TRANSACTIONS

OF THE

Astronomical and Physical Society of Toronto,

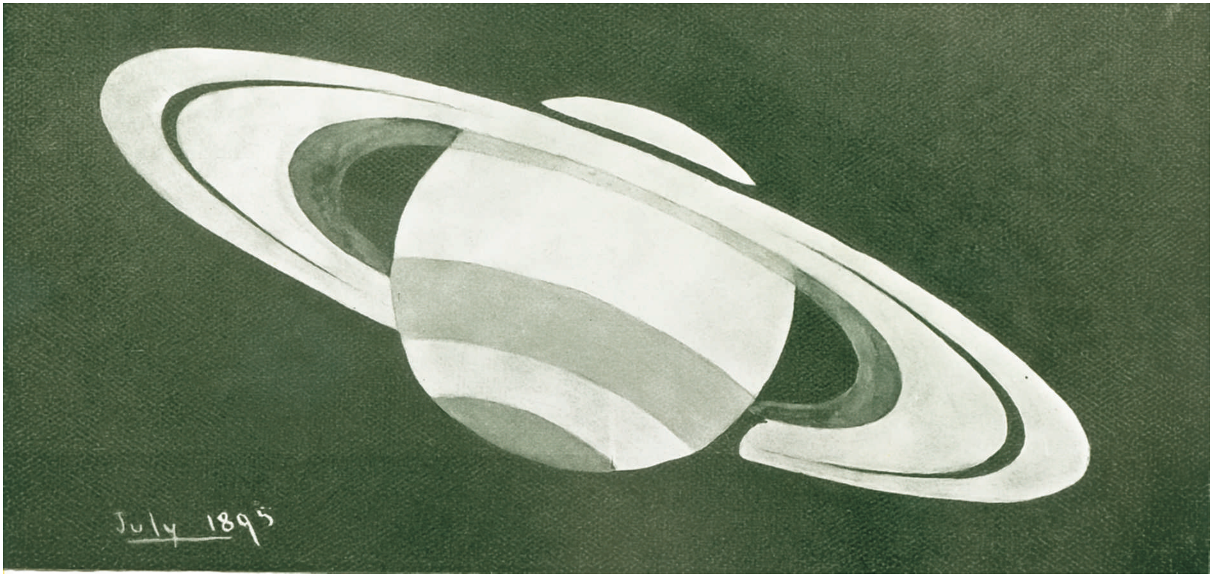
FOR THE YEAR 1895,

INCLUDING SIXTH ANNUAL REPORT.

PRICE ONE DOLLAR.

TORONTO:
ROWSELL & HUTCHISON,
Printers to the Society.

1896.



Saturn, as seen in Dr. J. J. Wadsworth's Reflector, Simcoe Ont.
Drawn by Miss Eva M. Brook.

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[Authors are alone responsible for views expressed in papers or abstracts of papers published in the *Transactions*.]

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TRANSACTIONS

OF

The Astronomical and Physical Society

OF TORONTO,

DURING THE YEAR 1895.

FIRST MEETING.

January 22nd; the President, Larratt W. Smith, D.C.L., Q.C., occupied the chair.

The meeting being called to order, Dr. Smith expressed his appreciation of the honour of re-election to the presidential chair, conferred upon him at the annual meeting, held January 8th. He regretted that he had been unavoidably absent on that occasion, when the various reports had given such excellent evidence of the Society's general prosperity and its activity in the scientific world. Although he had been absent from Toronto for a considerable part of the past year, he had continued to take the deepest interest in the work which the Society had undertaken; there was much to be proud of, and still much that might by united effort be attained. It had given him much pleasure to read the review of astronomical work throughout the world, which had formed the subject of the Vice-President's address at the annual meeting. Continuing, Dr. Smith referred to several matters of business which it would be the special province of the Council to consider, and suggested that the question of designing a suitable seal to be attached to the Society's official documents, and which it was necessary for a corporate body to adopt, be discussed as early as possible. The President concluded his remarks by saying that, however much he regretted that by reason of advancing years and failing strength, he was unable after the day's work was done to attend the evening meetings of the Society, as frequently as he wished, he could assure the members his heart was ever with them, and that, the Society might rely upon his warmest support in whatever might be undertaken, to promote the common objects they had in view —the great work of popularizing science in Canada. (Applause.)

The Secretary read an official communication from the Director of the Observatory of the Vatican at Rome, announcing the death of the Very Rev. Father Denza. Mr. Arthur Harvey addressed the meeting at some length in review of the life-work of the astronomer and physicist. It had been his privilege to enjoy a personal acquaintance with Father Denza, and he spoke in the highest terms of the genial personality of the distinguished priest, while all the world knew of the researches made by him in the domain of solar physics—researches most valuable, and which had thrown great light upon many important questions in the new astronomy. Shortly before his decease, Father Denza had given his assent to the proposed change in astronomical time reckoning. This, indeed, had been almost his last official act.

On motion of Mr. Harvey, seconded by Mr. G. E. Lumsden, a resolution was adopted conveying to the Roman College an expression of the Society's sympathy with the scientific world in the loss sustained by the death of Father Denza.

Mr. W. B. Musson reported the progress of the Library Committee in the work of preparing a descriptive index of the contents of the Library shelves; a full report would be presented in the near future.

The chairman then announced that papers had been prepared by several members on the general subject of "Star Clusters and Nebulæ," and called upon Mrs. A. G. Savigny, who read some notes on the telescopic study of the nebulæ, and the pleasure to be derived from it. Many of the objects within the power of a small instrument were described, as also the results of the labours expended upon the research by the great astronomers at large telescopes. Referring to the probable future of such objects as the great nebula in Andromeda, Mrs. Savigny quoted the opinion of Miss A. M. Clerke, "Even though stars without excep-ion have sprung from nebulæ, it does not follow that nebulæ without exception grow into stars;" instead of becoming ultimately a gigantic sun, a great nebula might be reasonably expected to form the beginning of what would be evolved into a cluster of stars.

Continuing, Mrs. Savigny said :—As we return to the exploration of the depths of space, sparkling with countless suns—countless, save to Him "who telleth the number of the stars and calleth them all by their names," we feel that some definite plan must have been formed in the Master Mind other than that these should merely lend additional light to us of Earth, or be a dazzling puzzle in an everlasting field of conjecture. Had the stars been created merely to beautify our firmament, we think that the Creator would have implanted in the minds of other than the few, the desire to look above and see "how the floor of heaven is thick inlaid with patines of bright gold." And as we still gaze upwards, fascinated and adoring, our being filled with wonder, mayhap a meteor falls, and the robe of exaltation falls from us, to give place to the material, as we remember it is even possible to touch with loving fingers the very clay abounding in those beauty spheres—familiar clay, reminding us of worlds made like ours, of beings such as we, perhaps sympathetic with our endeavours to understand the cosmogony of the universe

Let us take the curb from our imagination and revel in limitless space, where myriads of planets revolve, freighted with beings, perhaps attained to cosmic consciousness, that intellectual illumination of which Dr. Bucke tells us; knowing the plan and order of the universe; having telepathy with their neighbouring planets, mayhap now, themselves invisible, looking in upon us, with electric wings close pinioned, on which through space they've flown! We men and women of the Earth bury our minds in things of matter, learn too little of the stars, gaze but seldom into the sublime stellar depths. Men fetter their eagle spirits, which fain would soar, by grovelling for some petty place or aim. We women, weave our mental shrouds at pink luncheons, green teas or—blue dinners. Would that we might rather, in the words of a distinguished member of this Society, "search out new truths and endeavour to expand the field of knowledge."

Mr. T. Lindsay then read some notes on the origin of the nebulæ :—

In the telescope the nebula is a patch of white light; under inspection by proper physical apparatus it is a mass of glowing gas, and about that there seems to be no doubt whatever. There is field for the study of a life-time in determining data sufficient for a meagre catalogue of the nebulæ how long before we have data upon which to found a theory regarding their origin? If we could even outline the history of these mysterious bodies we would be a step nearer to the ultimate.

It is a mark of man's ambition that he has even tried to solve this problem, nor is there anything in the effort that would lead us to expect a stern rebuke for attempting to look so far beyond. We are still in the realm of phenomena, the origin of the nebulæ is a physical problem, and it seems under certain lights to be solvable, nay, one of our members has suggested an experiment even, by which something might be learned bearing most directly upon the question. When we allow our minds to dwell upon the picture of a vast mass of gaseous matter suspended in space, isolated, it certainly seems at first glance as if the simplest theory to account for its existence would be special creation, called into sudden existence by the power behind all law.

But there are very few who, giving the question a little thought, would accept this as a solution of the existence of the nebulæ. We feel somehow that such vast bodies must have been evolved from more simple forms, although we must ultimately arrive at a form so simple that it could of necessity only have been either specially created or co-existent with eternity, whatever meaning we give to that expression.

In the effort to trace back from the nebulæ, or, indeed, in any effort to explain phenomena, we want to traverse the shortest, least encumbered route. The moment we have a complication we may reject the whole evidence, for it cannot be true. Nature's method is a synonym for simplicity. The nebulæ, then, are waiting to be interrogated. They will answer any question put to them, therefore man's ingenuity is to be employed in asking the proper questions. One has been answered, and the message is brought to us from the nebulæ that they are composed very largely of hydrogen gas; and it is the self-same hydrogen that we have around us and experiment with in the laboratory. Distant hundreds of years of light travel, there is the same hydrogen; and truly it does seem as if there was once a connection between all the bodies in the universe. Either that or the matter composing them has been evolved from a simpler form, which is all pervading. So far as we can guess at the thing there does not; seem to be any room for a third explanation.

Either all the bodies in the universe were once connected as one mass, filling eternity, we may so express it, or a process of evolution has been going on in different parts of the universe, but with the same results, because the same primal form was everywhere. It may be said that there is no way in which either of those propositions can be proved, therefore it is a waste of time to consider them. Suppose that to be true, it has not deterred and will not deter philosophers from keeping on trying. It would be a pity, indeed, if what is truly within the domain of physics, though far down we confess, should be abandoned, and time for discussion freely given to the metaphysicians who have for thousands of years been trying to move the world without a place to stand upon, trying to embrace phenomena while in the vortex of phenomena.

The student of nature will try to trace the origin of the nebulæ. The theory of the late Dr. Croll was that a nebula is the resulting body after the collision of two suns in space. Whether this be true or not it still leaves us to account for the existence of the suns, so that the problem resolves itself into this: whence came the matter composing all the bodies that people space?

This is certainly a stupendous problem, yet an answer to it may be gathered from chemical philosophy, though no one will be in any way dogmatic about it. Thus, as the laws of nature are indisputably from the will of one law-giver, and as these laws within our experience, may be treated as a unit, namely motion, it seems to follow that matter to which motion is imparted is a unit; that no form of matter known in chemistry is an elementary, but that all forms are compounds of one. It does not matter what we call this one, all we can do is to give it a name; we cannot bring it within the range of our experience, but we believe it exists, and is the medium pervading the interstellar spaces, the same medium by which or through which energy acts.

Mr. Arthur Harvey followed with some remarks upon the destiny of the nebulæ :—

In "Nature" of February 14th last, is an important paper by Mr. Norman Lockyer, on this subject of nebulæ, which is now attracting so much attention, and I am naturally delighted to find the weight of that great name added to the theory of the functions of meteorites, and the formation therefrom of stars, which I brought forward at a meeting of the Canadian Institute in January, and alluded to more lately and perhaps more slightly here. We must all remember our Mr. Elvins' paper on the probable feeding of the Sun's fires by meteorites encountered on his way through space. I do not think, however, that Mr. Elvins, though he seems to have considered the universe as full of meteorites, carried his views so far as mine, which are, and I proved them by their number, non-periodicity, and varying inclinations to the plane of the Earth's orbit, that space is full of these bodies, moving in all directions. They must have mutual attractions, and while some of these comets (for I regard them all as such) come within the Sun's influence, and may be absorbed or dissipated, others go on from star to star and may become mighty aggregations, the nuclei of new stars. Prof. Lockyer in his list of new views of nebulæ expresses it thus, in his 8th claim: "Space is a cosmical plenum."

I stated in my paper that the reason we could not see these aerolites before they came within the range of the Earth's attraction was because of their small size; they did not reflect enough light to be visible, but I added that the condensation of any considerable aggregation would reflect heat and light enough to make it visible to us as a comet, while as to very large and distant bodies, the condensation upon the solid nucleus which must have been forming would give independent light enough to make them visible as stars. I found this a reasonable method by which new stars may be formed, or, as I have expressed it, the mode in which stars are born into the family. The next problem is how they die. Now Prof. Lockyer in his second claim agrees with me and says: "The first stage in the development of cosmical bodies is not a mass of hot gas, but a swarm of cold meteorites." He appears, however, to say that light and heat are not developed until there is a collision of swarms.

Spectroscopic evidence must decide the point. I see as yet no ground for thinking that nebulæ are in process of condensation or dispersion, or any necessity for supposing two swarms of meteorites to have clashed to make a *nova*; once light up the nucleus—once bring the condensation to the point at which light would be emitted, and the heat would rapidly permeate the whole of a single swarm, and cause it to flash into brilliancy. The particles on the farther side of the nucleus would be hurrying towards it, and towards us; those on the hither side would be hurrying towards it and from us. Hence may arise the diversion of the spectroscopic lines.

SECOND MEETING.

February 5th; Mr. Arthur Harvey, F.R.S.C., in the chair.

A communication was read from the Superintendent of the Nautical Almanac Office, Washington, covering the transmission of copies of the almanac, for which the thanks of the Society were due. A letter was a1so read from Mr. David E. Hadden of Alta, Iowa, referring to the very general interest being taken in popular science.

Mr. G. G. Pursey reported his observations of the Sun during January; he was of opinion that the maximum was not yet passed. On one occasion there had been a belt of spots from the east to the west limb of the disc. Mr. Elvins called attention to the prediction made some time since that auroral displays would be frequent during January and February. So far this had not been fulfilled. Several members reported having observed a very beautiful paraselene on the evening of January 31st.

Mr. T. Lindsay referred to a question which had been asked on several occasions regarding the phenomena of Jupiter's satellites, it was well known that the eclipses, occultations and transits of these bodies are given in our astronomical almanacs, but it is not stated what kind of a telescope the observer is supposed to use. There will be found, however, in the appendix to the earlier volumes of the Greenwich Nautical Almanac a full account of the best method of observing, and it is stated that the predictions are given for observation with a three-inch achromatic telescope. This, it was thought, had never been altered, and should be remembered by those wishing to verify prediction by observation.

The chairman read some notes on meteoric showers, and presented for inspection a fragment of the now historic Winnebago meteor which he had obtained through the kindness of Dr. Gustave Hahn. The stone is a typical chondrite, porous, crust rather thin, opaque black, not shining. Interior colour is grey, spotted with brown. On one broken surface there was a scum of a black substance which would mark paper, probably graphite. The metal in it is in exceedingly small globules and thin flakes, but contributes to the mass 45 per cent. of its weight. The metal consists of 93 per cent. iron, 6 per cent. nickel, and 1 per cent. cobalt. This stone fell in the daytime, and commenting upon this point, Mr. Harvey showed that in considering meteoric showers we may readily settle the direction in which they are moving when we remember that if showers are met in the daytime they must be coming from the Sun, while if they fall at night the direction must be towards the Sun, the darkened hemisphere of the Earth being in the path. The Winnebago meteor had passed perihelion some weeks before the Earth met the general shower, and was, therefore, in the outgoing part of its orbit.

THIRD MEETING.

February 19th; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

Mr. T. W. Gibson, of the Bureau of Mines, Toronto, was elected an active member of the Society.

A communication was read from John Hoskin, LL.D., Q.C., thanking the Society, in the name of the relatives of the late President, Mr. Chas. Carpmael, for the engrossed copy of the Society's testimonial.

The librarian reported the receipt of two very old and rare volumes on natural science, donated by Mr. G. H. Robinson, to whom the thanks of the Society were due.

Several members had been interested in observing Mercury at the recent elongation. Miss A. A. Gray had found no difficulty in getting several views of the planet without optical aid, and described also its appearance as observed in a three-inch telescope. In reporting some observations of the Sun's surface, Mr. G. G. Pursey introduced the much debated question as to whether the spots are elevations or depressions. Mr. A. F. Miller called attention to a very simple experiment tending to prove that a sun-spot is not an elevation. He stated that if a black mark be made upon a white globe, and the apparatus viewed at some distance, in the telescope, a "notched" appearance will in every case be the result. But as a matter of fact a notch on the Sun's limb is extremely rare, notwithstanding that so many observers have most carefully noted the coming round of a spot by the rotation of the Sun. Mr. Miller held, that if the spots were indeed elevations above the Sun's photosphere, then in all cases the "notch" would be seen.

Mr. Arthur Harvey read, by special request, a popular account of the work recently accomplished by Lord Raleigh and Prof. Ramsay, which had resulted in the discovery of argon, the new constituent of atmospheric air. The paper included a brief but instructive sketch of the work of all the great chemists, since the time of Cavendish, and enabled those who had not read the details of the work done, to very thoroughly grasp the state of chemical science in the present day. Mr. Harvey was requested to favour the Society still further with a popular review of such work as might be done by the co-discoverers of argon.

FOURTH MEETING.

March 5th; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

The assistant-secretary and editor laid upon the table copies of the *Transactions* for 1894. These were distributed to the members. The frontispiece to the volume was an excellent likeness of the late President. The report of the Earth Current Committee was appended, giving the details of the disturbances noted at the Canso, Nova Scotia, station of the Commercial Cable Company.

Mr. W. B. Musson read some extracts from press reports forwarded by Mr. J. Todhunter, relating to parhelia and related phenomena. Among these was an account of a lunar rainbow seen in January on the Isthmus of Panama, and which, it was stated, was the first seen there since the 16th century. Some discussion following, Mr. Harvey said that the account was probably correct, as such phenomena would be extremely rare in the tropics.

Dr. J. C. Donaldson, writing from Fergus, reported having observed Mercury without optical aid on five evenings during February, namely, the 4th, 5th, 11th, 14th and 16th. It was quite easy on the first three occasions, more difficult on the 14th, and required considerable attention on the 16th. Mr. G. E. Lumsden referred to the above as an excellent record of naked eye observations of the planet.

Mr. A. F. Miller then read a paper on

THE SPECTRA OF THE NEBULÆ,

of which the following is the full text :—

Among celestial objects which earliest attracted the notice of telescopic observers, it is not surprising to find star clusters holding a prominent place; for, however serious the defects of the astronomical telescope in its primitive forms, its immensely exalted light-grasp, as compared with the unaided eye, gave a peculiar charm and significance to those investigations conducted with its aid, whereby the numerous whiteish cloud-like streams and patches, known for ages as fixed markings upon the celestial vault, but hitherto unexplained, or their nature at best but vaguely conjectured, were now triumphantly resolved into stellar groups. Such surprising results naturally led to the inference that the irresolvable nebulous masses, which by the use of small telescopes were being discovered in great abundance, required only an increase of optical power to appear also as clusters of distinct suns. A long interval elapsed, however, during which little advance in this direction was made, till at length the era of large telescopes dawned, with the great Herschel as its most zealous apostle. This famous astronomer was a devout believer in the resolvability of nebulæ. His powerful reflectors gauged many of their secret recesses; and his chief reason for aiming at the construction of yet more colossal instruments is traceable to the confidence he felt that by their means still fainter groups would be resolved; since in each nebulous mass he was persuaded he saw a galaxy, differing only because of its remoteness, from the star-girdle of the nearer heavens,—the familiar Milky Way. From his day forward others shared his faith and trod the self-same path towards the goal he had sought; till the efforts at space-penetration and light-grasp culminated in the erection of Lord Rosse's giant reflector. The talented designer of this famous engine of research was as satisfied as had been his predecessors that the mode in which he treated the problem of the stellar depths was the right one; and when more than forty years ago he announced that nebula after nebula was yielding up its secret to the penetrating gaze of his great mirror, who was to gainsay the dictum of the astronomer owning and employing so assiduously the greatest (and, of course, in common belief the best), telescope on earth? Even the great nebula of Orion was deemed resolvable; all below the trapezium Rosse thought he saw as a mass of stars. Theorists, whose fancy had beheld in nebulæ a fiery mist, the matrix from which suns and their systems might be evolved, were silenced; and scientific research, linking hands with pious credulity, gazed in a sort of calm stupidity upon results of creative power, untroubled by the vain, or perhaps impious, desire to fathom the methods by which these results bad been achieved. When, therefore, in August, 1864, Dr. Huggins directed a telespectroscope upon the nebula H4374 and found its light to emanate from glowing gaseous constituents, it was but natural so unexpected a result should be received with incredulity or distrust. This, however, little troubled the great spectroscopist, who hastened to procure confirmatory evidence from other nebulæ and his observations were joyously hailed and rapidly extended by Vogel and other investigators. As is usual with Dr. Huggins's work, from the very outset he did every thing in the most thorough manner. Not merely did he discover the gaseous nature of the nebulæ, but went on to identify the elementary character and physical condition of the gaseous constituents. His instrument was, almost necessarily, of his own design, with a dispersive power of two prisms ; and using the comparison method he detected with certainty the coincidence of two hydrogen lines ; while the brighter nebular line was referred, in default of better agreement, to a closely adjacent green ray in the complex nitrogen spectrum. The latter conclusion was, of course, erroneous, since, so far as our present knowledge extends, nitrogen is *not* a constituent of the nebulæ. But Dr. Huggins was himself the first to recognize and correct an error into which he had very naturally been betrayed by the inadequacy of his instrumental means; and his subsequent conclusion, reached after he had equipped himself with instruments of greater dispersion, has held good to the present hour, despite much opposition and effort to prove the contrary; viz., that while in the gaseous nebulæ the hydrogen series is fully represented, the chief nebular line (λ 5007·05) and a series of other lines clearly associated with it, originate in matter unknown to terrestrial chemistry. *

Space forbids more than the merest reference to the interesting researches of Huggins, while engaged in investigations on the origin of the unknown nebular lines. The displacement of the chief one, as compared with the nitrogen line, was for a while referred to motion of the nebulæ in the line of sight; but this theory was soon abandoned as unsatisfactory; and necessarily so, since the hydrogen lines also seen, were in their normal positions. Then, too, *all* the nebulæ observed could hardly have the same proper motion. It must suffice to say that none of the important points in the chain of reasoning appear to have escaped the notice of this careful investigator.

Of course not *all* nebulous-looking celestial objects give discontinuous spectra. Many are truly what Herschel and Rosse believed them to be, *star-clusters*, and the prismatic analysis of their light only confirms the honest judgment of these great telescopic observers.

Other nebulæ again are in part gaseous; and still others, while

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* This was written before Crookes had made public his determination of the wave-lengths of the chief lines in the "blue" spectrum of argon. The exact coincidence of the principal nebular line with a line measured by Crookes, as well as the close resemblance in position of other lines in both spectra, formed the subject of a note read before the Society on a subsequent occasion.

seemingly not gaseous at all, are as surely not genuinely stellar. Such a case may be instanced in the great Andromeda nebula : Its curiously curtailed spectrum, destitute of, or much reduced in, the red extremity, has long been a spectroscopic enigma; the doubtful maxima which some observers declare they have recognized, being hesitatingly accepted, or even denied by others. The stellar nucleus, which in 1885 shone forth through the depths of the mass, shared all the prismatic singularity of its nebulous surroundings (as I am glad to say I was able to notice at the time), and at present, the highest authorities are disinclined to advance hypotheses as to the composition or character of this singular, though not unique, object. For neither in spectral peculiarity nor in temporary stellar outburst does it stand alone: The small nebula 80 M situated in Scorpio, has a similar prismatic constitution, and, like its great congener, was the seat of a luminous outbreak (in 1860), closely resembling that which disturbed the great Andromedean vortex, twenty-five years later.

Not merely have we learned the composition of nebulæ by aid of the prism, but the same means has availed for their discovery. Prof. Pickering, employing a spectroscopic ocular, has detected quite a number of planetary nebulæ otherwise quite indistinguishable from stellar points. He was led to this mode of sweeping by observing that in several instances such bodies had been recognized on the plates of the Draper Memorial, owing to their having recorded themselves thereon in discontinuous lines instead of the elongated spectra characteristic of true stars. Dr. Copeland also has successfully employed the same method.

When astronomers all the world over, ever since the time of Huggins's memorable discovery, have bestowed great and increasing attention upon nebular spectroscopy, it is not surprising to learn that, spite of all the difficulties inseparable from such work, many lines have been recorded. The latest results, tabulated by Dr. Scheiner, give no less than seventy-five lines, the positions of which have been accurately determined. Photography has largely supplemented visual observation in these researches; and to this fact is traceable, in very great measure, the remarkable success just mentioned. Nor can we notice without sincere pleasure that the venerable pioneer of nebular spectroscopy is still leading in this new direction. The photographs of nebular spectra, taken as many as thirteen years ago by Dr. Huggins and his gifted wife, are among the best that have yet been secured ; while their determinations of wave-length based upon their negatives or on visual observations, are everywhere accepted with that confidence to which the well-known accuracy of the observers entitles them.

One of the most important discoveries yet made in the spectroscopy of gaseous nebulæ was achieved by Dr. Ralph Copeland in 1887. I refer to the identification of the line D_3, λ 5876 as a nebular constituent. Dr. Copeland, who was then working at Dun Echt with a new spectroscope of specially suitable construction, noticed in the spectrum of the Orion nebula a line slightly more refrangible than the characteristic sodium pair. His inference that the ray in question was coincident with that known as the helium line, received speedy confirmation; and is now a well established fact. This valuable research has supplied an important link in the chain binding together the nebulæ, gas-stars, bright-line stars, and temporary stars; and points towards a possible kinship of such peculiar bodies with the chromospheric envelopes of more fully developed, and therefore (from our standpoint), more normal suns; thus suggesting a direction along which the rather doubtful course of stellar evolution might be assumed to travel. I hasten to add that the important observation just cited has been repeated in the case of several other nebulæ; and the line D_3 is now always sought in these spectra by investigators having sufficiently powerful appliances. Copeland has also noticed a considerable amount of continuous spectrum in the central visual region of the prismatic band. This is of interest too, as it clearly indicates the formation of solidifying particles, or perhaps masses, amid the vast expanse of highly attenuated nebulous material.

Having spoken at such length of the nebular spectral lines I venture here to insert the positions of the chief ones in the visual region. The wave-lengths have been determined by Keeler, and are based on Rowland's scale:

Name of Line.	λ in $\mu\,\mu$.	Remarks.
C ($H\alpha$)	6563·05	Rarely seen ; only three recorded instances:—The Orion nebula *not* one.
D_3 (Helium)	5876·	Invisible or faint in all but very large instruments.
Chief Nebular line.	5007·05	
Nebular line	4959·02	
F ($H\beta$)	4861·05	
($H\gamma$)	4340·66	Extremely faint in all but large instruments.

At this juncture, I will just mention, that, owing probably to the essays of sensational writers on science, some errors relative to the origin of the chief nebular line have insinuated themselves into a few older works purporting to deal with astronomical spectroscopy. In this way we sometimes meet with the statement that the chief nebular line is coincident with the solar coronal line commonly known as 1474 K ; with a bright line in the aurora, and with a suppositious line in the spectrum of the zodiacal light. These statements, which almost certainly originated in the manner indicated, had no true observational basis whatever, and are consequently not entitled to serious regard. The most careful determinations of the wave lengths of the chief nebular line place it, as I have said, at λ 5007·05 on Rowland's scale. With equal certainty the coronal line 1474 K is known to lie at λ 5316·89 ; this is really a triple line, its most refrangible member having coincidence with a line of iron, while the others are of unknown origin. The chief auroral line has a well determined position at λ 557·1. As for the zodiacal light, as all careful observers are well aware, its spectrum is a faint and very partial *continuous* one, having no bright lines whatever. Before quitting the subject of the auroral spectrum, I briefly remind you that the researches of Vogel appear to identify all its rays with lines in the air-spectrum.

I have trespassed on your time, perhaps unnecessarily, in pointing out the errors to which I have just referred. Curiously, an error in connection with nebular spectroscopy led to the most important discovery which has yet been made in that branch of astrophysics. With a brief notice of this I will close my paper. You are all doubtless familiar with the theoretical principles of Lockyer's meteoric hypothesis. The well known English physicist has for many years assiduously laboured to propagate a cosmical theory, partially original, but also largely derived from sources to which he has not always accorded whatever credit may be their due in this connection. The theoretical part has been supplemented by a very great amount of fine experimental and observational work of his own; and also by no small percentage of bold assertion. To state the position very briefly: Everything celestial and terrestrial was originally of meteoric origin; comets, nebulæ and even stars, were now, or had been at some period of their evolutionary development, made up of meteorites, more or less closely associated; and this, we were taught, obviated all difficulties and explained the most incomprehensible phenomena. True, the explanation seemed itself to require explanation;

but that was a mere detail. Clashing meteoric stones could be made to account for everything ; the very chemical elements melted with fervent heat under this furious bombardment, and, yielding up the ghost, appeared to the daring physicist, dissociated, and in the truly *spectral* guise of helium, of coronium, or of the perilous stuff nebulæ are made of. As links in a chain of reasoning, too often oracularly vague, what could better suit than the vague and ill-defined nebulæ? They contain hydrogen, and so do meteorites. Iron, a prominent meteoric constituent, is unrepresented by even one of its all but countless lines; but the case of magnesium seemed more hopeful. True, with the electric spark, or arc spectrum nothing could be proven; their evidence was all against theory; but the flame of burning magnesium, or of magnesium salts, yielded more pliable results; and the scientific world was duly apprised of the complete and triumphant coincidence of the nebular line and a band characterized by the somewhat poetical designation of "the last magnesium fluting." These results had been arrived at by experiments conducted with means scarcely adequate to settle a fundamental question, and physicists did not feel justified in according them that hearty acceptance to which, in the opinion of their originator, they were entitled. A few, indeed, were bold enough to deny flatly the asserted coincidence; and more or less scientific bitterness seemed likely to follow. At this juncture, Dr. Huggins, whose results had entirely differed from those obtained by Lockyer, fortunately invoked the aid of Professor James Keeler, the Spectroscopist in the Lick Observatory, and referred to his judgment the whole question at issue. Acceding with graceful alacrity to the request of his great English confrère, Prof. Keeler, in 1890 and 1891, carried out an investigation no less remarkable for the refinement of its scientific accuracy than for the singular, and in many respects the unexpected, conclusions to which it led. I cannot detain you while describing Prof. Keeler's elaborate study of nebular spectra, which the vast light-grasp of the great Lick refractor permitted him in many cases to investigate by the aid of the diffraction spectrometer, a form of instrument never before employed in work of this kind. He was speedily satisfied that the nebular lines had no relationship whatever with the magnesium fluting, and proved conclusively that Lockyer's observations were vitiated by grave errors which rendered confidence in them impossible. But as the absolute positions of the chief nebular lines were of fundamental importance, he proceeded to determine these with the most extreme accuracy;

and while thus engaged, reached the surprising result that the wave-lengths as obtained by the diffraction spectrometer (the most exact of all methods), had slight but perfectly obvious differences in the case of different objects. Cleared from all displacements due to terrestrial motions and motion of the solar system in the direction of the apex of the Sun's way, (effects which had to be carefully considered and allowed for in an investigation so delicate), there was still an outstanding displacement affecting not one line only, but *all* the lines in each spectrum, and in equal proportion. The conclusion was inevitable, nor did Prof. Keeler hesitate one moment in assigning it to its proper cause. The shift was due to *motion of the nebulæ in the line of sight*. By means of many observations on nebulæ in widely different regions of the heavens, which of course might, and must reasonably be assumed to be, moving in all possible directions as regards the observer, the mean wave-lengths or scale positions of the chief nebular lines were determined; thus furnishing datum-points from which displacements in any particular case might be measured, and the velocity of the corresponding motion inferred. Subsequently, comparisons with the fixed lines of hydrogen were employed to confirm the results of the other method. From the displacements thus observed, a number of velocities have been computed, but I will only trouble you with two or three results as instances of the rest. The velocity is stated in kilometres, and the – and + signs imply respectively motions of approach and of recession.

Orion;	+ 17·7
N.G.C. 6790;	+ 48·4
G.C. 4628;	– 49·7

NOTE. – A kilometre = 1000 metres, = $\frac{31}{50}$ mile.

