royal Geographical Society, held ten days ago, with the object of hearing an account of some of his proceedings during his painfully memorable Arctic expedition, the traveller dwelt so largely upon the conditions of the ice on the open Polar sea, &c., that one was led to believe that he was talking *at* opinions—spoken or written—by some one adverse to his own; possibly those given by myself in the communication published in NATURE of December 10 last may have been meant. Should this be so, anything that Major Greely has said does not in the slightest degree affect the statements made by me in the above-mentioned letter.

Major Greely tells us that Hayes, as well as *Kane* (it should be *Morton*), saw "an open Polar basin." Payer, in as high or a higher latitude at Franz Josef Land, saw, at a much earlier date in spring than Hayes and Morton did, a larger pool of open water, with "myriads" of water-fowl, but did not think of calling it an "open Polar basin," or part of one.

This idea of a great open Polar sea is almost, if not wholly, confined to our American cousins, where it seems to have taken firm root for at least thirty years past, and has, I should imagine, a spiritualistic origin, for Dr. Kane was a believer in spiritualism.

With the fear of appearing tedious, I shall quote briefly the perfect meaning, if not the exact words, of part of a letter which a distinguished spiritualist, Major ——, sent to me prior to one of my Arctic expeditions. In this letter I was told that Franklin was still alive (clear proof had been obtained that he had been dead some years before the date of a *part* of this letter), and was residing at 132 (?), St Peter Street, in a seaport town called Joppa, having a population of more than 100,000 persons, on one of the lands near the Pole!

There was a large population, the Government Republican, and a fine, healthy, and salubrious climate. "These *people were descendants of one of the lost tribes of Israel*"!

The postscript was curious, and written at a later date than the letter itself, immediately after the death of Dr. Kane, as follows:—"Have just had communication with the spirit of Dr. Kane, whose first visit after death was paid to Franklin in Joppa, where he was still alive and well, but praying to get home."

Major Greely seems to confound two forms of ice having very different origins—namely, the floeberg, of which I have already said enough elsewhere, and the freshwater-ice, which, he says, is derived from the ice-caps of far northern lands, a mass of which he saw, having very considerable extent and "a thickness of one-sixth of a mile! with a deep valley containing a number of boulders."

This great mass of ice, 880 feet thick, with valley and rounded stones, may have been readily formed on the shores of one of the high headlands—one of which is named as having an altitude of nearly 3000 feet—along the northern portions of which Lieut. Lockwood skirted during his sledge journey on the coast of Greenland.

True, I was never in these high latitudes, but a person may sometimes be permitted to reason from analogy, as I shall attempt to do.

In 1848 I saw on the northern shore of America, in lat. 68° 40′, not far from the Coppermine River, a snowdrift against a cliff about 100 feet high, and in 1849 I and my party were detained at the same place for a good many days, during which we had ample time and opportunity to examine this snowdrift, nearly all of which was converted into ice that seemed permanent, except when parts broke off and floated away.

The slope of this snowdrift tapered towards the sea with so gentle a descent that our boat was easily hauled upon it to protect it from the ice-pack, and we with great facility carried our baggage up the ascent, and pitched our tent on the top of the cliff. A part of this snow-drift ice had broken off and drifted away, showing a very distinct stratified section, similar to that described by Dr. Moss and Major Greely.

The height of this section above sea-level was only, as far as I can remember, about 10 or 12 feet, for the water is shallow on this coast,[1] but, if Major Greely's measurements are correct, the

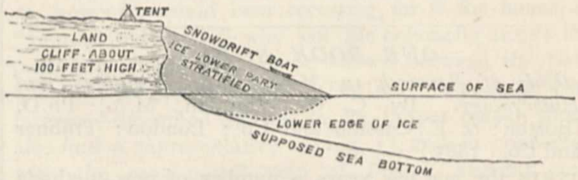


FIG. 1.—Actually seen by J. Rae in 1849, at a headland north of Coppermine River, lat. 68° 40′.

water close to the Greenland shore must be pretty deep—at least 100 fathoms—so as to float ice one-sixth of a mile thick.

My contention is that, if in latitude 69° a drift-bank of snow and ice is kept up from year to year against a cliff 100 feet high, the same thing may take place in latitude 82° to a far larger extent, where the shore is 2000 feet high, steep or precipitous, and the sea deep, so that masses of ice 800 or 900 feet thick may break off and float away.

That such great sloping snowdrifts do occur on the northern Greenland coast was proved by the difficulty met with by one of the officers of the English Expedition in travelling along them in 1876 with sledges, being forced to do so in many places by the rough ice outside, which stopped the way along the level floe.

As regards boulders, they are to be found of various sizes, more or less numerous, almost everywhere on Arctic lands high above the present sea-level, and they might have been transported to the "valley" spoken of by Greely in other ways than that supposed by him. They may have been moved downwards very slowly, by the alternate freezings and thaws of the snow and ice round them, by storms and snowdrifts, then down the slope of the valley to its lowest level, or they may have been carried by one of those streams of water similar to that mentioned as running down over the snow-caps of Grant Land. In fact, all that is wanted for this purpose would be two high,

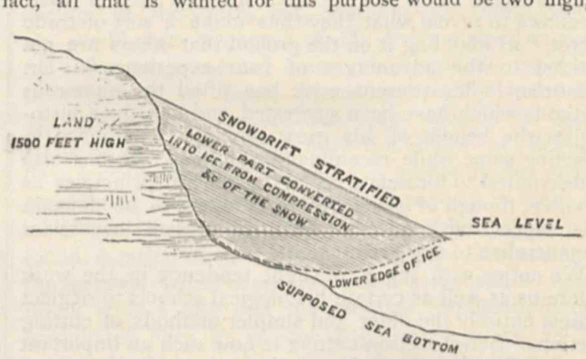


FIG. 2.—Supposed headland on the northern part of Greenland, about 1500 feet high. Greely says these headlands (or one of them) are nearly 3000 feet, having a northern or north-eastern aspect.

steep bluffs, with a deep narrow ravine between. The bluffs would give the thick masses of snow and ice-drift, and the ravine might form the bed of a stream carrying stones into the valley.

Neither Dr. Moss nor Major Greely, as far as I have noticed, have accounted for the very distinct stratification seen in the form of ice described. In all parts of Arctic America where I have been, a fall of snow is usually either accompanied or followed by a gale of wind more or less strong, chiefly from one

[1] In the very rough sketch sent, the water is made to appear much too deep; in fact, there is no pretence at correct proportion of heights and distances.—J. R.

direction, with thick snowdrift, which cuts away earth and sand in minute particles from the windward side of any hill or rising ground in its course, and these particles are carried along until they find a resting-place under the lee of some steep bank or cliff.

These foreign substances, when mixed with a great depth of snow, are not readily seen, but when the spring evaporation and thaws remove a great part of the snow, a stratum—more or less thin—of coloured matter, is visible on the surface, and this marks clearly the stratum or formation of one season. No doubt, sometimes, if there happen to be a minimum of snow-drift during the winter, followed by an unusually warm summer, all the winter deposit of snow may be removed, and the earthy deposit (naturally smaller than usual) will be added to that of the previous year.

It may be asked why I did not speak of these matters in Major Greely's presence at the meeting of the Geographical Society? This is easily explained: Major Greely's address was so long that little time was left for discussion, and this time was most properly given to the officers (four of whom were present) of the English Government Expedition of 1875-76, who, to my surprise and amusement, let the astute citizen of the United States have things pretty much his own way. In fact, one of these officers made matters rather worse than better by what he said. JOHN RAE

4, Addison Gardens, Kensington, W., January 1

## Hydrophobia—A Further Precaution

IT may be taken as an accepted fact that mongrels are more liable to rabies than well-bred dogs, both from the ill-treatment they commonly receive, and from the unnatural mingling of species that has led to their production. Statistics show that over 90 per cent. of mad dogs are retrievers, or animals so-called. In addition to these two points, it can be safely maintained that no kind of dog gives birth to so many mongrels as the retriever. Pointers, setters, terriers, and hounds will not readily breed from dogs of another class, but the reverse is true of the retriever, and the result is the production of a horrible progeny that ought to be immediately destroyed. Owners of a kennel of sporting dogs are constantly subjected to the annoyance of one of their true-bred retrievers having a litter of pups that only resemble retrievers in their coats. I would therefore suggest that the Retriever Stud Book should be kept by a Government official, and that all owners of retrievers should be obliged to send notice to him when a litter arrived; and that the police should be empowered to destroy any retriever whose owner was not provided with a certificate of registration. A few inspectors of litters could travel the country, and at a cost of a few hundreds a year prevent the development of countless mongrels—valueless for sporting purposes, hideous to look at, and sure promoters of canine madness. H. M. TOMLIN

## Rotation of Mars

PROF. BAKHUYZEN is right in regard to the number of days counted in error by Kaiser in comparing Hooke and Huyghens with recent observations. I wrote away from books, and with no means of determining whether Kaiser had made Hooke's observation a day too early or a day too late in comparing it with Huyghens's—which was what in reality he did. I saw that three days' correction would about bring matters right, and knew that in 1873 I *had* brought matters right; so concluded that was the way. But, being in London for a few days, I have looked up my paper of 1873, and find that the correction was obtained by omitting two days from Kaiser's count between Huyghens and himself, and adding one day to his count between Hooke and Huyghens.

I have not seen Prof. Bakhuyzen's paper, and the pressure of more serious business (life-duties) prevents me from giving time to such examination of it as I gave to Kaiser's in 1873. The results, however, were and are before me. It was natural I should infer that he had taken Kaiser's results as they stood. For, the comparison of either Hooke or Huyghens, using Kaiser's own dates and estimates (following him, in fact, in everything except his clerical errors in regard to the New Style date for Hooke's observations, and to the number of days in 1700 and 1800), gives no such results as Prof. Bakhuyzen has presented. Kaiser made the interval between November 1, 1862, 6h. 10·1m., and August 13, 1672, 12h. 10·3m. (at which epochs he found Mars to have been in the same position in regard to sidereal rotation), to be 69476d. 17h. 59·8m., and in this period, he said, Mars made 67,719 rotations: the resulting estimate of the rotation-period is 24h. 37m. 22·64s. In reality the interval was 69474d. 17h. 59·8m., and in this interval Mars made 67,717 rotations: the resulting estimate of the rotation-period is 24h. 37m. 22·71s. Again, using the observations of Hooke and Huyghens combined to give a mean, and the mean of the best observations between 1830 and 1870, we deduce the period 24h. 37m. 22·71s., which was, I find, the value I indicated as the most probable in 1873. Using observations up to those in 1884, I find for the period 24h. 37m. 22·703s. I find no noteworthy correction on using Maraldi's or W. Herschel's observations, with which, indeed, my inquiry began. I am satisfied the seconds are nearer 22·7 than 22·64.

RICHARD A. PROCTOR

## A Meteor

AT 4.47 p.m. yesterday, whilst returning home with two friends, I saw a large meteor pass slowly downwards in an east-north-easterly direction. Unfortunately it was twilight and very cloudy at the time of the observation, and the "fireball," as one of my friends called it, was consequently shorn of much of its brilliancy. It was, however, distinctly visible behind a thin veil of cloud, and when seen for a couple of seconds in the open it seemed to have an apparent diameter about four times that of the planet Venus, which, with the crescent moon, were the only other conspicuous objects in the heavens at that time.

Brighton, January 10 W. AINSLIE HOLLIS

## Meteorological Phenomena

I HAVE just received the inclosed notice of a meteorological phenomenon which you may consider of sufficient interest for publication in NATURE. HENRY TOYNBEE

Meteorological Office, 116, Victoria Street, London, S.W., January 6

LEAVING the port of Kingston, Jamaica, at dusk on November 23, 1885, the night was fine and starlit overhead, but about 8 p.m. a heavy bank of cloud obscured the island, and all around the upper edges of this cloud-bank brilliant flashes of light were incessantly bursting forth, sometimes tinged with prismatic hues, while intermittently would shoot vertically upwards continuous darts of light displaying prismatic colours in which the complementary tints, crimson and green, orange and blue, predominated. Sometimes these darts of light were projected but a short distance above the cloud-bank, but at others they ascended to a considerable altitude, resembling rockets more than lightning. This state of matters continued until about 9.30 p.m., when all display of light ceased. As I have never seen such a phenomenon in any other part of the world, I have deemed it an unusual occurrence, and worthy of record.

R.M.S. *Moselle*, Southampton T. MACKENZIE

I SHALL be obliged if you will allow me to record in your columns the following account of some remarkable phenomena witnessed during a voyage from Sunderland to London, and I trust that if you are good enough to insert this letter, it may be the means of eliciting some explanation from yourself or your readers as to the causes producing such strange effects. Capt. Herring, of the s.s. *Fenton*, reports to me as follows:—

"We left Sunderland at 3 p.m. on the 7th inst. bound for London, wind west-south-west, with snow squalls and strong sea; towards midnight wind increased, and the squalls cyclonic. When between Flamborough Head and Scarborough, the vessel became enveloped with phosphorescence, the mast-heads exhibiting the curious phenomenon known by sailors as 'Composants' (*corpus sancti*), which in this instance were shaped like a top, about two feet at the widest part, resembling a bunch of mistletoe illuminated. The standing rigging and all protruding objects were in like manner illuminated, and the most extraordinary effect was produced when the mate, who was on the bridge with me at the time, raised his head above the canvas weather-sheeting; the whole of his hair, exposed, and beard were instantly illuminated, and in like manner his hands when elevated became phosphorescent on the outline of his mittens. When under cover of the sheeting there was no appearance of phosphorescence; it would therefore appear that the effect of the wind produced the phenomenon. The weather towards morning moderated, and brilliant flashes of lightning were seen to the eastward." CHARLES WEST

Lloyd's, London, E.C., January 11

### The Admiralty Manual on Terrestrial Magnetism

IN a recent number of NATURE you mention that the new edition of the Admiralty Manual on Terrestrial Magnetism is being edited by me. It gives me great pleasure to be able to inform those interested in this work that I have obtained the advice and assistance of Capt. Creak and Mr. Whipple as to the changes required in the description of the ship- and land-observations respectively. From the guidance of such able specialists I feel that the work will have a value that it could never have had from my unaided exertions.

GEO. FRAS. FITZGERALD

Trinity College, Dublin, January 6

### Anchor Frosts

ON the night of Friday, January 8, there was an anchor frost in the Cherwell such as has not been known for twenty years, according to people who have lived at a mill on the river (Clifton Mill, near Aynho Station) for that period. In a mild form the phenomenon is fairly frequent there. The most marked effects are seen in comparatively still water.

Thus, in the mill-pond, where the current is stopped by the mill during the night, the whole stream becomes semi-viscous. Roots beneath the water, the brickwork at the sides of the mill-pond, &c., are seen to be coated with ice beneath the water as far down as can be seen, and between this ice and the surface ice-crystals form, not in a sheet or block, but interlaced loosely, like snow crystals in a drift. The mass thus formed blocks the channel, and it is said that water coming upon it from above will rise in level and flow over it, as over a solid obstruction. This I have not seen myself.

When the mill is started, at first the water will hardly flow past the wheel; but at length the crystals are forced to the surface, where they remain in floating masses, under which the water flows as usual.

The surface is not covered with a sheet of ice in these frosts.

In a broad, shallow ditch at right angles to the river, where the water is comparatively still, similar effects could be seen: the pebbles at the bottom coated with ice and the water filled with loose crystals. One consequence of the bottom ice forming on this occasion was that the floodgates were frozen down on the Friday evening, so that they could not be drawn up as usual, and the river overflowed during the night. In the morning, when they were at last raised, the water would hardly flow through, as already mentioned in the case of the water-wheel. T. HANDS

Clifton Mill, near Aynho Station

### Curious Phenomenon in Cephalonia

I BEG leave to forward to you an extract from a letter which I have recently received from a friend and former pupil who is at present an officer on board one of Her Majesty's ships in the Mediterranean. I have never seen any reference to the phenomenon which he describes. If you can insert the extract, perhaps it may evoke further information with regard to it. I would not forward the statement unless I had every confidence in the writer, so that I do not think he would be likely to be easily deceived or mistaken in his observations. He is a gentleman who took an excellent position in the Cambridge Mathematical Tripos. E. LEDGER

Barham, January 7

"By the way, at Cephalonia there is a very remarkable phenomenon. The sea runs into the land in a strong stream, turning a water-wheel on the way, and disappears in the earth about a hundred yards from the entrance. Can you explain this? I believe no one has yet done so. No part of the island is below the level of the sea, nor is there any salt lake or spring in the island. I imagine this water must be converted into steam, which comes out either at Naples or Stromboli."

## *SIR F. J. O. EVANS*

CAPTAIN SIR FREDERICK J. O. EVANS, R.N., K.C.B., F.R.S., late Hydrographer of the Admiralty, died at his residence, 21, Dawson Place, on December 20, 1885, in his seventy-first year.

