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.

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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.:—

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 dist ibuting 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:—

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.

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