In the case of an object so prodigious as the Orion nebula must unquestionably be, it might reasonably be asked whether motion in the line of sight detected in one region was shared by other portions of the vast structure. This point was not overlooked by Prof. Keeler while conducting his research on the position and character of the lines; and with a view of solving the problem which such a question presents, he made a number of careful measurements designed to detect changes of wave-length clue to this cause. The results, however, were purely negative, as it was found that, except from a very limited area, the nebular light was so faint as to be lost in the process of dispersion. Curiously enough, the characteristic spectrum was obtained undiminished from the seemingly dark abyss surrounding the trapezium, thus indicating that contrast rather than reality explains the familiar telescopic appearance of that region.

A similar search was made for shifting of the lines obtained from opposite sides of several planetary nebulæ the object being to detect movements of rotation. These experiments were also fruitless, though their successful accomplishment within the near future should by no means be regarded as either impossible or improbable.

The spectral characteristics of the stars involved in the great nebula have received no small share of attention, it being expected that thus the question of their actual position in space might be settled. Till quite recently the balance of evidence seemed to favour the view that these bodies were really placed within the nebulous matter; the researches of Huggins, Vogel and others indicating that the spectra of the trapezium stars, at least, contained groups of bright lines coincident with groups in the nebular spectrum. This view, while not unreasonable, involves the difficulty that the components of the multiple θ Orionis must be assigned to a spectral class of their own, since no other truly stellar bodies on record show the nebular lines; though the star-like nuclei of planetary nebulæ quite frequently do. Keeler, in his earlier observations, believed he saw the nebular lines brightest where they crossed the star-spectra; but subsequently be reached the conclusion that the involved stars had spectra with *dark*, hot bright, lines; and thus, so far as his results extended, they were not necessarily situated within the nebula at all. He adds, however, that many of the bright lines of the gaseous matter seemed to be coincident with dark stellar lines, and this counterparting appeared to affect not merely the included stars, but other and quite distant ones in the same constellation, where, indeed, similarity of spectral peculiarity has long been thought significant. The photographic evidence obtained by Huggins, Vogel and Schemer, should not be too readily discredited in favour of visual observation; and at any time the earlier conclusion may be confirmed with unanswerable assurance.

And now you may not unnaturally ask me what is the portion of the humble amateur telescopist in investigations and researches of so difficult a character that they tax to the uttermost the skill and patience of the ablest trained observers of Europe and the United States, though armed with the most powerful and perfect instruments the world has yet seen

I answer that if he cannot *repeat* the observations, he may and should *understand* them ; and his lot, however lowly, will be brightened and cheered by the thought that an Almighty Power is working everywhere, even upon the confines of the universe, so far as we can judge of them, according to laws which are present and in operation here on this little terrestrial speck where his lot is cast. He thus learns to regard calmly waning years, decreasing bodily energy, or the loss of opportunity, as he views the genesis of universes of suns, and in the truest sense sees life and immortality brought to light. But to fully appreciate that of which he hears or reads, he should behold for himself some of these wonders; for despite much that may be urged to the contrary, seeing and believing will always be closely associated in the human mind. Having then adapted a simple spectrum ocular to his telescope, he readily notes the nebular line in such a familiar object as the Ring-nebula in Lyra, or another of the same class. But for larger nebulæ a slit-spectroscope becomes necessary, together with a certain degree of skill in its use. Follow me then in fancy for a few moments while, as an amateur, I show you by description, a few of the wonders an amateur's small instruments are capable of revealing. Suppose you stand beside me in my small observatory on a clear cold winter's night, such as best suits for viewing the Orion nebula. The observers should be warmly clad, but the hands are uncovered despite the frosty and nipping air, for as a cat with mittens catches no mice, similarly a gloved spectroscopist catches few lines. A small telespectroscope is adjusted to the eye end of the telescope, the slit being carefully set parallel to the diurnal motion, and the focus adjusted for greatest distinctness in the region of the green and blue. The finder serves to bring the object upon the slit, which already has been placed by mechanical adjustment in the principal focus of the objective. Bringing the eye to the ocular, at once three beautiful gas-lines shine out; the brightest, and the fainter one which lies close to its more refrangible edge, are the nebula lines at λ 5007 and λ 4959, and are of unknown origin; the blue-green line at λ 4861 is $H\beta$, corresponding with the solar line F., and is the line chiefly employed for comparison experiments by the visual method. $H\gamma$ is invisible in so small an instrument. Some glimpses of Copeland's continuous spectrum are also caught as a delicate glow intertwined about the bright discontinuous maxima. But now what beautiful sparkling rays are these, which, shooting up, cross the gaseous spectrum at right angles? They are the continuous spectra of the trapezium stars :— The stellar images, being points, are elongated by the prism-system into true geometrical lines, in which the eye can distinguish only the terminal colours, but no spectral details; though such exist, and have been brought out in the photographic plates of Hugging and Vogel, who find numerous fine bright lines close to the widened star spectra, while similar fine lines are incorporated therein. A cylindrical lens over the ocular, though it spreads out the stellar streaks reveals nothing of these delicate rays, which indeed, have only been seen by means of large instruments, or on the photographic plate; and with our modest appliances our expectations must be proportionally moderate. A spectrum ocular would probably show bright lines if such exist in these stars, were it not that the series of nebular images in light of the chief nebular lines overlap and hide every thing else. Withdrawing the cylindrical lens we again see the crossing lines held steady by the driving-clock and the observer's practiced hand. A hydrogen tube before the reflecting prism sends in comparison rays, which, properly subdued, do not obliterate the nebulous lines, and a coincidence with Hβ is noted. A faint sodium flame gives an idea of the location of D_3 but here our optical power again proves inadequate; and possibly the line itself is variable in brightness. The complex nitrogen spectrum has, as Huggins early felt, nothing in common with the other visible lines. The observation is finished so far as our means will allow; and I fancy I hear you breathe a sigh of relief as we cap our small object glass and hasten to the fireside. Yet if our frames be somewhat chilled, our hearts are warmed with enthusiasm and renewed admiration for the grandest branch of the grandest science. Our minds are refreshed, and for a while toil and care are forgotten. However sceptical, we cannot look upon the still progressing work of the Great Architect of the universe without admiration and awe. As if by some subtle response of our inmost souls to an invisible power, our lips unbidden repeat the poetry of ancient days, old but still new, true to-day as thousands of years ago, sublime as any utterance the ear of man has ever heard :—" The heavens declare the glory of God, and the firmament showeth his handy-work. Day unto day uttereth speech, and night unto night showeth knowledge. There is no speech or language where their voice is not heard. Their *line* is gone out through all the earth and their words to the end of the world."

FIFTH MEETING.

March 19th; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

Mrs. J. Fletcher, of Toronto, was elected an active member of the Society.

Communications were read from Dr. J. Morrison, Dr. M. A. Veeder, and Mr. S. W. Burnham.

Several members and correspondents had forwarded notes of observations made on the occasion of

THE LUNAR ECLIPSE, MARCH 10.

The chairman reported the work done at the Wilson telescope, and, referring to the distinctness with which the Moon could be seen when entirely in the shadow, quoted several authorities as to the conditions which might result in the total disappearance of the Moon. "If the region of the atmosphere through which the solar rays pass be everywhere deeply loaded with vapours, the red rays will be almost totally absorbed, as well as the blue, and the illumination of the Moon will be too feeble to render her surface visible." Mr. P. G. Ross stated that this view had been accepted by many well-known observers.

Mr. Thos. Lindsay stated that on the occasion of the eclipse a small party had assembled on the grounds adjoining the *Mail and Empire* building, and had enjoyed witnessing the phenomenon in Mr. James Foster's three-inch Bardou telescope. Times of immersions of several lunar features were noted accurately, and also occultations of some faint stars in the Moon's path. The very general interest taken was very encouraging to those who had endeavoured to bring before the popular mind the fact that celestial phenomena were worthy of observation and study.

Mr. Arthur Harvey, who presented a coloured drawing of the eclipsed Moon, said :—" The eclipse was punctually on time, though no eye could see to the minute when the Moon swept into the penumbra, which has no definite edge. As this light shade covered the *maria* it caused a smoky look, which prepared the observer for the appearance of the shadow proper. This was at first of the colour of soot, a simple blackness, enough to blot out the lunar features, though some of the bright spots were visible where the gloom had completely covered them.

When about a third of the surface was covered by the shadow proper, I was called away, and only returned to my post when totality had set in. A more wonderfully beautiful sight could not be imagined. The upper edge of the Moon was of a beautiful blue, below and on each side was the loveliest tint of rose, then followed a yellow, a copper colour, the darkest parts of which were bronze. Though several eclipses recur to memory, none so beautiful is among them. The colours of the sunset were in a manner reproduced, which leads me to think that the causes of the unusual colour were in the cold upper air."

Rev. C. H. Shortt, M.A., referring to the beauty of the Moon's appearance, thought that some of the colours might be due to contrast, rather than to a physical cause.

Miss A. A. Gray and Mr. C. T. Gilbert reported having observed several occultations of faint stars in the Moon's path during totality. Similar observations were reported by members of the Meaford Astronomical Society.

Mr. A. F. Miller reported his spectroscopic observations with small telespectroscope and four-inch refractor :—

The Moon's spectrum was very bright and distinct, being visible from beyond A to beyond H_2. The lines in the red were very faint; the lines most distinctly seen being those in the green, blue and violet. On shifting the slit to fall on the darkened part, strong, general and selective absorption was evident. The violet end disappeared, and the blue was shadowed and rendered faint. The darker red region also became very faint, while from A to F many dark bands appeared, evidently telluric absorption lines When the slit was placed on the darkest smoke-coloured regions of the disc, the spectrum became very faint and contracted in length. In the green illuminated region the centre of spectrum was brightest. It was possible to compare the spectrum of the obscured region with that of the still illuminated portion, which showed the shadowing and the absorption bands fairly in comparison; but owing to the ill-defined edge of the shadow and its rapid motion, the comparison spectra could not be maintained long side by side.

Mr. G. E. Lumsden presented the following notes :—

March 10th, 1895. The total eclipse of the Moon this evening was well observed. The sky, which during the earlier half of the day, had been cloudy with snow-falls, cleared off towards night and permitted all the phases of the eclipse to be observed in a manner which left nothing to be desired, if one may omit reference to the temperature, which was steadily falling and, during the night hours, reached the zero point. Observations were made with the naked eye, opera glass, a two-inch telescope, and a pocket spectroscope. So far as could be detected, the penumbral eclipse of the Moon was so slight as to be incapable of estimation; possibly the only standard of comparison, an impossible one, would be the full Moon itself shining under normal conditions. From the commencement of the total phase, interest continually deepened. The actual instant of beginning could not be definitely decided, but once the darkening of the Moon's limb was noted, every minute seemed to add to the area undergoing eclipse. As the shadow wore on, the lunar areas most affected appeared to be those depressed regions usually called "seas." The higher levels took on and preserved throughout a bright copper hue, which, in places, somewhat resembled the metal when burnished. One most persistent feature was the brilliant bright line which bounded the limb where first it entered the Earth's shadow. The contrast between this sharply defined line and the neighbouring portions of the lunar landscape was, perhaps, more perceptible to the naked eye than in the telescope. Indeed, the beauty of the eclipse suffered nothing by naked eye observation, with the possible exception afforded by the lovely tints which formed the terminator or arc of the advancing shadow. Though strongly suffused by greenish and bluish tones, there seemed to be present many of the scintillations of the opal, due probably to the frost particles floating in the upper atmosphere. As the eclipse became total, and the Moon was shorn of her light, surrounding stars, hitherto invisible, appeared in the telescope; of these, seven or eight were in the telescopic field and close to the Moon. Though most of the last mentioned were about to be occulted, they are not found in the almanacs, so that it may be assumed they were of magnitudes less than of the sixth. When completely in the shadow, the phenomenon presented by the full round lunar orb, still easily visible, surrounded by bright stars, and itself marked by variegated tints, was something not to be forgotten. The effect was greatly heightened by the entire absence of moonlight and, consequently, of the shadows on the Earth to which one is accustomed. About 9.50, as the neighbouring stars were beginning to be perceptible in the telescope, my attention was caught by a bright point of light which seemed to be hanging centrally upon the western limb of the Moon. After some seconds' observation, this object began to flicker.

It then seemed to enter on the lunar disc, only to disappear instantaneously. I could not explain this until some minutes later, when the same object was re-observed, this time a little further down the limb. The mystery was now explained by the fact that a star had passed behind the edge of the Moon. About the same moment that this star was occulted, I noticed what seemed to be a light a little to the right and below the centre of the Moon, as inverted in the telescope. This stellar point was very puzzling, and, for want of higher magnifying power, was difficult to identify on the disc, though reference was immediately had to maps. The object seemed to be situated exactly on the point of the tongue of darkened landscape projecting northwards into Sinus Æstuum, in the vicinity of Eratosthenes. This bright point was observed steadily for some minutes. Presently, however, it became unsteady and began to disappear and to re-appear at longer and still longer intervals. Finally, it flickered and went out almost at the moment of total eclipse. I have been at a loss to account for this, but of one thing I am confident, and that is a light was there.

Among other reports of general interest, Mr. Lumsden read some notes upon a

PARHELION, MARCH 14,

which he had observed at 6 p.m., and which, though rather poorly defined, was noteworthy because of the very peculiar cloud formation prevailing at the time. While the extreme northern and southern portions. of the sky were filled with confused and broken cloud-masses, there was a clear belt, some thirty or forty degrees in width, extending from the westward, where the Sun was setting, immediately over the observer's head, to the eastern horizon. Stretched throughout along the centre of this, belt, but diametrically across it, lay a series of very similar convoluted forms at, apparently, equal distances from each other. These torpedo-shaped, cylindrical clouds, for they were invariably thickest in the middle and tapered gradually to more or less blunted ends, pointed north and south. Those from the point overhead and towards the western horizon, were most perfect in shape and were peculiarly beautiful and graceful, lighted up as they were around their edges by the setting Sun, which suffused them with warm grey tones deepening to ruddy tints along their outlines. Some of the prettiest effects were to be noticed in those clouds which had a rolled-up appearance, as if they had been the sport of the wind, which, at the time of observation, was only a gentle breeze.

In a few of them might be seen convolutions, rendering them not unlike objects formed of wrappings. These clouds were noticed by many passers on the street and, by some of them, compared to a numerous fleet of large white torpedo boats lying side by side, at regular intervals, and anchored in mid-air. Mr. Lumsden counted fifty-eight of these forms. In half an hour most of them had yielded to the effects of the breeze, and, beside drifting southerly, had become more or less ragged along their outlines and, in some cases, broken up. Two or three, however, preserved their first observed shape for a few minutes longer. Miss Gray said she had observed the clouds, and with others, had been much interested in their striking appearance. Mr. R. F. Stupart, Director of the Observatory, stated that the clouds had been described to him by several persons who had seen them and were curious to know how they had been caused.

A paper on

EARTHQUAKE AND VOLCANIC PHENOMENA

was read by Mr. A. Elvins. The following is a brief synopsis :—It was held that the Sun's energy is the great source of terrestrial heat, and that as the Earth's axial rotation and annual revolution cause all portions of the surface to be presented alternately to the Sun, the solar energy is distributed pretty evenly over it; also that the immense amount of solar energy converted into heat at the Earth's surface keeps the temperature from lowering as a whole, so that the heat of the Earth may remain constant or possibly may increase. Mr. Elvins referred to the fact that different geological strata expand differently with the same degree of heat; sandstone, for instance, expanding much more than marble. The strata are in some cases of great thickness, and the weight must be enormous. As a result of this difference of expansion, the friction produces great heat at the junction of two strata. The expansion of a stratum when covering a large area causes crumpling of the rock beds, and mountains are the result.

The greater expansion of an underlying stratum sometimes ruptures the less expansive rock lying on the surface, causing fissures, through which water sometimes finds its way to the heated rock beneath, where it is converted into steam. Explosions occur, causing earthquake shocks, and sometimes the fused matter and steam force a vent, and a volcano is the result. Mr. Elvins contended that solar energy converted into heat at the Earth's surface would not be wholly radiated into space, and that *heating up*, instead of *cooling off*, is what ought to be expected.

He remarked that "we never see a roasting fowl get cooler whilst it is turning on a spit in front of a fire," nor should we expect the Earth to cool off whilst it rotated on its axis, turning its different parts regularly sunward. The solar heat, combined with the heat produced by the fall of meteoric matter on the Earth, and the impact of ether waves on every side of it, is, in Mr. Elvins' opinion, causing a rise in the Earth's temperature, and the consequent expansion of its rocky strata causes earthquakes and volcanic action.

SIXTH MEETING.

April 2nd; Mr. A. Elvins in the chair.

Mr. J. F. Parkyn, of Toronto, was elected an active member of the Society.

On motion of Mr. G. E. Lumsden, seconded by Rev. C. H. Shortt, M.A., the Right Reverend J. A. Newnham, D.D., of Moose Factory, Bishop of the Anglican Diocese of Moosonee, was duly elected a corresponding member of the Society.

Mr. Lumsden said it had been his privilege, in another place, to hear Dr. Newnham describe the meteorological conditions which, especially in the winter season, prevail in the neighbourhood of St. James' Bay. These descriptions included accounts of auroræ, some of which had been rather remarkable. Not infrequently, displays lasting through several nights are witnessed, the supposition being that they really are active day and night during the time, but, as suitable instruments for detecting evidence of this are not yet in the country, the fact cannot be established. As a rule, the auroræ belong to the ordinary type, the whitish glow being, however, intensified to a degree far in excess of that to which we, in southern Canada, are accustomed. The light, which is said to be at times, quite equal to that of the full Moon, is often very steady, a feature of which it is His Lordship's intention to take advantage during the coming winter, to ascertain whether the landscape can be photographed by its means. At times, the skies are made gorgeous by the splendidly hued fan-like streamers, or rays, and by curtains with golden draperies whirling, or in rapid undulatory motion, and by tints, more or less strongly defined, of blue, yellow, green, white, and red, everywhere apparent over the violently agitated heavens. Though the prospects of success are not assured, it is the Bishop's intention, as special occasion offers, to expose some very rapid dry-plates for the purpose of ascertaining whether any of the auroral effects can be reproduced by photography. Dr. Newnham had more than once heard the crepitating or rustling sound associated by some observers with brilliant displays, and described it as being quite distinct at times. The intensity of the auroræ reported, is confirmatory of Humboldt, who, during an aurora witnessed by him, on the 7th of January, 1831, was able to read printed characters, and of Bravais who states that in Finmark he could read by the auroral light "a page of small print almost as easily as by the light of the full Moon." The Bishop evinced great interest in the work of this Society, and promised to send to it, from time to time, an account of such noteworthy observations as he was able to make during his residence at Moose Factory and during his journeyings through his vast Diocese in the performance of the duties of his office.

Mr. G. G. Pursey presented a series of drawings of the telescopic appearance of the Sun, extending over a period of three months. One conclusion to which he was led by his observations, which extended altogether over some twelve months, during which careful notes had been taken, was that in all cases when a spot is about to come over the disc it is preceded by faculæ. So regularly had this been observed that it had been found possible to predict the appearance of the spots regardless of the date of return by rotation.

Mr. Arthur Harvey drew attention to the recent earthquakes in Sicily and the southern parts of Italy, which had followed at no great interval the earthquakes in Greece and at Constantinople. He thought it not unlikely that this volcanic activity might be the precursor, within a few years at most, of a more important seismic disturbance there, and indicated the concurrence of a very low barometer, and a new or full Moon as the time to expect the same.

Mr. J. Van Sommer then read the following

NOTES ON AURORAL DISPLAYS : ARE THEY DUE TO SOLAR RADIAL ELECTRIC CURRENTS?

There are some features of the aurora with which we are not at present sufficiently acquainted, but which are mere matters of enquiry and correspondence; accurate knowledge of those things which we can find out would probably lead us in the right direction towards the solution of the problem of this, one of nature's most attractive displays. Every one is ready to watch that soundless movement of rapidly scintillating fire from an unknown source in the northern heavens.

Some of the questions are :—(1) As in the northern regions where the Sun is below the horizon in winter an aurora is seen any time in the twenty-four hours, do our magnetic needles show us any signs of auroral display in the day time, when by reason of the light they would otherwise pass unobserved ? (2) When we see an aurora at any certain longitude west of Greenwich, is there one to be seen at the corresponding longitude east ? (3) If not, how many degrees will the appearance compass? (4) Also, if not, will an aurora follow the night around the northern hemisphere, or, if the light moves round the pole, which way does it travel, east or west? (5) When there is an aurora at the north is there one seen at the south pole at the same time? These five questions are very simple, and admit of an early solution, but on the answers (especially to No. 2 and 5), depend to a large extent any theory we may be building up.

It has been suggested that the auroral light is seen over large tracts of land containing mineral deposits, which may be several degrees south of the north pole. If we find that at the same time the display was visible on the corresponding and opposite side of the pole, we can safely say that the layers of mineral in New York state, as I have seen it suggested, or in our own North-West, as another writer has proposed, had nothing to do with producing the display. If we further find that there is a correspondence in time between a display at the north and one in the south, I cannot see that the question as to the origin of the phenomena can remain any longer open to controversy as to whether it is due to a magnetic current or not, but that its appearance at the two poles at the same time, if not always but sometimes, would be sufficient proof that it was magnetic—and further still that it was due to a magnetic current passing through the Earth—and further still, that if there was this current passing through the Earth, the terminals of the current must be somewhere outside our globe.

First of all I ask your admission to what seems to me to be simply a scientific truism, namely, that if these currents are passing through our Earth from pole to pole their terminals are outside our globe. This seems a simple admission, but once admit the theory, once admit what we can almost call a self-evident fact, and you launch out on a course of investigation which, I believe, will be new, and fraught with much interest, namely, where are the terminals of this current passing through our Earth? Where do they go, or from where do they come; on what great electric line are we, with what undiscovered country does this ethereal cable connect us, where is the all-powerful dynamo that for ever keeps this line of unknown length in use? I will say here that this paper is more for the purpose of helping on to discovery, than of bringing positive proof; facts I will try to bring before you, the *deductions* I leave entirely to yourselves.

During a display the magnetic needle of a compass is uncertain and the telegraph wires are weak. It is, as we might say, that the electric "tension" for the time being becomes relaxed, the current is either diminished, or, like the tide in a river, is flowing in against the stream.

With a small piece of steel you can deflect the needle of a compass a certain number of points east or west, but take away the counter attraction and the needle quickly returns; but during an electric disturbance, with which an aurora is generally in connection, and which is always present during an aurora, the needle can be turned much more, and its return is slow and uncertain, or rather inexact.

The idea has been expressed that during an aurora there is a rush of electricity upward from our Earth,—in fact, it is described as if the northern magnetic circle or pole was in a state of magnetic ebullition, or giving forth electricity. If this were so, the natural result to be looked for would be, that the electric tension, as we have called it, would be stronger, in a state of abnormal activity. We find, however, that this is not so; on the contrary, it is confused, so we do not see that there is any addition to the strength of these streams of electricity through our Earth, during an aurora.

The normal state of the electric stream is probably the usual rapid flashing stream, and its direction northward and onward.

Observers believe they have established the fact that the aurora is more or less periodical—about every seventeen days or twenty-three days. What is periodical is always dependent on outside causes, so if the aurora is periodical it would lead us without any doubts to look for the cause off the globe.

I throw myself on the mercy of the Society, which I know they are ready to accord to all suggestions, however wild, and venture to make the following suggestion as to the cause of auroral displays; it is this :—

That there is a stream of electricity passing through our Earth from pole to pole, originating from some mighty outside influence, and that the immediate cause of the aurora is some intervening body along this electric circle, as for instance a nebulous meteoric cloud, or shower, circling perhaps around us outside the Moon, or between us and the Sun, where we know they do exist; or some other body that deflects, or causes counter attraction to our magnetic current, and that while this temporary break, or partial divergence of our current is taking place, the consequent scintillation of the electric current is seen in the rarified space at our poles, due to this disturbance along the electric circuit.

The arc seems the natural line of galvanic electricity, the direct line is the line of magnetic attraction. If you place a card on a horseshoe magnet and sprinkle iron filings on it, and gently tap the card, so as to make the filings move, they arrange themselves in direct lines between pole and pole, and also in radial lines on either side forming circles between pole and pole; it is these radial lines that I wish you to study. You might find that they possess different qualities.

By the kindness of the University authorities I have lately made some experiments with these radial lines.

If you take a vacuum glass of some size and place it on the plate of an exhaust pump, and have a covered rod inserted through the top, attaching the wires from a coil to the rod and plate, when the current is turned on there will be a diffused line of the electric light from the rod to the plate. But if you insert the end of the rod in a glass cup, lined with tin foil or half filled with water, when the current is turned on the direct rays seem to be obstructed, while the outer curved radial lines will rise from the glass cup and form a cascade of electric streams to the plate below. The very form of the lines proves that these outside streams of electric energy do not seek the shortest route from point to point, but that their direction of movement is a curve or circle. If the current is reversed, the electric fluid does no longer rise in the form of a cascade, but clings close to the glass, and in one case, when it was left for some time, the streams had formed two distinct lines north and south. Why this difference some of our enterprising members might study; at present I can think of no explanation.

An electric current, then, through space would be an arc; all movement in space is in a circle. Once admit this idea to your mind, of an electric current passing through space, and through our Earth, and we immediately think of the other end of this vast electric arc. Now we know that if you revolve a steel wheel round an iron axle, but isolated from it, you start an electric current through the axle at right angles to the motion of the wheel, and if the wheel is formed of magnets the axle will revolve as we see in the electric motors. The current would probably take the form of an arc as soon as it escaped from the ends of the axle.

If we had means to show it, we should probably find the radial curves forming a semi-circle from one end of the axle to the other. One end is negative and the other positive, and there must be union of the electric current before we obtain any effect.

The Sun is the centre of terrestrial revolution, and, knowing as we do that it is impossible to have movement without starting an electric current, or to have fire either, as that also will immediately produce electricity, the amount of electric energy produced by the production of heat in the Sun, its rapid revolution, and our rapid movement, must produce wonderful currents of the electric force in the space around us. The motion would be at right angles to the Sun's equator, that is from the poles or axle of the Sun. As the light streams to us from the Sun, as the force of attraction reaches us in a direct line from the Sun, is it not possible that these great galvanic circles also reach us, and the other planets as well? Might not these great cables be the means by which we receive the effect of the storms or other disturbances in the Sun? Might not their use be to hold us, as it were, in the plane of the Sun's equator, as if between the points of an immense pair of tongs ?

One further point and I have finished. In vacuum tubes, and in the particular kind known as Crookes' tubes, there is shown the electric light passing from point to point, and in some of them a small piece of metal is delicately balanced between thin wire points, and this the current will cause to revolve. The special point to mark here is that the motion cannot be due to magnetic force, but to some other motive power in the electric stream. This motive power was the object of my experiments at the University; but in the large bell-glass, with the apparatus I used, this motion did not show, though it doubtless exists. To illustrate this motion is another point that I would commend to the attention of those who have the time to give to it, especially for this reason, that if we are in a magnetic circle with the Sun, and the current is a motive power, it must have a direct effect on the motion of the Earth, perhaps regulate it or be the cause of it.

The three conclusions which these theories seem to lead to are:

That the Sun is the centre of electric currents, extending in circles at right angles to its equator, and reaching to the planets;

That they are either a direct cause, or are regulators of, our motion and position ;

And that these currents are the cause of the auroral light seen at the two magnetic poles of our Earth.

SEVENTH MEETING.

April 16th; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

Communications were read from Lieutenant Winterhalter, Naval Observatory, Washington, and from Dr. François Terby, Louvain, Belgium.

Miss A. A. Gray read a letter received from the pupils of the senior class of Wellesley school, asking for the pleasure of an evening at the telescope. Several members agreed to assist Miss Gray and place their instruments at the service of the school on the first suitable occasion.

It was moved by Mr. A. Elvins, seconded by Mr. A. F. Miller:—*Resolved*, That this Society deeply sympathizes with its esteemed fellow-members, Mr. G. G. and Miss Jeane Pursey in the great loss which they have sustained by the death of a wife and mother, and wishes to express the hope that they will be supported by that infinite power, intelligence and love which governs the universe and doeth all things well.

In moving the resolution, Mr. Elvins referred to his long friendship with Mr. Pursey and his family, and to the very high regard in which they had been held by all who were privileged to enjoy their acquaintance. Mr. Miller, in seconding the resolution, referred to the many very pleasant meetings which had been held, in the early days of the Society's existence, at Mr. Pursey's house, and to the kindly hospitality of Mrs. Pursey, who had always taken very great pleasure in making welcome those who met together for recreative study. The Chairman, in tending the sympathy of the Society to Mr. Pursey, paid a high tribute to the Christian character of his deceased wife, and to her zeal in the Christian work in which she had been long engaged.

Dr. J. C. Donaldson, of Fergus, on the subject of double-star observations, wrote :—" On the evening of April 10th I observed with my 3½-inch the *comes* to λ Geminorum. I found this star sufficiently difficult for my eye with the full aperture of my glass, although it is given in a list of stars at page 281 of vol. I., *Popular Astronomy*, as a space penetrating test for a 3-inch glass. In the fourth edition of *Webb's Celestial Objects*, the author adds in his comments 'not seen.' I think it will require a pretty fair 3-inch telescope to show the *comes* to λ Geminorum."

The Secretary read the following communication, received from Mr. W. N. Greenwood :—

GLASSON DOCK, LANCASTER, ENGLAND, 5th April, 1895.

The Secretary, Astronomical and Physical Society, Toronto, Canada:

DEAR SIR,—Seeing in one of our scientific journals a few words to the effect that the French Bureau des Longitudes had given in its adhesion to the unification of civil and astronomical time, in response to a circular issued by a joint Committee of the Canadian Institute and your Society, will you kindly give me confirmation of this fact. I thought possibly the journal of your *Transactions* for 1894 might give me the information, but I have not succeeded in obtaining a copy in this country.

The unification of the above times and the 24-hour method of recording the time has been matter of much interest to me during the last ten years, and in my humble way I have done what I could to make the adoption of both methods an accomplished fact; but I do not claim much for my success, though I have adopted the system throughout in my Tide Table and Nautical Almanac during the whole of the ten years in question. I forward you a copy of the book with my compliments; please to accept it; if only to show what has been done for some time past to make the uniform time more popular on this side the water, it may be of interest to you. I wish I could say that the success had been marked, but it is not so, though some time since when I proposed to change the system, finding it telling against the sale of the work itself, I was informed, in some quarters at least, the change would be resented. This was on the part of the foreign going users only; the home customers wished it the other way. Seeing, therefore, as I said, the note as to the action of the French Bureau, I was much interested in it, for possibly it brings us a step, and not a small one, nearer the time when the civil and astronomical and 24-hour day will be the same all over the civilized world.

Trusting you will excuse the liberty I take and oblige me with the information asked, or, if more convenient to you and to be found therein, a copy of your journal of *Transactions*.

I am, dear sir, yours faithfully,

W. Nelson Greenwood,

F. Imp. Inst.; F. R. Met. Soc.; etc.

The Secretary was requested to reply to Mr. Greenwood, forwarding a copy of the Society's Transactions and expressing the hope that Mr. Greenwood would favour the Society with a paper on the general subject of the unification of time at his earliest convenience.

Mr. A. F. Miller read the following notes on the coincidence of some of the spectral lines of argon with lines in the spectra of the gaseous nebulæ :—

"Prof. Crookes, F.R.S., having published his determinations of the wave-lengths of thirty-nine lines in the blue spectrum and twenty-four lines in the red spectrum of argon, I compared the positions thus derived with the positions of about seventy-five nebular lines tabulated by Prof. Dr. Schemer, as follows :—

Blue Argon. λ	Red Argon. λ	Nebulæ.		Remarks.
500·7		5007·05	(The chief nebular line.)	5 nebulæ.
	518·58	5183		
496·55		4959·01	(Second nebular line.)	5 "
439·95		4390		3 "
436·9		4363·8		5 "
425·95	425·95	4265		2 "
416·45		4167		1 "
415·95	415·95	4154		1 "
410·5		4101·85		5 "
394·85		3959		1 "
386·85		3868		4 "
376·6		3768		1 "
372·98		3727		1 "
12	3			
5				

In cases where the coincidence is very close, though not exact, comparisons of relative intensities of the lines would be of value. Crookes gives the intensity, but this point is wanting in the table compiled by Scheiner. However, the complete coincidence of the chief nebular line and the very near coincidence of the second nebular line, are, to say the least, highly significant. It must also be remembered that Professor Ramsay has published a brief note that he has found associated with argon (in a mineral termed cleveite) another gas, the spectrum of which is a line at λ 5876, coincident with D_3, or the characteristic line of helium.