This eminently scientific officer entered the Royal Navy in the year 1828, and served in H.M. ships *Rose* and *Whinchester*, on the North American station, until 1833, when he was transferred to H.M. surveying-vessel *Thunder*, Commander Richard Owen, and was employed until 1836 in surveying operations in various parts of the West Indies.

It was in this ship, and under the guidance of her able Captain, that he imbibed those scientific tastes which formed his character later in life, and laid the foundation of a career of usefulness, uninterrupted to its close, and which has perhaps rarely found a parallel in the naval profession.

Mr. Evans subsequently served in the *Caledonia*, the flag-ship in the Mediterranean, the *Asia*, the *Rapid*, the *Rolla*, the *Dido*, and *Wolverene*, of which two latter ships he was acting master. He was confirmed in that rank in 1841, and was then appointed to H.M.S. *Fly*, Capt. F. P. Blackwood, fitting for special exploring and surveying service in Australia and New Guinea, where he was continuously employed until 1846. He took a very leading part in the examination of the Coral Sea, the Barrier Reefs of Australia, Torres Strait, and the neighbouring shores of New Guinea, regions then comparatively unknown. After a short period of surveying service on the home coasts, Evans was appointed to the *Acheron*, under the late Admiral Stokes, and was engaged until 1851 in exploring and surveying the coasts of the then young colony of New Zealand; in both these important enterprises he took a very conspicuous part, and gained for himself the reputation of a skilful and scientific surveying officer, second to none in the profession.

During the Russian war Evans was employed in the Baltic on special reconnoitring service, and was attached to various ships of the fleet, taking an active part in the operations against Bomarsund and among the Aland Isles, for which he was mentioned in gazetted despatches.

It may be truly said that for many years of his life Evans was a zealous contributor to magnetic science. He had already begun to make observations of the three magnetic elements whilst employed on hydrographic work in H.M. ships *Fly* and *Acheron* in the Australian Colonies and New Zealand, between the years 1842–1851; but it was not until 1855, when he became Superintendent of the Compass Department of the Royal Navy that he was able to devote himself entirely to the magnetism of iron ships, a subject which was then growing yearly in importance, from the increasing amount of iron used in fitting as well as construction even before iron plating had brought about an actual crisis.

Sagaciously foreseeing the important part the science of magnetism was destined to play in the Navy, then being revolutionised by the change from wood to iron, he devoted his whole energies to the study of the subject until he had made himself completely master of it.

In 1865 Capt. Evans was appointed Chief Assistant to the Hydrographer, retaining his position as head of the magnetic department; this post he continued to hold until the early part of 1874, when a vacancy occurring in the Hydrographership of the Admiralty he was selected to fill it, and continued to do so with equal ability and conscientiousness until within a little more than a year of his death.

From the time of his first appointment in 1855 as Chief of the Admiralty Compass Department until his death Capt. Evans (in happy co-operation during a great part of the time with that great mathematical genius Archibald Smith) devoted himself heart and soul to the solution of what was really a question of life and death to the British Navy, and indeed to seafaring people all over the world. The question was whether it was possible so to deal with the disturbing element of iron, then entering largely into the construction of ships of all kinds, as to prevent the time-honoured compass from becoming a useless toy, or even a misleading guide. Now that the difficulty has been grappled with and conquered, we have half learned to forget the magnitude of the peril. But for the scientific and practical progress due to the labours of Capt. Evans and Archibald Smith we might almost with advantage have thrown all our compasses overboard. The attraction due partly to the inherent and partly to the induced magnetism of iron ships, and especially of plated ships, was so violent as to induce in some vessels, in certain positions, errors of two, three, or four points of compass indication. Something had been done to explain the causes of the mischief and to suggest palliatives. Famous old Flinders, at the beginning of the century, had spelt out the mystery so far as it was disclosed by the wooden ships of his time, but he had to deal with comparatively minute errors due to induction alone, and was never brought face to face with the stupendous difficulty which iron shipbuilding and iron ship-plating, afterwards created. The late Astronomer-Royal had done good and sound work in the earlier days of iron, but much more was needed to overcome the serious trouble which the newer types of mercantile and still more of naval vessels threatened to bring upon us.

It was at this critical epoch that Capt. Evans and Archibald Smith began to work together. Years of experimental labour and mathematical research went to the production of the "Admiralty Manual on Deviations of the Compass"—a book perhaps as perfect in its kind as a book could be. It is hard to do justice to the elegance of the mathematical handling, and, above all, to the happiness of the graphic methods which are found in the Manual, without seeming to indulge in extravagant laudation. An enthusiast in such matters once pronounced it a piece of lovely work, and one need not be an enthusiast to appreciate all that the epithet was meant to carry. Capt. Evans would have been the last to deny that the larger part of the purely theoretical investigation was due to his brilliant fellow-worker. Indeed his modesty often prompted him to claim less than his fair share of the credit due to both. The subject was one which called for the combination of practical sagacity and experience with refined scientific method—and if Archibald Smith was the stronger on the one side, Capt. Evans was his master on the other; nor was either of them without large powers, even in the special department of their joint labour in which he owned the supremacy of his friend. It was an undertaking which called for the united effort of just two such men as were fortunately brought together to do it, and the result has been a triumph to England and a blessing to the world which will preserve the memories of its authors as long as the ocean remains the highway of Englishmen and of the world.

The death of his old colleague did not abate the zeal of Capt. Evans, and few years passed since that time without some notable addition from the hands of the Hydrographer to our existing stock of experimental knowledge and scientific theory upon the subject which he had made his own. Much of his work will be found in the *Philosophical Transactions* of the Royal Society, and in 1870 he published an elementary manual supplementary to the Manual; both these works have been freely translated on the Continent, and are the acknowledged text-books in our own and foreign Navies to the present time.

The various steps of Evans's work may thus be stated:—

In 1858 a Chart of Curves of Equal Magnetic Declination, compiled by him for that epoch, was published by the Admiralty. This chart appeared most opportunely, for, with compass errors growing in amount and complexity, the mariner was by means of it enabled to ascertain in any part of the navigable world how far his compass deviated from the magnetic north.

In 1859 he read a paper on the magnetism of iron ships at the Royal United Service Institution. This was a valuable *résumé* of all that had been hitherto done in order to obtain a knowledge of the magnetism of iron ships and the treatment of their compasses. He also communicated some results of Archibald Smith's method of analysis as applied to the errors of the compass found in H.M. ships.

Evans's next paper consisted of a Report to the Hydrographer of the Admiralty on Compass Deviations in the Royal Navy. It treated of the magnetic character of the various iron ships in the Navy, and also of the *Great Eastern* steamship. The results of this paper were (1) to show the best direction for building an iron ship; (2) the best position for placing her compass; (3) the various sources of error affecting a compass under favourable conditions. This report was communicated to the Royal Society, and published in their *Transactions* in 1860.

In 1861 he read a paper of similar import before the Institute of Naval Architects.

Reference has already been made to his work on the *Great Eastern*, and an important result of it was the experimental investigation which he was led to make as to the cause of the abnormal errors of the compasses in that vessel, proceeding from the application of Airy's system of magnet and soft-iron correctors when long single-compass needles are used.

With Evans principally as an experimentalist and Archibald Smith as the mathematician, a valuable paper on the proper length and arrangement of the needles on a compass card, together with exact information as to the proper arrangement of magnet and soft-iron correctors with respect to it, was presented to the Royal Society in 1861, being the result of the joint work of those ardent investigators into the compass question in iron ships.

Commencing with this latter paper, we find Evans and Smith as we have said above, generally working together, and under their joint editorship there appeared in 1862 the first edition of the "Admiralty Manual for Deviations of the Compass." The introduction, however, of armour-plated ships soon rendered a new edition necessary, and in 1863 it was published. This work was again revised in 1869, and became the text-book of the world on the important question of the deviations of the compass in iron ships of whatever form, being translated into all the principal European languages.

In 1865 Evans and Smith produced another important paper on the "Magnetic Character of the Armour-plated Ships of the Royal Navy," which was published in the *Phil. Trans. Roy. Soc.* The novelty of the form of ships thus discussed as regards their magnetic character caused the results to be of more than usual interest, and showed with what degree of confidence compasses might be placed in positions where both helmsman and officer might have armour protection.

The practicability of determining the magnetic coefficients without swinging, and also of ascertaining the heeling error without inclining the ship, was also demonstrated, and has since been largely adopted in the Royal Navy.

In 1866 proposals were made by Mr. Evan Hopkins, C.E., to depolarise the iron hulls of ships by means of electro-magnets, and he was allowed by the Admiralty to experiment on the *Northumberland*, an armour-plated ship lying in the Victoria Docks. With the increasing difficulty of finding suitable positions for the compass, the prospect of being able to depolarise an iron ship was very attractive. In an able paper, however, read before the Royal Society in 1868, Evans showed that, so far from the hull of the *Northumberland* being depolarised, a portion of it was only temporarily, and therefore dangerously, polarised, and afterwards returned to its normal condition, thus preventing similar experiments being tried with other ships of the Navy.

It was only natural that a joint editor of the "Admiralty Manual for Deviations of the Compass," who knew the difficulties of that work for his fellow-seamen, should wish to present the subject, on which he had worked so long, in an elementary form more suitable to their every-day requirements. Evans therefore, in 1870, published his "Elementary Manual for Deviations of the Compass," a work which has been very well received by the nautical world, and has been translated into various European languages.

With the exception of some papers read at certain meetings of the British Association, and two lectures read at the Royal United Service Institution in 1865 and 1872, Evans subsequently relaxed his personal investigations into the magnetism of iron ships, and turned more to terrestrial magnetism.

Thus, in 1872, he contributed a paper to the Royal Society, on the magnetic declination in the British Islands, and compiled the magnetical instructions for the voyage of H.M.S. *Challenger*, being again assisted in this, and for the last time, by his old fellow-labourer, Archibald Smith.

Lastly, in 1878, Evans read an able and instructive lecture, on the magnetism of the earth, before the Geographical Society, showing the distribution and direction of the earth's magnetic force and the changes in its elements as then known.

Capt. Evans was elected a Fellow of the Royal Society in 1862. He sat for many years on its Council, and was more than once a Vice-President. He was also a Fellow of the Royal Astronomical and Geographical Societies; he served for many years as member of the Meteorological Committee of the Royal Society, and on the change in the constitution of that body became a member of its Council.

In recognition of his public services the Companionship of the Bath was conferred upon him in 1873, and in 1881 he was advanced to the Commandership of the same order.

Sir Frederick Evans's last public service after his retirement from the Admiralty in 1884 was as the British Delegate at the Congress of Washington for the establishment of a prime meridian and questions kindred to it.

## *JOHN MORRIS*

PROFESSOR JOHN MORRIS died on Thursday, January 7, having been laid aside by illness for several months. Born at Homerton in 1810, he spent almost the whole of his life in or near London. For many years a pharmaceutical chemist at Kensington, he passed all his spare time in exploring the neighbourhood of the metropolis and in collecting from field and book the great store of geological knowledge which was one of his especial characteristics. But science claimed more and more of his time, and at last he abandoned business entirely. In 1855 he was appointed Professor of Geology at University College, London, which post he held till 1877, when he was succeeded by Prof. Bonney. In 1878 the honorary degree of M.A. was conferred upon him by the University of Cambridge.

Morris was elected a Fellow of the Geological Society in 1845, and, whilst in health, was a constant attendant at its meetings. He received the first Lyell Medal in 1876, and has four times received the Wollaston Donation Fund. In the Geologists' Association he has been an earnest worker, having been twice its President, and always one of the foremost leaders at its excursions.

The earliest publication by Prof. Morris was "Observations on the Strata near Woolwich," in the *Magazine of Natural History* for 1835. Most of his own descriptive papers refer to the south-east of England and in the Oolitic districts, but in association with others he has done important work elsewhere. His paper with Murchison, "On the Palæozoic and their Associated Rocks of the Thüringerwald and the Harz," read before the Geological Society in 1855, is still one of the best accounts of those districts in the English language. He was joint-author with Dr. Lycett of an important monograph for the Palæontographical Society on the Oolitic Mollusca.

Considering the enormous amount of information stored in Morris's mind, one is surprised that comparatively so little original work came from his pen, and especially that so few species of fossils except those of the Great Oolite bear his name as their author. For this, however, we may perhaps be thankful; he may have been equally well employed in reducing the number of those already in use. This he did to good purpose in his "Catalogue of British Fossils," the first edition of which was published in 1843, the second in 1854. From that date onwards he was engaged in collecting materials for a third edition, which unfortunately he did not complete. Every working geologist and palæontologist has made constant use of this book, and those who have used it most best know the vast amount of labour which its preparation entailed. It is not a mere list, compiled from various authors; but nearly every species has been critically examined and the synonymy carefully traced.

Fond of conversation, a ready and pleasing public speaker, Morris was always glad to impart his knowledge to others. This knowledge was varied and exact; minerals, rocks, and fossils were equally familiar to him, and he was well read in the wider questions of physical geology.

He was held in high regard by all who knew him; and those who gathered around his grave at Kensal Green came to pay the tribute of personal friendship not less than that of admiration for scientific worth.

## *DISTRIBUTION OF DRIVING-POWER IN LABORATORIES*

A NOVEL arrangement has been adopted at the Physiological Laboratory at Cambridge, and at the Owens College, Manchester, for driving instruments in various rooms by means of a central motor. At the Brown Institution shafting has been used for the same purpose. This method is commonly used for driving machines which require a good deal of power, but it is not suitable for laboratories where the power is often required in many rooms, on different floors and some distance apart, thus causing great complication in the fittings. Again, when shafting is used, the instrument to be driven must be placed opposite a pulley on the shaft; in the arrangement about to be described, the instrument may be moved to any part of the tables, and the tables can be fixed in any part of the rooms.

We will now describe in detail the arrangement as applied in the Laboratory at Cambridge. The motor is an Otto gas-engine. It was found most convenient to place it in the cellar.

In Fig. 1 a pulley, B, fixed to a short length of shafting, is driven by a cord from the fly-wheel of the gas-engine, shown at A. The small pulleys at C are necessary to guide the cord in the required direction. This direction is vertical, hence no sag can compensate for changes of length due to stretching and the varying moisture of the atmosphere. The following arrangement was therefore adopted. Two grooves are turned in the pulley B, over which the cord passes twice, having between the first and second time passed under a pulley which supports a weight, W. Thus, the only effect of a change of length in the cord is to raise or lower the weight.

The short length of shafting driven in this manner by the pulley B is used to distribute the power to various rooms. A cord runs to each room, and forms a separate system, which can be stopped or started independently. This is done in the following manner. Fig. 2 shows a side view of the shafting, to which a number of pulleys are fixed. One of these, E, drives a second pulley, F, which is supported by a frame turning about a pivot, G. The pulley F has three grooves in it. A short leather band passes round the centre groove and round the pulley E; thus, if the centres of the pulleys E and F are brought together, the leather band which receives its motion from E will slip round F without driving it; but on the other hand, if the centres are pulled apart, F will be driven. When the cord H is pulled, the centres are brought together, and when released, a weight, K, gives the necessary

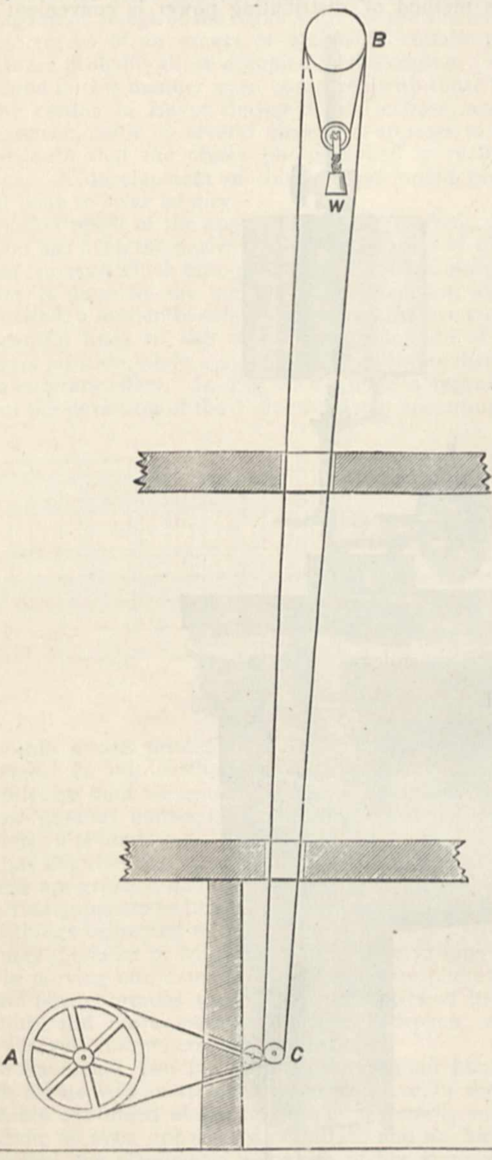


FIG. 1.

tension to the leather band. The endless cord L, which conveys power to one of the various rooms, is driven by the pulley F. This arrangement is something like that adopted for connecting the fly-wheel of the gas-engine with the shafting. The cord L first passes three-quarters of a turn round one of the grooves in the pulley F, under the pulley, M, which supports the weight K, and a quarter of a turn round the other groove of the pulley F. The cord then passes away in a horizontal direction, and is guided by pulleys round angles, either in a vertical or horizontal plane, to the rooms where the power is required. It is then guided in the same manner along the edge of each of the tables on which the instruments are to stand. The weight K, besides giving the necessary tension to the leather band, will take up all slack in the cord L. The cord runs near the ceiling, and is either led over pulleys and down to the tables in the rooms, or up through the ceiling to tables standing on the floor above.