Only one line in the red argon spectrum approximates in position to a nebular line, not already matched in the blue argon spectrum. Taking into account the difference of physical condition under which the blue and red spectra are produced, the result can hardly but be considered as a significant indication of the existence of a higher temperature in the nebulæ than has been hitherto assumed. The blue argon spectrum is produced by a very intense and hot spark of relatively high E. M. F. and negative in origin; the red argon spectrum is produced by a positive spark of considerably lower intensity and temperature.

Mr. Arthur Harvey then read, by request, a popular account of the recent discovery of helium, to which Mr. Miller had referred, reviewing the work of Prof. Ramsay and Mr. William Crookes, which had resulted in the identification of the hitherto hypothetical element in the upper atmosphere of the Sun. The work of M. Berthelot also was briefly reviewed, and an account given of the experiments which led him to conjecture that the enigma of the aurora may be solved by considering it to be a fluorescence produced by argon or its congeners, under the influence of electrical discharges developed in the atmosphere under certain physical conditions.

It was announced that on the first favourable occasion for observation a special meeting would be held for the purpose of testing in actual work an eight-inch reflecting telescope which Messrs. J. R. and Z. M. Collins had recently constructed.

EIGHTH MEETING.

April 30th; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

Mr. G. E. Lumsden was appointed to act as delegate representing the Astronomical and Physical Society at the annual meeting of the Royal Society of Canada, to be held in Ottawa, May 15th, and to present a report of work done during the year 1894.

A letter from Rev. W. G. Hanna, Ph.D., of Uxbridge, was received; it was decided to forward to Dr. Hanna the publications of the Society thus far issued, and to request him to consider whether a branch of the Society could be established in his locality.

The Librarian reported that he had received from the Astronomical Society of the Pacific, to whom sincere thanks were due, several back numbers of its publications, thus completing all the volumes to date.

Mr. A. F. Miller read the following notes of observations :—

April 28th, 1895, 9 *h*.

Micrometrical:—Measures of large oval-shaped spot in group nearing west limb, Sun's north hemisphere.

Umbra	=	$1{\cdot}67^t$	=	$26{\cdot}66''$	=	12,100 miles.
Penumbra	=	$3{\cdot}493^t$	=	$55{\cdot}76''$	=	25,500 "
Projecting tongue in Umbra	=	$0{\cdot}415^t$	=	$63{\cdot}3''$	=	3,100 "

Spectroscopic :—Group of bright prowinences seen at from P. A. 50° to 88° = 38° of the Sun's limb = say $\frac{1}{9}$ of circumference, 4 principal forms and several smaller. The largest was arborescent, another pillar-like, and one a pointed flame. Heights 20,000 to 28,000. The lines of H and D_3 were very bright.

Mr. Arthur Harvey read the following notes on

THE BEHAVIOUR OF MINERALS AT VERY HIGH TEMPERATURES.

Profiting by the kindness of the owners of a Barton electrical furnace here, I have made a few experiments which bear on the effect of the application of intense heat to the surface of specimens of minerals resembling in some respects the materials of which aerolites consist. Some of these bodies, as you are aware, are almost altogether composed of nickeliferous iron, others have small particles of such iron disseminated through olivine, augite and the like, thirty to forty per cent. of their mass being silica, a fourth as much alumina and a little magnesia. I took among others a specimen of quartz (silica) with small crystals of iron pyrites thickly scattered through it, about half the mass being quartz and half pyrites.

The principle of this furnace is that the electrical impulse proceeds through water to the negative pole, and an arc is formed where the mineral at this negative pole is brought to the surface of the water. Between the water and the mineral there is consequently an intensely heated layer of vapor of water, or, perhaps, of the electrolysed gases which are its constituents. In other words, the mineral is surrounded by an intensely heated gaseous envelope, which in a flash melts its surface; a condition which somewhat resembles that which obtains when a meteorite with a velocity of thirty or forty miles a second dishes into the tenuous upper air and incandesces by friction. The mineral immediately sputters off glowing particles, and one understands how trails are formed. Pieces crack off or scale away—the brightness of the light is dazzling, and when the specimen, after a few seconds, is withdrawn from the influence of the heat, which is, perhaps, 6,000° or 7,000º Fahr., the crust upon the surface much resembles that upon meteorites, while, where crystals of quartz have split off, there are pittings which are like those invariably seen on aerolites. There is also an appearance something like what is called the flow, though as the rush through the air cannot be reproduced in the experiment, the similarity may be merely accidental. The experiment is highly interesting and is instructive. The constituents of meteorites differ, and the difference in their visible colour doubtless results from this fact. The colours shown under the electric heat vary very strikingly as one selects specimens with quartz or with spar, also with various metallic constituents.

Mr. Lindsay announced that he had been in communication with Mr. T. S. H. Shearmen, of Brantford, a corresponding member of the Society, with reference to the work of the latter in telescope construction and had received from Mr. Shearmen some notes on

THE OBLIQUE CASSEGRAIN

which were then read, as follows :—

The recent publication in the *Monthly Notices* of the R. A. S., January, 1895, of a paper by Dr. A. A. Common, entitled "Preliminary note on a modified (oblique) form of Cassegrain telescope," has caused me to look up some of my own experiments in this direction, made so long ago as 1878. I convinced myself by certain experiments that an oblique Cassegrain was possible by properly adjusting and proportioning the mirrors. I also saw the advantages to be gained by using a Cassegrain reflector with the convex mirror so placed as not to obstruct the incident rays, by having mirrors tilted. Being convinced that it could be done, I wrote to Prof. Newcomb to know whether the plan was in actual use or not. He replied as follows :—

WASHINGTON, D.C., August 19, 1878.

Dear Sir,

Your plan for mounting reflecting telescopes is entirely inadmissible for the reason that an accurate image cannot be formed when the reflected rays are thrown so far to one side. The Herschel form of reflector is now entirely disused, owing to the impossibility of obtaining a good image by that method.

Yours respectfully,

S. NEWCOMB.

As my experiments had led me to expect at least a partial success, I tried the plan; and, after various trials found, as Dr. Common has recently done, that the astigmatism of the image from the concave mirror can, at certain angles, be almost eliminated by the opposite astigmatism of the small convex. I have not retained the sizes of the mirrors, but they would very closely approach the proportions given by Dr. Common, who finds good results to follow the use of a focal length of large mirror 10, aperture being 1, with radius of curvature of small convex three-fourths that of the concave and distance from the large mirror two-thirds of focal length. The angle between the first central incident and reflected ray is 5°30′, between the second 12°30′. Dr. Common's five-foot mirror thus fitted up gave fairly good definition of the Moon, but the stars showed trails. With the aperture reduced to thirty inches, however, the stars seemed round with low powers, but not quite so with a power of 1100. The Moon was very well defined and Jupiter was well seen in a quite dark field. With a speculum of one foot diameter and focus equivalent to eighty feet, he obtained very beautiful photographs of the Moon in two seconds.

Both the data regarding the proportions of the combinations and my original copy of the letter to Prof. Newcomb have been lost; but through the kindness of the distinguished Superintendent of the American Nautical Almanac I am enabled to reproduce my letter of 1878. I give it here not simply to assert a claim to the invention (an altogether hopeless and useless task in this age!) but to show how an invention can be hid away and apparently lost.

[copy.]

BELLEVILLE, ONTARIO, CANADA, August 12th, 1878.

Prof. S. Newcomb:

Dear Sir,—A plan for using the reflecting telescope has just occurred to me, that I cannot find described in what works on optics I have. It is this—

e. is the speculum, *ff*. the rays proceeding from the object. Instead of making a hole in the large reflector (*e*.) and viewing the small speculum through it, as in the Cassegrain or Gregorian construction, I propose to give the large reflector a slight inclination, and send the image to *c*., whence they are sent back to the side of the reflector at *d*.

The advantage of this construction over the Cassegrain or Gregorian, I conceive, lies in the large reflector having no hole in it, as that robs the reflector of the most important rays. Hoping not to be too intrusive on your valuable time, I would very well like to know whether the plan is in use or not.

Very respectfully,

THOMAS S. H. SHEARMEN

Prof. Newcomb, in sending the above copy of my letter, writes as follows:—

NAUTICAL ALMANAC OFFICE, NAVAL OBSERVATORY,
GEORGETOWN HEIGHTS, D.C., April 5th, 1895.

Dear Sir,—In reply to yours of the 2nd inst., I enclose a certified copy of a letter received from you in August, 1878, which is on file in this office. * * *

I note that the letter of 1878 said nothing about the really difficult point of the new construction, which is to so adjust and fix the second reflector as to correct the distortion produced by the obliquity of the rays from the first one.

Yours very respectfully,

S. NEWCOMB.

With reference to Prof. Newcomb's remark that my letter of 1878 did not call attention to the adjustment of the second reflector I would say (what I have already mentioned) that that letter was written simply to find out whether the oblique plan was in use or not. The adjustments of the mirrors were entered into after that letter was written. That there was anything new in thus adjusting the mirrors did not occur to me at that time, as a remark in that once popular work *The Practical Astronomer* of Dr. Thomas Dick had led me to believe that the principle of thus eliminating astigmatism was not new. In that work I found a reference to some experiments that Dr. Dick had made. In an account of the fitting up of a Herschelian of short focal length, he says "I had some ground for expecting success in this attempt from several experiments I had previously made, particularly from some modifications made in the construction of astronomical eyepieces, *which have a tendency to correct the aberrations of the rays of light when they proceed somewhat obliquely from a lens or speculum*." The italics are mine.

He further says: "The principal nicety in the construction of this instrument (the short-focus Herschelian) consists in the adjustment and proper direction of the eye tube. There is only one position in which vision will be perfectly distinct. * * * This position must be ultimately determined by experiment when viewing terrestrial objects. A person unacquainted with this construction of the telescope would, perhaps, find it difficult, in the first instance, to make this adjustment ; but were it at any time deranged, through accident or otherwise, I can easily make the adjustment anew in the course of a minute or two."

I presume it is not necessary to offer excuses for referring to the now almost forgotten work of Dr. Dick. This is the work that helped to shape Prof. Barnard's career in life; and has not Dr. Common (in *The Observatory*, November, 1888), said that Dr. Dick's "charming works are not held in such repute as they deserve, or, at any rate, do not seem to be read as they ought to be?" But I am digressing, as I merely wished to explain that experiment having shown me that a certain construction of the reflector was possible, I followed the precept of Dr. Dick to "ultimately determine by experiment" the *adjustment of* the construction.

Perhaps one reason why this construction did not then receive general consideration was that the time had not come for the employment of such large images in photography as the recent improvements in plates and processes have rendered possible.

In conclusion, I trust that these remarks and explanations will be viewed in a proper light and received in the spirit in which they are made.

Addendum:—The publication of Dr. Common's paper has been instrumental in bringing out considerable information regarding oblique reflectors. Since writing the preceding note attention has been called to the fact that such instruments were constructed many years ago in Germany and elsewhere; and in a letter to the *English Mechanic*, Prof. W. R. Brooks, of Geneva, N. Y., describes a reflector on this plan which he devised in 1884.

Such information had not reached this country, however, when my experiments were made in 1878.

NINTH MEETING.

May 14th; Mr. A. Elvins in the chair.

An abstract of the Society's work in 1894, to be presented to the Royal Society of Canada, was read by Mr. G. E. Lumsden and adopted. A short report from the earth-current committee was read by Mr. Lindsay, who stated that Mr. R. F. Stupart, Director of the Toronto Magnetic Observatory, was still engaged in comparing the records from Canso with his own observations at Toronto. Mr. S. S. Dickenson, Superintendent of the Commercial Cable Company's station, had been requested to send the siphon record of the earth-currents and had replied as follows :—

"I am delighted that the earth-current readings are proving of service to you and shall be glad to receive a copy of the paper thereon, as soon as published. When earth-currents become so strong as to prevent our working we put the cables direct to ground so as to guard against damage to our apparatus. I have known extremely strong earth-currents to fuse the signal coil and break down the insulation of condensers employed in cable working. I shall, however, comply with your request for the siphon record whenever it is practicable."

The chairman called attention to the great fall in temperature between May 12th and 14th, and noted that on this occasion the theory had been borne out, that there is such a fall generally in Europe and America on the dates named. Occasional references had been made by Dr. E. A. Meredith to this theory and also by the Chief of the Weather Bureau at Washington.

Some extracts from notes by Dr. J. C. Donaldson were read upon observations of a

DARK SPOT ON JUPITER.

May 6th :—When looking at Jupiter on the evening of the 4th instant with power 130, I thought I saw a dark spot on the lowest, *i.e.*, northern belt of the planet, but strange to say I could not see this spot with any of my other eyepieces, and suspect it may have been an ocular illusion; but, on the other hand, the spot seemed, when I looked with the 130 eyepiece, to change its position to correspond apparently with the revolution of the planet on its axis. On the whole, I am a little puzzled about it, and am rather inclined yet to the ocular illusion theory, but I would like to know if any of the other members of the Society happened to be looking at Jupiter on the evening in question.

May 10th :—In my letter of the 6th instant I mentioned having seen, or suspected seeing, a dark spot on one of Jupiter's belts on evening of 4th instant, using the 130 eyepiece, but thought it might have been an ocular illusion. Last evening (the 9th instant) about 7.35 E. S. T., I took out my glass again to have another look at Jupiter's moons, when with the same power 130 I saw a black spot nearly central on each of the two largest belts of the planet, which might easily have been mistaken for shadows of satellites in transit; but, of course, no shadows of satellites were then in transit; satellites I., II. and III. being not far distant from one another, and a considerable distance to the east, while satellite IV. was far to the west of its primary. I tried another eyepiece (170) and the two spots were still visible on the belts of the planet, but to make assurance doubly sure, I replaced the 130 eyepiece and another person who was present had a look at the belts and confirmed my observation in all respects. No doubt these spots have been seen by others but it may be well to remember that they will again be central on Jupiter in the early evening of the 14th instant, being an interval of five days, or about twelve revolutions of the planet.

Other reports of observations were received from Mr. J. G. Ridout and Mr. G. G. Pursey. Referring to solar phenomena, the latter stated his belief that the faculæ were more directly connected with terrestrial magnetism than were the spots. He described the method in use at the Toronto Observatory, by Mr. F. L. Blake, to sketch the appearance of the Sun's disc daily.

Mr. George E. Lumsden then addressed the meeting on the proposed visit of the British Association for the Advancement of Science. A resolution had been introduced in the Provincial Parliament to contribute a certain sum towards the expenses of the Association, provided that the meeting of 1897 should be held in Toronto, and this had been supported in a most hearty and public spirited manner. As there was every probability that the invitation to visit Toronto would be accepted, it would be wise to spread as widely as possible a knowledge of what the Association had done, and to so arouse public sentiment that a hearty reception would be accorded.

The resolution had been moved by the Honourary President of the Society, Hon. G. W. Ross, LL.D., Minister of Education, and who had acceded to the request of the Society that the text of his address to the Legislature be read to the meeting and published in the *Transactions* for the current year.

Mr. Lumsden then read :—

ADDRESS BY THE HON. G. W. ROSS, LL.D., ON THE VISIT OF THE BRITISH ASSOCIATION.

The British Association for the Advancement of Science is one of the oldest and most influential of all the associations of a similar nature in the British Empire, if not in the world. It has numbered among its Presidents the most eminent men in literature and in art and science which the British race has produced. Its sessions have been presided over by men of royal blood, by Premiers of the British House of Commons, and by philosophers none the less noble although less favoured by the accident of birth and opportunity.

In 1884 this association held its meeting in Montreal, under the Presidency of Lord Rayleigh, whose address on the uses of electricity for motive purposes was regarded as one of the most suggestive papers ever submitted to the association. Sir William Thompson, now Lord Kelvin, delivered a very powerful address on the relation of chemistry to commerce and the industrial arts; and he was ably supported in the views which he presented by Sir Henry Roscoe, who, as a writer of text-books on this subject, is well known in Canada. Sir Lyon Playfair, a very warm patron of technical education, as well as a leader in the politics of England, was also present. The geological section was represented by Sir William Dawson and by Sterry Hunt, both Canadians of great distinction; the ethnological section by Sir Daniel Wilson, whose connection with the University of Toronto will not be soon forgotten by the people of Canada. The geographical discoveries of the day were discussed by

Sir J. B. Lefroy ; mechanical science by Sir F. J. Bramwell ; agricultural chemistry by Sir J. B. Lawes and Dr. Gilbert. Several distinguished Americans also assisted in giving interest to the proceedings by reading important papers connected with American institutions. The mere mention of such distinguished gentlemen as I have named would give character to the association, no matter what were its scope and purpose. When I say, however, that it extends its investigations to every department of science, and when I say that its aim is to be practical and instructive, to teach lessons in political economy which touch the masses, in sanitary matters which improve the public health, in physics which are calculated to lighten the labour of the working classes, in the higher departments of science, such as astronomy and biology, which elevate the taste and widen our knowledge of the mysteries of nature, I have said all that is necessary to commend to this House the object of my resolution from a scientific point of view.

There are two or three other considerations, however, which should not be lost sight of.

(1) A meeting of so many distinguished men, closely identified with the best thought of Great Britain, would undoubtedly quicken in many Canadians that spirit of scientific research which has done so much for the advancement of civilization the world over during the century. To many the nineteenth century is regarded as chiefly a century of invention. To a certain extent this is true, but with equal truth it may be said to be particularly a century of scientific inquiry and research. No other century has given us so much profound philosophy; no other century has applied the scientific spirit so extensively or so fully to every department of activity. This is the case with every question to which the human mind has applied its powers. The scientific spirit in theology has given us the higher criticism; in literature it has given us a new inspiration; in philosophy it has given us the theory of evolution ; in physics it has given us electricity in its application to light, heat and power. The diffusion of this spirit among the people of Canada would certainly be an epoch in our history, and would quicken and inspire many of our people to pursue those lines of research which have been such a powerful stimulus to the educational forces of the world, and from which such extraordinary results have followed.

THE BONDS OF EMPIRE.

(2) Such a meeting would, in my own opinion, strengthen the bonds of empire. My honourable friend from South Toronto, Mr. Howland, has contributed to the literature of this subject a very valuable volume which no one can read without having a wider conception of his duties as a British subject. To come in contact with those who live at the seat of empire, and who are responsible in many respects for the evolution of those forces which in more than a scientific sense prevent its decay, would give to the people of Canada a higher conception of the power of that empire and of the relations which exist between its central vital forces and the extremities. As a part of that empire the Dominion of Canada holds no unimportant position, and if we are indebted to the strength of the Imperial arm for the security of our institutions we might very well reciprocate that obligation by receiving as fellow-subjects, with a generous hospitality, those men who are in a position to influence the public opinion of England in favour of more extended privileges should we require them and greater protection should the necessity arise. To be geographically a portion of the British Empire is not the ambition of Canadians alone. The savages of South Africa can occupy that position. It is our duty to be British subjects, with all the sympathies which constitute a loyal attachment to the institutions under which we prosper, and to that end we should ally ourselves to every force which contributes to the development of an empire in which intelligence and morality would dominate all other forces.

(3) A meeting such as is proposed would widen the mental horizon of our people. The great danger of colonists everywhere is to become parochial. The necessary concentration of energy imposed upon us by the struggle for existence has naturally prevented us from observing as fully as we might the great scientific movements of the day. It may not be our fault that we occupy our time with matters of business and with details of government which necessarily have this narrowing effect, but we certainly would be to blame if we neglected any reasonable opportunity by which such tendencies could be corrected. A meeting of the British association will give us such an opportunity, and will no doubt lift us to a higher altitude, where, in the clearer atmosphere of scientific thought, we may be able to unite our feelings and our sympathies with the thought of others who are recognized leaders in their respective departments.

(4) An advantage of meeting the living exponents of the best scien-

tific thought of the world should not be underrated. Naturally a great deal of interest attaches to the person and habits of every man who has made a name for himself in any department of activity. Many of us would give much to see such an exponent of educational opinion as Sir Lyon Playfair, others such a President of the association as Lord Salisbury. To know these men personally is to take a special interest in the work in which they are engaged. Every article they may write in a magazine or every discovery they may make would have thereafter a personal interest to us. The influence of their addresses would reach through the press the remotest hamlet and school house in the country, and thereafter every Canadian who is of a scientific turn of mind would work in his department with greater enthusiasm because of having worked side by side with a master spirit at the British Association in a similar department of thought.

I commend, therefore, to the House this resolution because of the national purpose it will serve, of the stimulus it will give to scientific inquiry and of the breadth and freedom of investigation which it will necessarily assist in developing among the people of Canada.

The resolution was supported by Mr. Howard, who, in a few well-chosen words, bore his testimony to the work of the British Association and of the eminent men enrolled among its membership. Mr. Haycock, on behalf of the Patrons, approved heartily of the resolution moved by the Minister of Education.

TENTH MEETING.

May 28th; Mr. A. F. Miller occupied the chair.

A cordial letter was read from Prof. J. McK. Cattell, editor of *Science*, who had published in the columns of that journal, issue of May 24th, a sketch of the origin and progress of the Astronomical and Physical Society of Toronto, in the hope that it would be of interest to men of science generally throughout America and possibly influence others to form similar societies in other places. The thanks of the Society were due to Prof. Cattell for the very liberal space given to the sketch referred to.

The Secretary was directed to forward a set of the Society's *Trans-*

actions to Columbia College, New York, a request for the volumes having been received from the librarian of that institution.

Communications were read from Rev. W. G. Hanna, of Uxbridge, in reference to the prospects for forming a branch society in that town, and from Dr. Sandford Fleming, C.M.G., with regard to the work of the Committee on the Unification of Time.

Miss A. A. Gray reported having spent an evening at the telescope with the pupils of Wellesley School, who were much interested in observations of Jupiter and Saturn. With the assistance of another member, she had arranged to give the senior classes of the public schools as many opportunities as possible to engage in practical telescopic work. The general interest taken and the order that prevailed during the observations had been very encouraging. Several members repeated their desire to assist Miss Gray in this work, which was directly in line with the Society's objects.

Mr. C. P. Sparling read some extracts from an article in the Belleville *Sun*, copied from some American source and dealing most extravagantly with theories recently announced concerning Mars. The object in reading was to show how much is to be done in order to disabuse the lay mind of certain ideas regarding the power of the telescope and the work of astronomers. When articles like the one in question were published without note or comment by reputable journals, many readers would be quite likely to acquire ideas regarding astronomy so utterly erroneous that the labours of others striving to make popular the beauties of the sublime science might be well nigh fruitless.*

Mr. A. Elvins, in speaking of theories regarding Mars, stated that his attention had been directed to a simple phenomenon, readily observed any day in winter, and which threw as much light upon the origin of the surface markings as any other of the now multitudinous theories. During the recent cold spell a circular vessel containing water had been exposed to the night air, and in the morning ice crystals had shot from the sides, some towards the centre and some as chords, forming a network over the surface. A very careful drawing of the resulting appearance had been made, and laid by the side of the Lowell drawings of the

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* Remarks in much the same strain were subsequently made by the editor of *Popular Astronomy*, when censuring a great American daily for publishing the article to which Mt. Sparling here so strongly objected.

Martian surface, when there was a remarkable similarity observed at once. Mr. Elvins wished it understood that he did not advance any theory regarding the planet's surface ; the drawing was presented merely to show how much care should be exercised before forming any decided opinions on Martian phenomena.

On motion of Mr. Arthur Harvey, seconded by Mr. T. Lindsay, it was resolved to reorganize the Opera-glass section, which had not held meetings for some time past, and to meet for active work on alternate Tuesday evenings at the residence of the Vice-President, Mr. John A. Paterson, 23 Walmer Road.

This was carried unanimously. After some discussion as to the best manner of conducting the meetings, it was suggested by Mr Harvey that no director be formally appointed, but that the member who took charge on one evening should name his successor for the next. This was agreed to.

Notes of observation were received from Mr. J. G. Ridout and Mr. A. Elvins. The latter presented a drawing of a sunspot group which had just passed the central meridian of the disc. He had also examined the records of the Toronto Observatory with a view to determining whether any magnetic disturbances had been coincident with the recent marked fall in temperature. The curve had been normal; there was in this case nothing to indicate a connection between the temperature curve and the fluctuations of the magnetic needle. In view of the fact that all conditions should be studied in the effort to reach the basis of meteorological science, Mr. Elvins thought this worthy of record.

The Chairman having referred to the recent work of Prof. J. E. Keeler at Allegheny, an interesting discussion rose regarding the significance of the result reached by the distinguished observer in his spectroscopic study of Saturn's ring. It had been demonstrated experimentally that the inner part of the ring revolves more rapidly than the outer, thus supporting and confirming the theory announced in 1859 by Clerk-Maxwell, that the rings are composed of solid independent bodies, meteorites as it were. Mr. Harvey read some notes from a popular description of Prof. Keeler's work published in *Science*, illustrating by diagrams the method employed. The classic mathematical analysis of the great physicist and the refinement of Prof. Keeler's work with the spectroscope would be ever remembered together.

Mr. J. Phillips announced that he would shortly present a paper dealing with the bearing of these results on the nebular hypothesis.

ELEVENTH MEETING.

June 11th; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

The members generally had taken advantage of the very favourable observing weather, and several reports of telescopic work, etc., were presented. A meeting of the Opera-glass section had been held at the residence of the Chairman, chiefly for the purpose of arranging details of the work which it was intended to take up. In addition to small telescopes and field-glasses, which were to be employed in constellation study, it had been thought advisable to make use of the Wilson telescope whenever practicable. Mr. Paterson stated that Messrs. J. & Z. Collins had recently repolished the metallic mirror and adjusted a new flat of silver-on-glass. The performance of the instrument was now all that could be desired.

Mr. A. Elvins presented a drawing of Venus taken at his three-inch telescope, showing a very bright patch at the northern pole, which he thought might possibly be a snow-cap. In observing he had used the full aperture of the objective, with a power of 300. The glare around the planet was found thus to be entirely removed, while the quality of the objective permitted very excellent definition.

Mr. J. Van Sommer reported having seen a very brilliant orange-coloured meteor falling in a direct line from Ursa Major to the horizon at about 10 p.m. on June 3rd. After breaking, the several portions appeared to become incandescent as they passed from the point of explosion.

Mr. J. R. Collins described an auroral cloud seen on the evening of June 1st in the north-western heavens, in the form of an inverted arc. He believed this appearance to be very rare.

Some notes were received from Dr. J. C. Donaldson, in connection with observations of the satellites of Saturn. He thought that a member who had reported having seen five of the moons in a three-inch telescope must have mistaken some small stars in the neighbourhood of Saturn for members of the system. He quoted Prof. Pickering, who gave the stellar magnitude of Rhea, the second in order of brightness, as 10·8; this object Dr. Donaldson found more difficult than the well-known test for small apertures, the companion to Polaris. Referring to the above, Mr. J. G. Ridout stated that it was not unusual to see two others besides

Rhea and Titan (the latter the brightest of all and an easy object), in a good three-inch. He called attention also to the fact that the four next to Titan were all discovered by Cassini in the 17th century, before the invention of the achromatic telescope which we have now.

Mr. Geo. E. Lumsden then addressed the meeting with reference to the advisability of forming a section of the Society for the special study of the Moon. He had prepared a large outline map of the lunar surface, and described the several interesting objects which appear day after day as the Moon's age progresses; many of these might be studied advantageously with telescopes of moderate power only. The work of a Lunar section such as might be formed without impairing in any way the work of the Society as a whole, was briefly outlined; this would consist of systematic observations whenever practicable, throughout each lunation; the becoming better acquainted with the special features of the surface, the nomenclature in common use, and careful searching for small details which have not yet been generally noted. The work of the section might also include photographing the Moon and making drawings of special features.

Dr. J. J. Wadsworth, of Simcoe, who was present, called special attention to the extreme beauty of the telescopic appearance of the Moon. We speak of the grandeur of terrestrial scenery, where nature has been lavish of her gifts, but the eye can be impressed with only a very small part of the Earth's surface at once; we direct the telescope to the surface of our satellite, and the view is incomparably grander, presenting on what is really a vast scale compared with the limit of vision on the Earth, mountain and valley, great plains and ocean floors, rifts, craters, and immense boulders, on a world which has done its work, and hangs now in space, inviting study regarding its origin and true history.

The discussion becoming general, many interesting points were brought to notice. A drawing of the full Moon, made some years ago by the late Mr. S. E. Roberts, of Mimico, was shown, and much admired. Mr. Elvins wished observers to note one peculiarity of the lunar surface which was to him very suggestive. Adjoining the *maria* are frequently found circular plains, which in many places have the mountain surrounding them apparently broken down; and where this break is seen at all, it is always on the side next the *mare*.

It was finally decided to meet on the following Tuesday at 23 Walmer Road, when details regarding the forming of a section would be discussed.

TWELFTH MEETING.

June 25th; Dr. Larratt W. Smith, Q. C., President, in the chair.

After the reading of copies of correspondence in connection with the work of the Committee on Time-Reckoning, Mr. R. F. Stupart, Director of the Toronto Observatory, was elected a member of that Committee, taking the seat rendered vacant by the decease of Mr. Chas. Carpmael.

Reference was made to the loss which the scientific world had sustained by the death of Prof. Daniel Kirkwood, LL.D., of Riverside, California. The distinguished astronomer had been one of the first to render assistance to the Society immediately after incorporation in 1890, and had honoured it by accepting honourary membership. Mr. A. Elvins paid a high tribute to the deceased, with whom he had had the pleasure of corresponding for many years. Dr. Kirkwood's researches on the subject of planetary rotation and on the orbits of the asteroids were most valuable, so much so that he had been referred to by his contempories as "the Kepler of the 19th century."

The Committee appointed to assist the Librarian in the rearranging of the Library, announced the completion of the work, and presented a report in detail, which was adopted, the thanks of the Society being tendered to the members who had been engaged.

Mr. W. B. Musson, the Secretary of the Committee, read some notes upon the general subject of indexing, and described the method which had been finally adopted as the most preferable. A card catalogue had been compared, which gave the references by authors' names, by subjects, and also by titles alphabetically. It was possible by this system to make the catalogue practically a bibliography, at least while the Library was yet a humble one in point of number of volumes.

Among other notes of observations Mr. Z. M. Collins stated that he had observed a bright spot at the south pole of Venus, bordered by a dark irregular streak. The spot at the north pole of the planet, reported by Mr. Elvins at the previous meeting, had been very widely observed, drawings having been published in several journals devoted to astronomical work.

Referring to observations of the Sun, Mr. Pursey stated that an interesting group of spots on the disc between June 9th and 17th had presented all the various changes of accretion and disintegration, to observe which it is generally necessary to study the solar surface for a very long time.

The President gave a brief account of a journey recently made to Florida, and described the zodiacal light as seen in the South. He had been gratified to find a very wide-spread knowledge of astronomy among people whom he had visited, especially among American ladies.

An interesting letter was read from Dr. J. J. Wadsworth, of Simcoe, who had forwarded by request a photograph of his 12-inch reflecting telescope. This was much admired, particularly as the instrument had been constructed by the Doctor himself. The Secretary was requested to communicate with Dr. Wadsworth and ask him to use his reflector specially in examining the more minute details of the lunar surface.

Dr. A. D. Watson read some notes on astronomical inaccuracies to be found in the works of novelists and poets. Examples were given from Shelley and Coleridge to show that even the greatest had made errors which might easily have been avoided, though it was not claimed that the beauty of these great poets' writings was really marred by mistakes of this kind.

THIRTEENTH MEETING.

July 9th ; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

Mr. F. L. Blake, chief observer at the Toronto Observatory, was elected an active member of the Society. The Secretary read a most cordial letter from Dr. Isaac Roberts, F.R.A.S., who had replied to a very interesting question which had been raised as to possible changes in the structure of the nebulæ. Photographs of the great nebulæ in Andromeda and in Orion, with an interval of seven years between the times when they were taken, had been examined, but no evidence could be found of changes having taken place in the nebulosity during that period.

The thanks of the Society were due to Dr. Roberts for his valuable gift of a copy of his volume of "Photographs of Star Clusters and Nebulæ." An interesting letter was also read from Mr. W. D. Barbour, of Leeds, Eng., who had forwarded copies of the *Transactions* of the Leeds Astronomical Society. These were subsequently read with much interest. It was very gratifying to note the success which the Society had met with in its efforts to widen the circle of students of astronomy.

The Librarian reported the receipt of a monograph on time-measurement, by M. de Rey-Pailhade, President of the Geographical Society of Toulouse.