The power is transmitted as follows to the instruments standing on the table from the cord running along its edge. Fig. 3 shows a piece of apparatus designed for this purpose, which has been called a driving-pulley. It is clamped to the edge of the table, and the cord is then made to pass round the pulley, P, as shown. This has the effect of raising the weight K, Fig. 2. The instruments are driven by a light cotton band passing round one of two grooves in a pulley fixed to the same spindle as the pulley P. The larger of the grooves is shown at Q, the smaller is not visible in the figure. The band is kept tight by means of a pulley and a weight, in the manner previously described; this arrangement also allows the

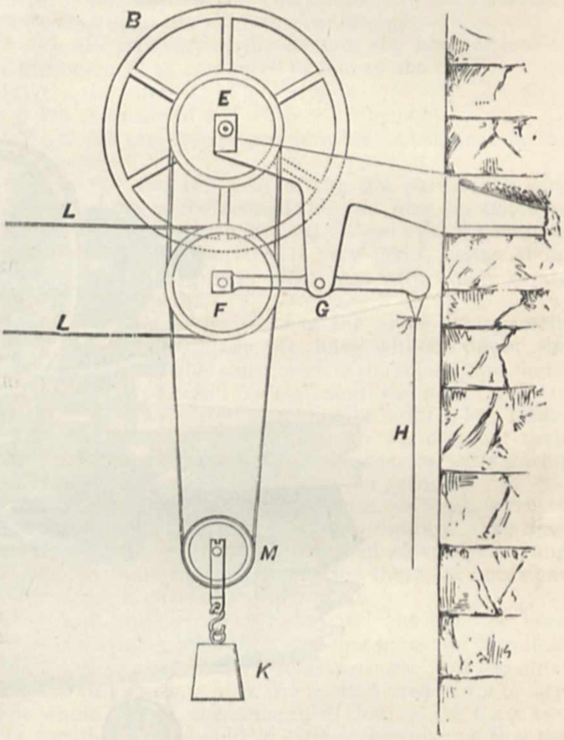


FIG. 2.

instrument to be moved to any distance from the driving pulley without altering the length of the band.

In the figure, the instrument being driven is shown at R. It is essential that it may be stopped without interfering with the cord which supplies power to the room. This is done in the following manner, which we believe is new. The handle T is fixed to a brass piece formed in the shape of a sector of a pulley, with an overhanging edge. It is arranged to turn about the bearing through which the spindle of the pulley Q passes, and can therefore rotate about the same axis as this pulley. The groove in the overhanging edge is in a line with the groove in the pulley. In the figure the cotton band is shown resting in this groove, and not in the groove of the pulley Q; now, if the sector is turned through half a revolution by the handle T, the cotton band will fall from it into the groove in the pulley Q. Thus the instrument is started; the reverse action not only throws it out of gear, but the friction of the cord running in the groove in the sector acts as a brake, and brings it quickly to rest. From the foregoing description it is evident that, once moved, the sector is required to remain in that position until moved again; in fact, it must turn with a certain amount of stiffness. A short piece of cord lies in a groove cut in its edge as shown at S; as the cord is prevented from moving and kept in tension by an india-rubber band, its friction in the groove must be overcome when the sector is turned. The smaller of the two grooves on the pulley Q is not visible in the figure, neither is the arrangement shown for lifting the cotton band out of this groove. The instrument being driven is a rotating cylinder for recording any vertical movements in the ordinary manner. Five grooves of different sizes are cut in the pulley R, and, as there are two grooves in the pulley Q, ten different speeds are possible. The sizes of these pulleys are such that the ten speeds form a geometrical series in which two consecutive speeds are in the ratio of 100 : 140.

The cotton band as well as the main driving-cord can be slipped off the driving-pulley without being cut; it can then at once be removed from the table. From this description it will be seen how the instruments may be driven, as before stated, whilst standing on any part of the table.

This method of distributing power is convenient for

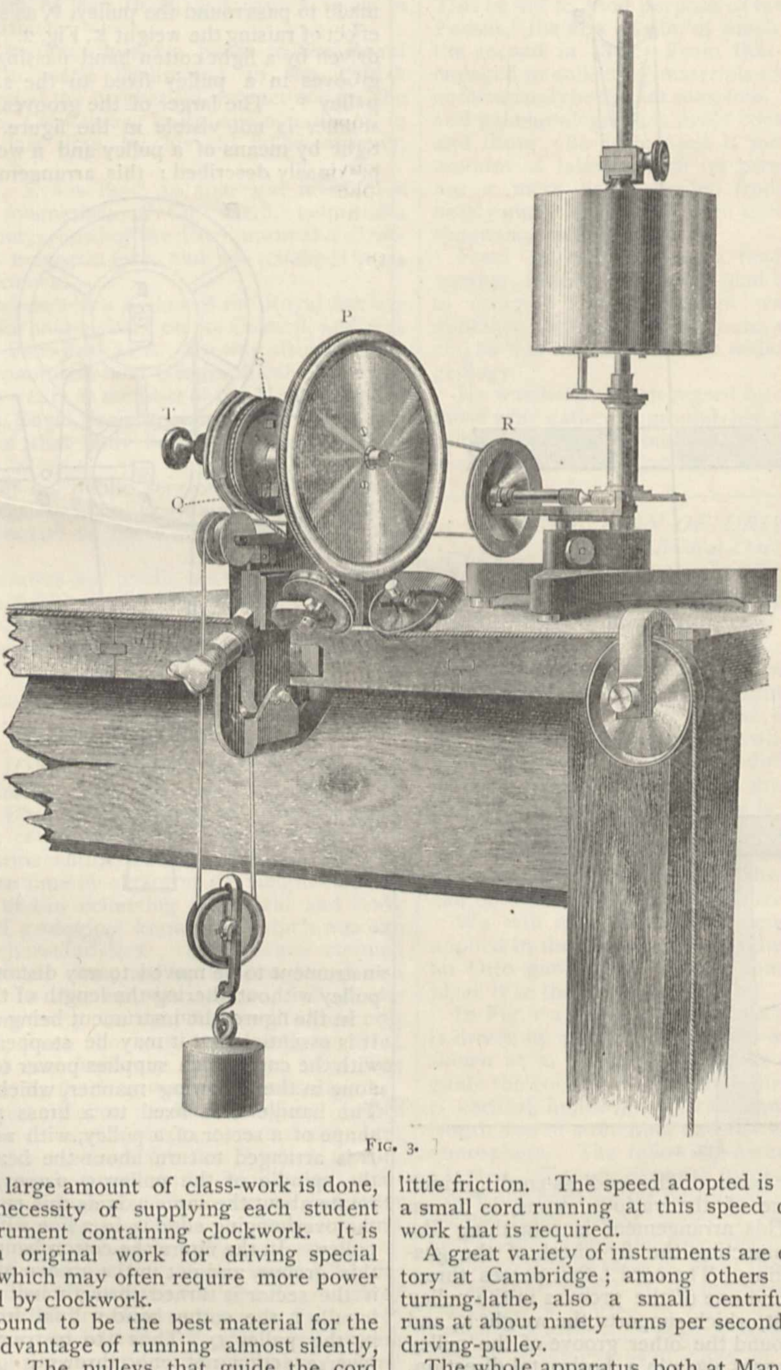

Fig. 3.

laboratories where a large amount of class-work is done, as it removes the necessity of supplying each student with a separate instrument containing clockwork. It is also most useful in original work for driving special pieces of apparatus which may often require more power than can be obtained by clockwork.

Cotton has been found to be the best material for the cords; it has the advantage of running almost silently, and is very durable. The pulleys that guide the cord from the shaft to the various rooms have been designed to run a long time without requiring oil, and with very little friction. The speed adopted is 10 feet per second; a small cord running at this speed can easily do all the work that is required.

A great variety of instruments are driven in the laboratory at Cambridge; among others we may mention a turning-lathe, also a small centrifugal machine which runs at about ninety turns per second, requiring a special driving-pulley.

The whole apparatus, both at Manchester and at Cambridge, has been designed and constructed by the Cambridge Scientific Instrument Company.

## RADIANT LIGHT AND HEAT[1]

### IV. (*Continued*)

### *Radiation and Absorption—Celestial Applications*

THIS is perhaps the most suitable place for alluding to a method of obtaining a picture of the corona on ordinary occasions, recently introduced by Dr. Huggins, and which has already met with considerable success.

By using a suitable absorbing medium Huggins has been able so greatly to diminish the proportion between the terrestrial glare and the light from the corona, that a photographic image of the regions around the sun exhibits visible traces of an excess of action in certain places which are probably those occupied by the corona. Plates prepared in this manner were compared with those taken of the corona in Egypt during a total eclipse, and the comparison, made by several observers, appears to leave little doubt that the object photographed is really the corona. A development of this method would prove a great boon to solar inquiry.

Another result of the application of the spectroscope to the sun has been the determination of the rates of motion of the currents which take place in the solar atmosphere.

This is done by the method of displacement already mentioned, a motion of solar gas towards the eye pushing its spectral lines to the more refrangible side of their ordinary position, while a motion in the opposite direction has a contrary effect. In Fig. 20 we have a representation of the deviation of the F line in a spot spectrum.

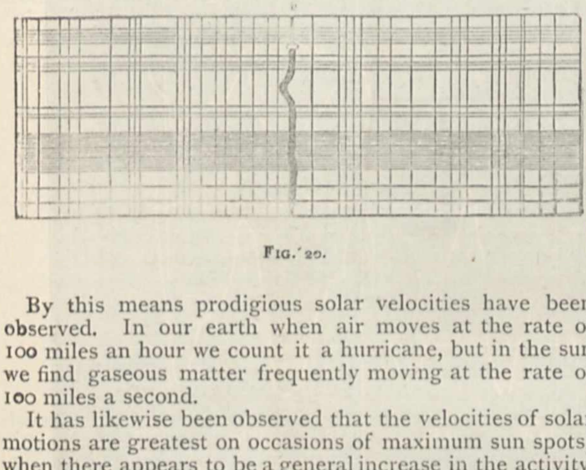

FIG. 20.

By this means prodigious solar velocities have been observed. In our earth when air moves at the rate of 100 miles an hour we count it a hurricane, but in the sun we find gaseous matter frequently moving at the rate of 100 miles a second.

It has likewise been observed that the velocities of solar motions are greatest on occasions of maximum sun spots, when there appears to be a general increase in the activity of all things belonging to our luminary.

It may therefore be said that all our observations combine in proving how extensive the solar atmosphere must be, and how enormous must be the velocities of its constituents, and more especially of the hydrogen, which enters largely into its composition.

We have thus two facts connected with our luminary which fill us with amazement. For we have, in the first place, his continued ability to radiate powerfully without cessation, or even apparent diminution, and we have, in the second, the astounding velocities of his atmospheric motions.

A little reflection will, however, serve to convince us that these two wonderful facts are intimately connected together and serve to explain each other, and that in truth the atmospheric motions are the very machinery which enables the sun to continue his radiation.

For let us inquire what is the essential condition of such continued radiation.

Clearly we must have some process by which there shall be a continuous and very rapid stream of fresh particles sent to the surface of the sun. These are there required to give out their light and heat, and then promptly to retire, being replaced by fresh particles from beneath, which again in their turn give out light and heat and then rapidly retire. It is necessary that there should be some powerful machinery of this kind, in virtue of which fresh recruits shall continually be carried to the front, while the exhausted battalions are promptly marched behind into the magazine.

Now such machinery is supplied in the vast and intense solar convection currents by means of which the cold matter from above is rapidly carried down, forming a sun spot, while the hot matter from beneath is rapidly carried upwards, forming a facula.

This ceaseless system of ascending and descending currents gives rise, no doubt, to the mottled appearance of our luminary, while in certain districts of the sun and on certain occasions the system is swelled out into gigantic proportions, and we have a large sun spot, with its accompanying faculæ. Nor is it difficult to understand why convection currents should be so powerful in the atmosphere of our luminary. The intensity of such currents will depend upon the following conditions:—

(1) On the intensity of the heat of the hot portions of the arrangement as compared to that of the cold.

(2) On the intensity of gravity.

(3) On the scale of the whole arrangement.

(4) On the presence of condensible substances in the atmosphere.

Now in the sun the heat of the hot particles is very great, while the space around the sun may be taken to represent something without heat. Again the intensity of gravity at the sun's surface is very great, being about twenty-eight times greater than that with which we are familiar on the earth.

In the third place, the scale of the whole arrangement is very great; and, lastly, we have without doubt the presence of condensible substances in the solar atmosphere. All these are powerful causes, and we must bear in mind that they have not merely to be added, but rather multiplied together. Can we therefore wonder that their joint effect is such as to raise the violence of solar storms into something like 60 or 100 miles per second?

These considerations may likewise, perhaps, serve to throw light on the question of solar variability. We have seen that sun spots have a period of eleven years, and that near the minimum of this period there are occasions when the sun is entirely without spots.

Now it is sufficiently obvious, and has likewise been proved experimentally, that a sun spot gives out less light and heat than the ordinary solar surface. On the other hand, the proportion which the spotted area bears to that of the whole disc is insignificant, so that, taking these two facts together, we should at first sight imagine that the sun ought to give us very slightly less light and heat on those occasions when there are most spots.

I think, however, that this direct action is probably inappreciable, and that sun spots are rather to be regarded as *symptoms* of a particular state of the sun, implying an increased activity of solar convection currents. Now, inasmuch as the outpouring of solar light and heat is kept up by means of these convection currents, we might therefore expect that on occasions when such currents are peculiarly powerful the sun should give out most light and heat. To use the words of the late Sir J. Herschel, the "*sun pot*" may on such occasions be boiling very rapidly.

On the whole, therefore, theory would lead us to infer that the sun will be found most powerful in its radiation on those occasions when it has most spots on its surface. It will, however, be noticed that this is merely a theoretical conclusion, and has to be supported by evidence which must of course be terrestrial. Have we, then, any terrestrial evidence that the sun is more powerful in its

[1] Continued from p. 38.

radiation at times of maximum than at times of minimum sun spot frequency?

In reply to this question, we must acknowledge that of direct evidence derived from the actinometer we have hardly any. Certain preliminary observations made by Mr. Hennessey at the headquarters of the Trigonometrical Survey in India may perhaps induce us to imagine that the sun may be most powerful on occasions of maximum sun spot frequency, but this is far from conclusive. We have, however, very strong indirect evidence in favour of this conclusion. This is derived partly from the facts of terrestrial magnetism and partly from those of terrestrial meteorology, that from the former being the stronger of the two. It is well known that the sun produces changes in the magnetism of the earth. It gives rise, for instance, to the solar-diurnal variation of the needle which is a systematic change; and it likewise produces magnetic storms, these words being employed to denote changes of a peculiarly abrupt and irregular kind. Now undoubtedly these both imply an energetic action of some kind on the part of the sun, and we have strong grounds for supposing that this energetic action is connected with the radiating power of our luminary. But both of these solar actions upon the magnetism of the earth are decidedly stronger in times of maximum than in times of minimum sun spot frequency.

Such electrical phenomena as the aurora borealis and the currents which take place in the crust of the earth are likewise peculiarly developed on the same occasions.

In Fig. 21 we have a diagram representing the connection between sun spot maxima, the maxima of declination range, and the frequency of the aurora borealis.

When we come to meteorology, the evidence before us is not so conclusive, although here also what we have tends, I think, in the same direction. Mr. Meldrum has shown that there are most cyclones in the Indian Ocean

SOLAR SPOTS, MAGNETIC DECLINATION, AND AURORAL DISPLAYS.

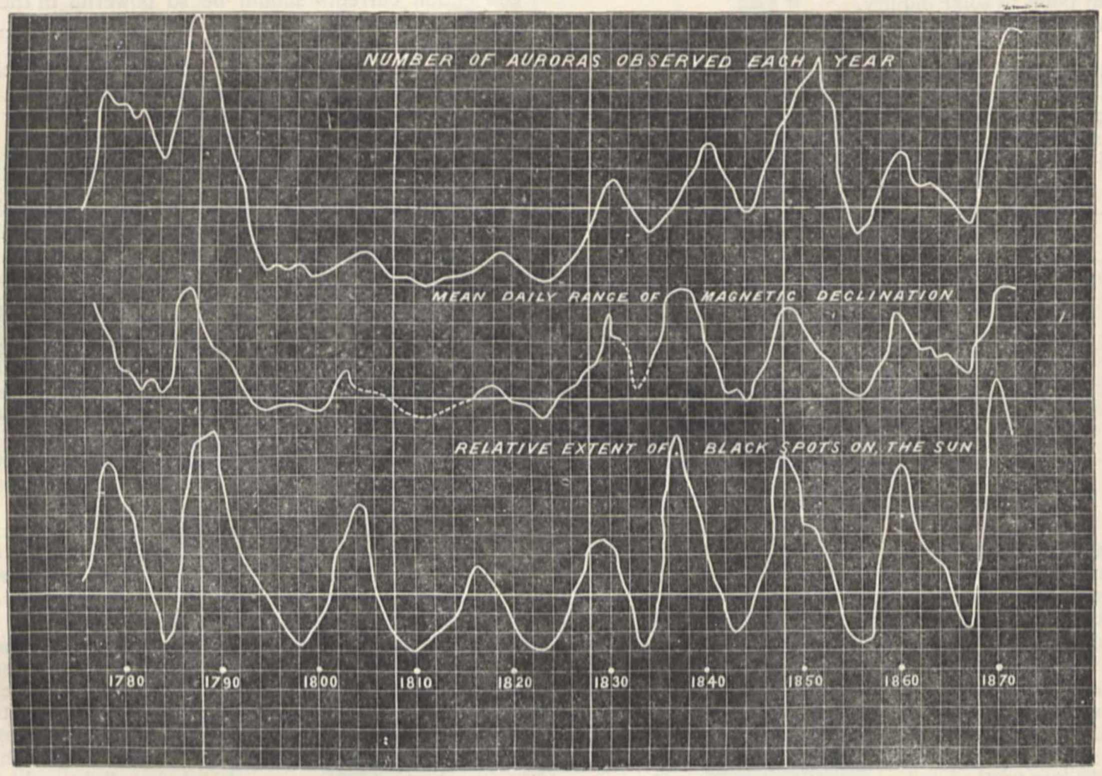


FIG. 21.

about times of maximum sun spots, and M. Pöey has proved the same thing with respect to West Indian hurricanes. On the whole, too, and in the majority of stations, the rainfall is greatest on the same occasions.

Again, let us take the barometric pressure of the air. Here a little reflection will convince us that the peculiar variations in the distribution of such pressure are really caused by the sun. For instance, we know that Western Siberia during the winter season has a pressure decidedly above the average, and we should therefore imagine that in years when the sun is peculiarly powerful the winter pressure in Siberia would prove to be particularly high. Now this is just the state of things which Mr. Blanford has found to correspond with years of maximum sun spot frequency, and thus the evidence is in favour of these being also years of maximum solar power. The Indian meteorologists have derived similar conclusions from the observations made in India. In fine, we may, with the balance of probability in our favour, adopt the conclusion deduced by Mr. Baxendell at a comparatively early period, who found that the forces which produce the movements of the atmosphere were apparently more energetic in years near maximum than in years near minimum sun spot frequency.

Let me now proceed to indicate the nature of the information which the spectroscope gives us regarding the planets and comets of our system. Since the moon and the various planets are illuminated by the sun, their spectra will necessarily be built upon that of the sun. As a matter of fact, this is found to be the case; but we ought to bear in mind that the solar rays that reach us from a planet must have penetrated some distance into the atmosphere of that planet. In doing so they will most probably have suffered spectral absorption, the nature of which may thus suffice to throw light upon the constitution of the planet's atmosphere.

Working in this manner, Dr. Huggins has observed no trace of an atmosphere in the moon, but in the spectrum of Jupiter lines are seen which indicate the existence of an absorbing atmosphere. One band appears to correspond in spectral position with dark lines due to our earth's atmosphere; but another band is different from

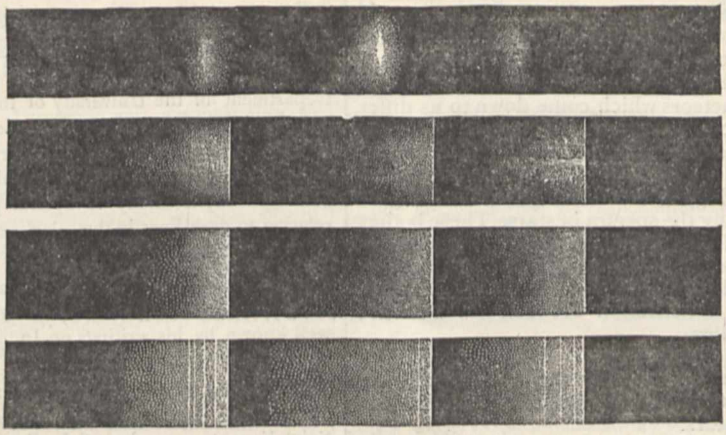


FIG. 22.—(1) Spectrum of Brorsen's comet; (2) Spectrum of Winnecke's comet; (3) Spectrum of carbon in olefiant gas; (4) Spectrum of carbon in olive oil (Huggins).

any line caused by our atmosphere, and indicates most probably the existence of some unknown constituent. Saturn has likewise a band common to the earth, so that aqueous vapour probably exists in the atmospheres of Jupiter and Saturn.

The spectrum of Mars indicates in like manner the


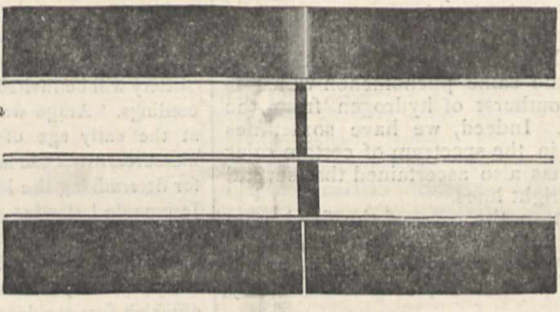

FIG. 23.

existence in the atmosphere of that planet of matter similar to that which occurs in the earth's atmosphere.

The absorption spectra of the far distant planets have likewise been examined.

Padre Secchi and M. Janssen agree with the conclusion that the vapour of water probably exists in certain planetary atmospheres.

In the absorption spectrum of our atmosphere Professor

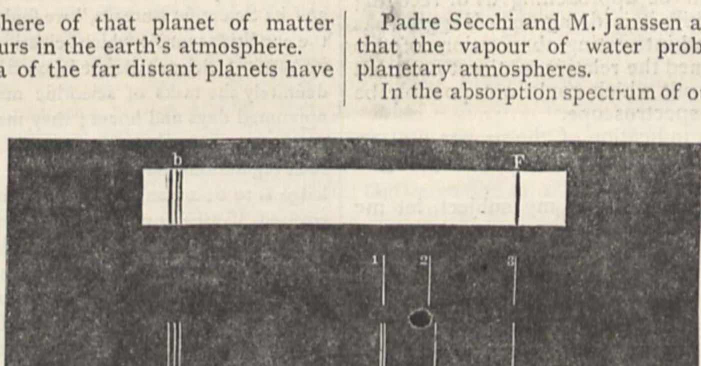


FIG. 24.—First, the solar spectrum; next, the spectrum of the nebulæ; 1, 2, 3 the lines observed. Below, the bright lines of magnesium, nitrogen, barium, and hydrogen.

Piazzi-Smyth has noticed a band which appears to be associated with the presence or possibility of rain, and which he has termed the rain-band. Strangely enough in the bright spectrum of the aurora borealis, which comes from our atmosphere, there is a line which we cannot identify with the spectrum of any known terrestrial substance.

Dr. Huggins has likewise studied the spectra given by the brighter portions of comets, and has obtained for many of these bodies lines which resemble the spectrum of carbon, as taken in a hydrocarbon.

In Fig. 22 we have, first, the spectrum of Brorsen's comet; secondly, the spectrum of Winnecke's comet; thirdly, the spectrum of carbon in olefiant gas; fourthly, that of carbon in olive oil.

More recently the same observer has found other specimens of this class of bodies, which give spectra essentially different from the hydrocarbon type, and he remarks that as the meteors which come down to us differ greatly in chemical constitution, so it is not surprising that a similar difference should be found in comets which we know to be very closely allied to meteors.

Dr. Miller and Mr. Huggins were amongst the first to give us information regarding the spectra of stars. These bodies exhibit spectra very similar to that of the sun—that is to say, they give us an underlying continuous spectrum, intersected with dark lines. We have thus evidence of a similarity in physical constitution between our sun and these very distant bodies.

The position of the dark lines is the great object of interest in stellar spectra, and by the method already described the presence of various terrestrial elements has been detected in the stars.

In the most brilliant bluish-white stars, along with the presence of numerous fine lines, we have a comparatively small number of prominent absorption lines, the substances which these indicate as present being hydrogen, calcium, magnesium, and sodium. We shall return to this subject on a future occasion.

Miller and Huggins were so fortunate as to obtain the spectrum of a star which suddenly blazed out in May, 1866, and found in it the unmistakeable presence of bright hydrogen lines.

Such a star is probably to be regarded as exhibiting on an enormous scale the same phenomenon which is frequently seen in the outburst of hydrogen from the interior of our luminary. Indeed, we have sometimes bright lines of hydrogen in the spectrum of certain solar regions. Padre Secchi has also ascertained that several very small stars exhibit bright lines.

I need hardly say that in the case of the fixed stars where the disc is a mere point, we have no possibility of differentiating between various portions of it, or of ascertaining the velocities of its atmospheric motions by spectral displacement.

We have, however, the means of ascertaining by this method whether the star be approaching us or receding from us, and how rapidly it is doing so. Huggins has made many laborious and interesting observations of this nature, and has determined the relative motion to or from us of many stars—a result which could not possibly be ascertained without the spectroscope.

In Fig. 23 we have an indication of the proper motion of Sirius as seen by the displacement of the hydrogen line F in the spectrum of that star.

Before concluding this branch of my subject, let me briefly allude to the light thrown by spectrum analysis on the composition of certain of the nebulæ. On directing his stellar spectroscope to the planetary nebula in the constellation Draco on 20th August, 1864, Dr. Huggins found that its spectrum consisted of three bright lines on a dark background—in fine, that it was the spectrum of incandescent gas.

Of these three lines one is in all probability the hydrogen line H; another appears to coincide with one of the lines of nitrogen, while the third does not coincide with any known line.

Other nebulæ have since been found by Huggins to give us similar spectra.

Fig. 24 denotes the general spectrum given by the nebulæ.

It thus appears that we have already derived great information regarding the constitution of the heavenly bodies by means of spectrum analysis.

BALFOUR STEWART

(*To be continued.*)

## NOTES

THE death is announced, in his seventy-first year, of Dr. John Christopher Draper, Professor of Chemistry in the Medica Department of the University of the City of New York. Dr. Draper was the eldest son of the eminent Prof. John W. Draper. His scientific papers, apart from those on the science of medicine, are devoted to chemical and physical subjects; and among the latter chiefly to optical phenomena. His last one, relating to dark lines in the solar spectrum, attracted some attention at the time, 1878-79.

THE death is announced of Mr. James Fergusson, F.R.S., on the 9th inst., at the age of seventy-eight years. Mr. Fergusson was well known by his writings on Indian architecture, and also by his magnificent work on "Tree and Serpent Worship in India."

THE deaths among French Academicians have been unusually numerous of late; we have to record this week that of M. de Saint-Venant, a member of the Section of Geometry.

THE Russian Academy of Science has elected Mr. David Gill, Astronomer-Royal at the Cape of Good Hope, a Corresponding Member.

SEVERAL French papers have published articles on the opportunity of celebrating the centenary of Arago's birth, this celebrated astronomer and physicist having been born at Estagel, a country town of the Department of the Pyrénées Orientales, on Feb. 26, 1786. As he was a Copley Medallist and a foreign member of the Royal Society of London, it is expected that this Society will be invited to send a delegate to take part in the proceedings. Arago was elected a member of the Paris Academy at the early age of twenty-three, after having achieved the measurement of the meridian arc from Dunkirk to Formentera, for determining the length of the metre. His predecessor was Jerome de Lalande. In 1830 he was elected Perpetual Secretary, and he continued to act as such during twenty-three years up to his death, which took place in 1853. His works have been edited by Barral, and fill seventeen large octavo volumes, of which four are devoted to "Astronomie Populaire."

IN a very excellent article in *Science* on "The Government and its Scientific Bureaus," we find some wholesome remarks on the conditions under which scientific work can be performed at its best. "Science cannot be carried forward by prescribing too definitely the tasks of scientific men. They may be bound by appointed days and hours; they may be told to perform specific duties,—and if only the maintenance of routine work is required, such regulations may secure fidelity and efficiency. But if knowledge is to be advanced, if better methods of work are to be discovered, if greater accuracy is desired, if unknown facts are to be ascertained and recorded and discussed, and, in short, if there is to be real progress, the methods of freedom are to be employed, not those of petty regulation. By this we mean that if the great undertakings which the Government has in charge, if especially its surveys of the coast and of the interior are to go forward, discretion must be given to the chiefs of bureaus, and they must be held to accountability for the aggregate success of their work. Honesty, economy, clear and accurate statement of accounts are, of course, to be demanded in every office: nobody questions this. But the determination of what shall be undertaken in a given year, to whom it shall be assigned, what allowances shall be made for instruments, books, and assistants, —these are questions which experience and judgment must decide. Somebody who has all the facts in mind must make the determination, and he must not be too quickly condemned, because the immediate results of the investigations which he has undertaken are not yet apparent. The highest personal character should be found in every one who is called upon to direct the labours of a scientific corps; he should be faithful, watchful, careful that all the interests intrusted to him may be promoted; but he should be free within the limitations of his office to select his subordinates, determine their duties, and prescribe their methods. Only by such regulated freedom as this can the highest results be obtained. Discretion with responsibility, in all the higher work of science, will bring the best services from those whose moral attitude is what it should be: no others should be intrusted with the leadership."

FROM the "Washington Letter" of *Science* we learn that in the retiring address of the President of the Chemical Society there Prof. F. W. Clarke gave "an able and entertaining *résumé* of the growth of chemistry in Washington during the past twelve or fifteen years." The President concluded with a plea for the establishment of a national laboratory, which, in its dimensions and equipment, should be commensurate with the importance and dignity of the science. Arguments to show the economy in, and the necessity for, such an establishment were not lacking, either in number or force. Examples of duplication or useless repetition of work, multiplication of instruments and facilities with no increase in efficiency, and frittering away time and energy on work properly belonging elsewhere, were given with a convincing emphasis, which made it a little difficult, at the close of the address, to believe that there were two sides to the question.

THE programme of the Congress of French Learned Societies for 1886 has been declared by a decree published by the Minister of Public Instruction. Amongst other subjects to be discussed by the fifth, or Natural Sciences Section, we find the following:—Study of the topographical distribution of the species inhabiting the French coasts; detailed study of the fluviatile fauna of France, indicating the species which are migratory and those which are permanent, and in the former case the dates of arrival and departure, noting also the time of laying the eggs, and the influence of the composition of the water; study of the migrations of birds, and of the periodical phenomena of vegetation, noting the coincidences of budding, flower, and maturity, with the appearance of the principal kinds of insects injurious to agriculture; the influence of winter temperatures on insects and their duration; study of honey- and wax-producing insects; a study, from an anthropological point of view, of the different populations which have occupied, in whole or part, a certain part of France since the most remote times; the course and duration of the great epidemics of the Middle Ages and of recent times; a comparison between the Tertiary vertebrates of the various French formations in view of the successive modifications which the types have undergone; a comparison of the Quaternary vertebrates with similar species of the present epoch; a comparison of the flora of the southern departments of France with that of Algiers; the Eucalyptus and its uses; the influence of the chain of the Cevennes in the propagation northwards of the Mediterranean species of plants and animals; a study of the general movements of sand in Asia and Africa, noting the region in which it is retreating and that in which it is advancing.

WE lately commented on a suggestion made, on economical grounds, that Jamaica should diminish the amount spent on, and by consequence the usefulness of, the Colonial Botanic Gardens. We are pleased to notice that similar views do not prevail elsewhere in the West Indian Islands, for at a late meeting of the Legislative Council of Grenada, the Governor announced that it had been decided to establish a Botanic Garden on the island. The site, he said, was selected on Government land, and an annual grant of 300*l*. was placed on the estimates for the expense of preparing the ground and paying a superintendent, who has not yet been engaged. The sum appears small, but is really a considerable amount for an island which is small and far from wealthy.