A report of the first meeting of the Lunar section was received from Mr. G. E. Lumsden, who had been appointed director. The members had met at the Toronto Observatory on July 2nd, and had spent a most enjoyable evening. Several telescopes had been set up on the adjoining lawn, while the large telescope of the Observatory had been also placed at the service of the meeting by the kindness of Mr. R. F. Stupart. A paper on the "Present Condition and Past History of the Moon:" had been prepared by Mr. Elvins, but the time was so largely taken up in observation that the reading of the paper was postponed until a future regular meeting of the Society.

The following notes of

OBSERVATIONS OF SATURN

were received from Dr. J. J. Wadsworth of Simcoe :—

I began observing Saturn this year, 1895, on April 4th, with 12½-inch reflector. Made notes on him on seven evenings in April, three in May, and nine in June. Will give the substance of what I have seen:

1. The Cassini division has been steadily seen on the ansæ, and on several occasions nearly up to the globe; but not on the part of ring between us and the globe. 2. The shadow of the ring on the globe to the south of the ring has been very distinct during the month of June. In the early part of the month the eastern end of this shadow was much wider than the central part, which seemed strange. On April 4th I saw a distinct dark line to the north side of the ring; was it the shadow, or the crape ring, or both? It reached across the globe just below the ring. It grew fainter after May 2nd. 3. The shadow of the globe on the rings has been clearly visible. It is growing wider and has a concave outline, the cause of which I cannot imagine. The concavity points to

the east, causing the ring to look like a blunted horn, where it meets the shadow. The outline of a sphere projected obliquely on a plane should be convex on the outside, approximating to a straight line in extreme projection. Yet to me and to two others this shadow is clearly hollowed out. 4. I see the following belts on the north half of Saturn: (*a*) bright equatorial belt; (*b*) dark belt; (*c*) another bright belt, not so clear as (*a*); (*d*) north polar dark shading. 5. The crape ring is very plainly seen as a dusky-reddish shading, plain in the vertex of the sky lune, and extending about one-third of the way to the globe. On two occasions lately I have seen a faint line across the body of the planet immediately north of the bright inner ring. This I believe to be the crape ring, as seen between us and the planet. 6. As to satellites, Iapetus, Titan, Rhea, and Dione are often seen; and other stellar points I am not sure of, fixed stars being numerous in the neighbourhood.

Accompanying Dr. Wadsworth's notes was a very beautiful drawing of Saturn, made by Miss Eva M. Brook, of Simcoe, to whom the thanks of the Society were due. The concave outline of the shadow was plainly seen in the drawing.

Mr. G. G. Pursey presented a series of drawings of sun-spot groups covering the period of the last three months, eighty-four observations in all. Mr. R. F. Stupart stated that the magnetic records of the Toronto Observatory had shown no unusual disturbance since last November; as the Sun had been fairly active for some time past, it was thought by some of the members present that this would tend to furnish evidence on the negative side of the much disputed theory as to whether solar outbursts and magnetic storms are coincident. Mr. A. Harvey described a solar halo which he had observed on July 5th. Referring to the cause of these phenomena, Mr. Stupart said that they were most frequently seen when an area of low pressure was passing over the Ohio valley. Mr. Elvins called attention to what he considered a noteworthy fact, that twice during the present year a very hot wave had been followed immediately by a sudden fall in temperature.

Mr. Thos. Lindsay then read the introductory chapter to an

HISTORICAL SKETCH OF THE GREENWICH NAUTICAL ALMANAC.

A complete history of *The Greenwich Nautical Almanac* would be work requiring nearly as much research as was expended upon James Grant's great classic *The History of Physical Astronomy*. That work was completed before what we call the " new astronomy " began, for example, before solar physics became a science of itself, and is largely a history of mathematical astronomy. As the nautical almanac is concerned entirely with this subject, it follows that it would be necessary to look up the same reference as Grant consulted, if one wished to trace the work and all the particulars of its compilation from its inception. Grant has given all his references in copious foot notes, so that the way is already paved for the second historian. But it would be also necessary to refer continually to copies of the almanac itself year by year. So that, in short, it would be impossible to write a full history of the great book unless one were able to spend month after month in England, with occasional trips to the Continent. That being the case, it is extremely unlikely that the writer of this sketch will ever write anything more than a sketch. But as all these years have passed without anything at all being written on the subject, it is just possible that even a short account will be of interest to some. It cannot be to everyone, because in these days astronomy is divided into so many branches that no man can study them all; one generally chooses his particular line of work and follows it out, perhaps becoming an authority in that branch and recognized as such by those whom he regards as authorities in other departments.

The sketch which is here presented has one most excellent feature; the reading of it can be stopped at the end of any paragraph and no harm done to anybody. As a matter of fact, it is the intention to read only a few pages to-night. I would hardly wish to inflict the whole story upon you at one sitting; it can be left off and taken up some other time to fill in half an hour. And even if it is never to be read, an effort will be made to write it, if I follow out a desire which I have had for many years, that is, to describe in popular language what has been to me ever since I first opened it, the most interesting book in the world. What may be learned from its pages by the student, and how intensely interesting the apparently dry columns of figures can be made I hope to have another opportunity of showing, not being specially concerned with those points in this chapter. I will just throw out one hint to all who may be engaged in teaching astronomy in schools, universities or elsewhere, with all deference to their opinion if they differ : if a student can be brought to study the almanac side by side with the text book he will get along just twice as fast; therefore the almanac should be in the hands of every learner who wishes to study what we call the "old astronomy," as distinguished from astronomical physics.

I said that the history of the almanac was still unwritten ; I believe that to be the case, but anyone interested specially in the subject might read with advantage the article "Almanac" in the *Encyclopædia Britannica*. It is not a long one, but many references are given to literature bearing on the subject generally. A series of articles on "Almanacs" is also now running in *Popular Astronomy*. The works from which has been gathered what follows here are, Grant's *History of Physical Astronomy* and copies of the almanac, such as I have been able to procure.

If we ask a student of history to give his opinion as to the time when England began to be a mighty nation he will answer according to the particular trend of his own ideas as to what the greatness of a nation means. One, lost in admiration of the days of chivalry in general, and the deeds of the Norman barons in particular, will quickly reply that the Conquest was the real beginning of what is now the greatest nation of all history; another, less disposed to credit the feudal knights with all the virtues man can ever possess, will go a little farther back, and declare that the days of Alfred the Great marked the commencement of the greatness of his country, and to the haughty descendant of the Norman will reply as did the sturdy English farmer, "If your ancestors did come over with the Conqueror, they found mine here when they landed." Still another, versed in the history of the glorious English tongue, will hold that the days of good Queen Bess marks the era of all eras,—the days when Shakespeare wrote,—in the language that had been evolved out of the mixture of Anglo-Saxon and Norman French. Then some other, with strong democratic tendencies, will affirm that in the moment when Cromwell told someone to "take away that bauble," England *was* great, but that she lapsed into the common rut of monarchical institutions at the Restoration. In fact, no two students of the fascinating study of English history will agree as to the date when England's present greatness began. It is, therefore, with a great deal of diffidence and unwillingness to argue the point at all, that we give our humble opinion in the matter. We do not wish to discuss it, merely ask you to receive it and think over it at leisure. Possibly then you may agree that this British Empire of ours, this empire upon which the

Sun never sets, this monarchical institution which is still the most democratic in the world, dates its right to eminence among the nations of the Earth from the year when the printing press, grandest of inventions, gave to the English people the noblest of its productions—next to the Bible—*The Greenwich Nautical Almanac*. A nation becomes great when she begins to extend her commerce to the farthest regions of the Earth; she cannot do this with perfect satisfaction until she either has or borrows from some other nation an ephemeris of the Sun, Moon and planets of the solar system. Pretty ostrich feathers come from Africa; some one must go down to the sea in ships to bring them; he cannot make much headway unless he understands how to mark the trackless way that leads him thither. Is he content to take work of the astronomers of other countries as his guide? Well, perhaps he is, but when it dawns upon him, when it dawns upon the nation, that this is a very humble position to take, an astronomical ephemeris, in the language of the people, is demanded. This is the era when a nation becomes truly great.

Our country was not the first to reach the point when true greatness, as we have defined it, could be claimed. The great almanac of France, the *Connaissance des Temps*, has an unbroken record since 1679. To England the light did not come until 1767. A brief glance at the political, literary and scientific world at that time will show how ready she was to take the final step which made her in all things the foremost nation of the Earth. Farmer George had been king seven years; the great Chatham thundered against the impositions laid upon the Colonies, and which finally led to the American War of Independence; Captain Cook was about starting on his voyages which resulted in the addition of a Continent to the British possessions. Our territory in India was rapidly being enlarged. The cause of France had been ruined in that quarter some years before, and English bayonets were engaged in converting the benighted heathen. In the world of letters we have, model for all his successors, Oliver Goldsmith, his *Deserted Village*, not yet written; the poet Gray nearing his end; Samuel Johnson, his fame established, and happy in his grumbling and a pension; and David Hume still striving to invent a philosophy. Edward Gibbon was a young man forming the outline of his great work; Robert Burns was an urchin at school; Walter Scott, most loved of all, had not yet seen the light. Adam Smith was busy at the work which laid the foundation of a new science; Blackstone had published his Commentaries ; Edmund Burke was in the prime of life ; Robertson was engaged upon his great *History of Charles the Twelfth*, the gentle Cowper was enjoying a lucid interval in the household of the Unwins.

A brilliant era was this in the history of England's literature ; but England was ready for something even nobler than poetry, and we turn to the records of science and see how the world stood in 1767, because while a nation's literature is its own, science is universal. On the Continent, Euler, Lagrange, and Laplace, lived and worked. Laplace was then a young man, but still giving evidence of the genius that ultimately produced the *Mécanique Céleste*. In the domain of pure mathematics we may mention the name of Lambert, of Belgium, who had presented to the Belgian Academy of Sciences a rigorous demonstration that the circumference and diameter of a circle are incommensurable, thus settling for ever, the third great problem of antiquity. So that up to this time it was in order to attempt the quadrature of the circle, but since about the date of the issue of the nautical almanac the circle squarer has been following a phantom.

Newton had passed into immortality forty years before. The world had been in possession of his *Principia* for twice that time, and what we now call the Newtonian philosophy had been established firmly and for ever. Nevil Maskelyne, fifth astronomer royal, had been two years in office, and for four years had published the *British Mariner's Guide*, the forerunner of the great work we are about to describe. Tobias Mayer, of Germany, had constructed tables of the Moon which allowed of a very close determination of the longitude, and for which the British government subsequently paid a very substantial reward.

The achromatic telescope had succeeded the old form of single lens telescopes, and Herschel, settled in England, was beginning to dream of what he might accomplish with the great reflector. The government had just awarded the sum of £20,000 to John Harrison as a reward for his invention of the chronometer, which, after repeated trials on long sea voyages, had been found to be all that was claimed for it; the transit instrument was known, and the name of Ramsden appears in the list of opticians of the day. The time, then, was evidently ripe for the publication of a great national ephemeris.

Though I hope to be able to extract from such works as I have access to, a general outline of the rise and growth of *The Greenwich Almanac*, I fear I can tell but little of the lives of the men who compiled it, certainly nothing of their inner lives. Even of the men who compile the work now, in this very day, there is nothing to be learned from published literature ; they have, in fact, been entirely forgotten by historians, by sketch writers, by novelists, by everybody. Yet I have a kind of an idea that it would be just a little interesting to learn something of those men who are engaged in transforming intricate algebraic formulæ the whole live-long working day. What effect does it have upon them,—does it tend to make them narrow-minded or the contrary? From what ranks generally are they recruited? Do they lose all taste for what we call recreative reading, or do they fly to it as soon as they can get free from the unceasing figuring that occupies the day? Can they retain what there is of poetry and romance in every educated man to start with? If they do, they have not been treated fairly, for as I have said, the romancer and the poet have forgotten their existence, if, indeed, they ever knew of it. What far away trips from home does the novelist take to find a subject for a book! "Mr. So and So is at present in Africa looking up material for a new work; his last had an enormous sale, half cloth so much, paper edition very cheap," etc. Bless his innocence, he has passed the almanac office a thousand times, and yet remains unconscious of the vast mine of material for character work that is to be found in that building. Blasé at thirty, he sings in earnest as Byron sang in jest—

"I want a hero ——"

Express surprise and he will tell you: "Yes, all types of character are represented in fiction; what with Dickens and Scott and Thackeray as masters, and a whole host of followers, the entire field is covered." Well, it is not covered, it has not begun to be covered, and the brightest corner of the field is wide open for whomsoever wishes to explore.

"But the type of character to be found around the Almanac office is not suitable for fiction. It is not like the Foreign office or the Sealing-wax office, and so on." Well, I fancy it is a trifle superior to the style that gets into the *fin de siècle* novel. But it is the duty of the novelist and the dramatist to educate the public, to keep up the standard of morals; they debase their talents when they seek to lower it. Let them give us good healthy reading and see how quickly we will sweep all the other trash out of the field. They act as if we wanted their vile stuff; we do not, but we must read something and have no choice but to take what is given us. But I do not lay all the blame on the novelist; the historian and writer of biography must bear their share also. They too have ignored these men all through the century ; Grenville Murray has given us what he calls " Social Photographs," picturing various departments of the civil service, not a word about the almanac department. All our great political leaders have been held up to nature, and the minutest details of their lives given us, but not a line about the almanac computer. "He is only a secondary kind of character." Is he indeed? If every one may speak for himself, let me say here: I once had the distinguished honour of seeing Mr. Gladstone and Lord Rosebery riding together in a carriage (it was on the occasion of one of their triumphal entries into Edinburgh). They were bowing right and left to the people —I was one of the people. But I do not claim any superiority over the ladies and gentlemen here assembled because I had the honour of being bowed to by these great politicians, nor am I specially conceited because I once spoke to a man who had been intimate with the Prince of Wales. I could mention a score of books clearly outlining a system of government which would render entirely unnecessary anything like a politician or a royal prince. But I cannot see how man of the present age could enjoy life either on its practical or its æsthetic side without the assistance of the *Nautical Almanac* in some shape or other, perhaps for practical life not so elaborate as we have it, but to meet the wants of the beautiful in life we cannot have it elaborate enough. And I do admire those men and would be proud to know some of them, who have selected as their life work the unceasing, severe, wearying task of figuring out for us the paths of the orbs in space. I am aware the computer is but following out what was given to the world by the greatest genius of all history and a few brilliant minds succeeding, who dragged into the light of day that mighty instrument of research the "modern mathematical analysis," which had lain hidden among a few simple geometric truths. I am aware that the almanac computer need not be a genius, unless we accept literally Carlyle's definition, "Hard work is genius." But I am also aware that we accord a high place to the legislator, and no legislator has ever improved upon the laws of Moses. We give high place to sanitary engineers, but Moses was the first; we are quick to appeal for aid to the doctor of medicine, and praise him none the less because he is not a brilliant experimenter or discoverer; it is enough for us if he is pretty well up in what a few great investigators have discovered in the past.

However, as it is, unless we go to England specially to learn upon the spot, we can say but little about the compilers of the almanac. I have thrown out the idea, perhaps some one may take it up, of weaving a romance around those computing rooms. The writer of the future may even run in a serio-comic situation now and then—just fancy a clerk coming home, weary with his day's work, flinging himself into a chair, or, as it would be in a novel, "resting his tired frame on a richly upholstered ottoman."

To him, his wife, "You look worn out to-day, dear."

"Yes, that confounded Venus bothered me at the office all day."

"Indeed !"

"Yes; Jupiter and Mars were on the same side of the Sun, pulling at her like the mischief."

" Oh! that Venus!"

"Why, of course. What did you suppose I meant? And Mercury will be into the field to-morrow to make matters worse,"—and so on.

But one must have talent to write novels. A member of this Society whose opinion we value very much and to whom I was speaking of the lack of literature on this subject, suggested that I take the matter up myself. Well, of course it is not want of talent that would keep me from writing a novel; not at all! but I have an unfortunate failing for not being able to write anything that is not strictly true, so I must keep out of that field.

Proceeding now with strict recital :—We learn that the light reached England in 1767. The *Nautical Almanac* for that year came from the printing press in 1765, necessarily in advance, that navigators might have data from which to determine positions at sea, though their voyages might be prolonged—to be two or three years from home was common in those days. For four years prior to 1767 the British sailor had been in possession of the *British Mariner's Guide*, published by Nevil Maskelyne, and he it was who brought to the notice of Parliament the necessity for issuing annually under the authority of the Commissioners of Longitude, an astronomical ephemeris. The great problem of the preceding part of the century had been to find a method by which the navigator could readily determine his longitude. It need scarcely be said that there was no difficulty about latitude. Almanacs giving the Sun's declination had been common enough since Kepler's time, and there were extant several star catalogues; but to observe a star on the meridian of one place and know how long since it was on the meridian of another place, this was not so easy. Galileo had been the first to point out that there was one series of signals constantly recurring in the heavens, the configurations of Jupiter's satellites, and had constructed tables of these. Cassini towards the end of the seventeenth century had improved upon all who preceded him in this respect, and published an almanac which to a certain extent was useful for explorers, but for several reasons of little value to the navigator. In fact "dead reckoning" was the best kind of calculation which the sailor could bring to his aid until about the date of the almanac, when, as has been said, Tobias Mayer had given to the world his tables of the Moon. It must not be overlooked, however, that there were some great navigators in the world long before almanacs were printed; and the maps of the ancient world are pretty accurate when we remember that the unit of length for the geographer was simply a day's march. As for Columbus, there is all the more credit to him as a sailor, considering that he made his dash for America even before Copernicus had given his theory to the world.

Newton had made lunar tables possible, and had pointed out the advantage to be gained by considering the Moon as the minute-hand of the celestial clock; so it had long been the great aim of astronomers to perfect the lunar theory. Mayer died in 1762, and his tables being in the possession of the British Government, there was no man so well qualified as Nevil Maskelyne to put them to practical use. This great astronomer was born in 1732, and was a student of the stars from early manhood. In 1765 he succeeded Bradley in the position of Astronomer Royal, being the fifth to hold that office. Maskelyne is one of the illustrious men connected with the *Nautical Almanac* about whom something may be learned. It would be a pity, indeed, if the founder of the great book had been forgotten. It should be remembered that it was he who proposed to the Royal Society the historic Schehallien experiment to determine the mean density of the Earth, and also that he was the first astronorner to mark observations to tenths of a second. The recognized authority in England on matters connected with astronomy and navigation, we find him correcting those who, undertaking to publish almanacs for common use, were not very careful of their figures. In the *Gentleman's Magazine* for 1767, he calls attention to the fact that the Moon's phases, as given in the *Stationer's Almanack*, are grossly erroneous, and that it would not avail to plead they were not intended for the current year, because an almanac was supposed to be compiled for the year in which it was dated.

Maskelyne, as the chief computer of the *Nautical Almanac*, and the responsible head of the Royal Observatory, acted under the direction of the Commissioners of Longitude, and laboured for nearly fifty years. We hope to find much information regarding his personality, as we proceed with the examination of his works, the monuments of his genius.

Mr. Lindsay here brought the introductory chapter to a close, and stated his intention of presenting a continuation of the subject in the near future.

FOURTEENTH MEETING.

Ju1y 23rd; the Vice-President, Mr. John A. Paterson, M.A., in the chair.

Letters were read from the Director of the Greenwich Observatory; from Dr. J. J. Wadsworth, of Simcoe, and from Mr. G. F. Townsend, of Austin, Texas. From the last named were received two very fine photographs of the lightning flash taken during a storm on the night of May 5th at Austin. These were much admired, and formed a valuable addition to the Society's album of photos.

It was moved by Mr. J. Todhunter, seconded by Mr. A. Elvins, that the following named gentlemen be added to the Committee appointed to consider the establishment of a popular observatory in Toronto: Mr. A. Harvey, Mr. R. F. Stupart, Dr. Larratt W. Smith, Mr. J. A. Paterson and Mr. G. E. Lumsden. This was carried.

It was moved by Mr. Pursey, seconded by Mr. Elvins, and resolved, that the Society express its sympathy with the family of Miss Bambridge, in the loss sustained by the death of a brother in British Columbia.

Mr. E. A. Meredith, LL.D., read some notes on

THE CHEMISTRY OF STEEL MAKING

As illustrated in the recently discovered, "Doherty Process," now in operation at Sarnia, where he had an opportunity of investigating it. Iron masters and authorities in chemical science regarded this as an epoch making discovery, and the methods employed as being as much superior to the Bessemer system as the latter was to the methods in common use when it was given to the world in 1857. In the Bessemer system, two distinct and separate processes are necessary to convert the pig-iron into steel. The first is to melt the iron in a furnace, and when melted, air and steam are forced into the molten mass of metal, in order to burn out the carbon, sulphur, and other impurities in the iron. The second step is to pour this molten mass, now converted into malleable iron, into another furnace, called the converter, where it is mixed with a certain proportion of fused carbonized iron containing manganese in the form of ferro-manganese, and converted into steel. By the Doherty process, on the other hand, the steel is produced by a single furnace, and more cheaply than malleable iron by the old method. A pipe containing air meets a pipe containing steam (regulated by a cock), just before they enter the furnace; the result is that hydrogen gas is set free, which, in combustion, gives out an intense heat, fusing the metal very rapidly, at the rate of about one ton in fifteen minutes. By the intensity of the heat also, the impurities, or mineral poisons, as they are called, the manganese, phosphorus, and sulphur are burned out. The molten mass, freed from all these impurities, is poured out in a white stream, at the rate of forty pounds in twelve seconds. The metal, when poured into the ladles, throws up innumerable jets of flame, caused by the burning of the carbon, and must be allowed to cool for some seconds before being poured into the moulds. Dr. Meredith stated that the inventor of the process claims that the steel so produced is harder and more ductile, and in other respects superior to that produced in the ordinary way, while it is very much cheaper, as the quantity of fuel used is only one-tenth of that required in the old method.

After the reading of Dr. Meredith's notes, the chairman called upon Mr. A. Elvins to read the paper which had been prepared for the Lunar Section meeting, but which it had been decided to receive at a general meeting, as it was, in chief part, an introduction to the study of the Moon which several members desired to take up.

The following is the text of the paper, which was illustrated by several of Mr. Elvins' own drawings, some photographs from the Society's collection, and the beautiful map of the Moon published by Messrs. Poole Bros., of Chicago.

THE MOON'S PRESENT CONDITION.

I am glad that a few of our members intend to form a section for the study of the Moon's surface. As in the case of most amateurs it was about the first object which attracted my attention, and it has lost none of its charms during the many years that have passed, since I was first struck with the grand scenery of its mountains, and the quiet appearance of its mighty plains; indeed the interest grows on me, as I think I can observe in its present condition some evidence of great changes which must have taken place in the ages long gone by.

Turn your telescopes on the Moon now as it is at its full; you see some parts of dazzling brightness, other spaces much darker.

We know that our satellite is seen by reflected sunlight ; the quantity of light which falls on one part must be about the same as that which falls on another part; let us examine these bright parts and see what they are. We shall have a far better view of these bright spots at the first or second quarter than at full Moon; the Sun is just rising on its central meridian and any elevations near this part (called the terminator) will cast shadows on the side *from* the Sun, while hollows will have shadows on the side nearest the Sun. A little after first quarter we see a bright marking running not far from north and south, extending quite a distance on the surface of the Moon. We easily see that on the side facing the Sun it is very bright, while on the opposite it is dark. The dark portions look like shadows cast by the bright ridge; is this the case, or are these dark spaces real differences in the colour of the surface? On the following night we will get an unmistakeable answer to our question. We now see that the sunlight stretches far out into what was quite dark last evening, and what we rightly suspected to be shadows are now much shorter, and we feel sure that the bright line is really a ridge of mountains, and the dark portion joining the bright hills are truly shadows thrown by those hills on the plains below. This conclusion will find corroboration as we see the shadows grow shorter and shorter every night, until the Sun shines down perpendicularly on the hills at full Moon, and then we fail to see any shadows at all. But all the proof that this ridge is really a mountain range is not yet exhausted; you will look after the Sun has passed the Moon's meridian (about the third quarter) and you will see the hills and shadows again, and now they are reversed; the shadows cover the side which was bright at the first quarter, and the side which was then in darkness is bright now.

This range of mountains, the lunar Apennines, is an example of many others which you will become acquainted with as you prosecute your systematic observations. We will notice another feature, these extended dark spaces which we first observed. These have a very different appearance from the mountains, they are for the most part flat plains, broken here and there by isolated hills and in some places by deep cavities. You will have examples of both on the floor of the sea of Showers (Mare Imbrium); Bessel and Plinius in the adjoining sea of Serenity are also of this class. Why are those extended plains so dark? Of course the hills might be expected to be brighter when the Sun is rising on them, but we see the plains less bright even at the full, when the Sun casts no shadows. Something in the nature of the *mare* itself must absorb part of the light. Again I am sure you will be struck by those great ring-plains, some more than 100 miles in diameter surrounded by high circular hills. You will see a large group on the terminator about the first and third quarter; each of these is a study in itself. At the full Moon we see many bright rays which converge at a point near the southern limb of the Moon and spread outwards from that point (Tycho) over a large part of the lunar surface; what they are is an unsolved problem, it will be one of the questions which you will seek to answer. I must cease description but did time permit I would be glad to continue on these lines. I wish to direct your attention to thoughts in relation to changes which have struck me during my observations. There are several instances in which circular walled plains occur on the borders of the dark spaces which I think have properly been called seas. Le Monnier on the border of the sea of Serenity, and Fracastorius, on the sea of Nectar, are of this class. Now in both these cases and in many others the wall has been in part broken down, and the breakage is on the side which enters the sea, or seems on the sea bottom, where it would have been washed by the tides and waves if they ever existed.

I cannot repress the conviction that the so-called seas were at one time filled with water, and that the walled plains were broken down by the action of the tides, and that the debris scattered (being in some cases still visible) where strewed by the water action in ages long past. Then again, we see some walled plains and craters which seem to be partly covered by some deposit; in the Mare Humorum and near Bullialdus are several cases of this kind; to me they have always conveyed the impression of gigantic submerged ring plains, and craters of smaller size, also submerged.

If we have been merely viewing the effects of water action, nothing is more natural than for us to inquire what has become of the water?

As to the origin of the features which we observe, I do not think we have any certain knowledge. Several theories have been published, but the one most generally entertained is what we may call the *volcanic*. This theory regards the mountain ridges and ring-plains, craters, etc, as the result of earthquake and volcanic action in past ages. Many of the lunar features certainly have this appearance, and this view is fully treated and sustained in an able paper (in the June number of the publications of the A. S. of the P.) by Prof. Suess, an eminent Austrian geologist. This paper should be carefully read. Another, which we may call the *meteoric* theory, is presented in a paper read and lately published by G. R. Gilbert of the American Geodetic Survey, who made an exhaustive study of the subject. He regards the chief features of the lunar surface as the result of the gathering up of a ring of meteoric bodies, which he thinks once surrounded the Earth, into one mass, and holds the lunar walled plains, craters, etc., to be the result of the heat produced by the impact of these masses when they struck the surface. This theory, strange as it may appear, is supported by close reasoning and a mass of experiments, which though not demonstrating the theory should be carefully studied as you follow your work in the lunar section.

The Moon having been formed, it must be studied in relation to the modifications resulting from the existence of great tidal action. The great mass of the Earth would produce tides in any body not perfectly rigid. Air and water would produce results which may still be traced if either or both of those ever existed there.

The glacial theory—I have long thought the possible existence of ice and snow may have had much to do with the present condition of the Moon's surface, and this question has been ably stated, illustrated and defended in papers by our fellow-member, S. E. Peal, of Asam, India, but as his views are so very similar to my own I shall not refer to them here, only to state that his paper "Lunar Surfaceing," is bound up in our 10th volume of Monographs, which I hope you will read carefully, and you will be well repaid.

In whatever manner the Moon's mass may have been formed, it is quite possible and I think quite probable, that sometime in the past it rotated on an axis in much shorter time than it revolved around the Earth. The axis of rotation might be somewhat inclined to the plane of its orbit. Under such conditions there could be no reason why the Moon should not have had an atmosphere, and oceans, lakes and rivers, as we have on the Earth. This state of things I think really existed, and if I am right, if it possessed large seas there must have been enormous tides. In consequence of the Earth's greater mass, its lifting power on the Moon's surface would be much greater than the Moon's power to raise a tide on the Earth. Sir Robert Ball in his very interesting book *Time and Tide* has shown clearly that our tides serve as a break to retard the Earth's rotation, and most certainly the lunar tides would have had a similar effect on the rate of the Moon, causing it to rotate more and more slowly, until at last its rotation in relation to the Earth ceased and in relation to space the revolution and rotation were completed in the same time. Such is the present state of the Moon. If a rigid bar were thrust through the body of the Moon and one end of it brought to the Earth that end would be at the centre of the Moon's orbit, and the Moon would always have the same side turned earthwards, (libration only excepted), which is known to be the fact. During the period in which it rotated more rapidly than it revolved in its orbit, the great tides in its seas and oceans would doubtless make sad havoc with the rocks forming its sea-shores. The rocks on the sea of Showers at the foot of the Apennines (which were doubtless the shore of that sea) are just what we would expect to find there as a result of tidal action, and the breaking down of the ring mountains on the side next the sea would be a natural result of the Moon's enormous tides. But as soon as the time of the Moon's rotation was retarded to such an extent as to cause the times of rotation and revolution to be equal, the lunar tides would no longer pass around the Moon but would remain stationary, and if the Moon became stationary, or ceased to revolve in its orbit, the raised water or tide would exist at the point nearest to the Earth. But the Moon has not ceased its onward motion, it still moves onwards in its orbit, keeping the same hemisphere earthwards all the time.

This motion would cause a centrifugal tendency in the atmosphere and waters of the Moon, which passing backward would form a high tide, opposite the one tide raised earthward by the Earth's attraction; the plus of atmosphere and waters raised on the radial line of the orbit would leave a minus of atmosphere and water at right angles to the line joining the Earth and Moon. The atmosphere and water would thus accumulate toward the Earth and on the more distant point, and be greatly reduced or removed altogether from the sides, or from points at right angles to the radius of the lunar orbit. This removal of the atmosphere would lower the temperature of the Moon's surface, the moisture in the remaining atmosphere would be thrown down as snow, and the waters on the surface would be changed into ice. The oceans would be converted into vast icefields, glaciers would gradually form on the highlands and pass downward into the plains below, so that what was formerly fertile and full of life has now become a barren waste, a world covered with *ice and snow*.

In studying the lunar surface you will be reading a history of great events long past, and multitudes of questions will be presented to your minds, some of which you may solve, others never. I can only assure you that you have entered on a very interesting study, and I think you will believe me when I say that I wish you many happy hours, and an increase of the pleasure which your studies in the past have given you.

Before I close I must ask you to read carefully Sir Robert Ball's interesting work *Time and Tide*; it opens up many questions full of interest and will enable you to see the cause of the retardation of both Earth and Moon by tidal action. Gilbert's paper on the cosmogony which you will find in our Library should be read with care; and the papers of S. E. Peal, on the glacial theory of the lunar surface are very suggestive, and must by no means be overlooked in following out your studies of the Moon.

Addendum.—We may reasonably ask, how is it possible that the Moon ever rotated more rapidly than it does now ? I see a difficulty here, if it had always been a satellite of the Earth, but perhaps it may not have always been such. In the distant past it may have revolved as a primary planet around the Sun, in an orbit differing but little from the Earth's orbit; its motion is such that at present its orbit always presents its concave side to the Sun, and I think Proctor suggested when here that it might be treated as a primary to advantage even now. It would then be so distant from the Earth that the latter would not control it and determine the time of its rotation. But some perturbation acting just at the critical moment, might have brought it to the point where its projectile force was balanced by the Earth's attraction, and so it might have taken its present orbit around our globe.

FIFTEENTH MEETING.

August 6th; the Vice-President, Mr. John A. Paterson, M.A., occupied the chair.

The following communications were received: From Rev. T. E. Espin, of Tow Law, England, who forwarded circular No. 42, of the Wolsingham Observatory; from the Secretary of the Meaford Astronomical Society, transmitting the annual fee for affiliated membership; from Mr. W. N. Greenwood, of Lancaster, England, who wrote as follows:

GLASSON DOCK, LANCASTER,
13th July, 1895.

The Secretary, Astronomical and Physical Society, Toronto, Canada:

DEAR SIR, —

Long ere this, I expected to be in a position to give you a satisfactory answer to your letter of the 20th April last, by sending your Society a paper which you kindly ask on Unification of Time. Circumstances are against me, however, and I cannot make them move so fast as I could wish, nevertheless, in a short time I hope to be able to accomplish the end in view, and my intentions *re* the paper in question. That you may see what I am doing in respect to it, I enclose you a circular letter that I have addressed to a large number of representative nautical men, individually and collectively, through their Societies in this country. It is their answer to the three or four questions asked that is delaying me just now. Please, therefore, consider that I have the matter in hand and at heart, and accept my apology for any seeming delay in acknowledging your kindness and the courtesy of your Society in requesting my opinion on such a subject as the Unification of Time.