MESSRS. WODDERSPOON AND CO., Serle Street, Lincoln's Inn, are publishing in one sheet on paper for sixpence, an easy guide to the principal constellations and stars visible in Great Britain. We have first a map of the circumpolar stars, and another showing those north and south of the equator; the guiding lines, by which the stars can be easily found from the instructions carefully given, being printed in red ink. It is very clear that the author has embodied in this cheap chart the results of much labour for the teaching of people uninstructed in such matters. We learn, indeed, that many copies have already been given away among coastguardsmen, labourers, and others, who, after a little instruction, use them greedily, and delight in having them to refer to. This is distinctly a good work, and we trust that some of our readers will follow it up.

WE hear that the reconstruction of the Naval Astronomical Observatory of Japan, which has been deferred for several years owing to the extraordinary outlay required, is to be commenced at once according to the plans originally prepared.

IT is stated that M. Paul Bert, the French physiologist, has been appointed Resident in Annam, Tonquin, and Cambodia.

A COMPLETE set of observations of the new star in Orion discovered by Mr. J. E. Gore on December 13, 1885, was, *Science* states, obtained at Harvard College Observatory on December 16—the very evening on which the despatch was received from Lord Crawford—settling the non-identity of the star with D.M. + 20°, 1172, the star named in the despatch. A meridian circle observation by Prof. Rogers gave for the position of the *nova* R.A. 5h. 49m. 4·25s., Decl. + 20° 9′ 15″·6. Prof. Pickering's photometric measures made the magnitude 6·2, and the spectroscope showed the existence of bright bands. Two excellent photographs fixing the position of the star with reference to neighbouring stars were obtained, and one photograph of the spectrum. The indications are suggestive of the new star being a long-period variable, and there was a slight suspicion of a diminution in magnitude during the first six or seven hours it was under observation.

MR. WOOD-MASON has, it is stated, undertaken to prosecute a thorough inquiry into the silk-producing larvæ of India.

SOUTH-EASTERN ROUMELIA was visited on the evening of the 8th and morning of the 9th inst. by earthquake shocks, some of which were of a violent character. At Philippopolis the movement is described as being in a south-south-easterly direction.

ON December 29, 1885, there was an earthquake at Ismidt, in Asia Minor, not far from Constantinople. It took place at half-past one in the afternoon. The oscillations were slight, and passed from west to east.

AN interesting discovery of bronze hatchets and other prehistoric warlike instruments has been made at Llanwitt Major, Glamorganshire. As a number of workmen were engaged in digging a foundation for a building, they discovered three spear-heads, six hatchet-like celts, and several other interesting relics, which were concealed under an ancient wall. Some bones were also found. A further search is being organised.

SOME interest is being excited among geologists in Kent by the great depth to which a boring has been sunk at the Dover Guard Prison for a water supply. The boring, which is close to the sea, has now reached a depth of 1000 feet, being 700 feet below the sea-level.

THE additions to the Zoological Society's Gardens during the past week include a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mr. T. Risely Griffith; a Gray Ichneumon (*Herpestes griseus*) from India, presented by Capt. J. Cutting; a Gray Squirrel (*Sciurus cinereus*) from North America, presented by Mrs. Charles Neck; a Golden Eagle (*Aquila chrysaëtos*), European, presented by Mr. H. V. Knox; a Bronze-winged Pigeon (*Phaps chalcoptera*) from Australia, presented by Mr. Augustus F. Spry; a —— Hangnest (*Xanthosomus icterocephalus*) from Venezuela, a Song Thrush (*Turdus musicus*), British, deposited; a White-thighed Colobus (*Colobus vellerosus*), a Moustache Monkey (*Cercopithecus cephus*), a Ludio Monkey (*Cercopithecus ludio*) from West Africa, received in exchange.

## *OUR ASTRONOMICAL COLUMN*

THE LEYDEN OBSERVATORY.—Prof. H. G. van de Sande Bakhuyzen has published his Report for the year ending September 15, 1885. The work to which the meridian circle was devoted during the year was the continuation of the observation of a selected list of fairly bright stars situated in the immediate neighbourhood of the Pole. It is expected that this series of observations will be finished off during the present winter. With the 7-inch refractor, nine observations of Wolf's comet were made. Between October 1884 and March 1885, a series of measures have been made with Airy's double-image micrometer attached to this equatorial, for the purpose of determining the systematic errors of the measures of the diameters of Mars and Uranus obtained in former years. For this purpose, Prof. Bakhuyzen has measured the diameters of artificial disks, formed by circular holes in a copper plate, made so as to resemble, both in size and brightness, the planets themselves. The results of these investigations will be published shortly. The reduction of the meridian observations, 1877-85, is in a forward state, some parts being nearly completed. This work is intrusted to Dr. E. F. van de Sande Bakhuyzen, the First Observer. Some progress has also been made in the reduction of the zone observations, 1874-76. Prof. Bakhuyzen himself has been chiefly occupied with his monograph on the rotation-period of Mars, now published. In March 1885 work was commenced in connection with the erection of the new 10½-inch objective, and the instrument is now ready for use. The mounting has been supplied by the Repsolds, and the object-glass by Alvan Clark and Sons. Its performance, so far as it has yet been tested, appears to be remarkably good, and does not compare unfavourably with that of other instruments of similar size. In Prof. Bakhuyzens' hands it will doubtless do good work.

FABRY'S COMET.—Dr. H. Oppenheim gives the following ephemeris for this comet for Berlin midnight:—

| 1886 | R.A. h. m. s. | Decl. ° ′ | Log Δ | Log r |
|---|---|---|---|---|
| Jan. 17 | 23 31 4 | +21 53·4 | 0·2304 | 0·2025 |
| 19 | 23 29 58 | 22 5·3 | | |
| 21 | 23 28 58 | 22 18·2 | 0·2316 | 0·1857 |
| 23 | 23 28 3 | 22 32·1 | | |
| 25 | 23 27 14 | 22 46·9 | 0·2319 | 0·1682 |

BROOKS'S COMET.—The following elements and ephemeris have been computed for this comet by Dr. J. Palisa:—

$$T = 1885 \text{ Nov. } 28{\cdot}2136 \text{ Berlin M.T.}$$

$$\left.\begin{array}{l}\pi = 301^\circ\ 29'\ 50''\\ \Omega = 262\ 30\ 48\\ i = 42\ 31\ 27\end{array}\right\}\text{Mean Eq. } 1886{\cdot}0.$$

$$\log q = 0{\cdot}04091$$

Error of the middle place (O – C).

$$d\lambda \cos\beta = +4''{\cdot}7 \qquad d\beta = 4''{\cdot}5$$

*Ephemeris for Berlin Midnight*

| 1886 | R.A. h. m. s. | Decl. ° ′ | Log. Δ | Log. r | Brightness. |
|---|---|---|---|---|---|
| Jan. 14 | 21 5 25 | +12 8·6 | 0·2921 | 0·1261 | 0·74 |
| 18 | 21 20 46 | 13 48·6 | 0·2989 | 0·1377 | 0·68 |
| 22 | 21 35 48 | 15 25·2 | 0·3064 | 0·1495 | 0·62 |
| 26 | 21 50 48 | +16 57·4 | 0·3146 | 0·1614 | 0·57 |

The brightness on December 28 is taken as unity.

BARNARD'S COMET.—For Barnard's comet Dr. H. Oppenheim gives the following ephemeris, also for Berlin midnight:—

| 1886 | R.A. h. m. s. | Decl. ° ′ | Log Δ | Log r |
|---|---|---|---|---|
| Jan. 17 | 2 37 45 | +11 14·7 | 0·2136 | 0·3193 |
| 19 | 2 34 22 | 11 38·4 | | |
| 21 | 2 31 8 | 12 2·5 | 0·2173 | 0·3068 |
| 23 | 2 28 4 | 12 27·0 | | |
| 25 | 2 25 9 | 12 51·8 | 0·2213 | 0·2937 |

GORE'S NOVA ORIONIS.—Dr. Copeland, examining the spectrum of this object at Lord Crawford's Observatory, Dun Echt, finds distinct evidence of a spectrum of bright bands superposed on a well-marked spectrum of the third type; these bright bands corresponding to those ordinarily seen in cometary spectra, and obtained in the spectrum of a coal-gas flame.

## *ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 JANUARY 17-23*

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

*At Greenwich on January 17*

Sun rises, 8h. 0m.; souths, 12h. 10m. 24·5s.; sets, 16h. 21m.; decl. on meridian, 20° 42′ S.: Sidereal Time at Sunset, 0h. 9m.

Moon (Full on January 20) rises, 13h. 53m.; souths, 21h. 44m.; sets, 5h. 39m.*; decl. on meridian, 18° 13′ N.

| Planet | Rises h. m. | Souths h. m. | Sets h. m. | Decl. on meridian ° ′ |
|---|---|---|---|---|
| Mercury | 6 40 | 10 36 | 14 32 | 22 59 S. |
| Venus | 9 12 | 14 39 | 20 6 | 7 14 S. |
| Mars | 21 30* | 4 0 | 10 30 | 5 9 N. |
| Jupiter | 22 39* | 4 38 | 10 37 | 1 6 S. |
| Saturn | 14 15 | 22 25 | 6 35* | 22 36 N. |

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

*Occultations of Stars by the Moon*

| Jan. | Star | Mag. | Disap. h. m. | Reap. h. m. | Corresponding angles from vertex to right for inverted image ° ° |
|---|---|---|---|---|---|
| 17 | 117 Tauri | 6 | 15 38 | 16 28 | 33 264 |
| 18 | 130 Tauri | 6 | 0 54 | 1 59 | 124 310 |
| 18 | 26 Geminorum | 5½ | 20 37 | 21 44 | 43 271 |
| 20 | 1 Cancri | 6 | 2 3 | 2 49 | 61 338 |
| 22 | 37 Sextantis | 6 | 19 16 | 20 7 | 33 224 |

*Variable-Stars*

| Star | R.A. h. m. | Decl. ° ′ | | h. m. |
|---|---|---|---|---|
| U Cephei | 0 52·2 | 81 16 N. | Jan. 18, | 0 2 *m* |
| | | | ,, 22, | 23 41 *m* |
| Algol | 3 0·8 | 40 31 N. | ,, 19, | 20 0 *m* |
| ζ Geminorum | 6 57·4 | 20 44 N. | ,, 18, | 5 0 *m* |
| | | | ,, 23, | 7 30 *M* |
| S Cancri | 8 37·4 | 19 27 N. | ,, 19, | 3 23 *m* |
| δ Libræ | 14 54·9 | 8 4 S. | ,, 19, | 17 20 *m* |
| | | | ,, 22, | 1 11 *m* |
| U Coronæ | 15 13·6 | 32 4 N. | ,, 19, | 23 6 *m* |
| U Ophiuchi | 17 10·8 | 1 20 N. | ,, 17, | 18 41 *m* |
| | | | and at intervals of | 20 8 |
| β Lyræ | 18 45·9 | 30 14 N. | Jan. 19, | 19 0 *m* |
| η Aquilæ | 19 46·7 | 0 43 N. | ,, 17, | 0 0 *m* |
| δ Cephei | 22 24·9 | 57 50 N. | ,, 19, | 0 0 *M* |
| | | | ,, 22, | 19 0 *m* |

*M* signifies maximum; *m* minimum.

## *MR. AITKEN ON DEW*[1]

THE first point referred to in this paper is the source of the vapour that condenses to form dew. A short historical sketch is given of the successive theories from time to time advanced on this point, showing how in early times dew was supposed to descend from the heavens, and then afterwards it was suggested that it rose from the earth, while Dr. Wells, who has justly been considered the great master of this

[1] Abstract of Paper read before the Royal Society of Edinburgh on December 21, 1885, communicated by permission of the Council of the Society.

subject, thought it came neither from above nor from below, but was condensed out of the air near the surface of the earth. He combated Gersten's idea that it rose from the earth, and showed that all the phenomena observed by Gersten and others which were advanced to support this theory could be equally well explained according to the theory that it was simply formed from the vapour present at the time in the air, and which had risen from the ground during the day, and concluded that if any did rise from the ground during night, the quantity must be small, but, with great caution, he adds that "he was not acquainted with any means of determining the proportion of this part to the whole."

A few observations of the temperature of the ground near the surface and of the air over it, first raised doubts as to the correctness of the now generally-received opinion that dew is formed of vapour existing at the time in the air. These observations, made at night, showed the ground at a short distance below the surface to be always hotter than the air over it; and it was thought that so long as this excess is sufficient to keep the temperature of the surface of the ground above the dew-point of the air, it will, if moist, give off vapour; and it will be this rising vapour that will condense on the grass and form dew, and not the vapour that was previously present in the air.

The first question to be determined was whether vapour does, or does not, rise from the ground on dewy nights. One method tried of testing this point was by placing over the grass, in an inverted position, shallow trays made of thin metal and painted. These trays were put over the ground to be tested after sunset and examined at night, and also next morning. It was expected that, if vapour was rising from the ground during dewy nights, it would be trapped inside the trays. The result in all the experiments was that the inside was dewed every night, and the grass inside was wetter than that outside. On some nights there was no dew outside the trays, and on all nights the inside deposit was heavier than the outside one.

An analysis of the action of these trays is given, and it is concluded that they act very much the same as if the air was quite still. Under these conditions vapour will rise from the ground so long as the vapour-tension on the surface of the ground is higher than that at the top of the grass, and much of this rising vapour is, under ordinary conditions, carried away by the passing air, and mixed with a large amount of dryer air, whereas the vapour rising under the trays is not so diluted; and hence, though only cooled to the same amount as the air outside, it yields a heavier deposit of dew.

Another method of testing this point was employed, which consisted in weighing a small area of the exposed surface of the ground, as it was evident that if the soil gave off vapour during a dewy night, it must lose weight. A small turf about 6 inches (152 mm.) square, was cut out of the lawn and placed in a small shallow pan of about the same size. The pan with its turf, after being carefully weighed, was put out on the lawn in the place where the turf had been cut. It was exposed for some hours while dew was forming, and on these occasions it was always found to lose weight. It was thus evident that vapour was rising from the ground while dew was forming, and therefore the dew found on the grass was formed of part of the rising vapour, trapped or held back by coming into contact with the cold blades of grass.

The difference between these experiments in which the exposed bodies *lose* weight, and the well-known ones in which bodies are exposed to radiation, and the amount of dew formed is estimated by the *increase* in their weight, is pointed out. In the former case the bodies are in good heat-communication with the ground, whereas in the latter, little or no heat is received by conduction from the earth.

Another method employed for determining whether the conditions found in nature were favourable for dew rising from the ground on dewy nights, was by observations of the temperatures indicated by two thermometers, one placed on the surface of the grass, and the other under the surface, amongst the stems, but on the top of the soil. The difference in the readings of these two thermometers on dewy nights was found to be very considerable. From 10° to 18° F. was frequently observed. A minimum thermometer placed on, and another under, the grass, showed that during the whole night a considerable difference was always maintained. As a result of this difference of temperature, it is evident that vapour will rise from the hotter soil underneath, into the colder air above, and some of it will be trapped by coming into contact with the cold grass.

While the experiments were being conducted on grass land, parallel observations were made on bare soil. Over soil the inverted traps collected more dew inside them than those over grass. A small area of soil was spread over a shallow pan, and after being weighed was exposed at the place where the soil had been taken out, to see if bare soil as well as grass lost weight during dewy nights. The result was that on all nights on which the tests were made the soil lost weight, and lost very nearly the same amount as the grass land.

Another method employed of testing whether vapour is rising from bare soil, or is being condensed upon it, consisted in placing on the soil, and in good contact with it, small pieces of black mirror, or any substance having a surface that shows dewing easily. In this way a small area of the surface of the earth is converted into a hygroscope, and these test-surfaces tell us whether the ground is cooled to the dew-point or not. So long as they remain clear and undewed, the surface of the soil is hotter than the dew-point, and vapour is being given off, while if they get dewed, the soil will also be condensing vapour. On all nights observed, these test-surfaces kept clear, and showed the soil to be always giving off vapour.

All these different methods of testing point to the conclusion that during dewy nights, in this climate, vapour is constantly being given off from grass-land, and almost always from bare soil; that the tide of vapour almost always sets outwards from the earth, and but rarely ebbs, save after being condensed to cloud and rain, or on those rarer occasions on which, after the earth has got greatly cooled, a warm moist air blows over it. The results of some of the experiments are given, showing, from weighings, the amount of vapour lost by the soil at night, and also the heat lost by the surface soil.

It seems probable that when the radiation is strong, that soil, especially if it is loose and not in good heat-communication with the ground, will get cooled below the dew-point, and have vapour condensed upon it. On some occasions the soil certainly got wetter on the surface, but the question still remains, Whence the vapour? Came it from the air, or from the soil underneath? The latter seems the more probable source: the vapour rising from the hot soil underneath will be trapped by the cold surface-soil, in the same way as it is trapped by grass over grass-land. During frost, opportunities are afforded of studying this point in a satisfactory manner, as the trapped vapour keeps its place where it is condensed. On these occasions the under sides of the clods, at the surface of the soil, are found to be thickly covered with hoar-frost, while there is little on their upper or exposed surfaces, showing that the vapour condensed on the surface-soil has come from below.

The next division of the subject is on dew on roads. It is generally said that dew forms copiously on grass, while none is deposited on roads, because grass is a good radiator and cools quicker, and cools more, than the surface of a road. It is shown that the] above statement is wrong, and that dew really does form abundantly on roads, and that the reason it has not been observed is that it has not been sought for at the correct place. We are not entitled to expect to find dew on the surface of roads as on the surface of grass, because stones are good conductors of heat, and, the vapour-tension being higher underneath than above the stones, the result is, the rising vapour gets condensed on the under sides of the stones. If a road is examined on a dewy night, and the gravel turned up, the under sides of the stones are found to be dripping wet.

Another reason why no dew forms on the surface of roads is that the stones, being fair conductors, and in heat-communication with the ground, the temperature of the surface of the road is, from observations taken on several occasions, higher than that of the surface of the grass alongside. The air in contact with the stones is, therefore, not cooled so much as that in contact with the grass.

For studying the formation of dew on roads, slates were found to be useful. One slate was placed over a gravelly part of the road, and another over a hard dry part. Examined on dewy nights the under sides of these slates were always found to be dripping wet, while their upper surfaces, and the ground all round, were quite dry.

The importance of the heat communicated from the ground is illustrated by a simple experiment with two slates or two iron weights, one of them being placed on the ground, either on grass or on bare soil, and the other elevated a few inches above the surface. The one resting on the ground, and in heat-communication with it, is found always to keep dry on dewy nights, whereas the elevated one gets dewed all over.

The effect of wind in preventing the formation of dew is referred to. It is shown that, in addition to the other ways already known, wind hinders the formation of dew by preventing an accumulation of moist air near the surface of the ground.

An examination of the different forms of vegetation was made on dewy nights. It was soon evident that something else than radiation and condensation was at work to produce the varied appearances then seen on plants. Some kinds of plants were found to be wet, while others of a different kind, and growing close to them, were dry, and even on the same plant some branches were wet, whilst others were dry. The examination of the leaf of a broccoli plant showed better than any other that the wetting was not what we might expect if it were dew. The surface of the leaf was not wet all over, and the amount of deposit on any part had no relation to its exposure to radiation, or access to moist air; but the moisture was collected in little drops, placed at short distances apart, along the very edge of the leaf. Closer examination showed that the position of these drops had a close relation to the structure of the leaf; they were all placed at the points where the veins in the leaf came to the outer edge, at once suggesting that these veins were the channels through which the liquid had been expelled. An examination of grass revealed a similar condition of matters: the moisture was not equally distributed over the blade, but was in drops attached to the tips of some of the blades. These drops, seen on vegetation on dewy nights, are therefore not dew at all, but are an effect of the vitality of the plant.

It is pointed out that the excretion of drops of liquid by plants is no new discovery, as it has been long well known, and the experiments of Dr. Moll on this subject are referred to; but what seems strange is that the relation of it to dew does not seem to have been recognised.

Some experiments were made on this subject in its relation to dew. Leaves of plants that had been seen to be wet on dewy nights were experimented on. They were connected by means of an india-rubber tube with a head of water of about 1 metre, and the leaf surrounded with saturated air. All were found to exude a watery liquid after being subjected to pressure for some hours, and a broccoli leaf got studded all along its edge with drops, and presented exactly the same appearance it did on dewy nights. A stem of grass was also found to exude at the tips of one or two blades when pressure was applied.

The question as to whether these drops are really exuded by the plant, or are produced in some other way, is considered. The tip of a blade of grass was put under conditions in which it could not extract moisture from the surrounding air, and, as the drop grew as rapidly under these conditions as did those on the unprotected blades, it is concluded that these drops are really exuded by the plant. Grass was found to get "dewed" in air not quite saturated.

On many nights no true dew is formed, and nothing but these exuded drops appear on the grass; and on all nights when vegetation is active, these drops appear before the true dew, and if the radiation is strong enough and the supply of vapour sufficient, true dew makes its appearance, and now the plants get equally wet all over, in the same manner as dead matter. The difference between true dew on grass, and these exuded drops, can be detected at a glance. The drops are always exuded at a point near the tip of the blade, and form a drop of some size, while true dew is distributed all over the blade. The exuded liquid forms a large diamond-like drop, while the dew coats the blade with a pearly lustre.

Towards the end of the paper the radiating powers of different surfaces at night is considered, and after a reference to some early experiments on this subject, the paper proceeds to describe some experiments made with the radiation-thermometer described by the author in a previous paper. When working with this instrument it is placed in a situation having a clear view of the sky all round, and is fixed at the same height as the ordinary thermometer-screen, which is worked along with it, the difference between the thermometer in the screen and the radiation-thermometer being observed. This difference in clear nights amounts to from 7° to 10°. By means of the radiation-thermometer the radiating powers of different surfaces were observed. Black and white cloths were found to radiate equally well; soil and grass were also almost exactly equal to each other. Lamp-black was equal to whitening. Sulphur was about 2/3rds of black paint, and polished tin about 1/7th of black paint. Snow in the shade on a bright day was at midday 7° colder than the air, while a black surface at the same time was only 4° colder. This difference diminished as the sun got lower, and at night both radiated almost equally well. In the concluding pages of the paper some less important subjects are considered.

## *TELESCOPIC SEARCH FOR THE TRANS-NEPTUNIAN PLANET*[1]

IN the twentieth volume of the *American Journal of Science*, at page 225, I gave a preliminary account of my search, theoretic and practical, for the trans-Neptunian planet. I say *the* trans-Neptunian planet, because I regard the evidence of its existence as well-founded, and further, because, since the time when I was engaged upon this search, nothing has in the least weakened my entire conviction as to its existence in about that part of the sky assigned; while, as is well known, the independent researches in cometary perturbations by Prof. Forbes conducted him to a result identical with my own,—a coincidence not to be lightly set aside as pure accident.

That five years have elapsed since this coincidence was remarked, and the planet is still unfound, is not sufficient assurance to me that its existence is merely fanciful. In so far as I am informed, this spot of the sky has received very little scrutiny with telescopes competent to such a search; and most observers finding nothing would, I suspect, prefer not to announce their ineffective search.

The time has now come when this search can be profitably undertaken by any observer having the rare combination of time, enthusiasm, and the necessary appliances. Strongly marked developments in astronomical photography have been effected since this optical search was conducted; and the capacity of the modern dry-plate for the registry of the light of very faint stars makes the application of this method the shortest and surest way of detecting any such object. Nor is this purely an opinion of my own. But the required apparatus would be costly; and the instrument, together with the services of an astronomer and a photographer, would, for the time being, be necessarily devoted exclusively to the work. While, however, the photographic search might have to be ended with a negative result, in so far as the trans-Neptunian planet is concerned, there would still remain the series of photographic maps of the region explored, and these would be of incalculable service in the astronomy of the future.

In the latter part of the paper alluded to above, I stated the speculative basis upon which I restricted the stellar region to be examined; also the fact that between November of 1877 and March of 1878 I was engaged in a telescopic scrutiny of this region, employing the twenty-six-inch refractor of the Naval Observatory. For the purposes contemplated I had no hesitation in adopting the method of search whereby I expected to detect the planet by the contrast of its disk and light with the appearance of an average star of about the thirteenth magnitude. A power of 600 diameters was often employed, but the field of view of this eye-piece was so restricted that a power of 400 diameters had to be used most of the time. I say, too, that, "after the first few nights, I was surprised at the readiness with which my eye detected any variation from the average appearance of a star of a given faint magnitude: as a consequence whereof my observing-book contains a large stock of memoranda of suspected objects. My general plan with these was to observe with a sufficient degree of accuracy the position of all suspected objects. On the succeeding night of observation they were re-observed; and, at an interval of several weeks thereafter, the observation was again verified." Subjoined to the original observations are printed these verifications in heavy-faced type.

In conducting the search, the plans were several times varied in slight detail,—generally because experience with the work enabled me to make improvements in method. Usually I prepared every few days a new zone-chart the region of over which I was about to search; and these charts, while containing memoranda of all the instrumental data which could be prepared beforehand, were likewise so adjusted with reference to the opposition-time of the planet as to avoid, if possible, its stationary point. The same thing, too, was kept in mind in selecting the times of subsequent observation. Notwithstanding this precaution, however, it would be well if some observer who has a large telescope should now re-examine the positions of these objects.

Researches in faint nebulæ and nebulous stars appearing likely to constitute a separate and interesting branch of the astronomy of the future, it has seemed to me that the astronomers engaged in this work may like to make a careful examination of some of the stars entered in my observing-book under the category of "suspected objects." The method I adopted of

[1] By David P. Todd, M.A., from the *Proceedings* of the American Academy of Arts and Sciences.

insuring re-observation of these objects was by the determination, not of their absolute, but only of their relative, positions, through the agency of the larger "finder" of the great telescope. This has an aperture of five inches, a power of thirty diameters, and a field of view of seventy-eight minutes of arc. Two diagrams were usually drawn in the book for each of these objects,—the one showing the relation of adjacent objects in the great telescope, and the other the configuration of the more conspicuous objects in the field of view of the finder. Adjacent to these "finder" diagrams are the settings,—to the nearest minute of arc in declination, and of time in right ascension,—as read from the large finding-circles, divided in black and white. The field of vew of the finder is crossed by two pairs of hair-lines, making a square of about twelve minutes on a side by their intersection at the centre. The diagrams in all cases represent the objects as seen with an inverting eye-piece. As the adjustment of the finder was occasionally verified, as well as the readings of the large circles, there should be no trouble in identifying any of these objects, notwithstanding the fact that no estimates of absolute magnitude were recorded. The relative magnitudes, while intended to be only approximate, are still shown with sufficient accuracy for the purpose of the research, and the diagrams are, in general, faithful tracings from the original memoranda.

[Mr. Todd transcribes the observing-book entire.].

## PRIME MERIDIAN TIME[1]

ON the first day of the month, the President of the United States, in his message at the opening of Congress, referred to the International Meridian Conference lately convened in Washington, in the following words:—"The Conference concluded its labours on November 1, having with substantial unanimity agreed upon the meridian of Greenwich as the starting point whence longitude is to be computed through 180° eastward and westward, and upon the adoption for all purposes for which it may be found convenient of a Universal Day, which shall begin at midnight on the initial meredian, and whose hours shall be counted from zero up to twenty-four."

The Canadian Institute is peculiarly interested in this announcement. No society, literary or scientific, has taken a more important part in the initiation of the movement to reform our Time-system, of which the success is, to some extent, indicated in the President's words. It therefore appears to me fit and proper that I should recall to your attention the various steps which from time to time have been taken, so that we may possess a record of the events which have led to the now almost general recognition of the necessity for a new notation.

Six years ago on several occasions the meetings of the Institute were engaged in discussing the subject of Time-reckoning and the selection of a Prime Meridian common to all nations. Papers were read and arguments were advanced, with the view of showing the necessity of establishing a cosmopolitan or universal time, by which the events of history might be more accurately recorded, and which would respond to the more precise demands of science, and generally satisfy the requirements of modern civilisation. The *Proceedings* of the Institute for January and February, 1879, give at considerable length the views submitted and the suggestions offered to meet the new conditions of life. While on the one hand it was argued that the introduction of a comprehensive scheme by which time could be universally reckoned was highly desirable, it was equally maintained that the determination of a common Prime Meridian for the world was the key to its success, and that the establishment of such a meridian, as a zero, recognised by all nations, was the first important step demanded.

These *Proceedings* were brought under the notice of His Excellency the Marquis of Lorne, then Governor-General of Canada. In the name of the Institute, they were submitted, in the form of a memorial, with the hope that His Excellency would see fit to lay them before the Imperial Government, that they would by these means obtain the attention of the several scientific bodies throughout Europe, and that some general systematic effort would be made in the right direction to secure the important objects sought to be attained.

Through the good offices of His Excellency, copies of the Canadian Institute *Proceedings* found their way to the British Admiralty, the Astronomer Royal, Greenwich, the Astronomer Royal for Scotland, Edinburgh, the Royal Society, the Royal Geographical Society, the Royal Astronomical Society, the Royal United Service Institute, and other societies of eminence and weight in the United Kingdom. Copies of the papers were likewise sent through the Imperial Government to the governments of the following countries, viz.:—

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| France, | Germany, |
| Italy, | Norway and Sweden, |
| The United States, | Russia, |
| Austria, | Belgium, |
| Brazil, | Denmark, |
| Japan, | The Netherlands, |
| Spain, | Portugal, |
| Switzerland, | Turkey, |
| Greece, | China. |

In the year following, the American Metrological Society issued a Report of the Committee on Standard Time. The report bears the name of Mr. Cleveland Abbe, the Chairman of the Committee, and the date of May, 1879. It draws attention to many of the causes calling for the establishment of accurate time, and the attempts made since the establishment of the electro-magnetic telegraph to make the notation of time synchronous. While pointing out that this result had been obtained in Great Britain through the efforts of Prof. Airy, Mr. Cleveland Abbe gave a list of the various observatories on this continent which are in possession of the necessary apparatus and force proper to furnish astronomically accurate time by telegraph. Writing in February, 1880, while giving the resolution adopted by the society, recommending the adoption of accurate time by telegraph from an established astronomical observatory, Mr. Cleveland Abbe points out that the subject of accurate time has been taken up by the Horological Bureau of the Winchester Observatory of Yale College, and that the most perfect apparatus had been received for the purpose of distributing New York time with the highest degree of uniformity and accuracy.

Mr. Cleveland Abbe's own remarks on the subject are of high value. He forcibly points out the difficulties and inconveniences under which railway operations in America labour from the want of a proper system of time. To show this fact in greater force, he gives the 74 standards then followed. These several standards he proposed to set aside and replace by standards each differing one hour, or 15° of longitude.

While recommending this course, the report sets forth that the change could only be regarded as a step towards the absolute uniformity of all time-pieces, and the Society passed resolutions, that absolute uniformity of time is desirable; that the meridian six hours west of Greenwich should be adopted as the National Standard to be used in common on all railways and telegraphs, to be known as "Railroad and Telegraph Time;" that after July 4, 1880, such uniform Standard Time should be the legal standard for the whole country, and that the State and National Legislatures should be memorialised on the subject.

Mr. Cleveland Abbe in this report alluded to the previous *Proceedings* of the Canadian Institute.

The active sympathy of the Marquis of Lorne greatly aided the movement of Time-reform in its early stages. In 1879, in his official position as Governor-General, he had been the recipient of the papers published by the Canadian Institute, and had transmitted them to Great Britain, and through the Imperial Government to the several European centres. In 1880, it was learned that the Report to the American Metrological Society, above alluded to, would shortly be issued. Accordingly, advance copies were obtained from New York, and, together with additional papers issued by this Institute, they were transmitted by His Excellency to the following European Societies, and the special attention of their members was directed to the documents themselves:—

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| 1. | The Institut de France . . . . . | Paris. |
| 2. | Société de Géographie . . . . . . | Paris. |
| 3. | Société Belge de Géographie . . . . | Brussels. |
| 4. | Königliche Preussische Akademie der Wissenschaften . . . . . . | Berlin. |
| 5. | Gesellschaft für Erdkunde . . . . | Berlin. |
| 6. | Kaiserliche Akademie der Wissenschaften . . . . . . . . . . | Vienna. |
| 7. | K. K. Geographische Gesellschaft . . | Vienna. |
| 8. | Nicolaievskaia Glavnaia Observatoria . | Pultowa. |
| 9. | Imper. Rousskae Geograficheskoe Obschestou . . . . . . . . . | St. Petersburg. |

[1] This paper, giving the early history of a movement which is now attracting such general attention, we extract from a recently received volume of *Transactions* of the Canadian Institute.

10. Imper. Akademia Nauk . . . . . St. Petersburg.
11. Société de Géographie . . . . . . Geneva.

By this means attention was obtained for the subject in Europe, and when I submit evidence of the fact, I think you will agree with me, that no little of the success which has attended the movement is owing to our late Governor-General. We must all acknowledge how much we are indebted to him for the great personal interest he has always shown on the subject. We are certainly warranted in forming the opinion, that the dissemination of these papers, under such distinguished auspices, awakened attention to the arguments they contain, and prepared the way for the subsequent action taken at the International Geographical Congress at Venice, at the Geodetic Congress at Rome, and more recently at the Conference at Washington.