Accept my thanks also, for the copy of your Journal so kindly sent. I find it most interesting reading, and until I fulfil my obligation to your Society, believe me,

Yours faithfully,
H. NELSON GREENWOOD.

Mr. Elvins presented a drawing of a part of the lunar surface; he had chosen the sea of Nectar as illustrating another example of what he believed to be the evidence of the action of water in past ages in breaking down the wall of a ring plain. Mr. G. G. Pursey reported having observed on August 5th, between forty and fifty spots on the Sun's disc. He thought these had chiefly broken out on the side of the Sun turned towards the Earth. Mr. Miller had observed these also, but had noted among them some five or six which had come round the limb by rotation. Mr. Miller had also observed on the evening of August 5th, a very brilliant meteor, giving the appearance of a translucent glowing cloud rather than that of a solid body raised to incandescence. He estimated the diameter to have been about fifteen minutes of arc. The meteor had passed between γ and δ Ursa Majoris and Arcturus.

Mr. Phillips then read some notes on the law of the "conservation of areas," and maintained that there was no evidence that this had been followed in the evolution of the solar system, as it must have been if the nebular hypothesis were correct. Mr. Phillips held that the equations which apparently prove that the Sun rotated in the same time as a given planet now revolves when it was extended out to the latter's orbit, are entirely useless and have no bearing whatever on the question. He instanced particularly the Earth and Moon system and said that no matter what value be given to the rotation of the Earth at present we have always the same result at the distance of the Moon. He held, therefore, that there is no real connection at all between the axial rotation of the Earth and the sidereal revolution of the Moon. The discussion became general and several members took part; the views of some authoritative writers on this question were read adverse to Mr. Phillips. The latter stated, however, that these authors were most certainly in error.

SIXTEENTH MEETING.

August 20th; Rev. C. H. Shortt, M.A., occupied the chair.

It was moved by Mr. Andrew Elvins, seconded by Mr. Arthur Harvey, F.R.S.C., and

Resolved, That as some mark of the appreciation of this Society of the original work done and of the important results obtained by him in the field of spectroscopy, Prof. James Edwin Keeler, D. Sc., of Allegheny, Pa., be and is hereby elected an honourary member of this Society in the room and stead of Prof. Daniel Kirkwood, LL.D., deceased. This was carried unanimously.

On instructing the Secretary to enrol Prof. Keeler as an honourary member, the Chairman stated that the Society would be highly honoured by having such a distinguished astronomer so intimately connected with it, one who had on many occasions shown the utmost courtesy to its members.

A letter was read from Mr. H. C. Howard, of Winnipeg, making inquiries regarding the work of the Society, etc. The Secretary was instructed to forward to Mr. Howard a copy of the Society's last publication, and to request him to endeavour to form a branch of the Society in his locality. Mr. Thos. Lindsay stated that he had during the past week received a visit from Mr. W. J. L. McKay, of Orangeville, and in company with him had called upon several of the members and also at the Toronto Observatory. Mr. F. L. Blake, Chief Observer, had very kindly placed the large telescope at the service of his visitors. Excellent views of Venus were obtained, at 3 o'clock in the afternoon. A power of 500 was used with very fair success; a power of 300 gave a most satisfactory view of the planet, surface markings being clearly seen.

Mr. McKay was elected an associate member of the Society.

Several members reported having observed the Perseid Meteors, between August 10th-13th. The display had not been particularly noticeable but some very fine meteors had been seen by Mr. W. B. Musson on the evening of August 10th. Mr. Pursey reported that the solar surface was less active than it had been earlier in the month. Two groups were near the middle of the disc, four or five spots in each, faculæ not very conspicuous.

Mr. Harvey read some interesting notes on the rings of Saturn. He had studied the drawing made by Miss Brook at Dr. Wadsworth's 12-inch reflector and had also observed with his own 3-inch refractor, having faintly glimpsed the concave outline of the shadow of the ball upon the ring. By a series of diagrams and a model Mr. Harvey demonstrated that, granting the accuracy of the drawing, a section of Saturn's ring must be either lenticular or elliptical—the ellipse having its major axis perhaps eight or ten times as long as the minor. It was shown from the principles of projection that if the rings were flat the edge of the shadow would, in effect, be elliptical, with curvature toward the planet. Following Prof. Keeler's confirmation of the theoretical proof that the rings are composed of solid bodies, the inner moving faster than the outer, this observation of the shadow and the deduction therefrom appeared to reveal an interesting fact.

SEVENTEENTH MEETING.

September 3rd ; Rev. C. H. Shortt, M.A., in the chair.

A cordial letter was read from the editor of *Knowledge*, London, Eng., who had kindly forwarded specimen copies of the periodical to the members of the Society. Special attention was called to the plates reproducing Dr. Roberts' photographs of the sidereal heavens.

The Corresponding Secretary read the following letter from Prof. James E. Keeler :—

ALLEGHENY OBSERVATORY, ALLEGHENY, PA., Aug. 24, 1895.

G. E. Lumsden, Esq., Corresponding Secretary of the Astronomical and Physical Society of Toronto:

MY DEAR MR. LUMSDEN,—Your letter, informing me that the Astronomical and Physical Society of Toronto elected me an honorary member at its last meeting, has just been received. It is needless to say that I value highly this signal mark of the Society's approval, and I beg that you will convey to the members my heartfelt thanks, and the assurance of my sincere appreciation of the honor they have done me.

With regard to my spectroscopic observations of Saturn, it is proper to point out that Prof. Seeliger, in a very interesting article in the *Astronomische Nachrichten* (No. 3,295), has taken exception to my conclusion that these observations afford a proof of the meteoric constitution of the ring, as distinguished from a confirmation of the accepted views. I have lately written a note, which will probably be printed in the *Astronomische Nachrichten*, in defence of my original position; at the same time I have no intention of claiming for these observations anything that is not justly their due. What their value may be I am well content to leave to the estimation of others.

For some time past I have been engaged (among other things) in experiments on photographing planetary spectra, and have succeeded in obtaining excellent photographs extending to a considerable distance below the D lines. The dispersion is pretty high (the same as that employed in my Saturn photographs) and the D lines are widely separated. These spectra, therefore, include the principal telluric bands of water vapour about which there has recently been so much said in connection with the spectrum of Mars. At the next opposition of Mars all the questions relating to the bands of water vapour in its spectrum can be settled by photography. Unfortunately Mars was out of reach before my experiments began. The observations, however, to be of real value as a test of the existence of an atmosphere, will probably have to be made in some more favoured climate.

Yours very sincerely,

JAMES E. KEELER.

A report of the work of the Lunar section was received. The last meeting had been held at Mr. Lumsden's residence, when his 10¼ inch reflector had been very successfully used in the observation of lunar features. A copy of the work on the Moon by Mr. T. Elger, F.R.A.S., had been shown to the members and much admired. The maps were thought to be of greater excellence than any yet published, while the style and arrangement of the text were such as to commend the whole work at once to the amateur and the more advanced student. Mr. W. B. Musson, referring to the work on the Moon which amateurs might engage in, pointed out the advantages to be gained, as he had learned through experience, by studying a small portion of the lunar disc at one time and not attempting more until that had been thoroughly learned.

Mr. A. F. Miller read some notes on observations of the Sun and especially of the large spot then upon the solar disc. On September 1st the spot had been well seen with the eye unaided. The umbra measured 16,240 miles in length, by 10,680 in width. A peculiarity was a bright bridge extending partially across the middle of the umbra. This measured 6,500 miles in length by 4,000 in width. A drawing of this object was shown by Mr. Lumsden, who had also observed it in his reflector.

THE UNIFICATION OF TIME.

Mr. G. E. Lumsden addressed the Society in review of the work of the Joint-Committee on the Unification of Time and presented the various reports which had been received through the Governor-General's Office from the Colonial Secretary, embodying the replies of the Ephemeris-publishing nations interested in the proposed change in Time Reckoning.

[COPY.]

UNITED STATES OF AMERICA.

WASHINGTON, 26th October, 1894.

MY LORD,—With reference to Your Lordship's circular-despatch of this series of the 22nd ultimo, transmitting a copy of a Memorial from a Joint-Committee, appointed by The Canadian Institute and the Astronomical and Physical Society of Toronto, advocating that the Astronomical and Nautical Days should be arranged everywhere to commence at Mean Midnight and that this change should take effect on the first day of the next century, I have the honour to inform Your Lordship that I brought the matter to the notice of the United States' Government. The Secretary of State in his reply, informed me that the members of the United States' Naval Observatory are adverse to the Canadian proposition and he furnished me with a copy of their report, which I have the honour to enclose herewith to Your Lordship. I have, etc.,

(Sgd.) W. E. GOSCHEN.

The Earl of Kimberly,
etc. , etc. , etc. , etc.

[ENCLOSURE.]

UNITED STATES' NAVAL OBSERVATORY,
WASHINGTON, 16th October, 1894.

1. In compliance with the Bureau's second endorsement of the 8th instant, covering a Memorial relative to the Astronomical Day, transmitted through the regular official channels from a Joint-Committee appointed by The Canadian Institute and The Astronomical and Physical Society of Toronto, we have the honour to submit the following report :—

2. The Memorial, in question, proposes the Unification of the Astronomical, Nautical and Civil Days by making them all commence at midnight. This proposal was originally made by The Washington International Meridian Conference of 1884, and was, subsequently, very carefully considered by the official astronomers of the leading countries which publish astronomical ephemerides, but the general consensus of opinion was so far against the proposed change that no steps were taken to bring it about.

3. The present practice of counting astronomical mean time from noon, or, in other words, from the transit of the Sun across the meridian is in exact conformity with the practice of counting sideral time from the transit of the vernal equinox across the meridian, and both systems have been adopted because of their superior convenience for astronomical purposes. The resulting advantage will not be less important in the future than in the past, and to make any change in either system would only introduce needless incongruity.

4. The change recommended by the Joint-Committee would necessarily involve the ephemerides in other systematic changes relating to sideral and solar time, respecting which the Memorial makes no suggestions, but which, we think, would be very difficult to effect without causing confusion.

5. As the astronomical observations and ephemerides made prior to the year 1900 must continue to be used for many centuries, the proposed change would greatly complicate the work of astronomers by compelling the constant employment of two different systems of counting days, viz., one for all observations prior to 1900, and another for all observations subsequent to that date.

6. All navigators are now accustomed to nautical almanacs in which the hours are reckoned from noon, and the introduction of new ones in which the reckoning was from midnight would be so confusing to them that it would probably cause many errors in the determinations of the positions of ships.

7. We believe the Memorialists are mistaken in supposing that the proposed change offers any advantages sufficient to compensate for all these inconveniences. The use of astronomical time is confined to astronomical work, and cannot possibly affect the people at large because they have never anything to do with it. Why

then make a change which will inflict permanent inconvenience upon those most concerned without benefit to others?

8. In view of the above facts, we are decidedly opposed to any change in the existing mode of reckoning astronomical time, and, therefore, recommend that no departure be made from the present system.

(Sgd.) F. V. McNair,
Captain U. S. N., Superintendent of the Naval Observatory.

(Sgd.) S. Newcomb,
Professor of Mathematics U. S. N., Director of the Nautical Almanac.

(Sgd.) Wm. Harkness,
Professor of Mathematics U. S. N., Astronomical Director of the Naval Observatory.

[copy.]

Spain.

Madrid, 10th November, 1894.

My Lord,—With reference to the circular from Your Lordship's department of the 22nd September, I have the honour to transmit herewith translation of a note from the Spanish Government, stating that they are prepared to adopt the scheme for the Unification of the Astronomical and Nautical Days in 1901, if it appears that the majority of nations who publish astronomical ephemerides are desirous of the change. I have, etc.,

(Sgd.) H. Drummond Wolff.

Earl of Kimberly, K.G.,
etc., etc., etc., etc.

[enclosure]

Ministerio de Estado, 7th November, 1894.

Your Excellency,—I have the honour to inform Your Excellency in answer to a despatch from the Embassy, dated 25th September last, in regard to the Unification of Nautical and Astronomical Days, that my colleague, the Minister of Marine, to whom I referred the matter, without for his part proposing or desiring the said change, is quite prepared for his Government to accept the change in 1901, if before 1896 it is shown that the majority of the ephemerides' offices which regularly issue nautical almanacs are in favour of it. I have, etc.

(Sgd.) Alexander Groizard.

His Excellency Sir H. Drummond Wolff.

[copy.]

France.

Paris, October 16th, 1894.

My Lord,—I have the honour to inform Your Lordship that I have addressed a note to the French Government in the terms of Your Lordship's despatch-circular of this series of September 22nd, and that the matter has been referred to the Ministers of Marine and Public Instruction. I have, etc.,

(Sgd.) C. Phipps

To Her Majesty's Principal Secretary of State for Foreign Affairs.

No official reply having been received from France, at Mr. Lumsden's request, Mr. Arthur Harvey, F.RS.C., read the following summary of the report to the Ministry by the Bureau of Longitudes, published at length in *Cosmos*, February 2nd, 1895 :—

1. The Bureau of Longitudes is favourable to the principle of the reform proposed by the Canadian Institute for the change in the beginning of the Astronomical Day.

2. The Bureau thinks that as the Lords of the Admiralty observe, the reform would not be efficacious without an understanding among the Governments which publish the principal ephemerides.

3. Finally, considering that the unification of time will not be complete until the Civil Day is reckoned from 0 to 24 o'clock, as is done in Italy, the Bureau hopes this reform will be realized at the earliest possible date.

In the body of the Report, which is interesting and exhaustive, the Bureau says that if the *Connaissance des Temps*, the *Nautical Almanacs* of Greenwich and Washington, and the *Berliner Jahrbuch* were to adopt the unification of time simultaneously, all other publications would have to follow them.

Some members of the Bureau were disposed to advise that all risks be run, and the new system adopted in the *Connaissance des Temps*, but the majority thought that the confusion which might ensue would be too considerable, as posterity ought not to have the trouble of making corrections for each country in reducing astronomical observations.

That the time would come when the reform would be made they did not doubt, for the demand for it becomes more and more imperative every day, and it cannot come too soon.

[COPY]

HUNGARY.

VIENNA, 22nd May, 1895.

My Lord,—With reference to my despatch of this series No. 23, of the 3rd further note and its enclosures, as marked in the margin, with regard to the proposed amalgamation of the Astronomical and Nautical with Civil Days. I have, etc.,

(Sgd.) Edmund Monson.

Her Majesty's Principal Secretary of State for Foreign Affairs.

[COPY.]

FOREIGN OFFICE, 28th May, 1895

Sir—I am directed by the Secretary of State for Foreign Affairs to transmit to you, to be laid before the Secretary of State for the Colonies, a despatch as marked in the margin, from Her Majesty's Ambassador at Vienna, on the subject of the Unification of the Astronomical, Nautical and Civil Days. It is requested that this despatch may be forwarded in due course to the Admiralty. I am, etc.,

(Sgd.) E. GREY

The Under Secretary of State, Colonial Office.

[COPY.]

DOWNING STREET, 10th June, 1895.

My Lord,—I have the honour to transmit to you for the information of your Government, with reference to previous correspondence on the subject, copy of the documents noted below, regarding the proposed Unification of Astronomical, Nautical and Civil Days. I have, etc.,

(Sgd.) R. H. MEADE (for the S. of S.).

The Officer administering the Government of Canada.

(Enclosures.)

[COPY.]

Translation of a Report of the Sub-Commission of the Third Section of the Hungarian Academy of Science of Budapest with regard to the Memorandum of The Canadian Institute and of The Astronomical Society, respecting the Unification of the Astronomical and Nautical with Civil Day:

BUDAPEST, February 18th, 1895.

The Commission appointed by the two above-mentioned Academies of Science, and consisting of seven members, under the presidency of Mr. Sandford Fleming, proposes putting an end to the numerous and often serious mistakes, caused by the three-fold system of reckoning the Civil, Astronomical and Nautical Day, and, to this end, moves that, from January 1st, 1901, the Astronomical Day, like the Civil Day, should begin at midnight. This concession on the part of astronomers should find a response in the adoption of the system numbering the hours from 0 to 24, as it now exists in Italy, Canada and the East Indies. The Commission, on April 21st, 1893, put the question to a large number of astronomers all over the world, whether they were ready or not to accept this alteration. Of these, 108 answered "Yes," and 63 "No." Unfavourable answers were received only from Germany, Norway, Holland and Portugal. The Sub-Commission appointed by the Third Section of the Hungarian Academy of Science fully shares the view taken by the Commission of Toronto, and deems it advisable that from the first day of next century there should be a unification of the Civil, Astronomical and Nautical Day, which, however, would make it necessary for the Nautical Almanacs, and all such year books published for nautical purposes, to reckon the day accordingly.

(Sgd.) NICOLAUS KONKOLY.
DR. GUSTAV KONDOR.
AUGUST HELLER.

[copy.]

Translation of an extract from the protocol of the sitting held by the Third Section of the Hungarian Academy of Science on February 18, 1895:–" 6. The proposal made to the Academy by The Canadian Institute, and referred to its Third Section, respecting the unification of time reckoning, has been dealt with by the Sub-Commission appointed to consider it, whose report was read out. The Section adopts this report unanimously, and presents it to the General Assembly."

[COPY.]

May 16th, 1895.

In continuation of their note of the 18th ult., the Ministry for Foreign Affairs has the honour to transmit to Sir E. Monson, translation of the report of The Hungarian Academy of Science respecting the Memorandum of the Committee of The Canadian Institute and of The Astronomical and Physical Society of Toronto, dealing with the Unification of Time reckoning.

(Sgd.) CZIRAKI.

His Excellency Sir E. Monson, G.C.M.G.

[COPY.]

Translation of an extract from the protocol of the sitting held by the General Assembly of the Hungarian Academy of Science, on March 19th, 1895:–"72. The Third Section moves the acceptance of the innovation proposed by The Canadian Institute and The Astronomical and Physical Society of Toronto, whereby the Day, Civil, Astronomical and Nautical, should begin at Midnight from January 1st, 1901, onward, and also that the hours of the day should be numbered from 0 to 24." This motion passed.

[COPY.]

BRAZIL

DOWNING STREET, 15th March, 1895.

My Lord,—I have the honour to transmit to Your Lordship, for communication to your Government, with reference to previous correspondence, a copy of the under mentioned document. I have, etc.,

(Sgd.) R. H. MEADE, for the S. of S.

The Officer Administering the Government of Canada.

[ENCLOSURE]

RIO DE JANIERO, 30th January, 1895.

My Lord,—With reference to Your Lordship's circular-despatch of this series of the 22nd of September last, on the subject of the arrangement of Astronomical and Nautical Days during the next century, I have the honour to report that I have been informed, by the Minister of Foreign Affairs, that the Brazilian Government have decided to adhere to the proposed arrangement, which they will adopt in all publications of a scientific or technical nature from the 1st of January, 1901. I have, etc.,

(Sgd.) GEORGE GREVILLE

The Earl of Kimberly, K.G., etc., etc., etc., etc.

[COPY.]

MEXICO.

Mexico, 23rd January, 1895.

My Lord,—On receipt of Your Lordship's despatch-circular of this series dated 22nd of September last, H. M. Charge d'Affaires invited the Mexican Government to state their views as to the desirability of the Astronomical and Nautical Days being arranged to commence at Mean Midnight from the first day of the next century, as advocated by The Canadian Institute and The Astronomical and Physical Society of Toronto, copy of the Memorial of the Joint-Committee being annexed for their information. I have now the honour to forward to Your Lordship copy and translation of a note which I have received in reply from the Mexican Minister for Foreign Affairs, informing me that in the opinion of the Secretary for War and Marine, no objection exists to the adoption of the course advocated by the Joint-Committee above referred to. I have, etc.,

(Sgd.) HENRY MEVILL DERING.

The Earl of Kimberly, K.G.

[ENCLOSURES.]

Foreign Office, Mexico, January 19th, 1895.

M. LE MINISTRE,—With reference to the note which I sent to your Legation on the 9th of November last, I have the honour to forward to Your Excellency copy of a communication from the Department of War and Marine, from which you will observe that it finds no difficulty in approving the proposal made by the Joint-Committee of The Canadian Institute and The Astronomical and Physical Society of Toronto, which has for its object the abolition of the present Astronomical Day, arranging for it to commence, from the first day of the next century, at Mean Midnight in all parts of the world. I avail myself, etc.,

(Sgd.) IGN. MARISCAL.

His Excellency, Henry Nevill Dering,
etc., etc., etc., etc.

MEXICO, January 16th, 1895.

MINISTRY OF WAR AND MARINE, Mexico, Department of Special Staff Corps Fifth Section, No. 16, 719

In reply to your courteous despatch, dated the 10th of November last, in which you state that Her British Majesty's Consul sent to your Department note enclosing a Memorial of the Joint-Committee of The Canadian Institute and The Astronomical and Physical Society of Toronto, advocating the Unification of the Civil, Astronomical and Nautical Days, I have the honour to inform you that as far as this Department is concerned, no objection exists to the approval of the proposal for the said unification in the terms suggested, already approved by the majority of astronomers of the various nations of the world, to take effect of the first day of the coming century.

(Sgd.) J. M. ESCUDERO.

To the Minister for Foreign Affairs, Mexico.

A lengthy reply had also been received from the Government of Austria, reviewing the proposal, and finally agreeing to follow the example of the Lords Commissioners of the Admiralty regarding the Ephemeris.

Mr. Lumsden then stated that the text of the Third Report of the Joint-Committee would be read at the next meeting of the Society.

Mr. Arthur Harvey, F.R.S.C., then read the following paper on

THE CONTRAST BETWEEN LUNAR AND TERRESTRIAL FEATURES.

Until we disprove the conclusions of Laplace, which thus far present the only theory which satisfies the conditions observed in the celestial universe, we shall do well to keep our speculations within the bounds of his nebular hypothesis, viz.: that the Sun's atmosphere once extended to the orbit of the furthest planet, and that as this gaseous agglomeration rotated, and the central parts of it contracted, successive spherical shells were under the influence of centrifugal force thrown off or left behind—equatorially Placed with respect to their parent Sun. They did not maintain themselves as rings, but, in due course, became condensed into the several planets, in whose case the process was repeated on a smaller scale, the most perfect of the secondary systems being the microcosm of Saturn, a belted orb which is ringed with meteors and controls eight circling satellites.

Each shell generally produced one planet, but that interior to Jupiter broke under his influence into hundreds of planetoids, and when the ring was thrown off which has become our dwelling, either two centres of attraction were set up in close proximity, or, as Laplace believed, the Earth, yet nebulous, separated into two. Twin planets thus came into being, Terra and Luna, with present diameters of 7,926 and 2,160 miles respectively.

If the Moon was whirled off by the Earth, her materials, drawn from the outer layers of a condensing body, would be comparatively light. If she was formed about an independent centre of attraction, it was the feebler one, so that the particles directed to it by gravity would be the tightest and least numerous. Either view is consonant with the fact that the density of the Moon is only 0·63 to the Earth's unity, while 0·165 represents the force of gravity upon its surface, compared with unity for the Earth; little more than half what that force is at the surface of Mars. A much smaller portion of its core than of the Earth's may consist of metals.

In consequence of Prof. Keeler's spectroscopic confirmation of Maxwell's mathematical proof that Saturn's rings are not coherent, and that their parts revolve with different velocities (the outer ones the slower), nebulæ will soon be subjected to new discussions. The successive births and the order of them will be questioned. The rotation of the whole, as one coherent though vaporous body, may prove to be an unnecessary and erroneous assumption. It may be found that instead of a slowly turning Sun of enormous size, increasing in velocity as he contracted, there was first formed within a spiral nebula a small body with a rapid gyration, becoming slower as it drew in matter, harmoniously sorting into a concordant family of planets what was beyond its power to absorb.

We can well suppose that the Sun-nebula was glowing throughout when it gave off these shells, and that as each planet had its birth, its substance blazed out as new stars are known to do. The Earth and the Moon were then radiant with independent light and heat, though, like all the other planets save, perhaps, Jupiter, they have long since settled down to a dull existence, with merely reflected splendour. Radiation into space has cooled their surfaces, though, at first, they, perhaps, had photospheres and chromospheres like the Sun's.

Cooling, on the Moon, would cause slag to form upon its surface, and the action of currents in the melted fiery mass would make such scoriæ collect in the region of the poles. The aggregation of ice in the polar districts of the Earth is quite analogous. Examine the Moon through any glass, and resist if you can the impression that you are looking at a quantity of such floes, which have drifted together to form extensive rough and broken surfaces. The greatest collection is at the south, but Neison correctly speaks of the region about Timœus, near the northern pole, as "a wild labyrinthical mass of mountains." Where such areas were driven together, huge mountains might well be heaped up, and at such junctions, presenting the lines of least resistance to eruptive forces, volcanoes would develop, if at all. But it should not be assumed that lunar craters were ever volcanoes similar to those which now exist upon the Earth. Their great size and the regularity of their circular outlines forbid the idea. They are divided, it is true, into walled plains, ring plains, crater plains and craters, but the division is very arbitrary. The walled plains, which differ, indeed, but little from small *maria* are from 40 to 150 miles across, the ring plains from 20 to 60, the crater plains a few miles less, and the craters from 4 to 12, but, in Neison's language, " no distinct hue of separation is possible." Then we have craterlets and crater pits. If all or many of these were caused by eruptive forces, there must have been great collections of the vapors of metals and other elements pent up under the stiffening surface, which raised it in huge dome-like shapes to burst like other bubbles—the sides remaining like circular walls, the centres falling back and remelting at a lower level. Such a process, repeated at the same vent, would give rise to the terracing observed within some craters, and the lava would congeal into a smooth floor, far below, when the eruptions had subsided. This congelation, proceeding from the edges towards the centre, might well leave room there for the egress of enough material to form the *mamillæ* so often noted. If such vapors found their way out without a violent outburst, the consequent subsidence would form circular pits, without walls, or with walls of slight and gentle elevations, where the edge of the abortive volcano bad been. The expansive, force of pent up vapors must be as violent upon the Moon as here. It is irrespective of gravity, nay, we may well conceive that, exerted in such a way, it would lack the corrective forces of gravity and air-pressure which here exist. Under several thousand feet of crust it would have enormous power, and the crust being lighter than that of the Earth, volcanoes on the Moon would be larger than they could be here, not only in proportion but in actual size.

As a variation of this theory we may suppose that such vapors, on meeting at the surface the cold of space, might condense and build up the walls we see, which would be circular, for in the absence of air no wind-influence would carry off the products of condensation, which would fall with great regularity as to distance around the central duct. In the later stages of the existence of such a lunar volcano, these condensing vapors would form the *mamillæ* or central mounds which exist in most of them.

It is evident that some craters, with more or less distorted outlines, are more ancient than others. The circular ones were plainly formed after the crust had become well solidified, and those we see upon the low lying plains or *maria* are perhaps the most recent of them. all. They are surely of later date than the *maria*.

The *maria* are badly miscalled. They seem to be comparatively flat depressed surfaces of rock, and it is quite intelligible that, as the cooling of the Moon's surface proceeded, some shrinkage in its bulk would occur, so that the spaces last left fluid would sink a little lower in level than the shores. The fiery pools, however, as the centuries stole on, lost the heat of youth and solidified also, as our Kilauea might do now. This derives some probability from the fact that in most craters the floor is lower than the surrounding level. Chacornac is reported by Neison as saying that the surface of the great grey *maria* appears to have been fluid long after the principal formations of the Moon had become permanently rigid.

If recent deductions from the kinetic theory of gases, as to the ultimate velocity of their molecules, are well founded, there can never have been air on the moon like ours, and hardly water. Free hydrogen leaves even the Earth, for gravity here is not strong enough to overcome the velocity with which its molecules are credited. Free oxygen and nitrogen would leave the Moon. This decides adversely the possible existence of an air like ours—a mixture of oxygen and nitrogen with a little argon, helium and carbon dioxide. As hydrogen has been seized by oxygen on the Earth before escaping, to form water, so oxygen might possibly be seized by carbon on the Moon, and give us an envelope of carbon dioxide. But no evidence of it is established, nor can the writer see the least sign of water. The first thing to examine is the testimony afforded by the Moon's superficies. Now terrestrial valleys begin with babbling brooks, narrow and shallow, they receive many branches, they widen and deepen, and finally debouch into the ocean, with broad estuaries, on each side of which there are often noble cliffs. Lunar valleys are not infrequent. Loewy and Puiseux describe several, *e.g.*, the valley of the Alps to the west of Plato—that which extends south east of Rheita—those between Herschel and Hipparchus and between Bode and Ukert—all these being among the deepest. But they in no respect resemble terrestrial valleys. They are almost straight, have no branches, maintain about the same breadth and seem to be equally depressed from one end to the other. No place can be seen where the products of erosion have been deposited, nor are they anywhere even partially filled up with alluvium; with high powers that seem to have a flat floor, "as if the crust had been cracked to the depth where the interior was fluid, and this had afterwards solidified." These celebrated celestial photographers find that many lunar valleys are roughly parallel, such as the three which touch the southern edges of Arzachel, Albategnius and Ptolemy, and the two others which touch the craters of Reaumur and Flammarion. Another system, at an angle of about 70° with the above, is formed by valleys which are tangential to both sides of Albategnius and Alphonsus, and the rampart of Ptolemy to the north-east, and stretch towards Moesting. The circle of Albategnius is thus inscribed in a parallelogram of valleys, and even the sharper angles of the parallelogram are cut by fairly visible clefts which make it look like a regular hexagon. The vast depression which Tycho occupies has the well defined form of a parallelogram. In the northern legions Eudoxus is enclosed between four rectilinear furrows—two being similar in direction to the valley of the Alps—and examples could be multiplied. These cracks lead one to think of cleavage planes in a sort of rough crystallization and probably result from contraction of the superficial layers. In all this, one is forced to observe how unlike it is to a water-worn surface, for these valleys or clefts, sometimes called rills, stride across mountain and plain with a directness quite unlike the course of a tortuous terrestrial river. They never extend from mountain top, down widening valleys, in a tortuous course, to end in a *mare*. The opinions of Messrs. Loewy and Puiseux are very valuable, for their photographic appliances are almost perfect. To photograph the Moon, one must not only have a telescope mounted on principles like the usual equatorials, but the movement must be adjustable to the varying inclination of the Moon's orbit to the ecliptic, and the clock so set as to suit the eastward motion of the Moon among the stars. It requires the best skill of the mathematician and the instrument maker combined to meet these requirements, but, notwithstanding the rapidity of our sensitive plates, only when they are fulfilled can the perfectly sharp negatives be taken which are necessary to show the minute features on which the progress of selenography must henceforth depend. Such plates are now examined with high powers, or are many times enlarged by photographic methods, or are thrown upon screens by enlarging lanterns, and the French observers named think they have about reached the limit when the toughness of the granular surface of a plate becomes the chief obstacle to further distinctness of small features. This roughness of grain it is which mars to some extent the well-known photographic enlargements of Weinek from Lick Observatory negatives.

It is only just to say that Prof. Pickering describes some short clefts, widening slightly from origin to ending, that ending being usually in a pear-shaped craterlet. He hints at these as possible channels for a small quantity of fluid. I have observed such clefts, and since seeing his remark have re-observed them. They are not numerous, and they do not convey to me the idea of water courses at all. The craters in which they end are probably among the oldest, for they are the most distorted, and with that distortion the ravines may be connected. These clefts are nearly as wide as the craters, and are broad at their beginning.

The first solidification of the Earth may have exhibited phenomena not dissimilar to those we notice on the Moon, except as modified here by air, but almost every earthly thing of such a nature has long since been masked by the products of erosion. Sedimentary rocks have covered the primitives, oxygen has rusted the crust for enormous depths, water has not only covered the greater part of the surface, but through wave, rain and ice action has profoundly modified the archæan forms. It is, however, interesting to note that in places where glacial processes have cleaned off everything down to the primitive rocks, traces of melting and re-melting, of the forming of domes and bosses, and of the cracking of the surface in the early youth of our world have been revealed.

Writing of the Rainy lake region of Ontario, Prof. A. P. Coleman, Geologist to the Ontario Bureau of Mines, says in this year's report :— "The statements of Lawson regarding the eruptive relationship of the gneisses to the Huronian rocks above have been fully borne out by our investigations in the region; and we must suppose that by the deposit of perhaps ten miles' thickness of Couchiching and Keewatin rocks combined, the isogeotherms or levels of equal temperature gradually ascended towards their normal distance from the surface, and as a result the once solid Laurentian was brought into a condition of igneo-aqueous fusion or semi-fusion—welling up at some points and at others allowing the schists resting upon them to sink into sharp anticlinal folds. That fragments of the solid schists were thus broken loose and floated away, and that apophyses, irregular veins of molten matter, were injected into fissures of the rocks above, any one may satisfy himself by a day's canoeing among the islands on the eastern part of Rainy lake."

In another part of his report he speaks of "Laurentian gneisses, fluid enough to carry off blocks of the already solid green schist," of "a layer of Huronian, lifted on the shoulders of the upswelling gneiss or granite * * in part dissolved and lost to view in the molten rock beneath" ; of "granite bosses, many of them come up through the Laurentian granitoid gneiss * * an oval patch more than ten miles long * * on Bat lake,"—the roots, according to Lawson, of an old volcano.

Now, as the oldest rocks we know upon the Earth are these gneisses, with their early derivatives, pyroxenic rocks and granites, through which traps or diabasic (sometimes called basaltic) rocks have been forced up in dykes, it is not unnatural to suppose that most lunar rocks are similar to them, and resemble the forms familiar in the northern lake regions of Canada, and in the boulders which bestrew the whole of our country.

To those who are intimately acquainted with the great regions north and west of our great lakes, where there are hundreds of square miles of exposed primitive rocks, the volcanic cracks and dykes and dioritic overflows and veins and faultings will at once disclose analogies to the lunar surfaces, in some cases very striking. Here, however, the similarity appears to end; that is, at the period of first transition from a fluid to a solid state.

It is not easy to conceive of the processes which followed the solidification of a planet without air pressure or water currents. There could be no rains, no winds, none of the great chemical changes due to hydrogen or oxygen, no consuming fire or darkening smoke. As the Moon's surface hardened into stone, so it remained without any of the numerous changes we observe on Earth, due to erosion, sub-aerial action and vegetation. There could be no shales, limestones, freestones, sands or clays. Here we have always had oxygen to unite with carbon, and water or vapour of water to dissolve calcium. This has given us our enormous supplies of limestones, including chalk and marble, and the removal of the carbon dioxide by union with lime has made life possible here in its present forms. The only changes on the Moon would be those due to expansion from internal or from solar heat, followed by contraction through its loss. The former cause is, however, alone sufficient to account for the contortions of the lunar surface. It is just conceivable that calcium and some other metals, once vaporized, now covers portions of the lunar primitives with dust, and that in the deeper valleys and *maria* some sulphur, or perhaps carbon, may have combined therewith, not because they settled there from an atmosphere, but because they issued as vapours from these localities, and were at once condensed. This, as well as differences in the composition of the rock first solidified, and that which congealed later, may cause the variations in light-reflecting power, or shade, or even in colour, which are discernible. The differences in colour, by the way, are, jealous, illusory.

The constancy of these features is another evidence of the absence of water. If there were any upon the surface, it would, during the lunar night, be ice or snow, for all authorities agree that the temperature must run down to –200° centigrade, or thereabouts. "Our atmosphere," says Prof. Hallock, of Columbia College (*Science*, August, 1895), "acts like a valve, transmitting in almost undiminished strength the short, quick waves of energy radiated to us from the Sun, but refusing, absolutely, to return the long, slow waves in which the Earth tries to radiate the energy back into space; without this atmosphere we should all have been frozen long ago." This is a conclusion reached by Lord Rosse and others, many years since, but the language indicates its recent confirmation by means of the bolometer. Notwithstanding this, there might be some melting during the fourteen days of continuous sunshine—some change from a uniformly white due to a mottled shade, some variation of brilliancy. But as the surface shows at the beginning of the long lunar day, so in its broad features it continues through noon and evening, the only apparent change being in the slowly increasing or decreasing length of the black shadows of the tremendous cliffs. The only escape from this argument is the admission that what was water is now quite continuously frozen. But if so, it seems that its existence as water must have been too brief to produce any noticeable effects. Subjected to the cold of space, it must have congealed almost as soon as formed.

But, says one of our most original thinkers, there has been water, now absorbed in the Moon's interior. His lively imagination and playful fancy lead him to see the contents of the sea of Fecundity dashed through the seas of Tranquillity and Serenity into those of Mists and Showers and Little Storms, and so around the sphere, retaining through more southern maria into the Moon's cavernous inside. May one suggest as a slight obstacle the varying levels of these plains, for the sea of Serenity is much deeper than that of Tranquillity, and somewhat below the lake of dreams. The mare Frigoris is high, Imbrium is low.

Again, if there was ever water in these connected basins, driven by the fearful tides that must have prevailed, there must have been well-worn and continuous channels, and we should not look in vain for striking evidences of erosion. Our friend thinks the not infrequent breaking away of crater walls on the side towards the *mare* is such evidence, and he gives some beautiful illustrative drawings. I am surprised that so careful an observer and close a reasoner does not perceive that the breaking away is too complete, it proves too much. Where a crater-ring beside a *mare* is broken, scarcely a trace of it remains, according to his drawings, and in cases none—no islets, reefs or ruins to mark the site of the old precipice walls. It is as if the side had sunk and been engulfed in the lake of fire around it, or as if it had melted away completely, like so much wax. This total disappearance can better be accounted for by re-fusion and by settlement before renewed solidification. If it be argued that a tidal drag must have existed to bring the Moon to a rotation period equal to that of its revolution around the common centre of gravity of the Earth and itself, proof may well be demanded that no other reason accounts for this, and the enquiry made why a water drag need be imported into this question without any evidence of water? Moreover, it is not reasonable to suppose that water should have existed, to bring about exact concordance, and should then have mysteriously vanished. A finished work is not in accordance with the methods of nature, which seems to keep no Sabbath. Such retardation as the tidal-drag hypothesis demands would be still progressing, the rotation period would be tending towards identity with the period of revolution, but that approach, though getting closer and closer, would not be finally accomplished, or evolution is a myth. The Earth may be twirling with less velocity than in the Silurian or Archæan days, but it can never stop until its dissolution, and an ending nobody can better comprehend than a beginning.

Another feature on the Moon which seems different from anything on Earth is the streak system. Broad bright streaks diverge from at least seven principal points, of which Tycho is easily the chief. There are numerous minor streak-centres too. The points from which these rays diverge are usually craters, and the lines are, like the clefts, almost always straight, while they hold their course without respect to other features, such as plains or mountains. No conjectures as to their nature have yet been made that satisfy the mind, and observers are still puzzled by them.

The writer has frequently examined that great streak which crosses the Mare Serenitatis, which is often supposed to be a prolongation of a notable bright ray which extends from Tycho northward and is lost among or under the mountains which border the *mare*, to reappear on reaching it. It may as well be thought to begin at Thales and run southward across the *mare*, to be stopped by the Hœmus mountains. It must be scrutinized at the lunar sunrise, noon, and sunset, and preferably during our afternoons. When the glare is thus diminished and the seeing good, it seems to be a ridge or shoulder at the junction of two slightly inclined surfaces. A few other streaks have been examined and seem to catch some light as the terminator advances, but even so, we get no further: other streaks are not similar in this particular. Mr. Percival Lowell says the streaks in Mars are the results of organized labour, but we may be very sure these on the Moon are not, though we might as well say they are roads as call the Martian marks canals.

It is quite interesting to note the analogy between the craters and streaks of the Moon and the oases and canals of Mars. Have they a similar origin and *raison d'être?* Will one of these globes throw light upon the other and lead to a rational conjecture as to the nature of their common features ? The curious thing about the Martian canals is that they extend straight from start to finish, and that, wherever they intersect, round spots are found, which Mr. Lowell calls oases. He says (*Atlantic Monthly* for August, 1895): "Dotted over all the so-called continent of Mars are an innumerable number of dark circular or ovate spots. * * Of them indeed are the forty lakes, found by Prof. W. H. Pickering. There appears to be no spot that has not two or more canals running to it * * apparently no canal junction is without its spot. The majority are 120 to 150 miles in diameter, some smaller ones not more than 75 miles across, or less. That all are fundamentally of a kind is hinted at by their shape and emphasized by their character. The Solis Lacus is an oval spot in latitude 28° S. A cordon of canals surrounds it to the north. Upon the cordon are beaded a number of spots." Do we not think at once of Tycho and the lunar craters and ring plains ! The association will not be welcome to the advocates of the irrigation theory for Mars.

I do not know if the author of *The Time Machine*, that ingenious romance which carries us forward by successive stages towards the era when he supposes the Earth will be a dying planet, drew from lunar studies his idea of the subterranean race with which at one of his far distant periods he peoples this world. The French astronomers above named speak of the numerous small sink-holes on the Moon, almost or quite destitute of ramparts, arranged in lines, often so close together that the intermediate walls are no longer apparent, and, indeed, often looking like beads on a rosary. A fine example of this can be seen, they say, forming a common tangent to the interior rim of Almanon and Albategnius, and another, a row of them, tangential to the exteriors of Ptolemy and Albategnius. Sometimes, they continue, the connecting plain between these pits remains firm, but their perfect alignment leads one to suspect some invisible connection. Such caverns, left by retreating fires, might exist, there being no water to fill them up or cause their destruction, but as for a population, with bodies at all similar to ours, it can only exist in the imagination of a novelist, though more cavities are visible the higher the powers used and when the seeing happens to be very good. The telescopes now made show those no larger than a city block. Sir William Herschel felt quite convinced there were inhabitants on the Moon. He also believed there were active volcanoes. Astronomy and physics have made great strides since his day. Yet if he was wrong, it was only in part, and the idea which has received extended credence that the Moon is a lifeless, worn-out body is not only repugnant but without any foundation in reason.

Prof. Goldwin Smith says: "Our satellite is either a miscarriage or a wreck." The notion is built upon pessimism and implies analogies which are quite fanciful. One of the noblest features of astronomy is that it shows how infinitely varied are the processes of nature in the heavens. Everywhere we now recognize motion, the stars are no longer "fixed," and motion implies life. The fires of Luna may still be at work within, and changes on her surface as important as those upon the Earth's may be in progress. No notice of them being given by flame or smoke or steam, which without air cannot exist, makes it very difficult to observe any, but Schröeter saw near Cassini, on the night side of the Moon, a brilliant white spot as large as a 5th magnitude star, which lasted for 15 minutes, and similar appearances have since been observed in the same region. Linné, a spot on the Mare Serenitatis, has been thought variable, while Schmidt once announced its disappearance. Some drawings show it, others do not. Prof. Pickering finds changes in the shade of several other patches on this plain.

In a few years we may have much more information, for the great map of the Moon the illustrious Dr. Weinek of Prague is compiling, 14 feet in diameter, will have impressed upon it by photography the outlines of all the lunar features which the grain of the gelatine upon a carefully made plate will allow to be distinctly marked. These photographs will tell the tale with a hundred fold the fullness and accuracy of a draughtsman. We may thereby be enabled to detect some seismic changes, but it will requite close scrutiny, for, short of the clouds of dust and vapours and flames, how many of the changes made upon the Earth by earthquakes and even by volcanic eruptions could be observed by the best of our astronomers if they had their finest instruments upon the Moon ? The new photographs show numerous small craterlets just outside the wall of the loftiest large craters. It would seem that there, as upon the earth, eruptive forces found their easiest vent at such lofty elevations rather than at the lower levels of the crater floor. The detection of colour-changes seems less easy, for they are very slight. Most observers agree that there is a change on the floor of Plato, from steel gray at one period of the lunation to dark gray at another, and this is difficult to account for except by an actual variation of its surface. There is said to be a pale green tint about the Mare Serenitatis in certain lights, but it is confessedly hard to see, and I have looked for it in vain. Perhaps the imagination of the observer or the peculiar incidence of the light at certain angles may account for both these changes. The concordance of a multiplicity of observers at different seasons and places, and of persons using reflectors as well as those who have refractors, is desirable, that a recognized conclusion may be reached.

It may be noted here that upon general principles we should not expect so much variety in lunar as in terrestrial features; the largest continent, or island, or body of water must, *ceteris paribus*, be the most diversified in all respects; the largest river valley must have the richest fauna and flora; and so our larger planet should bear the palm from its smaller twin.

But, says a veteran observer, it may be that air if not water still exists on the further side of the Moon. To this two answers may at once be given. While the greater part of the farther side is never revealed to us, we do, under favourable conditions of libration, see eighteen per cent. of it, for 0.59 of the surface of the Moon is visible at one time or another. The Leibnitz mountains, S.W. of Tycho, which the present writer made a measurement of upon the serrated edge last year and reported to this Society as being 30,000 feet in height, were barely traceable, a week ago, in Mr. Lumsden's fine reflector, and though owing to the less diameter and greater curvature of the surface of the Moon, a lunar mountain would disappear from view in a less distance than one of the same height upon the Earth, this will give an adequate idea of the great extent of the other side we do really see. And if an area of nearly one-fifth of that other side does not differ from this, as we see it does not, that is one good reason why the other four-fifths are similar too.

Then, there is still some doubt as to the form of the Moon. It may be egg-shaped, as eminent mathematicians have believed, as Schiaparelli says Mercury is, as at least one of Jupiter's satellites is reported to have been seen, at Arequipa. But the centre of gravity cannot be much further from the Earth than the centre of form, and if it is nearer, even by so little as 15 miles, as Newton calculated, the necessary consequence is that if there were an atmosphere, or water, there would be more on this side than on the other. No, it is not on the invisible side that we can place the features which would render possible biological conditions like ours; we must, at once, admit that there everything is and always has been very different, and that vital forces there are clothed with quite other envelopes than ours, possessing totally diverse characteristics, parts, powers and senses. The beings upon the Moon may be no bigger than beetles, and yet have keener intelligence than ours. It is futile to guess at their attributes. As we have no ideas that do not spring from experience, we are blocked as to a foundation even in fancy about beings whose works are like themselves, invisible. But if there be no life upon the Moon, we must conclude that she is in no sense an independent centre, but exists for the benefit of the Earth alone and is merely one of its organs, as much an adjunct to it as a crest of feathers to a bird, a wing case to a beetle, a thorn to a rose. We should be driven to work at the question what she has done or will accomplish for the physical benefit of the Earth. To suppose her as existing for the delectation of us poor incidental mortals seems absurd. But it is as well to avoid speculation on these points. We cannot measure the infinite by space, the everlasting by time, the Almighty by force, the absolute by matter. Within our limits there is ample scope for research into facts not beyond our ken, and into causes and effects we can learn to comprehend.

EIGHTEENTH MEETING.

September 17th; the Vice-President, Mr. John A. Paterson, M.A., occupied the chair.

Mr. E. F. Churchill was duly elected an active member of the Society.

A cordial letter was received from Mr. W. F. King, C.E., corresponding member of the Society, and who had been engaged on the International Boundary Commission in Alaska. Mr. King promised to prepare a paper on instrumental work at the earliest opportunity. Referring to the work of the Boundary Commission, Mr. Lindsay said it was now well known that special instruments for use in photogrammetry had been devised by Surveyor-General Deville; he called attention to the fact also that the work of constructing the instruments had been entrusted to a fellow-member, Mr. James Foster, who, it was most encouraging to note, had received orders from several foreign governments for instruments of the same design, since Mr. Deville's methods had been made public.

A short note was read from the Editor of *Science*, referring to the observation reported by Dr. Wadsworth of the concave outline of Saturn's shadow on the ring. After naming several writers who had treated this subject, it was said the apparent outline of the shadow will always be greatly influenced by various optical illusions, and observations made with ordinary telescopes under ordinary conditions cannot be safely relied on.

Reports of observations of the lunar eclipse, September 3-4, were received from several members who had observed the phenomenon in Toronto; from Mr. D. E. Hadden, of Alta, Iowa, a full account was also received, and which was subsequently published in *Popular Astronomy*. Mr. A. F. Miller presented the following notes: "Spectroscopic work was commenced September 3rd, 23h. 15m., and all observations were repeated and confirmed by Mr. C. A. Chant: The method was the same as on former occasions; an image of the Moon in the principal focus being received on the slit-plate of a small direct-vision telespectroscope, and held there by the clock movement. The spectrum of the uneclipsed part of the lunar surface thus was available for comparison with that of the darkened region. Occasionally as an aid to determination of positions when the light had become very faint, the flame of alcohol containing a sodium salt was employed for reference. There was no marked selective absorption in the lines, nor were any new lines produced by the advance of the shadow; but the blue end of the spectrum was almost entirely deficient; the blue-green, green and yellow parts became so faint as to be quite invisible as *colours* though seen as *light* ; the red end only remaining bright enough to affect the eye as a colour-sensation, but in this respect it was very distinct and specially struck Mr. Chant's eye. The appearance of the spectrum was similar in general absorption effects to what would be produced had the light passed through smoked or neutral-tint glass; the characteristic lines noted in connection with the eclipse of 1895, March 10, were not seen at all on this occasion."

The Chairman then called upon Dr. McCallum, President of The Canadian Institute, who was present by special invitation, to address the society on the subject of the proposed visit of the British Association in 1897. Dr. McCallum stated that the meeting would certainly be held in Toronto, and that it would be necessary to arouse as much enthusiasm as possible throughout Canada, so that the fullest success might be attained. He pointed out the many advantages that would accrue from the presence, in Canada, of such a body as the British Association, and hoped that The Astronomical and Physical Society would do its utmost to make the visit a memorable one for the distinguished visitors. The Chairman assured Dr. McCallum that the Society would take part in all committee work which the general committee might decide upon undertaking.

NINETEENTH MEETING.

October 1st; the Vice-President, E. A. Meredith, LL.D., in the chair.

The following named ladies and gentlemen were duly elected active members of the Society :—Miss Jessie Campbell, Mrs. J. E. White, Miss Workman, Miss Field, Mr. Hugh Caldwell, all of Toronto.

The Corresponding Secretary read the following

MINUTES OF A MEETING OF THE JOINT COMMITTEE ON THE UNIFICATION OF TIME :—

Pursuant to call, a special meeting of the Joint-Committee of The Canadian Institute and of The Astronomical and Physical Society of Toronto on the Unification of Astronomical, Civil and Nautical Time, was held at The Canadian Institute on the afternoon of the 25th of September instant. The members present were :—Dr. Sandford Fleming, C.M.G., etc., in the chair, and Messrs. Arthur Harvey, F.R.S.C., G. Kennedy, LL.D., G. E. Lumsden and R. F. Stupart, Director of the Toronto Observatory.

The Chairman submitted a draft of the Third Report of the Committee and of a letter intended to cover the transmission of the Report through the office of His Excellency the Governor-General to the Home Authorities. The drafts were read and discussed, clause by clause, and certain amendments made, whereupon,

It was moved by Dr. Kennedy, seconded by Mr. Stupart, That the Third Report of the Joint-Committee on the Unification of Time, as now read, be adopted and be transmitted to the Home Authorities for their consideration. Carried.

The Committee then adjourned *sine die*.

SANDFORD FLEMING,
Chairman of the Committee.

Toronto, 26th September; 1895.

The text of the Report referred to was then read, as follows :—

UNIFICATION OF ASTRONOMICAL, CIVIL AND NAUTICAL TIME—THIRD REPORT.

TORONTO, September 21st, 1895.

The Joint-Committee of The Canadian Institute and of The Astronomical and Physical Society of Toronto on the Unification of Astronomical, Civil and Nautical Time, as recommended by The Washington Conference, 1884, respectfully reports :—

The several communications on this subject which have been transmitted by the Home Government to the Governor-General of Canada and by His Excellency submitted to the Institute and to The Astronomical and Physical Society, have been referred by them for an expression of opinion by the Joint-Committee.

The fundamental object of The International Conference summoned by the Government of the United States in 1884, was to consider and to recommend a more satisfactory mode of reckoning time, and to endeavour to establish an international system for its unification on a broad and scientific basis.

The correspondence before the Joint-Committee has direct reference to that branch of the general system of Time-reform which forms the subject of the Sixth Resolution of The Washington Conference of 1884.

It has been generally recognized that mariners, together with the interests of commerce which they represent, would especially benefit by the adoption of the Sixth Resolution of The Washington Conference. In considering the subject on previous occasions, the Joint-Committee therefore assumed that mariners the world over would favour the adaptation of the *Nautical Almanac* to one uniform reckoning of time at sea, and that it would be in all respects advantageous to have the time-reckoning at sea in complete harmony with the time-reckoning on land. It was only with respect to the views that astronomers might entertain that the Joint Committee had doubts, inasmuch as the universal adoption of the recommendation of The Washington Conference in all matters relating to navigation would practically abolish "Astronomical Time" as it is now defined. In order to obtain the views of astronomers on this point, the Joint-Committee sent circulars to every known astronomer in all parts of the world whose address could be found. Nearly one thousand circulars were so forwarded, and replies were asked to the following

Question :—" Is it desirable, all interests considered, that on and after the 1st day of January, 1901, the Astronomical Day should everywhere begin at mean midnight?"

The replies received, classified according to countries, were to the following effect :—Austria, Australia, Belgium, Canada, Columbia, England, France, Greece, Italy, Ireland, Jamaica, Madagascar, Mexico, Romania, Russia, Scotland, Spain and the United States of America, eighteen countries in all, were in favour of the proposal; Germany, Holland, Norway and Portugal, four countries in all, were unfavourable to the change.

The replies received were not so numerous as the Joint-Committee expected, and it is difficult for the Committee to determine whether those who did not reply were influenced by indifference, or were prepared to acquiesce in the change recommended by The Washington Conference. It is, perhaps, not unreasonable to infer that all who were strongly opposed to the proposed change would have embraced the opportunity presented them of giving expression to their views. From the large number appealed to, 171 replies were received, 108 in favour of and 63 against the proposal. The result of this appeal to astronomers was referred to in the Second Report of the Joint-Committee, dated 10th May, 1894. This Report was communicated to His Excellency the Governor-General for transmission to the British Government, by whom it was referred to the Science and Art

Department, South Kensington, and, finally, to the Admiralty. The Lords Commissioners of the Admiralty, under whose supervision the *Nautical Almanac* is published, expressed their willingness to sanction such alterations as may be necessary to effect the change at the beginning of the new century, provided other nations who publish astronomical ephemerides are prepared to take the same action. The British Government thereupon instructed Her Majesty's representatives to ascertain the views of the Foreign Governments to which each was accredited, with regard to the proposed change.

The nations publishing ephemerides are, including Great Britain, nine in number, viz. , Austria, Brazil, France, Germany, Great Britain, Mexico, Portugal, Spain and the United States of America.

Of these nine Powers, six have formally given their assent to the proposed change. The remaining three, while they have not signified assent, have not expressed dissent.

The six nations formally assenting to the adoption of the recommendation of The Washington Conference on the 1st of January, 1901, are :—Austria, Brazil, France, Great Britain, Mexico and Spain. Of the remaining three, Germany and Portugal have not, so far as is known, sent any reply. A brief communication has been received from the Secretary of State at Washington, simply stating "that the members of the United States' Naval Observatory are adverse to the 'Canadian proposition,'" as they see fit to term it, and, at the same time, he sends a copy of the adverse report. This is the only report of a negative character which has been received, and it is the more surprising as this report is entirely at variance with the position taken by the United States throughout the movement for reforming the time-reckoning of the world during the last fifteen years. The United States have, indeed, taken a prominent part in the movement. Two Societies, comprising in their ranks some of the most eminent scientists of the country, have actively promoted it from the commencement —The American Metrological Society and The American Society of Civil Engineers. Moreover, both Houses of Congress have taken joint action in the matter. It was under the provisions of an Act of Congress that the President assembled The International Conference of 1884. At that Conference, it was the five distinguished delegates nominated by the Government of the United States who introduced the proposal respecting the Astronomical Day, a proposal which was carried without a dissenting voice by the representatives of the twenty-five nations constituting the Conference. It is to the United States we trace some of the first steps taken to establish an acceptable system of reform in the reckoning of time adaptable to the whole world. It is certainly to them that we owe the first national recognition of the movement and its first application to every-day life, that is to say, the Joint-Resolution passed by Congress in July, 1882, and the action of the gathering of Railway Managers in Chicago, which resulted in the Hour Zone system of time reckoning going into force throughout North America on November 18th, 1883.

With all the facts before us, it is impossible to consider that the adverse report signed by three officials of the United States' Naval Observatory, fairly represents the mind of the United States Government, of Congress, or of the people of the United States. The objections brought forward in this report are of old date and at various times have been answered—by the Bureau des Longitudes of France in an official report (*vide* Cosmos, February 3rd, 1895) endorsed by the French Government, May 6th, 1895 ;—by the reports of the Joint-Committee of April 21st, 1893, and May 10th, 1894, copies of which have been transmitted to the Home Authorities ; by the Astronomer Royal in a report to the Trustees of Greenwich Observatory which points out that the proposal can be easily introduced and with decided advantage to observers ;—by a former Superintendent of the United States' Naval Observatory, Commodore Franklin, December 11th, 1884, in a communication transmitted to Congress with other documents by the Secretary of State for the Navy, February 17th, 1885.

These several documents amply refute all the objections to the proposal and render any discussion of them in this report unnecessary. Their best answer is in the fact that the Governments of six ephemerides-publishing nations, comprising some of the most conservative countries in the world, have, under the advice of their ablest men, recognized the advantages of the proposal and have assented to it being carried into effect. The attitude assumed by the United States' Naval Observatory is so decidedly different from that of a few years back that the sole explanation which can be made is that the personnel of the Observatory has changed.

Immediately after The Washington Conference of 1884, Commodore Franklin, the head of the United States' Naval Observatory, desiring to give effect, without delay, to the resolutions passed, issued instructions to the Observatories of the United States to bring Astronomical Time into agreement with Civil Time. This officer was supported by three-fourths of the leading astronomers of the United States, who, doubtless, felt with him that it would be becoming on the part of the nation which had assembled the Conference to be the first to accept and give practical effect to its wise recommendations. There was one exception, however, Professor Simon Newcomb, who raised strong objections to any departure from the old system. This gentleman, whose name is attached to the adverse report of recent date, as Professor of Mathematics, U. S. N., and Director of the *Nautical Almanac*, did not, in objecting to the instructions of Commodore Franklin, express his adverse opinion on the general question for the first time. In 1882, two years before The Washington Conference, The American Society of Civil Engineers formulated a scheme of Time-reform, which, in its essential features, has come into use not only throughout the North American Continent, but also over large parts of Europe, Asia and Australia. In order to ascertain the views of scientific and practical men, this Society sent out circulars asking an expression of opinion respecting the proposed measure. A series of questions were drawn up and answers to them were respectfully invited. Among the many replies received and placed on record in the publications of that Society there is one from Mr. Simon Newcomb, whose words read strangely in the general record of assent and approval with which the measure was welcomed throughout the United States and Canada. They are given in the appendix to this Report.

The publications of The American Society of Civil Engineers affirm that replies were received from all parts of the United States and Canada and that 97 per cent. expressed opinions diametrically opposed to those of Mr. Newcomb. The unanimity of opinion was indeed remarkable. In one respect, Mr. Newcomb stood alone in his antagonism to this scientific reform. In marked contrast to his objections, we have the breadth of view and general enlightenment of the members of the Societies named, and of the managers of the great lines of transportation by land and water throughout the United States—the men who in 1883 adopted the Standard Time System. We have in still more marked contrast to the views of Prof. Newcomb the spirit which moved the highest constituted authorities, the Senate and the House of Representatives, in passing a joint-resolution requesting the President of the United States to assemble representatives from every civilized nation to consider the very questions which find so little favour with Mr. Newcomb. These remarks go to prove that this gentleman holds conservative views in the matter of Time-reform peculiarly his own and that it is impossible to accept a report expressing his opinions on this question as representing the voice of the United States. The evidence shows that the United States stands in the front rank in support of this important movement, and with respect to the proposed change in the Astronomical Day, a letter from the Secretary of the Navy to Congress, dated February 17th, 1885, brings out the fact that although the execution of the order was subsequently deferred until a general agreement could be reached, a General Order was actually issued on December 4th, 1884, by the head of the Naval Observatory, to begin the Astronomical Day at midnight in accordance with the recommendation of The Washington Conference of that year. Moreover, it may be added that the recommendations of The Washington Conference were endorsed by the President of the United States in his message to Congress of January 9th, 1883.

In view of the facts narrated, the Joint-Committee respectfully conceives that it is fully warranted in the opinion that the United States, as a nation, may be truly considered to be one of the nine ephemerides-publishing nations in favour of the proposal to bring the Astronomical Day into agreement with the Civil Day.

In a question of this kind, perfect unanimity is scarcely to be hoped for, nevertheless the Joint-Committee submits that the evidence establishes that there is so general an agreement among the nations as will admit of concerted action in regard to the introduction of the change proposed in the Sixth Resolution of The Washington Conference.

The Joint-Committee, in conclusion, respectfully expresses the hope that in accordance with the 11th Paragraph of the letter of the Admiralty of January 5th, 1886, the Lords Commissioners will be pleased to sanction such alterations in the *Nautical Almanac* as may be necessary to establish the change to the new reckoning, on the first day of the coming century. It is scarcely necessary to add that as the *Nautical Almanac* is usually prepared four or five years in advance, it is important that a definite decision should not be long delayed.

All of which is respectfully submitted.

On behalf of the Joint-Committee.

SANDFORD FLEMING,
Chairman.

APPENDIX.

Replies of Mr. Simon Newcomb, Washington, to Queries issued by the Specia1 Committee on Standard Time of The American Society of Civil Engineers, 1882:—

(Extract from the Publications of the Society.)

Question 3.—Do you consider it advisable to secure a Time system for this country (the United States), which would commend itself to other nations and be adopted by them ultimately?

Answer (by Mr. Newcomb). —No. We don't care for other nations; can't help them, and they can't help us.

Question 4.—Does it (the scheme for regulating Time), seem to possess any features which generally commend themselves to your judgment?

Answer (by Mr. Simon Newcomb).—A capital plan for use during the millennium. Too perfect for the present state of humanity. See no more reason for considering Europe in the matter than for considering the inhabitants of the planet Mars.

After the reading of the above, Mr. Harvey stated that the Report had been adopted by The Canadian Institute. It was thereupon moved by Mr. G. E. Lumsden, seconded by Mr. T. Lindsay, that the Third Report of the Joint-Committee meeting be adopted, and embodied in the *Transactions* of the Society. Carried.

In accordance with the resolution of the Joint-Committee the Third Report was subsequently forwarded to the Lords Commissioners of the Admiralty through the office of His Excellency the Governor-General.

A report of the work of the Lunar section was received from Mr. Harvey, and a series of sketches of the solar disc from Mr. G. G. Pursey.

Mr. J. A. Paterson read some notes from a number of *Science Siftings*, bearing upon the origin of the Moon and indirectly upon the nebular hypothesis.

A short discussion followed, in which Mr. Harvey and Mr. J. R. Collins took part. The former referred to the address of the President of the British Association, in which it had been stated that recent researches in thermo-dynamics proved the nebular hypothesis, as outlined by La Place, to be untenable. Mr. Paterson and Mr. Lindsay dissented from this view.

The Assistant Secretary presented an exhaustive paper on

SOLAR OBSERVATIONS,

contributed by Mr. D. E. Hadden, of Alta, Iowa. The following is a synopsis:

Systematic observations of the Sun's surface were begun in August, 1890, and have been continued to the present time. During this time I have observed the solar disc on 960 days and have kept a fairly complete record of the number of groups of spots, the total number of spots in these groups, and the number of faculæ; also their relative size, location and place of origin, whether on the visible disc or appearing by rotation. These observations have been published, somewhat in a condensed form, principally in the *Monthly Review of the Iowa Weather and Crop Service*.

The observations were made mainly with a 3-inch Brashear telescope, with various eye-pieces, powers and shade glasses; of late I have given preference to the Herschelian wedge prism, using ½-inch and ¾-inch eye-pieces, and neutral green-tinted shade glasses. The method of projection was also much employed. During the past year some observations of the prominences have been made with the spectroscope, using a 2-inch Rowland diffraction grating of 14,438 lines to the inch. A few photographs were taken, and numerous pencil sketches of the larger and more remarkable groups were made.

When the daily average number of sun-spots is plotted graphically marked fluctuations are noticed during the entire five years, but generally there is a constant increase in the number of spots during the period.

A small maximum appeared in July, 1891, and two maxima in 1892, viz., February and July; that of July being the highest during the five years.