Mr. Wilhelm Förster, director of the Berlin Observatory, enters into the subject at length in a paper "Zur Beurtheilung Einiger Zeitfragen, insbesondere gegen die Einführung einer deutschen Normalzeit." [A Review of some considerations on Time, especially against the introduction of German National Uniform Time.]

Mr. Förster proceeds to say: The British Government is now transmitting, through its representatives, although at the same time it declares itself neutral, a proposition which has been published by a society of scientific men in Canada, which aims at the establishment of a cosmopolitan normal datum (Prime Meridian) and of Universal Time, and also the establishment of 24 meridians of an hour apart, by which local time will be absorbed. The first proposal Mr. Förster describes as an important sign of the times and evidently favours it.

He strongly protests against the establishment of a National German Time; but for railway business, and for such matters of communication as require precision, also for the form of expression of all scientific relations to time, Mr. Förster points out that a Universal Time common to the whole world is to be recommended.

Dr. G. von Boguslavski, in the *Verhandlungen der Gesellschaft für Erdkunde* (*Transactions of the Geographical Society of Berlin*), commends the new scheme as it has been put forth in the Canadian Institute papers, and foretells that it will be a matter of fact in a short time.

Col. Aden, Director of the Military School, Belgium, has two papers in the *Bulletin de la Société Belge de Géographie*. He supports the proposal to establish Universal Time, and expresses the opinion that longitude throughout the world should have a common notation, dating from one universally adopted Prime Meridian.

Col. Wauverman, President of the Geographical Society of Antwerp, in the *Bulletin* of that society, 1882, advocates the change, and with ability meets the arguments raised against it, showing them to be groundless and arising from a want of thoroughly understanding the question.

In Spain, the proposals have met with full support. All the papers issued by the Canadian Institute have been translated and published in a paper of 80 pages by the Revista General de Marina. The translator, Don Juan Pastorin, an officer of the Spanish navy, is warm in his commendation of the scheme, and takes a wise and comprehensive view of the whole question. The Spanish Government secured the advantage of this gentleman's services as delegate to the Washington Conference.

M. Otto Struve, the well-known Astronomer and Director of the Imperial Observatory, Pultowa, reports on the papers transmitted by Lord Lorne to the Imperial Academy of Science, St. Petersburg. He gives his adherence to the establishment of Universal Time, based, as suggested, on a Prime Meridian common to the whole globe, and strongly advocates counting the hours in one series up to 24.

In England, the Royal Society considered favourably both the establishment of a Universal Time and the determination of a common Prime Meridian. While the present Astronomer Royal, Mr. Christie, takes a favourable view of the question, his predecessor, Sir G. B. Airy, reported unfavourably. The report of the Astronomer Royal for Scotland, Prof. Piazzi-Smith, is decidedly adverse. These documents have been transmitted to the Institute.

In Italy, the Italian Geographical Society has given its countenance to a work by Mr. Fernando Bosari, who, in a pamphlet of 68 pages, reviews the whole question at length, and lays down three principles: 1. The determination of a Zero-meridian; 2. The establishment of Cosmopolitan Time based upon it; 3. The notation of the hours from 1 to 24 in a continuous series.

The question of Universal Time and the selection of a Prime Meridian is discussed with ability in a paper published by M. Thury, professor at the University of Geneva.

At the meeting of the Association for the Reform and Codification of the Laws of Nations at Cologne, Prussia, in 1881, the question of regulating time on the new system was considered and resolutions moved.

In the same year (1881), the subject occupied the attention of the International Geographical Congress at Venice, at which a delegate from the Canadian Institute attended. The general question was warmly discussed, and resolutions adopted. The appointment of an International Conference to meet at Washington, specially to consider the question, was then suggested by the Canadian delegate, and warmly supported by gentlemen representing the government and scientific societies of the United States. The President of the Congress communicated the resolutions to the Italian Government, and Prince Teano, on behalf of the Italian Government, undertook to conduct the official correspondence. Out of this appears to have sprung the important discussion at the meeting of the International Geodetic Association at Rome, in October, 1883, when the utility of Universal Time was recognised, and a special International Conference for the establishment of a zero-meridian for longitude and time recommended.

Returning to this side of the Atlantic, the question of regulating time for railway, telegraph, and civil purposes generally, was considered at the Convention of the American Society of Civil Engineers, held at Montreal, June 15, 1881, and a committee of men engaged in the management, and familiar with the economy of railways, appointed to examine the question. The committee has reported from time to time. They recognised that a proposition to reform the general time system of the country was a problem beset with difficulties, but it did not appear to them insolvable. It was felt, however, that the question affected so many interests that any change could only be effected by general concurrence.

To attain the end proposed by this society, the papers bearing on the question were printed, and a scheme modified on the *Proceedings* of the Canadian Institute was drawn up, under the title of "Cosmopolitan scheme for regulating time."

I may briefly recall the features of the scheme.

There should be one standard of absolute time, a Universal Day, based on the mean solar passage, at one particular meridian, the Prime or initial meridian for computing longitude. This Prime Meridian, together with the Universal Day, to be observed by all civilised nations.

There should be 24 secondary or hour-meridians established, 15 degrees of longitude apart, beginning with the Prime Meridian as zero.

To distinguish the Universal Day from local days, it should bear the title of "Cosmic Day." [1]

Cosmic Time is intended to be used to promote exactness in chronology, and to be employed in astronomy, navigation, meteorology and in synchronous observations throughout the world. To be employed in ocean telegraphy and generally in all operations non-local in character.

The several 24 meridians to be used as standards for local time around the globe. Applying the system to North America, the effect would be to reduce the standards to four or five, as suggested by the Metrological Society.

A circular, dated March 15, 1882, signed by Mr. John Bogart, the Secretary of the American Society of Civil Engineers, was forwarded to the leading men in railway direction, either as general managers, superintendents, or engineers, and to men of scientific attainments throughout the United States and Canada. The paper thus circulated contained 11 questions, and categorical replies were invited to them.

Replies were received and reported on at a Convention of the Society, held in Washington on May 15, 1882. The scheme submitted was generally and cordially approved.

An emphatic and unanimous opinion was expressed, that there should be established as early as possible a comprehensive system of Standard Time for North America.

[1] [NOTE.—I may remark, that the designation "Cosmic" was first suggested, independently, by two Canadian gentlemen widely separated, by Mr. R. G. Haliburton, then in Algiers, and by Mr. Thomas Hector, of Ottawa. The etymology commends the use of the word. It has been accepted by a number of societies and by many individuals as appropriate and applicable.]

Of those who replied to the queries, 95 per cent. favoured the idea that there should be a common agreement between the standards of time in all countries. That while we must primarily look to our own convenience on this continent, it is proper to aim at eventually attaining general uniformity among all nations.

Seventy-six per cent. were in favour of reducing the standards in North America so that they would differ only by intervals of one hour, and 92 per cent. were in favour of a notation of the hours of the day by a single series from 1 to 24, instead of in two divisions, each of 12 hours.

The character of the replies received indicated that a remarkable unanimity of opinion prevailed in every section of the continent heard from. The Convention accordingly resolved that an attempt should be made to obtain European concurrence to the selection of a Prime Meridian on which a time-system could be definitely based. But, failing to obtain this recognition, the people of the Western Continent should determine a zero-meridian for their own use and guidance.

It was thereupon resolved to petition the Congress of the United States to take the matter into consideration. The American Metrological Society about the same time adopted a similar proceeding. The consequences were that a joint-resolution of the House of Representatives and the Senate was passed, authorising the President of the United States to call an International Conference to fix on and recommend for universal adoption a common Prime Meridian to be used in the reckoning of longitude and in the regulation of time throughout the world.

On the meeting of the American Association for the Advancement of Science in Montreal, in July, 1882, the subject was brought forward, and all the documents were submitted and discussed. It was agreed that the Association should co-operate with other bodies in furtherance of the movement.

On two occasions the Royal Society of Canada has had its attention directed to the matter, and this body has assisted in furthering the determination of the problem by its co-operation and by correspondence with the Government.

While some delay took place in summoning the International Conference by the President, in consequence of diplomatic correspondence on the subject, the question was ripening on both sides of the Atlantic for concerted action. Indeed, a decision with respect to the regulation of local time was anticipated by the railway authorities in North America, who adopted the system of hour-standards which had been prominently brought forward as described.

On November 18 of last year (1883) the new system of regulating railway time on this continent came into operation. There had been several preliminary meetings of railway managers; the last meeting was a Convention held in Chicago the previous October, and it was then determined immediately to carry out the change.

Mr. W. F. Allen, the secretary of this Convention, who also took a prominent part in effecting the adoption of the change, has given a history of the events leading to it. Upon this gentleman mainly fell the labour of arranging details, and he executed the difficult duties assigned to him with consummate ability. In the words of the historian, the transition from the old to the new system "was put into effect without any appreciable jar, and without a single accident occurring." According to this authority the first newspaper to advocate some change was the *Railroad Gazette* for April 2, 1870, and it is claimed that as early as 1869 Prof. Charles F. Dowd, Principal of Temple Grove Ladies' Seminary, Saratoga Springs, proposed a system of meridians based on the meridian of Washington at intervals of one hour, by which railways should be operated, and that an expression of his views was placed in the hands of the President of the New York and Canada Railroad. The proposition appears to have attracted attention in the *Travellers' Official Guide* of 1872. In 1873 it was brought before the Railway Association of America, not now in existence. A committee was appointed to examine into its merits; they failed to recognise its necessity, and recommended that the question of National Standard Time for use on Railways be deferred till it more clearly appeared that the public interest called for it.

Mr. Dowd's efforts to introduce a National Standard Time to meet the difficulties which were being developed were at the time imperfectly appreciated. He, however, has had the satisfaction of seeing a scheme unanimously accepted, and put in operation, which in essential features does not materially differ from that which he advocated; and he himself attended at the meeting of the American Metrological Society, and took part in the proceedings when the details of the new time arrangements were officially narrated.

Prominent among those who have earnestly laboured to advance the movement of time-reform is the distinguished President of Columbia College, New York. Dr. Barnard has from the first taken the deepest interest in the question, and few men have done so much to bring it to a practical issue. In the proceedings of the American Metrological Society for 1881 will be found a paper prepared by Dr. Barnard in 1872, and presented to an association which has since assumed an international character, and is known as the Association for the Reform and Codification of the Laws of Nations. In this paper Dr. Barnard recommends the selection of Greenwich as the Prime Meridian for the world, and he submits the views he held at that early date, which at this hour are of peculiar interest. He points out that "it is becoming a matter of greater importance every day that there should be established some universal rule for defining the calendar day for all the world."

I have alluded to the valuable report of Prof. Cleveland Abbe, of the United States Signal Service, to the Metrological Society, and I cannot deny myself the pleasure of acknowledging the services of the gentlemen with whom I have been associated on the special committee on Standard Time of the American Society of Civil Engineers, Mr. Charles Paine, of New York; Mr. Theodore N. Ely, of Altoona, Pennsylvania; Mr. J. M. Toucey, of the Hudson River Railway; Prof. Hilgard, Coast Survey, Washington; Prof. T. Egleston, of Columbia College: General T. G. Ellis, of Hartford, now unfortunately deceased, and Mr. John Bogart, Secretary of the Society.

The American Society of Civil Engineers, since meeting in Montreal, in 1881, has made persistent and continuous efforts in the common interest to advance the movement of time-reform, having greatly aided in bringing about the important change carried into effect a year ago. This Society is now directing attention to a reform of scarcely less importance, the notation of the hours of the day. At the Buffalo convention in June 1884, this particular question received prominent consideration in the address of the President, as well as in the report of the special committee. Since that date a correspondence has taken place between the Secretary and the railway managers in the United States and Canada Already replies have been received from the representatives of some 60,000 miles of railway, 98 per cent. of whom have given expression to their sympathy with the movement, to abandon the old practice of halving the day, designating the two sets of 12 hours by the abbreviations A.M. and P.M., and are prepared to adopt a simple notation of 1 to 24 in a single series. The great telegraph interests of the country are likewise in full sympathy with it. The President of the Western Union Telegraph Company, Dr. Norwin Green, states that their telegraphic traffic is equal to the transmission of 44,000,000 messages a year, and the general adoption of the 24 o'clock system (as it has been designated), would be cordially welcomed by telegraphers. It would reduce materially the risk of errors, and to the company over which he presides, he says it would save the transmission by telegraph of at least 150,000,000 letters annually.

The branch literature bearing on the two questions of Universal Time and the establishment of a Prime Meridian, has been enriched by a series of papers which have appeared during the past year in the *International Standard*, a magazine published in Cleveland, Ohio. These papers are by the following gentlemen connected with the International Institute:—Rev. H. G. Wood, of Sharon, Pennsylvania; Prof. C. Piazzi-Smyth, Astronomer Royal for Scotland; Prof. John N. Stockwell, Astronomer, Cleveland; Mr. Jacob M. Clark, C.E., New York; Mr. William H. Searle, Pennsylvania; the late Abbé F. Moigno, Canon of St. Denis, Paris; Commodore Wm. B. Whiting, U. S. Navy; Mr. Charles Latimer, C.E., Cleveland; and others.

It will be seen from what I have submitted, that the proceedings have neither been few nor without success, and that since this Institute published the first issue of papers on Time and Time-reckoning, the subject has received much attention on both sides of the Atlantic. Societies with kindred pursuits, men of recognised merit in the scientific world, have turned to its examination and aided in its development. Some few men have acted in concert. The labours of others have been independent. Some of these names I have been able to record, but I fear that I neglect to include many of eminence because they are not known to me. It is this varied and widely diffused effort which has rendered possible the realisation of the practical results which I have the gratification to record, and all the members of this Society must equally join in the common satisfaction in the measure of success which has been achieved.

Six years back, when the subject was discussed in this hall, there were probably not a few who viewed the propositions then submitted as merely fanciful theories. Others, who did not refuse to recognise their bearing, entertained the feeling that many grave difficulties presented themselves to interfere with any successful attempt to reform or modify usages so ancient as the computation of time. But the Institute, as a body, was hopeful. The action taken by the Council to extend the field of discussion and awaken the attention of foreign communities, evinced confidence, and we may now ask, was this confidence justified? What are the facts to-day? Twelve months have passed since an important change in the notation of railway time was made with general approval throughout the length and breadth of North America; a revolution in the usages of 60,000,000 of people has been silently effected and with scarcely a trace that it has happened. That proceeding has been followed by events of equal importance. On October 1 last a body of accredited delegates from the different nations, on the invitation of the President of the United States, met in Conference to consider the problem first submitted to the world by this Institute. The delegates were the representatives of 25 civilised nations. The Conference continued during the whole month of October, and, as a body, they came to conclusions affecting all peoples living under our theories of civilisation.

It was early understood that a determination with respect to Universal Time was not possible without the general recognition of a Prime Meridian. Hence the importance attached to its choice, that it should be universally accepted.

## SOCIETIES AND ACADEMIES

### LONDON

**Royal Society**, December 10, 1885.—Abstract of a Paper on "Preliminary Results of a Comparison of Certain Simultaneous Fluctuations of the Declination at Kew and at Stonyhurst during 1883-84, as recorded by the Magnetographs." By the Rev. S. J. Perry, F.R.S., and Prof. B. Stewart, F.R.S.

The authors remark that such fluctuations almost always occur as couplets or groups of couplets; a couplet meaning first an ascent and then a descent, or the reverse. In their opinion this duality and the other facts of their paper can best be explained by supposing that a recorded magnetic fluctuation is the joint result of two causes: the one of these being a true magnetic change, and the other a secondary current caused by this change. The secondary current would probably appear as an earth-current. Its maximum strength would depend on the maximum rate of magnetic change, but as this last element is quite unknown, we may perhaps suppose that this maximum strength will be practically proportional to the mean rate of magnetic change.

On this supposition the authors suggest the following formula as capable of being used as a preliminary working hypothesis.

Let $K$ denote the observed Kew change, and $k$ the true magnetic change at Kew, also let $t$ represent the duration; then $K = k\left(1 \mp \frac{\alpha}{t}\right)$ where $\alpha$ is a constant. Also if $S$ represent the observed simultaneous Stonyhurst change, then $S = k\left(\beta \mp \frac{\gamma}{t}\right)$, the sign − being applicable to the first limb, and the sign + to the second limb of the couplet.

It follows from this that $\frac{S}{K} = \frac{\beta \mp \frac{\gamma}{t}}{1 \mp \frac{\alpha}{t}}$, or, in other words, that the ratio between the Kew and Stonyhurst observed disturbances will be a function of the duration, quite apart from all theoretical considerations, which can only in the meantime be regarded as pointing out a method of treatment. The authors then practically discuss their results, and have obtained the following preliminary conclusions:—

(1) $S$ is always greater than $K$, or the ratio $\frac{S}{K}$ is always greater than unity.