A secondary maximum was noted in the summer of 1893, commencing in the month of June and reaching the last maximum in August. After this a steady decline set in, with slight irregular fluctuations during 1894 and first half of 1895.

The following tables gives the maximum and minimum number of sun-spot groups observed on any day for the months and years indicated :—

MAXIMUM DAILY NUMBER OF SUN-SPOT GROUPS

Months.	1890.	1891.	1892.	1893.	1894.	1895.
January	....	4	11	11	10	7
February	....	3	8	9	8	9
March	....	2	6	10	8	8
April	....	4	7	10	7	8
May	....	5	7	11	9	10
June	....	7	9	11	9	7
July	....	8	13	10	11	......
August	2	5	10	14	8	......
September	2	8	9	9	9	......
October	2	5	8	12	7	......
November	4	8	9	9	6	......
December	3	7	13	9	......	......

Months.	1890.	1891.	1892.	1893.	1894.	1895.
January	....	0	2	2	4	3
February	....	0	1	2	4	3
March	....	0	1	3	3	4
April	....	1	2	4	3	5
May	....	2	4	6	3	1
June	....	1	4	5	5	4
July	....	2	3	4	4	......
August	0	0	4	6	2	......
September	0	1	3	4	2	......
October	0	3	2	5	2	......
November	0	2	3	4	3	......
December	0	1	2	2	......	......

Mr. Hadden had also forwarded minute descriptions of many remarkable outbursts of sun-spots during recent years.* These were compared with notes which Mr. Elvins and Mr. Pursey had made on the same occasions. The comparisons were found particularly interesting.

* A detailed account of these has since been published in the Publications of The Astronomical Society of the Pacific, December, 1895.

TWENTIETH MEETING.

October 15th ; the Vice-President, Mr. John A. Paterson, M. A., in the chair.

Mr. Chas. P. Sparling, Recording Secretary, addressed the Society on the subject of a change in the place of meeting. A cordial letter had been received from Dr. J. O. Orr, Chairman of the Technical School Board; inviting the Society to make use of the building and the adjoining lawn, and to hold all regular meetings in the board room. Special meetings could be held in what was set apart as an electrical-room, and which contained apparatus for illustrating lectures in physics. On motion of Dr. E. A. Meredith, seconded by Mr. Thomas Lindsay, it was decided to accept the invitation and to remove the effects of the Society at once. The motion was coupled with an expression of the Society's appreciation of the kindness of the Young Women's Christian Guild, in whose rooms the meetings had been held since 1893. This was carried unanimously.

Mr. Arthur Harvey reported his observations of Venus. He had endeavoured, but with success, to make out irregularities on the terminator. This was, however, much less sharply defined than the outer edge, and though this is freely accounted for by the twilight belt on the edge of the sunrise curve, due to the vaporous atmosphere of Venus, it is possible that the shadows of eminences on her surface, and other surface features there, may contribute to this effect, which is noticeable on the Moon though without an atmosphere of similar nature. Some discussion arising regarding the visibility of the unilluminated portion of the planet's disc, Mr. Lindsay stated that this subject had occupied the attention of several members of the British Astronomical Association, and papers on the subject were embodied in the publications of that Society.

Mr. A. Elvins and Mr. G. E. Lumsden had observed the auroral display of September 12th. Mr. R. F. Stupart showed charts of the magnetic curves, while the aurora had been in progress. In this case the disturbance had not been specially marked.

Mr. Lindsay reported some observations made with an eight-inch reflector recently figured and silvered by the Messrs. Collins. The telescope was the property of Mr. E. W. Syer, of Chicago, and was to be mounted at Niagara-on-the-Lake for use during the summer months.

The mirror performed exceedingly well on many difficult objects, especially the ring-nebula in Lyra. It was thought to be a matter of congratulation that this instrument was to be brought into active service in Canada.

Mr. J. Phillips then read a paper on

THE RINGS OF SATURN AND THE NEBULAR HYPOTHESIS.

This paper shewed, from Mr. Phillips' standpoint, that La Place founded his hypothesis chiefly on the Saturnian ring-system. That only for this system he would, most likely, never have propounded his hypothesis; that in view of all the facts then known, the hypothesis was sound when framed; but that, from discoveries since made, it is proven to be untenable from La Place's own demonstrations.

The following is a part of Mr. Phillips' paper, beginning with one extract out of many translated from La Place :—

"We suppose according to this hypothesis, that the inner ring of Saturn being very near the planet, the duration of its rotation ought to exceed only by a very little that of the rotation of Saturn. In considering how small is the difference observed between these durations, it is difficult not to admit that the atmosphere of Saturn extended out to its rings, and that they have been formed by the condensation of its layers." —*Mecanique Céleste*, vol. 5, book 4, chap. 3.

Such were some of the chief grounds on which were founded the celebrated La Placean hypothesis, by one of the very greatest of all the great continental mathematicians of the latter half of the last century, the greatest the world ever saw, called forth by the exigency and demand of the times. One hundred and thirty or forty years ago, be it remembered, all classes of most of continental Europe, from the prince to the peasant, were on the *qui vive* about the solar system and the laws that govern it. Here were some of the questions that solutions were called for :—

"Is gravity a law of the universe, and, if so, does it vary precisely as the inverse square?"

Were Jupiter and Saturn perpetually receding from each other?" And above all, "Was our Moon being precipitated down upon us?"

From the earliest historic times the Moon has been drawing nearer and nearer the Earth, and her lunations growing shorter and shorter. Was this acceleration perpetual? Was it owing to a resisting medium, or to what else? And, if perpetual, when would the great collision occur, and the present state of things here on Earth come to an end ? Was this a sign set on high by the Creator to keep mortals in mind of the final consummation?

Philosophers, theologians, moralists, saints and sinners, were interested in those all absorbing topics. Governments vied with each other in encouraging learned men skilled in the physico-mathematical sciences. New modes of computation were devised, the infinitesimal calculus extended, the geometric method was superseded by the analytic on the continent, and astronomy took such a bound forward as it never did before and is never likely to do again in so brief a period. How such interest was awakened in Newton's teaching after its lying dormant for fifty years does not matter now; awakened it was.

Near the close of the century La Place published his hypothesis, about which so much has been said. Too often it is confounded with that of Emmanuel Kant by those who ought to know better.

Turning again to the Saturnian rings, La Place says "The outer ring rotates in 0·438 of a day, the ball of the planet in 0·427, just a little less, which accords with theory." He had no means of finding from observation in what time the inner ring rotated, but felt convinced it was between the 0·427 and 0·438 of a day, leaving only about sixteen minutes as the margin of time ; otherwise his hypothesis would be untenable—for, "The duration of a planet's rotation," says he, "must be *less* than the duration of the revolution of the nearest body that circulates around it."

This is an absolute rule in mechanics, and there was not one known exception to it in the planetary system while La Place lived. One such case, well established, would falsify his hypothesis—as Prof. O. M. Mitchell declared many years ago, in defence of that hypothesis, in which he was a strong believer. But what do we find to-day ? Not barely one, but millions of exceptions to this rule! This very year we have confirmed by Prof. Keeler the old idea of Cassini and the later contention of Clerk-Maxwell, that the Saturnian ring system is neither solid, liquid, nor gaseous, but is made up of an all but infinite number of meteorites, swinging round and round like a swarm of bees or a cloud of locusts, each meteorite an independent satellite revolving in its own orbit! And all of them are kept apart and retained, each in its own separate path by its rapid motion, and by the force of Saturn's attraction upon each and every one of them. Nor is this all, for we know besides that the whole system known as the "inner ring" a hundred years ago—instead of rotating more slowly than the planet, as the hypothesis calls for, rotates much faster. The meteorites composing the outer edge of it revolve in about the same time as the planet rotates; those of its middle, an hour and three-quarters faster; those composing its inner edge, in nearly three hours (2·9133 hrs.) less time. Again, the inside edge of the dusky ring, extending to within 5,900 miles of Saturn's surface, rotates in less than half the time the ball of the planet takes to turn upon its axis!

Had La Place found all this out a hundred years ago, he would have popped his hypothesis into the fire instead of sending it to the printer. Can any one doubt this for a moment after reading his own preceding words—to which ten times more might be added of the same import ? If there is any one thing more than another La Place insists upon, or that the science of mechanics calls for—whether it be a satellite or ring revolving about a planet, or a planet round the Sun (the revolving body being cast off by the centrifugal force of the rotating one)—it is that the planet or Sun must turn on its axis with a greater angular velocity than the revolving body it casts off by its rotary force, can sweep in a closed orbit around it. Every mechanic, every wood-man that ever ground an axe, every boy that ever turned the stone for him, knows that jets, drops, or specks of water thrown from the face of a rapidly turning grindstone are thrown with less velocity, certainly not with greater, than the circumference or face of the stone whirls round. They are left behind because they cannot keep up with its angular velocity. It is, therefore, physically impossible for either of the two inside rings of Saturn to have originated according to the La Placean hypothesis.

Yet, when first framed, the evidence in its favour was all but overwhelming. Only one valid objection could be urged against it then—and that was the Uranean moon system. Newton in his day had not much stronger grounds for laying down his theory of gravitation. Indeed there was a remarkable similarity between the two. Both were numerical verifications. Newton failed at first, owing to the incorrect measure of the Earth he applied. La Place from theory computed the period of rotation of the outer section of the ring system, which was afterwards verified by observation. Both theories had almost everything to support them that observation and reflection could bring to bear on them when first propounded. And what may seem a strange coincidence, each theory had one serious objection to it. The one was the motion of the lunar apsides not agreeing with computation, which Newton never cleared up. The other the peculiarity of the Uranean moon system, which La Place could never reconcile with his hypothesis.

This great objection to the Newtonian theory was met by Clairaut the very one who expressed doubts of gravity being able to explain it, about the middle of the last century. By the end of the century the doctrine of gravitation became so firmly established that it can never more be shaken. Not so with the La Placean theory, as we may call it. Misfortune followed in its wake all along. Not only was the primary objection to it never answered, but others loomed up as new discoveries were announced. About the middle of this century Neptune's moon entered his protest—a different kind of protest, too, from that of the Uranean moons, for his orbital plane lies nearly in that of the ecliptic. Next Phobos, the inner moon of Mars, appeared on the scene, and actually vetoed the La Placean hypothesis altogether. And last, but not least, now come along the two Saturnian rings, which have been muttering their dissent for some years past—or rather now emerge out of the mist, countless millions of tiny satellites, sweeping round Saturn in periods of revolution less than Saturn's period of rotation, and he rotates pretty fast—just as Phobos does round Mars, in less time than Mars rotates.

So the last prop is knocked from under the so-called Nebular Hypothesis! It was an enchanting dream, and entertained many in its time. It had many opponents, if not enemies, at first, but kept gaining friends all along. Every one will regret its departure, and many will revere its memory. It indeed leaves a blank behind not easily filled. A similar void was beginning to be felt a hundred and fifty years ago, when Clairaut declared gravity inadequate to account for the progression of the lunar apsides. It looked like having to lapse back into the days of darkness before Newton's time. It was only a passing cloud though, before the real dawn of day.

To those who feel the loss of the La Placean cosmogony I have a crumb of comfort to offer. If that is not the true theory, some other is, whether yet discovered or no. And the detection that one is not, may be the harbinger of another that is.

In a couple of years or so from this, if life, health and mental vigour hold out, I may offer a theory of the planetary system, by which the origin of all the planets, primary and secondary, and the zone of planetoids, including the Saturnian ring-system, will be accounted for by *one general law*, in perfect accordance with the teaching of Newton's philosophy.

TWENTY-FIRST MEETING.

October 29th; Mr. Arthur Harvey, F.R.S.C., occupied the chair.

The meeting was held in the board room of the Toronto Technical School, College street, opposite McCaul street. There was a large attendance of members and friends, who wished to become acquainted with the new location. The Board of Management had set apart a room to serve as a Library, and for the storing of apparatus, etc., and had promised to do all in its power to further the Society's interests.

A communication was read from Dr. M. A. Veeder, of Lyons, N.Y., in which he stated that he was obliged, for several reasons, to give up active work in auroral research. The Corresponding Secretary was requested to convey to Dr. Veeder the Society's appreciation of his labours, and to express the hope that he might still see his way to continue work which had been so productive in the past.

Among other exchanges, the Librarian reported having received several copies of the *Journal* of the Astronomical Society of Wales, an association recently formed, but already evincing great activity in the work of popularizing astronomical studies. The current number of *Popular Astronomy* was also laid on the table. Mr. Lindsay gave a short resumé of the matter contained therein, and referred especially to the articles by Dr. J. Morrison, formerly of Toronto, on the "Solar Ephemeris." It was stated that the articles contained information which was not to be found in text-books, certainly not in such as the amateur usually meets with.

Some questions which had been proposed at a previous meeting were discussed. A member had asked what was the least aperture with which the companion to Sirius could be seen. To this Mr. A. F. Miller replied that he had made the observation in a 10¼-inch reflector, when the companion had been well separated. The visibility of "dark auroras" was also discussed, and some notes by Dr. Veeder, bearing on the subject, were read.

The meeting then adjourned for the purpose of allowing the members an opportunity of inspecting the appointments of the Technical School and the advantages which the location would offer for regular and special meetings. It was thought that the out-door meetings of the Opera-glass and Lunar Sections could be held on the grounds very successfully; there was a good horizon for observation on all quarters, and the proximity of the building to the Observatory was also an advantage. All present expressed themselves as being much pleased with the new location.

TWENTY-SECOND MEETING.

November 12th; held in the Electrical Room of the Technical School, Mr. John A. Paterson, M.A., Vice-President, in the chair.

This was an open meeting, to which the public had been invited. There was a very large attendance of members and their friends. The chairman took the opportunity of returning thanks to the members of the School Board for their kindness in providing such excellent quarters for the Society.

Prof. Dr. Kirschmann, of Toronto University, was duly elected an active member. Routine business was then suspended in order to afford time for the reading of the paper which had been specially prepared for the evening by Mr. G. E. Lumsden, and which, it had been announced, would be followed by an exhibition of astronomical photographs, projected on the screen.

SOME NOTES ON CELESTIAL PHOTOGRAPHY

was the subject of the paper presented. Mr. Lumsden, after referring to the many investigators whose discoveries had, from the crudest beginnings, led to the present high standard to which photography has attained, thus alluded to its application in the field of astronomical research, and to some of the many respects in which astronomers are indebted to the Art for an enhanced knowledge of celestial objects :—

I have already spoken of the year 1839, and I recall it for the purpose of reminding you that in that year the French astronomer Arago, in announcing to The Academy of Sciences the improvements affected by Niepce and Daguerre, expressed the opinion that it would yet be possible "to make the Sun and Moon record their own features by photography."

Though thus encouraged by Arago, Daguerre appears to have failed signally in solar photography, his pictures being very faint and devoid of detail. With regard to the Moon, however, better, and perhaps the earliest, success was attained on this continent by Prof. J. W. Draper, an enthusiastic English physicist, who had become connected with the University of the City of New York. For some years, Draper had been at work along lines similar to those being followed up in Europe, and, as a result of his experiments, had himself introduced some improvements. It is claimed for him that so early as 1834 he had attempted to photograph the lines in the solar spectrum by using paper which had been coated with bromide of silver. He failed, but in 1843, by employing daguerreotype plates, he made "photographs of the diffraction spectrum by a grating, both by reflection and transmission." By 1840, Draper had succeeded in taking a picture of the Moon, with respect to which, in one of his papers, he says that he met with no difficulty "beyond that which arises from her motion," practically, it may be observed, the only difficulty met with even yet. Of course, in his day the difficulty was vastly greater, because the exposures were very long. To-day, the Moon may be photographed in a fraction of a second. In Draper's day the exposures lasted twenty minutes. The pictures were also very small, being only an inch in diameter, though taken with a photographic lens of five inches' aperture, armed with an eye-piece to increase the magnifying power. That Draper was far in advance of most of his fellows, was proved by the exhibition before you two years ago of a copy of the first portrait of a human face taken by sunlight. The subject of the picture was Miss Draper, the Professor's sister, who sat as still as she could before the camera for ninety seconds. The exposure was made on the roof of a flat building so as to get the full benefit of the sunshine, and Miss Draper's face was dusted over with white powder in order that, in impressing the sensitive plate, the Sun should be assisted in every way possible. It was my privilege to see the original daguerreotype at Chicago in 1893, where its authenticity was vouched for by Sir William John Herschell, who, on a search, suggested in that year by the University of New York, found it among the papers of Sir John Herschell, his father, to whom it had been sent by Draper in 1840. On the 2nd of April, 1845, Foucault and Fizeau succeeded in taking a daguerreotype of the Sun, which was reproduced, as a very fine plate, in Arago's Popular Astronomy. In 1850, the ingenious Bond using the fifteen-inch refractor of Harvard College, secured the first really valuable photograph of the lunar surface. Some of Bond's daguerreotypes were shown in England in 1850 and in 1851, and were so good that "they took the scientific world by storm," the language used by Dr. H. C. Russell, F.R.S., to whose paper on this subject I am indebted for some of the facts here enumerated. Thereupon, many English amateur astronomers made attempts to photograph the Moon. Of these, Mr. De la Rue was at once the most energetic and the most successful, though perhaps the first good daguerreotypes that would bear examination under a powerful microscope, were taken by Mr. Dancer. On the 17th of July, 1850, Bond, at Harvard, assisted by Whipple, took the first known daguerreotype of a star. The subject chosen was Vega, a brilliant bluish-white star in Lyra. The time of exposure is not now known, but it is on record that Polaris, a star of the second magnitude, made no impression on the sensitized plate no matter how long an exposure was given, and that the bright, easily separated, yellowish-white, first magnitude, double star Castor, gave only an elongated image. In 1858, Luther, of Koenigsberg, showed De la Rue a daguerreotype of the total solar eclipse of 1851, taken by Busch with a heliometer. Notwithstanding great difficulties. De la Rue, in 1852, eventually succeeded in obtaining some good lunar images about one inch and one-quarter in diameter. Others in England who worked with much zeal were Rutherford, Reade, Phillips, Bates and Hartnup. In 1854, Hartnup obtained ten collodion pictures of the Moon nearly one inch and one-half in diameter. From these he made, on glass, copies, enlarged, some of them, to four and one-half inches, which, in turn, were used to throw on a screen faithful images of the Moon eight feet in diameter. On the 24th of April, 1854, Sir John Herschell, in the interest of science, suggested a daily photographing of the Sun, and the Royal Society entrusted the work to De la Rue, for whom a special photo-heliograph was made by Ross the optician. Having, by 1857, secured a proper driving-clock, De la Rue succeeded in eclipsing all previous efforts wheresoever made, and in producing lunar photographs which "must still be classed as good," because, though but one inch and one-tenth in diameter, they bore magnification sufficient to show the existence of any object on the

Moon if it were two miles square. Having seen Lord Rosse's great telescope in 1857, Dr. Henry Draper, the worthy son of a worthy father, determined to construct a large reflector and to use it for astronomical photography. With a silver-on-glass mirror, made and mounted by himself, he, in the course of several years, took fifteen hundred photographs of the Moon, one of the best of which, made September 3rd, 1863, though but one and one-tenth inches in diameter, was successfully enlarged to three feet.

In the meantime Bond, who had persisted in his stellar work, succeeded in 1857 in photographing, in two seconds, Mizar, one of the components of Zeta Ursæ Majoris, and in eight seconds, Alcor, the other component which is green in colour. Both these intervals of two and of eight seconds respectively, are each fifty-three times as long as the periods now required to photograph the same stars with rapid dry-plates. Lord Rosse himself tried celestial photography with his six feet reflector, but his efforts, for want of a driving-clock, could not have been successful; at least no photographs taken by him were ever published. About this time it occurred to Bond that the photographic method could be employed for determining and classifying the magnitudes of the stars. As a result of his experimental work, he came to the conclusion that the photographic magnitudes of stars increase by equal areas for equal increases in times of exposure, which is said to be also true of eye-estimates. In 1860 and 1863, De la Rue succeeded in photographing sun-spots on a scale of three feet and of thirteen feet respectively. In 1858, Mr. Underwood exhibited what was claimed to be the first photograph of a comet, being that of Donati, taken in seven seconds with a small portrait-lens, which gave an image about one inch in length. In 1861, De la Rue disputed the accuracy of the claim, alleging that all of his own attempts to photograph the comet of 1861, as well as that of Donati, had failed, though he had employed both a telescope and a portrait-lens, and had given exposures as long as fifteen minutes. In 1860, De la Rue met with most gratifying success in photographing at Rivabellosa, in Spain, the total eclipse of that year. De la Rue took two photographs, which not only showed the red prominences, but proved that the prominences do not belong to the Moon. Father Secchi's photographs, taken at a point 240 miles from Rivabellosa, confirmed those of De la Rue, thus setting forever at rest "a much vexed question of those days." On the 27th of February, 1863, Dr Wm.

Huggins obtained a photograph of the spectrum of Sirius, but from instrumental inadequacy, got no indication of lines, though the spectrum "was tolerably well defined at the edges." In 1864, Rutherford, of Harvard, figured the first telescopic objective intended for photographic purposes, and with it succeeded in getting on his plates in three minutes impressions made by stars of the ninth magnitude. On the 11th of January, 1869, Janssen pointed out that it was possible to isolate any part of a spectrum by placing a second slit near the eye-piece,—"an idea which underlies some of the most remarkable results of the present day;" but which lay dormant until 1892. In 1870 by using an open slit on the spectroscope, Prof. C. A. Young photographed the solar prominences. In 1872, Draper having made a 24-inch reflecting telescope, photographed the spectrum of Vega, "showing four strong lines." In the same year, Dr. Ellery, of Melbourne, produced the finest photograph of the Moon "seen up to that date." In 1873, Sir George Airy acceded to a general request that during the approaching transit of Venus photography should be employed for determining the position of the Venus on the solar disc. The results, however, were not as successful as had been hoped for. The year 1876 was made notable by Huggins who, after many experiments, announced his preference for dry-plates, which had been introduced in 1871, and, as a result, the use of the collodion process in astronomical photography was gradually relinquished. One decided advantage derived from the adoption of the dry-plate was the acquired ability on the part of the photographer to extend his exposures, necessarily short with the wet-plate process, to hours, and even to days, did the faintness of any given object require it. Huggins was now able to photograph the spectrum of Vega, and to discover that of the seven hues recorded, two coincided with those of hydrogen—an important advance in knowledge. In 1877, Draper announced his discovery of oxygen in the Sun. In the same year, Janssen got solar images twelve inches in diameter, which displayed, in sharp detail, sun-spots, willow-leaves, rice-grains, and a fine photospheric net-work—an advance due to the use of an improved flashing shutter, which permitted of an exposure as short as the one two-thousandth part of a second. In 1878, Draper, during a total eclipse of the Sun, photographed the spectrum of the solar corona. In 1879, Huggins reported that he had secured impressions of the spectra of Sirius, Vega, Arcturus, Betelgeux, Capella, Rigel, and of Alpha and Beta Pegasi, and of Alpha Herculis and of Venus, Mars and Jupiter, and suggested photography for the discovery of variable stars. In 1881, Dr. H. C. Vogel, of Berlin, photographed the spectra of rarified oxygen ; Draper the spectrum of the Great Nebula in Orion, and Huggins, Draper and Janssen, independently, the spectrum of Comet *b* of that year. In 1882, Huggins and Draper secured still better photographs of the spectrum of the Orion Nebula, and, by using absorbing media, Huggins succeeded in photographing the solar corona without an eclipse. In 1883, Dr. E. C. Pickering designed a very ingenious star-camera for the purpose of making regular comparisons of star magnitudes. In 1884, the worthy brothers Henry, of Paris, while photographing the small stars in the ecliptic found upon their plates the trails of asteroids, and since then photography has been successfully resorted to as a means of discovering these minute planets, of which more than four hundred are now known to exist. In 1885, the Henrys, with a larger camera, discovered the nebula surrounding the star Maia in the Pleiades. They even succeeded in getting on their plates stars of the seventeenth magnitude—that is, stars which have never been seen by the human eye, though assisted by the most powerful telescopes of the age, thus incontestably proving the superiority of photography over visual work in some fields of research. In the same year, the value of Pickering's stellar negatives was shown by the convincing proof they afforded of the variability of the new star in the Orion Constellation which had been discovered by Mr. J. Ellard Gore. An examination of the plates at the Observatory revealed the fact that the star, as a much fainter object, however, had been photographed at Harvard five weeks before naked eye discovery. On the 15th of March, 1885, Tromholt, at Christiania, photographed a very brilliant aurora. In 1886, Dr. Pritchard, at Oxford, successfully applied the photographic method to the determination of parallax, selecting 61 Cygni for the purpose. In the same year, Dr. Isaac Roberts, who had been eminently successful in faint object photography, discovered that the Maia nebula was much larger than had been supposed, "many branchings seeming to form a background for the whole cluster of the Pleiades."

In 1887, agreeably with an invitation from Admiral Mouchez, Director of the Observatory, fifty-four astronomers of all nations met in Paris and agreed to undertake to photograph all the stars, partly for the purposes of measurement and for cataloguing, and partly for the purposes of comparison throughout future years—a labour of love, which has progressed far towards completion. Continuing our record, it may be mentioned that Huggins was the first to use the spectroscope for determining the motion of stars in the line of sight, as was Prof. J. E. Keeler to determine that of certain nebulæ, and, later, the composition of Saturn's rings. Vogel was the first, however, to employ photography to record spectra in order to determine star motions in the line of sight. Fifty-one stars were selected for this purpose, and their motions satisfactorily decided. On the 29th of December, 1883, Roberts surprised the scientific world by his famous photography of the great Nebula in Andromeda, which showed its extent and complex character. Under the supervision of Pickering, and as the result of the munificence of Mrs. Henry Draper, now a widow, and of Miss C. W. Bruce, all the spectra of stars above the eleventh magnitude are being photographed by Harvard astronomers for future reference. Already, as depending upon these plates, it has been discovered by two of the many ladies employed on the computing and measuring staff of the Observatory, that the stars Mizar and Beta Aurigæ belong to a hitherto unknown class of binary systems. These suns, even in the most powerful telescopes, appear as single stars, yet they are really composed of two, each major, or primary, star having in rapid revolution around it a companion whose existence had not previously been suspected. These solar systems are situated at a vast distance from us. The smaller suns, for they are suns, are so close to the larger ones that, at their respective distances, no telescope ever likely to be constructed will reveal them as separate stars. If you mark off into thirty equal parts a line equal to the diameter of the full Moon, as seen from the Earth, you will, roughly speaking, have thirty minutes of arc. If you sub-divide any of these minutes of arc into sixty equal parts, you will have sixty seconds of arc. You will then perceive that one second of arc is a very minute quantity to measure. Where two suns are said to be separated by one-half of a second of arc, it is meant that in a telescope sufficiently powerful there may be seen between them a space of black sky one-half of a second of arc in width. Stars separated by less than one-tenth of a second remain as single stars in all but the best telescopes in existence. There is, therefore, no hope that any instrument will ever be equal to dividing visually Mizar and Beta Aurigæ, because their spectra show them to be separated from each of their companions, that is as seen from the Earth, by a distance so infinitesimal as the one four-thousandth part of a second of arc. But for photography, which enables the astronomer to record and measure the spectra of the stars, this discovery might never have been made. But time presses, and we must pass on, though much could be said of the photographic achievements of able men like Holden, Deslandres, Lockyer, Seabrooke, Russell, Gill, Burnham, Schaeberle, Barnard, Brooke, Lohse, Zenger, Ricco, Gothard and others whose names will occur to you, their work having been more modern than that of those astronomers to whom previous reference has been made. Still, special mention must not be omitted of the original and valuable work of Dr. G. E. Hale, of Chicago, who has been appointed to be the Director of the new Yerkes' Observatory near Lake Geneva. Dr. Hale has been able, largely by means of ingenious devices of his own invention, to photograph, for the first time, all of the visible solar phenomena, including the faculæ on the central portion of the Sun's disc. Being yet a young man, with opportunities which seldom fall to the lot of young men, Dr. Hale, has, no doubt, before him a long and useful career.

A fundamental point to be settled with regard to the effectiveness of any photographic telescope is to determine its light-gathering power. The rapidity of the action of an object-glass, or of a mirror, for photographing a luminous surface depends, all other things being equal, on the square of the ratio of its aperture to its focus. A small lens of very short focus will photograph a much fainter surface than a large telescope can. With faint stars, if an enlarging lens be inserted between the focus and the photographic plate, still better definition will be secured. In a visual telescope, aperture is more important than focal length, which is of little consequence, the magnification depending on the eye-piece. In the case of a photographic telescope, the main question is one of focus, the aperture having nothing to do with the result; if, for instance, the aperture were to be made twice as large, other things being equal, the operator would simply give a much shorter exposure. It has been found by a study of the faint stars lying within the Huyghenian region of the nebula of Orion, that, for a region of this brightness, a photographic telescope whose focal length is sixteen feet, whatever its aperture, is equal to a visual telescope of eight inches' diameter, whatever its focal length. Outside of this region, and within that portion of the nebula that can be readily seen, the photographic instrument is equal to a visual telescope whose aperture is fifteen inches. Against a perfectly black background of sky, a photographic refracting telescope with two hours exposure should be nearly equal to a visual telescope of the same size. For stars, however, the rule is modified by the character of the image to be obtained. In general, with any form of telescope, apertures being equal, it is found that the shorter the focus the fainter the stars that can be photographed. Stars surrounded by a nebula form an exception. With a rapid doublet and an hour's exposure, the light of a bright nebula completely obliterates all the stars found within it. The light of a nebula is diminished in proportion to the square of the focus of the lens, while the brightness of the stars decreases much more slowly. Therefore, by giving a somewhat longer exposure, the star images will be as bright as before and the nebula fainter. With the focus sufficiently long, stars could be photographed at mid-day.

In a masterly discussion of the whole subject (see *Annals*, Harvard Observatory, Vol. XXXII., Part I.), Prof. W. H. Pickering, to whom I am largely indebted for the information collated here, explains that three kinds of telescopes may be employed in photography, viz., the refractor, the reflector and the doublet. The doublet, he says, is, in fact, a very large camera with lenses ground to curves like those of the portrait-lenses found formerly in photograph galleries, but now largely displaced by those of the rapid rectilinear varieties. Each of these photographic telescopes has its own field of work in which it would be useless to attempt to supplant it with either of the others. The refractor gives the best definition and has a moderately sized field of two or three degrees, depending upon the character of the definition demanded. Having a smaller angular aperture, it is particularly adapted to the photographing of the bodies composing the solar system, certain bright star-clusters and nebulæ and widely separated double-stars. The plates taken by it are also best adapted for the measurement of position. Reflectors and doublets can be made with large angular apertures and are, therefore, well suited for photographing faint objects of all kinds. The field of a reflector never exceeds one degree unless the angular aperture of the telescope is very small. On the other hand, a doublet has a very large field and gives good definition over a circle seven degrees in diameter, or throughout a square measuring five degrees on each side. For many purposes, a doublet will give sufficient definition over a field ten degrees square. In a word, for bright objects and sharp definition, use a refractor; for ordinary nebulæ and faint stars, use a reflector; for the faintest and very large nebulæ, and for charting use a doublet, and for the very largest nebulæ, use a small doublet with a wide angular aperture and, at the same time, a large field. With respect to the light-gathering power of a telescope, there is one correction which must not be overlooked, viz., the correction dependent upon the absorption of the glass of which the objective in a refractor, or the metallic surface forming the mirror in a reflector is made. Visually, absorption is matter of small account ; photographically, it is of great moment. A refractor of, say, fifteen inches, if made of perfectly colourless glass, transmits about forty per cent. of the actual energy it receives, but if either of the lenses forming the objective shows a small tinge of yellow, it will transmit very much less. A doublet transmits still less, say about twenty-five per cent. Here large reflectors have the advantage, silver-on-glass reflecting about fifty per cent. of all the actinic energy which strikes it. To a certain degree, however, the absorption even in the case of the reflector depends upon the colour of the light reflected.