(2) This ratio appears to depend in some way on the duration of the disturbance;

(3) But not, as far as can be seen at present, upon its magnitude.

Finally, they hope to make a more extended investigation of the subject, going over a greater number of years, and perhaps adding to their methods of treatment.

"On the Limited Hydration of Ammonium Carbamate." By H. J. H. Fenton.

The hydration of ammonium carbamate affords an example of a chemical action of the simplest type, namely, the direct union of two simpler molecules to form one more complex—

$$CO(NH_2)(ONH_4) + OH_2 = CO(ONH_4)_2.$$

There are but few actions of this type which can be investigated when all the substances concerned are in the liquid state, and all extraneous matter absent.

In a former paper it was shown that ammonium carbamate, when acted upon by sodium *hypochlorite* in presence of sodium hydroxide, yields one half of its nitrogen in the free state, the other half remaining in the form of sodium carbamate—

$$2CO(NH_2)(ONH_4) + 3NaClO + 2NaOH = 2CO(NH_2)(ONa) + 3NaCl + 5OH_2 + N_2.$$

Sodium *hypobromite* at once decomposes sodium carbamate, yielding the nitrogen in the free state. This, in fact, is claimed as a specific reaction for carbamates, since no other substance yet investigated will yield free nitrogen by action of a hypobromite *after* the completed action of a hypochlorite.

Based upon this reaction, then, we have a direct and simple method of determining the amount of carbamate existing in a solution at any given time.

Experiments were conducted with a view of examining the influences of time, mass, and temperature upon the hydration of ammonium carbamate, and also the reverse action, namely, the dehydration of normal ammonium carbonate into ammonium carbamate. The hydration is expressed by the ratio—

$$\frac{\text{molecules of water assimilated}}{\text{molecules of carbamate taken}}.$$

(1) *Influence of Time.*—Solutions of ammonium carbamate of different strengths were examined at stated intervals. In all cases the action proceeds rapidly at first, becomes progressively slower, and finally reaches a limit short of complete hydration.

The time required to reach a determinate state of hydration is less as the relative number of water-molecules is greater.

(2) *Influence of Mass.*—The hydration is shown to be a function of the number of water molecules present. As far as the action could be legitimately studied, the minimum hydration corresponded to the case in which the substances are present in equal molecular proportions.

(3) *Influence of Temperature.*—The hydration is in all cases less, as the temperature is lower. Probably at a sufficiently low temperature the hydration would be practically *nil* when the substances are present in equal molecular proportions—*i.e.* ammonium carbamate and water would practically not combine at all.

(4) *Dehydration of Normal Ammonium Carbonate.*—It was shown that this salt undergoes dehydration in solution, becoming in part converted into carbamate. The dehydration is greater, as the relative number of water-molecules is less. It seems not unlikely that if the same relative number of molecules could be started with, the same equilibrium state between carbamate, carbonate, and water, would be arrived at for the same temperature, whether ammonium carbamate or normal ammonium carbonate were initially taken.

Since there is a tendency for normal ammonium carbonate to become in part dehydrated in aqueous solution, and for the system to come to a state of equilibrium where the carbamate and carbonate co-exist, it seems probable that the hydrolysis of urea under the action of ferments may be less simple than is usually represented. The author proposes to attack the problem by a method based on the actions of sodium hypochlorite and hypobromite, by means of which it is possible to detect, and quantitatively estimate, urea, carbamic acid, and ammonia, when all present in the same solution. A preliminary trial of the method gave satisfactory results.

**Geological Society**, December 16, 1885.—W. Carruthers, Vice-President, in the chair.—Charles John Alford, Samuel Blows, James Warne Chenhall, William Farnworth, Paget

Henry Cater Fulcher, and Harold Temple Wills, were elected Fellows of the Society.—The following communications were read:—Old sea-beaches at Teignmouth, Devon, by G. Wareing Ormerod, F.G.S. The author stated that while old records show that no important changes have taken place in the level of the Teignmouth district during the historical period, the excavations made in recent drainage-operations in the present year showed the existence of at least two series of beaches. The oldest sea-beach, which is a few feet above the present sea-level, was partly washed away and then covered up by later deposits exhibiting evidence, in a number of delicate bivalve shells in an unbroken condition, of having been deposited in a calm sea.—On the gabbros, dolerites, and basalts of Tertiary age in Scotland and Ireland, by Prof. John W. Judd, F.R.S. In previous papers published in 1874 and 1876, it has been demonstrated by the author that there exist in Scotland and in Hungary igneous rock-masses presenting the most perfectly crystalline characters, and belonging to the Tertiary period. It was further shown that such highly crystalline, plutonic rocks are seen passing insensibly into volcanic rocks of the same chemical composition—gabbros into basalts, diorites and quartz-diorites into andesites, and quartz-andesites and granites into rhyolites—the lavas in turn graduating into the perfectly vitreous types known as tachylites and obsidians. The present paper deals with the basic rocks of Western Scotland and Northern Ireland, which are shown to exhibit the most marked analogies with rocks of the same age in the Faroe Isles and Iceland; these facts lend strong support to the doctrine of the existence of petrographical provinces. The Tertiary age of the Scotch and Irish rocks is placed beyond dispute by the fact that they overlie unconformably the youngest members of the Cretaceous system, and are interbedded with stratified deposits of Lower Tertiary age. With regard to the nomenclature of these rocks, the identification of the more crystalline forms with the gabbros, which was made by Zirkel and Von Lasaulx, is supported; while the use of the term "dolerite" as a convenient one for the connecting links between the gabbros and basalts is advocated. Of the original minerals contained in these rocks, plagioclase felspar (ranging in composition from anorthite to labradorite), augite, olivine, and magnetite, are regarded as the essential ones; while enstatite, biotite, chromite, picotite, and titanoferrite are among the most frequently occurring accessories. It is shown, however, that these original minerals may belong to different periods of consolidation. The Secondary minerals are very numerous, including quartz, epidote, zoisite, hornblende, serpentine, and zeolites, with many other crystallised and uncrystallised substances. There are remarkable variations in the relative *proportions* of the original minerals in different examples of the rock; and by the complete disappearance of one or other of the constituents, the gabbros are sometimes found passing into picrites, eucrites, or troctolites. In their microscopic structure these rocks present many interesting features. From the highly crystalline gabbros there are two lines of descent to the vitreous tachylites: one through the *ophitic* dolerites and basalts, and the magma-basalts wtth skeleton-crystals; and the other through the *granulitic* dolerites and basalts, and the magma-basalts with granular microliths. The former are shown to result from the cooling down of molten masses which were in a state of perfect internal equilibrium, while the latter were formed when the mass was subject to movement and internal strain. It is shown that in the most deeply-seated of these rocks (gabbros) the whole of the iron-oxides combines with silica; but, as we approach the surface, the quantity of these oxides separating as magnetite increases, until it attains its maximum in the tachylites. In all the varieties the order of separation of the different minerals is shown not to depend solely on chemical causes, but to be influenced by the conditions under which the rocks have cooled down. Although these rocks are not highly-altered ones, yet they afford admirable opportunities of studying the incipient changes in their constituent minerals. The nature of these changes is discussed, and they are referred to the following causes:—(1) The corrosive action of the surrounding magma on the crystals; (2) the changes produced by solvents acting under pressure in the deep-seated masses (these have been already described under the name of "schillerisation"); (3) the action of heated water and gas escaping at the surface; (4) the action of atmospheric agents on the rocks when exposed by denudation; and (5) the changes induced by pressure during the great movements to which rock-masses are subjected.

**Physical Society,** December 12, 1885.—Prof. Guthrie, President, in the chair.—Mr. C. F. Casella and Prof. T. E. Thorpe were elected Members of the Society.—The following papers were read:—On a magneto-electric phenomenon, by Mr. G. H. Wyatt. The author had conducted a series of experiments with a view of testing experimentally an expression obtained by Mr. Boys for the throw of a copper disk suspended by a torsion-fibre between the poles of an electro-magnet, when the current was made or broken, and communicated by him to the Society on June 28, 1884. Disks of various metals and of various dimensions were used, the results being such as to agree with the theory within narrow limits. It was, however, found that when the throw of the disk was used to measure the magnetic field, the value obtained from the throw at break was uniformly greater than that obtained on making the current. Prof. S. P. Thompson observed that the case presented was analogous to that of the ballistic galvanometer, and that for the theory it was necessary that the magnetic field should be made and destroyed before the disk had moved sensibly. Mr. Boys believed that the results of the experiments showed this to be the case, since the result of such a movement would be to increase the throw on breaking the current when the disk made an angle of less than 45° with the lines of force, and to decrease it when the angle was between 45° and 90°, whereas no such variation from the theoretical result was observed.—On some thermodynamical relations, by Prof. William Ramsay and Dr. Sydney Young. In this paper experimental proof is given of the following relations:—(1) The amount of heat required to produce unit increase of volume in the passage from the liquid to the gaseous state, at the boiling-point under normal pressure, is approximately constant for all bodies. (2) If these amounts of heat be compared at different pressures, for any two bodies, then the ratio of the amount at the boiling-point under a pressure, $p_1$, to the amount at another pressure, $p_2$, is approximately constant. (3) The products of the absolute temperature into the rate of increase of pressure with rise of temperature are approximately the same for all stable substances. (4) The rate of increase of this product with rise of pressure is nearly the same for all stable substances. (5) A relation exists between the absolute temperatures of all bodies, solid or liquid, stable or dissociable, which may be expressed in the case of any two bodies by the equation

$$\frac{T_A}{T_B} = \frac{T'_A}{T'_B} + c(T'_A - T_A),$$

$T_A$ and $T_B$ being the absolute temperatures of the two bodies corresponding to any vapour-pressure; $T'_A$ and $T'_B$, absolute temperatures at any other pressure; and $c$, a constant which may be zero or a small positive or negative quantity. (6) The variations from constancy of the expression $t\frac{dp}{dt}$, though small, may be expressed by a similar equation. (7) If $L_A$, $L_A'$, $L_B$, and $L_B'$, represent similar relations of latent heat at different pressures, the same for $A$ and $B$, it appears probable that

$$\frac{L_A}{L_A'} = \frac{L_B}{L_B'} + c(T'_A - T_A).$$

(8) The ratio of the heats of vaporisation of any two bodies at the same pressure is approximately the same as that of their absolute temperatures at that pressure. The authors conjecture that this statement is also true of dissociating bodies. A large part of the experimental work consisted in obtaining the relation between vapour-pressure and temperature of different substances, values of $\frac{dp}{dt}$ had been obtained from these observations in two ways, by plotting curves with $t$ and $p$ as co-ordinates and drawing tangents, and by the method of differences. Prof. Perry suggested that the curve should be expressed in such a form as

$$\log p = \alpha - \frac{\beta}{t} - \frac{\gamma}{t^2},$$

which Rankine has shown to be a very true expression for the relation between pressure and temperature, and that $\frac{dp}{dt}$ should be obtained from this by differentiation. Prof. Guthrie hoped the authors would experiment upon the vapour-tensions of mixed liquids, a subject to which he had himself given some attention.

EDINBURGH

**Royal Society,** December 21, 1885.—Prof. Douglas Maclagan, Vice-President, in the chair.—Mr. J. Y. Buchanan, communicated a paper on the temperature of Loch Lomond and also one on oceanic islands and shoals.—Prof. Herdman discussed elaborately the phytogeny of *Tunicata*.—Mr. John Aitken gave a communication on dew, which will be found in full at p. 256.—Mr. Frank E. Beddard read a paper on the structure of *Lumbricus complanatus*, Dugès.—In a paper on the salinity of the water about the mouth of the Spey, Messrs. H. R. Mill and T. Morton Ritchie show that the sea-water slowly forces its way like a wedge between the river-water and the bottom as the tide rises, and dams back the water further up the stream, while the surface-water always remains quite fresh, and a brackish zone separates the two strata. When the ebb sets in the salt water runs out very rapidly, and before half ebb there is only fresh water inside the bar. The salinity of the water in Spey Bay was also studied. The river-water could be traced as a stream sweeping to the north-east, with a sharply-defined western margin. Alkalinity and temperature observations were also given.—Mr. A. Wynter Blyth discussed the distribution and significance of micro-organisms in water.

January 4, 1886.—The President submitted notes on the recent experiments at the South Foreland lighthouse.—Mr. Omond, of the Ben Nevis Observatory, communicated an account of the glories, halos, and coronæ observed there. The small number of glories seen is remarkable. Only four have been noticed since the Observatory was established.—Prof. Crum-Brown read a note on the simplest form of half-twist surface.—The Rev. T. P. Kirkman submitted a discussion of the linear section $PR$ of a knot $M_{n}$, which passes through two crossings, $P$ and $R$, which meets no edge, and which cuts away a $(3 + r)$-gonal mesh of $M_{n}$.—Messrs. Rainy, Ellis, and Clarkson gave an account of the exploration of the central portion of the field of a Helmholtz galvanometer.—In a paper on systems of colliding spheres, Prof. Tait showed that Maxwell's law of the distribution of energy between two different sets of molecules is erroneous. If two sets of molecules at a given temperature and pressure be mixed, the resultant temperature and pressure will be the same, but the average kinetic energy of the less massive molecules will exceed that of the more massive molecules. In the case of hydrogen and oxygen the excess will be 25 per cent.

### PARIS

**Academy of Sciences**, January 4.—M. Jurien de la Gravière, President, in the chair.—On the potential of two ellipsoids, by M. Laguerre.—Researches on the sulphur of antimony, by M. Berthelot. Here the author determines the measure of the heat of formation of this compound under its various conditions, that of its chlorides and oxichlorides having already been ascertained by MM. Thomsen and Guntz.—Remarks on Dr. A. Sprung's treatise on meteorology ("Lehrbuch der Meteorologie"), recently published at Hamburg, by M. Faye. The author's comments are confined chiefly to the vexed question of the ascending or descending movement of the air in whirlwinds or cyclones. Three points he considers now settled: (1) that the movement of translation is inexplicable according to the old theory; (2) that this rapid movement of translation corresponds with the upper cirrus-bearing currents; (3) that a descending movement cannot be denied within the cyclones themselves. Another step, and the old ascending will give place to the new descending theory.—Note on the differential invariants of M. Halphen, by Prof. Sylvester.—Note on the angular movement which a vessel takes on a wave of given size and velocity, by M. L. de Bussy.—Rectangular co-ordinates and ephemeris of Fabry's comet, by M. Gonnessiat.—Note on the new star in Orion, by M. Ch. Trépied. The magnitude of this star is 6·7, its colour an orange-red, and its spectrum very remarkable, showing six dark bands, two in the red and orange, four in the green and blue; bright lines have also been doubtfully detected in the green.—Note on the transformation of the Fuchsian functions, and on the reduction of the Abelian integrals, by M. H. Poincaré.—A tentative application of the calculus to the study of colour sensations, by M. R. Feret.—On the emetics of tellurium, by M. Daniel Klein. The author has succeeded in preparing some tartrotelluric emetics with the tellurites of the alkaline bases, which are alone soluble, and treating them with tartaric acid in due proportion.—On the transformation of the essence of turpentine to an active terpilene, by MM. G. Bouchardat and J. Lafont.—Note on the employment of the metallic oxides for the purpose of detecting in wines colouring substances derived from coals, by M. P. Cazeneuve.—Note on the cultivation of beet-root in the Wardrecques district, Pas-de-Calais, during the year 1885, by MM. Porion and Dehérain.—On the toxic action of the alkaline salts, by M. A. Richet. From a series of experiments made on fishes, pigeons, and guinea-pigs, the authors conclude that in absolute weight the metals are the less toxic the higher their atomic weight, which reverses the law formulated by Rabuteau; also, that the chlorides are, in absolute weight, more toxic than the bromides, and these than the iodides. But, with equal molecular weight, the reverse is the case. In general the alkaline salts are toxic through their chemical molecule, and the higher the weight of this molecule the more toxic it becomes, although the difference is slight and the molecule always about equally toxic.—On the circulation in the ganglionary cells, by M. Alb. Adamkiewicz.—On the morphology of the ovary in insects, by M. Armand Sabatier.—Note on the trunks of fossil fern-trees occurring in the Upper Carboniferous formations, by MM. B. Renault and R. Zeiller.—On the present value of the magnetic elements recorded at the Observatory of the Parc Saint-Maur.—Notes were presented by M. Ch. Beaugrand, on the meteoric dust collected in the atmosphere on November 27–30, 1885; by M. L. Sandras, on the modifications of the human voice by means of inhalations; and by M. Durif, on a remedy for diseased vines.—This number of the *Comptes rendus* contains a complete list of the members of the Academy on January 1, 1886, and announces the election of M. Gosselin as Vice-President for the current year.

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