A photographic telescope intended for the most difficult classes of work necessitating long exposures, must not only be constructed according to the best models, but must be mounted in the most rigid manner possible, and be driven by a clock of the highest excellence. From the writings of those who have done such work and have used such apparatus, some idea may be gained respecting the extreme accuracy with which the mechanical adjustments must be made, and the care with which they must be employed. For instance, in a telescope of sixteen feet focal length, a second of arc is rather less than ·001 of an inch, a quantity quite invisible to the naked eye. The image falling upon a photographic plate must not shift from one side to the other from its true position, even by this small amount. If an enlarging lens be in use, a shift of ·0005 of an inch will be evident, and probably one of ·0002 would be noticeable. While this must be the rule in special work on the Moon and other bright surfaces, like accuracy is not required in the case of faint objects, such as stars where a slight elongation is not objectionable, as it is only by this elongation that stellar impressions, in certain cases, can be distinguished from the minute imperfections in the film on the plate. Even when these telescopes are driven by the finest clocks yet constructed, the operator is obliged to attend them during every second of time the exposures are being made. For reasons that cannot be explained here, it has so far been found to be impossible to construct a clock which, under all circumstances, will drive, for hours at a time, a telescope with the perfect accuracy that would render it independent of human supervision. The motion which must be counteracted by the clock, is that due to the rotation of the Earth on her axis. It is this motion, of which we are entirely insensible, which causes the Sun, Moon and stars to move apparently towards the West. The Earth carries everything on her surface in the opposite direction, or towards the East, at a speed varying as the distance from the equator, where every object is continuously moving at a velocity exceeding one thousand miles an hour, or about eighteen miles a minute. This real motion, of which we are not sensible, creates in the heavens an apparent motion of which the astronomer, and particularly, the astronomical photographer must take account every instant he is at work. The duty of the driving-clock is to move the telescope with the stars at a rate so exact that to any one looking into the instrument the stars stand perfectly still hour after hour. It is to this duty, which at present the astronomer must share, that Prof. Pickering alludes when he speaks of the necessity of keeping the photographic image always falling exactly upon the same spot. And this it is, which, while it makes celestial photography so arduous, renders the work of the successful photographer glorious and valuable, having regard to the purposes of future measurements and comparisons.

There are in this city two equatorially mounted clock-driven refracting telescopes which can and have been employed for the purposes of astronomical photography of the more difficult kinds. But for the majority of our members such instruments of precision are beyond their reach. For this reason, some of the special pleasures associated with the art are denied to some of us, but, happily, this denial is not so sweeping that we must be deprived entirely of a share in this work. With any telescope howsoever mounted, very good photographs of the Sun and Moon may be taken, the requisite exposures being practically instantaneous. If the images are small they can be enlarged by processes familiar to anyone conversant with photography in any of its branches. It is very easy to adapt to the eye-end of any telescope a light-tight box made of sheet-metal or wood, or even of paste-board, which, for one's first lessons, may be used as a camera in which to expose sensitive plates, which can be had of all sizes and speeds and at very small cost. Indeed, after one gains some experience, these very facilities will be found to be quite adequate for work of a character more pretentious than what might be characterized as experimental and elementary. I have here a camera made, in a couple of hours, of sheet-copper. It is very light and can in a moment be attached to the eye-end of a reflecting telescope. With it very fair photographs of the Sun and Moon have been taken. Similar, simple, but none the less effective, cameras have been made for other telescopes in this city and some very creditable negatives have been developed from the plates exposed in them. There is no reason why the pleasure of making instantaneous views of the Sun and Moon should not be participated in by scores instead of a dozen or so members of the Society. To this pleasure may be added the delight attaching to the development of one's plates and the printing of pictures from them. And now that facilities for making lantern-slides are within reach of any of us, there is still another pleasure to be derived from photography, namely, the exhibition of slides. With appliances such as have been described exposures can be made on the planets and the bright stars. But even for the fainter stars photographic telescopes are not absolutely essential if one has the necessary driving apparatus, some of the best photographs of the stars, of vast nebulæ, and even of comets having been taken with ordinary portrait-cameras similar to those found in studios scattered throughout the country. The lenses and focal lengths of these cameras are extremely well adapted to several most interesting varieties of photographic work. Nay, even the one and a-half inch lens belonging to a cheap oil-projecting lantern, such as are used at magic lantern entertainments, have been found to be capable of photographing objects utterly beyond any photographic telescope, though it cost many thousands of dollars. Such lenses have even made discoveries which would never have been made had discovery depended upon a telescope, or an ordinary telescopic camera. In proof of this, it may be mentioned that in 1890, Prof. W. H. Pickering, with a small portrait-camera, like that described, discovered the Greater Orion Nebula, which is vastly larger, though fainter than the "Great Nebula," with which astronomers have long been familiar. In 1894, Barnard, who had not heard of Pickering's discovery, independently found the same nebula on a plate he chanced to expose in a camera armed with a cheap oil-projecting lens such as has been referred to. This discovery was the result of an effort to see what such a "toy-lens" could do. This lens covers a field of thirty degrees, of which, however, only one-half degree is flat, yet Barnard assures his readers that it is adapted for photographing comets, meteors, and nebulosities. The new Moon herself has been photographed by it in from one to three seconds, that portion of her surface illuminated by Earth-shine being well brought out. By means of this lens, costing but a few dollars, clouds in the Milky Way have been photographed in ten to fifteen minutes and the Great Nebula in Orion has made a good impression on a plate in less than one hour. With it, too, Barnard last summer proved the existence in Scorpio of a hitherto unsuspected nebula, occupying a region of the heavens upon which telescopes have been turned for generations, but without revealing its presence.

But photography is a useful assistant not only to the astronomer but to the meteorologist, as I am sure Mr. R. F. Stupart, the Director of the Toronto Observatory, or any member of his staff, will demonstrate to any one who cares to examine into this branch of scientific work. The instruments of which a photographic record of changes is kept at the Observatory are the barometer, dry and wet-bulb thermometers, declinometer, and horizontal and vertical-force magnetometers. In each of these instruments the principle developed is the same, viz., the action of light on photographic paper wound around a cylinder, which involves by clock-work once in twenty-four hours. In the case of the barometer, the light from a gas-jet shines through the slit between the top of the barometer-case and the mercury in the tube and passes thence to the cylinder. In the case of the thermometers, a ray of light is passed through an air-speck in the mercurial column of a specially prepared thermometer, and is reflected back on the cylinder, which is placed within the building. In the case of each of the magnetometers, a slit of light is passed through a lens on to two mirrors, one of which is attached to the magnet and the other to the base of the instrument, and thence, after being brought to a focus by being passed through a semi-cylindrical lens, the two rays fall on the cylinder—the ray from the stationary mirror affording a base-line. In order to have a time-scale, an automatic cut-off of the light acts every two hours, and the accuracy of the time-scale is dependent on the bargraph-clock, which, by means of an electric attachment, cuts off the light.

When one glances over the field of astronomical photography and sees men like Roberts, Common, Russell, Ellery, the Henrys, Barnard, Pickering, and many others who, regardless of exposures during the long vigils of the night, often sharpened by bitter cold, and with their eyes glued to the finders of their instruments, compelled, as second after second grows into weary hour after weary hour, to stand or sit, some times in uncomfortable positions, guiding their telescopes, one cannot but exclaim, ALL HONOUR TO THEM ! Great as are the present admitted obligations of Science to these men, and to the men who have gone before them, it may be that the full measure of the value of their work shall not be known for centuries, or, at least, until, for the purposes of comparison and verification, the astronomers of the future have had occasion to refer their own observations to the enduring records now being compiled. In adding to the knowledge of his day, Ptolemy was in no small measure indebted to the labours of Hipparchus, and to the crude star-map that philosopher left behind him. Copernicus, in turn, was indebted to Ptolemy. Kepler was indebted partly to Copernicus and very largely to the long and unselfish labours of Tycho Brahé. Newton, for his immortal discoveries, was entirely dependent upon information derived from others. Some observations he used descended to him from the time of the Chaldeans. Others were provided for him by contemporaries, who toiled away in their observatories and computing rooms to wake, or reduce, observations necessary to the completion of his work. And so it may be with our own contemporaries, who, in her interests, are giving to science so lavishly of their time and of their means. Upon their work may be depending discoveries of immense importance, of which to-day no man is even dreaming.

Probably you will allow me to conclude this paper with a series of paragraphs which must be interesting to us all, and which should be useful to many, as they contain information compiled from various authentic sources, or kindly placed at our disposal by the staffs of leading observatories in Europe and America.

As a rule, experienced photographers will advise you to use, in terrestrial work, the slowest plate your subject will allow. Excellent plates of extreme speed have been made, but so far it is impossible to make rapid plates which possess the best qualities found in first-class plates of average rapidity. The same rule appears to hold good in astronomical work. Use the slowest plate that will do justice to the celestial object about to be photographed. Take two photographs of each object, with slightly different exposures, so as to guard against uneven films and possible insufficient exposure. On the Sun, use slow plates and reduce your telescopic aperture to at least three inches, depending, if necessary, upon enlargement, either by an eye-piece, or by the enlarging camera. On the Moon, use slow plates if you are photographing the brightest portions; the most rapid plates if you are exposing for the terminator. On stars, use rapid plates, selecting higher rates of speed as the faintness of the object increases. For the Moon, the most popular object for the amateur, one authority recommends that the photographs be taken in the principal focus of the telescope and be enlarged, though he cautions the astronomer that the grains of silver in the films, fine as they are, are still sufficiently coarse to give trouble on enlargement. A grain of silver measures about the one two-thousandth of an inch, which is equal to the ·07 of a second of arc, or to the ·08 of an English mile on the lunar surface as seen in the Lick telescope. In passing, it may be said that a trained eye can see with a high power on this telescope, extremely small but well-defined crater-pits better than, with the present coarseness of the silver-grains, they can be photographed. Still, there are other features which, owing to contrast, can be photographed better with an aperture of eight inches of this telescope than they can be seen with the whole thirty-six inches of aperture. Speaking of the performance of this telescope on the Moon, it is interesting to know that, when observing under favourable conditions, the view is "sublimity itself." Under exceptional aspects when the highest powers may be used, the lunar scenery is fascinating in the extreme. "You seem," Barnard says, "to be suspended only a short distance above the mouths of the stupendous craters. The dreadful feeling forces itself unconsciously upon you that you may at any moment lose your hold on Earth and be dashed to pieces within their yawning cavernous depths." These realistic effects cannot as yet be brought out in the photographic plate, but the future may have surprises for us even here.

The writer desires to express his obligations to the following gentlemen, who most cheerfully complied with his request for the information they have communicated so clearly and succinctly. The observatories are placed in alphabetical order.

GREENWICH OBSERVATORY.

At Greenwich the stellar photographs (those intended for the catalogue as well as those for the chart) are developed with eikonogen, using Dr. Andressen's cartridges. The only departure from the original formula being that the mixed developer is used at half strength, each cartridge being dissolved in seven ounces of water, preferably distilled. As the catalogue plates are exposed long enough to contain all stars down to the 11th magnitude under good average conditions of sky, and as any plate may contain stars varying by five or six magnitudes, the brighter stars will necessarily be over-exposed and must be left to take care of themselves, the development being prolonged sufficiently to give good measurable images of the 11th magnitude stars. Developing for thirty minutes with the developer prepared as above is found to answer the purpose quite well, and if proper care is taken in the manipulation there is no risk of getting the plates stained with this, and even much longer, time spent in development. The brighter stars will, of course, show considerable halation when the plates are not *backed*, but this has not been found to injuriously affect the measurement of smaller stars that may be included in the circle of halation. The plates at present in use are made for us by Messrs. Mawson & Swan, and are sufficiently sensitive to shew the stars of the 9th magnitude on Argelander's scale with an exposure of 20 seconds. Nothing can be seen on the plate during development, except the reticule and perhaps one or two of the brighter stars, and what success has attended the exposure of a particular plate can not be known till after the application of the hypo bath. It is the practice to leave the plate in the hypo bath some two or three minutes after all milkiness has disappeared, to insure the complete removal of the unaltered silver salt. After the hypo bath, the plates are washed in running water for about half an hour, then placed in the alum bath for a few minutes, and again washed for two or more hours. Only in the very hottest weather do these plates shew any sign of frilling, and even then very little.

G. S. CRISWICK.

The treatment of the solar photographs at Greenwich is necessarily entirely different from that for the stellar. For these "lantern" plates are used, partly because of their relative slowness, a great advantage when dealing with so bright a body as the Sun, partly because of the more delicate detail they permit, and partly because of their greater transparency, a point of great importance in plates intended for micrometric measurement. Hydrokinone is the developer always employed, and usually in a very dilute form. The average time that the plate is kept in the developer is ten minutes. As with the stellar plates, it is impossible to judge by the appearance of the photograph during development as to how the detail is coming out. When a distinct black image of the Sun can be seen through the back of the plate, it is assumed that sufficient density has been obtained. The exposure is the one critical point in solar photography. The plate must be *lightly* exposed. If the image begins to come up immediately the developer is poured on, the exposure has been *much* too long. There should be no sign of an image for a minute or two, and then it should come out slowly and gradually. I have seen several solar photographs by amateur astronomers, which left nothing to be desired as to focus, photographic manipulation, and so on; but in every case the result was unsatisfactory owing to the exposure having been much too long. This is a serious pity, for there is no department of astronomical photography that could be so well taken up by many amateurs as that of the Sun. The aperture used at Greenwich is only 29 inches for the production of eight-inch pictures, many of which are only inferior to the very best obtained by Janssen, with much more powerful means; and the exposure being instantaneous, no driving clock is needed.

E. W. MAUNDER.

HARVARD COLLEGE OBSERVATORY.

The plates generally used here for celestial photography are the most sensitive (No. 26 +) made by the M. A. Seed Dry Plate Co., of St. Louis. We have recently obtained some of these plates numbered 27, and presumably still more sensitive. For the work done here, long exposures are usually required. Short exposures can be made, without running the risk of shaking the telescope, by a screen held before the object-glass, or by turning the dome, if it runs smoothly enough to occasion no tremor.

The following developer is used :—In 384 oz. water dissolve 12 oz. sulphite of soda and 12 oz. carbonate of soda (use 6 oz. of carbonate when the so-called granular carbonate is used). In 16½ oz. alcohol dissolve 3 oz. salicylic acid and add to the first solution. Shake, or stir, until thoroughly mixed. This forms the liquid portion of the developer and may be kept indefinitely, being drawn off and used without dilution. When ready to develop, draw off the required amount of the solution and add pyrogallic acid in the dry form in the proportion of about 1 gramme to every 4 liq. oz. of solution. EDWARD C. PICKERING.

LICK OBSERVATORY.

The specific objects sought in development vary in different cases. In stellar photography there is practically no limit to the exposure which may be given—the longer the exposure the fainter and more numerous the stars photographed, and the development is expected to bring out the faintest objects possible. Development should here be continued to the point of fogging, or even a *little* beyond, to be certain of having reached the limit. The fog may be either chemical or due to diffused light. In the latter case, not uncommon with very long exposures, developer of only moderate strength should be used at the outset, to avoid *immediate* fog; the stars should be developed first; the strength may be increased from time to time as needed. I think it is well to continue development for some little time after the back of the film, seen through the glass, begins to turn dark. Where faint stars or nebulæ are photographed the very bright objects are necessarily over-exposed; the details in bright nebulæ are "burnt out," and there is no adequate remedy. Where particular details are sought, and the subject presents strong contrasts, it is well, if pyro be used, to begin with somewhat dilute developer, gradually add the alkaline solution until the details are sufficiently visible, then add pyro in the same manner until enough density is attained for printing or other use. It is always *safe* to commence with developer of only moderate strength, and then modify it according to the needs of the case. After some practice one acquires a sort of intuition as to what to do, and it is difficult to formulate definite rules. Practical acquaintance with the behaviour of some particular developer (*i.e.*, made after a certain formula) counts for a good deal. In a general way I regard the "rushing" of plates in development as of doubtful utility ; a moderate amount of "coaxing" may be resorted to with good result, especially in cases of under-exposure. To obtain a specific effect *both* exposure and development need to be considered. For general work I like a plate so fully exposed that the normal developer, instead of *driving*, seems to *allow* the image to come up in a comfortable, leisurely sort of way. Where considerable crispness of definition is desired the exposure should be so short that the requisite detail and density are to be had by a *little* coaxing, with strong developer. I am becoming more impressed with this point in the course of my experiments in making enlargements from our Moon negatives. In making direct photographs of the Moon for scientific purposes the terminator is, of course, chiefly considered. *Theoretically*, it is possible to expose and develop for a given effect upon a particular region, but I regard that as hardly practicable as there are so many factors of uncertainty, *e.g.*, the illumination of the region, the atmospheric absorption of light at different altitudes and different times, the strength and temperature of the developer, the rapidity of development, and the final density of deposit. The only way to get everything possible out of such a subject is to make a series of exposures, of different times, as we do here, and develop until the terminator in general is judged to be about right. The rapidity of plates has its effect upon the result, the slower plates giving the stronger contrast. For the Moon we use the most rapid plates obtainable to avoid, as far as possible, any motion of the image on the plate. The results at the terminator are quite satisfactory, but the plates could hardly be expected to give much contrast in the more brilliantly-lighted portions of the disc. On one occasion we used some very slow plates (Carbutt A) on the full Moon, as an experiment, and brought out, in strong contrast, the bright streaks radiating from Tycho. The merits of different developing agents are worth considering, but in this matter I have had but limited experience. For general negative work I doubt if anything better than pyro is to be had; it is prompt in action, fairly elastic in application to different exposure-times, and gives fine printing qualities. Unfortunately it is said to deposit the silver in courser granules than some other agents —an objectionable feature in the case of negatives to be enlarged from. The hydrokinone developer, described in our Vol. III, is *very* convenient to use, “keeps” well, is applicable to an extremely wide range of exposures, and gives a good colour for transparencies and lantern-slide’s, but is prone to give a chemical fog. For this reason I am inclined to discontinue its use, but have not fixed upon its successor. At present I am experimenting with the new agent, glycin, which seems to promise well. It has one good quality at least, it does not easily fog the plate. The solution does not keep well, and so should be made in small quantities. As to the Sun, it is our experience in using the photoheliograph of 40 feet focus, that only very slow plates will give the needed contrast, and that only with extremely short exposure—so short as to need considerable coaxing to bring the images to the required point. We generally use Carbutt A. So many unknown factors enter into the problem of development that it is not easy to formulate very definite rules. Much experimental work will be needed before this can be done.

A. L. COLTON.

PARIS OBSERVATORY—(TRANSLATION).

I employ a developer which differs a little according to the purpose in view. For stellar plates, which require a decided contrast between whites and blacks, I use the following formula :—

Solution A. —Water, 1,000 gr.; Oxalate of Potash, 400 gr.
Solution B.—Water, 1,000 gr.; pure Sulphate of Iron, 300 gr.

When ready to develop, I pour one part of Solution B into three parts of Solution A, and if the temperature does not exceed 16° Centigrade (60° Far.), I leave the plate in the mixture for at least thirty minutes. For lunar plates, I mix five parts of A with one part of B, to which I add several drops of a five per cent. solution of sulphite of soda. This addition of sulphite gives softer and clearer plates. I fix the plates in a twenty per cent. solution of hyposulphite of soda and then place them in a bath containing five per cent. of alum. I find it to be desirable to strengthen the lunar plates, in order to increase the intensity of the terminator, which is always too weak, by immersing them, moistened, for thirty seconds only, in a bath of four per cent. of bi-chloride of mercury, and, after a prolonged washing, in a feeble solution of ammonia in water, it may be useful to add that the photographing of the stars, in order to secure excellent results, does not require a very calm atmosphere. During windy, but very clear, weather I have always obtained fine stellar photographs. But, with regard to the Moon and the planets, on the contrary, an absolutely calm air is indispensable in order to secure first-class impressions, especially if you greatly enlarge the focal image. I need not say in addition, that in all cases, a very rigid position at the photographing point must be maintained to procure the maximum of effect. P. HENRY.

TORONTO OBSERVATORY.

I would like to add the developing formula used at this Observatory, which is one of the oldest and most important magnetic observatories in the world.

1. *Developer* :—Saturated solution neutral oxalate of potash . . . . 4 oz.
" " sulphate of iron . . . . 1 oz.
60 grains to oz. solution of bromide of potassium,
or ammonium . . . . 6 drops.
Also 1 drachm of citric acid to each pint of iron
solution to keep it from oxidizing.

2. *Fixing Solution* :— Hypo.-sulphate soda, 1 part to 6 of water.

3. *Clearing Solution* :—Sulphuric acid, 1 part to 80 water.

The photographic paper used is the Argentic gelatino-bromide manufactured by Morgan & Kidd, Kew, England. R. F. STUPART.

The paper was followed by a special exhibition, with an arc lamp, of lantern-slides comprising a series selected from the Society's collection ; thirty six loaned by a New York firm; seven loaned by Mr. John E. Lewis, a very talented amateur residing at Derby, Conn., and a dozen loaned by Mr. J. R. Connon, an experienced photographer at Elora, Ont. Mr. J. R. Collins took charge of the stereopticon in a very efficient manner, and Mr. Lumsden made comments on the various shades. Mr. Connon's series included several very good solar slides, and one from a negative of Elora Falls, made by his father thirty-five years ago, illustrated in a very marked degree the excellent work in the days of the wet-process. Mr. Lewis' collection included a slide of Holmes' Comet and of the Andromeda Nebula made by attaching an ordinary camera to a telescope and keeping the latter in motion for half an hour by a slow motion by hand; one of the Orion Nebula; two showing star-trails at the pole and at the equator; one of Gale's Comet, and two, one enlarged, of the bright meteor photographed by Mr. Lewis, January 13th, 1893, with his camera which has a 3½-inch lens of 13 inches' focus. The very fine slide from the original meteor-negative was presented by Mr. Lewis to the Society.

TWENTY-THIRD MEETING.

November 26th; this was an open meeting, held in the Physical lecture room of the University of Toronto, the Vice-President, Mr. John A. Paterson, presiding.

The attendance of members and their friends was very large, special invitations having been issued and the announcement made that the third of a series of popular lectures would be delivered by Mr. C. A. Chant, B.A., Lecturer in Physics, and that he would conduct several of te classic experiments of Hertz.

The usual routine business of the Society's meetings was suspended after the reading of the minutes, and the Chairman, having acknowledged the kindness of Prof. Loudon, President of the University, in placing the building at the service of the Society, introduced the audience to Mr. Chant, saying that he could not introduce Mr. Chant to the audience when the former was in "his own den, his own lair."

The following is the full text of the lecture :—

Electrical Radiation.

With radiant light and radiant heat we have long been acquainted, but to most of you present, I doubt not, radiant electricity is a veritable stranger. Only in recent years has he appeared openly amongst us ; but his growth has been almost phenomenal, and now he is a well-developed youth. In almost all the latest works on electricity you can learn about his appearance and his doings.

And yet, this branch of electrical science did not grow up with such wonderful suddenness after all. Fully fifty years must we look back to see its beginning. At that time the great Faraday was cross-examining nature with a penetration little short of intuition. He held a magnet near another magnet, or near some bits of iron, and even though many inches apart their mutual attraction or repulsion could be seen. Again, one electrified body was observed to influence another some distance away. He also discovered that when a current of electricity in one coil was started or stopped, a galvanometer attached to another coil, entirely disconnected from the first coil, was affected. This truly was not a weed which he drew up, but a most valuable fish.* Many a time also, he was impressed with the grandest of all these attractions, namely, that between the heavenly bodies—the force of gravitation.

Faraday learned, as generations have been taught, that one body attracts another, the force decreasing inversely as the square of the distance, but with that knowledge he could not rest. Such a delicate skeleton might suffice to bear the gauzy garments woven in the subtle brain of the mathematician—often, truly, of surpassing beauty, if you are able to see them—but the great investigator demanded something more tangible. If body A attracts body B when ten inches apart, there must be ten inches of something between them. The influence passes from *A* to *B*; it surely cannot pass if there is nothing to carry it. What then is the mechanism by which the transfer is effected? And, in the second place, how much time is required to make the transfer?

These were questions of amazing difficulty, but Faraday resolutely set himself to find the answers, and in his search he enriched our store of natural knowledge more than any other man has done. To the first, he obtained answers fairly satisfactory to himself; on the second, his work was not conclusive. But, to his death, he persisted in the belief that the action was not instantaneous, and that the time rate would be measured. Lord Kelvin says that the last time he saw Faraday at work he was in the cellar of the Royal Institution, which had been chosen on account of its freedom from disturbance; there he was arranging experiments to test the time of propagation of magnetic force from an electromagnet, through a distance of many yards of air, to a fine needle

* In a letter to R. Phillips, September 23rd, 1831, Faraday said: "I am busy just now again on electro-magnetism, and think I have got hold of a good thing, but can't say. It may be a weed instead of a fish that, after all my labour, I may at last pull up."—*Life and Letters, Vol. 2, p. 3.*

polished to reflect light; but no result came from these experiments. About the same time, his attempts to connect magnetism with gravity were unsuccessful.

To determine the part in the action played by the medium, Faraday placed various substances between mutually attracting or repelling bodies. By this means electrical and magnetic actions were very greatly modified, but by no means which the experimenter's fertile mind could suggest was the behaviour of the force of gravitation altered in the slightest degree. From these experiments arose two sets of fundamental constants which are ever recurring in electrical theory, and which are now usually represented by the letters K and μ. Suppose we have two small bodies similarly electrified and separated by an air space. The force between them will be numerically represented by $\frac{qq'}{r^2}$ where q, q' are the quantities of electricity on the bodies and r is the distance between them. Now immerse the bodies, charged as before, in coal oil, or paraffin, or any other non-conductor; the force of repulsion is altered and is equal to $\frac{1}{K}\frac{qq'}{r^2}$,—that is, it is only one-*Kth* part of what it was in air. This quantity K is usually called the *specific inductive capacity* of the *insulator*, or the *dielectric constant*. The values of K for many substances have been determined, and some are as follows:

Substance	K.
Glass, plate	84.5
Glass, extra dense flint	10.1
Paraffin	2.29
Sulphur	3.84
Mica	6.64
Resin	2.55
Petroleum	2.10
Carbon bisuiphide	2.67
Distilled water	75.7

The other quantity, μ, can be defined in a similar way, though that is not the usual method. If two magnet poles of equal strength, m, be placed a distance, r, apart, the stress between them will be $\frac{1}{\mu}\frac{m^2}{r^2}$, where μ is the *magnetic permeability* of the medium in which the poles are placed. The only substances whose magnetic permeabilities differ much from their values in air are iron and its compounds, and in good wrought iron the value of μ sometimes rises to 3,000.

In Faraday's footsteps followed James Clerk-Maxwell, but where the former was somewhat deficient the latter was strong, namely, in mathematical power and training. After carefully studying Faraday's researches, Maxwell set to work to translate them into mathematical language. Though great difficulty was experienced in dealing with Faraday's lines of force, polarized dielectrics, etc., the mathematician persisted in his work, and finally in 1873 gave to the world his two large volumes. The work was a masterpiece, and has proved the epoch-making book on the subject. The third edition was published in 1892, and is more studied now than ever before.

There is not a continuous consistent development of a single theory throughout the two volumes, the confusion being due to the attempt made to weld the old theories to new ideas. Maxwell applied the laws of mechanics to a discussion of the electro-magnetic field, and on pushing his theory to its logical conclusion he reached some very remarkable mathematical equations. On interpreting them he deduced the result that electric and magnetic disturbances are propagated through any medium with a velocity, $v = \frac{1}{\sqrt{K\mu},}$ where K and μ are the quantities I have already defined. Now there are two systems of electrical measurement, one called the electrostatic, the other the electromagnetic method. A quantity of electricity can be defined by both methods, and Maxwell further showed that the quantity v should numerically be equal to the number obtained on dividing the electromagnetic unit quantity by the electrostatic unit. Forthwith experiments were devised to measure this ratio (which is, as we saw, the speed with which electric and magnetic effects are transmitted), and the astonishing result was reached that (within the limits of errors of observation) this velocity agrees precisely with that of light.

Years before this the battle in defence of the wave theory of light had been fought and won, and it had been established to the satisfaction of most scientists that there is a certain all-pervading practically imponderable, medium throughout all space. This is now well known as the *ether*, and its vibrations are believed to be motions made in transmitting radiant heat; and light with a velocity of 185,000 miles per second. Now Maxwell's mathematics led him to believe that electromagnetic effects are transmitted with the very same speed, and that they also must be propagated by some medium. As all space was already filled with the luminiferous ether, it was hardly philosophical to postulate a new medium to transmit the new effects, and so the natural conclusion to arrive at was that electric and magnetic actions are transmitted precisely as light, and by the same medium ; in fact that the only difference is in the wave-length. That is how the electromagnetic theory of light arose. It was first published thirty years ago; now it has been completely verified, and is almost universally accepted in the scientific world.

Thus far theory led; how to test it was now the problem. If these disturbances are propagated through air and other insulators, some means should be possible for their detection. Light waves are evident from the manner they affect the retina; heat waves, impinging upon ordinary matter, exhibit well-known heat effects; but how can electric waves be made to manifest themselves?

Some seven or eight years ago intense interest was aroused in the subject by the announcement by Heinrich Hertz, a young German professor, that he had actually observed and measured these electric waves. No experimental work was ever received with more enthusiasm, and though the gifted worker died on New Year's Day, 1894, at the early age of 37, he carried with him to his grave the honor and admiration of every nation.

Hertz demonstrated experimentally that electrical energy is transmitted precisely as Maxwell predicted. He determined the best methods for producing and measuring the effect, measured the velocity of propagation, calculated the wave-lengths which he used, and in fact clearly identified electrical with the other forms of radiation.

In 1886, when experimenting with some apparatus which he was using for lecture purposes, he happened to notice a spark where he had not expected it, and at once a method for investigating electrical oscillations was suggested to him. For his first radiator he used two metal plates, 40 cms. square, from each of which ran a wire terminating in a knob. (Fig. 1.) These were then attached to the secondary of an

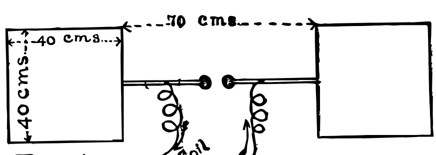
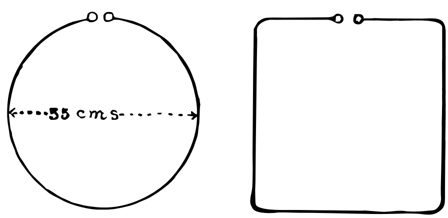


Fig. 1.

Hertz's Original Radiator.

induction coil, which caused sparks to pass from one knob to the other. In 1853 Lord Kelvin had shown mathematically that when a Leyden jar is discharged there is not a single spark, as generally supposed, but a series of them. The electricity does not simply rush across and stay there, but it behaves as does water in a trough when one end is quickly raised or lowered. In this case the liquid rushes to one end, then back to the other end, and after several motions back and forth equilibrium ensues. So it is with the jar; there is a series of motions to and fro across the spark-gap ; each time the electrification leaps across, a spark being produced. Four years later, Feddersen analysed the discharge in a rotating mirror and confirmed Kelvin's deduction. The sparks were separated, but by not more than a millionth of a second.

Now the arrangement adopted by Hertz is quite analogous to a Leyden jar, each plate and its knob corresponding to a coating of the jar. These very rapid oscillations set the surrounding ether in vibration and this motion is radiated in every direction. To receive this radiant energy he used a wire ending in knobs, and bent either in the form of a rectangle or of a circle. (Figs. 2 and 3.) Hertz found that when his



Figs 2 & 3.

Circular Receiver. Rectangular Receiver.

circle had a diameter of 35 cms. a spark would pass between the knobs (which were pretty close together), even when held some feet from the radiator. The radiator sent out the energy of undulatory motion, and this manifested itself in the shape of a minute electric spark.

I just said that the circle's diameter was 35 cms., and in every instance, for the best effects, the receiver had to be of definite dimensions, the rule being that the little sparks which occur across its air space must succeed each other at the same rate as do those of the radiator. In other words, the times for an oscillation in each must be the same. Perhaps I can best illustrate this from acoustics. I have here three tuning-forks. The first and second each give 1,024 vibrations per second, the third 1,016. When I vibrate the first I find the second, even though some distance away, responds; but no matter how vigorously I vibrate the first or second, no motion is appreciable in the third. This seems to indicate that a single swing or a single wave is ineffectual; but when a whole series of properly-timed waves beat against the second fork their efforts are added together, and finally elicit a decided response from the unison fork. It is quite the same with the radiator and receiver. The impulses from the former arrive at the proper times to increase the disturbance in the receiver until at last the rush of electricity is violent enough to make it leap over the gap and exhibit the spark.

The simplest method of showing this unison phenomenon is due to Prof. Oliver Lodge. Indeed I may say that nearly all the experiments I have to show to-night are due to this same physicist. At the time of Hertz's discovery he was engaged in researches which would probably have led him to the same result. Let us take two similar Leyden jars (Fig. 4). From the outside coating of one a wire, ending in a knob, is bent round until its knob almost touches that running up from the inside coating. The second jar

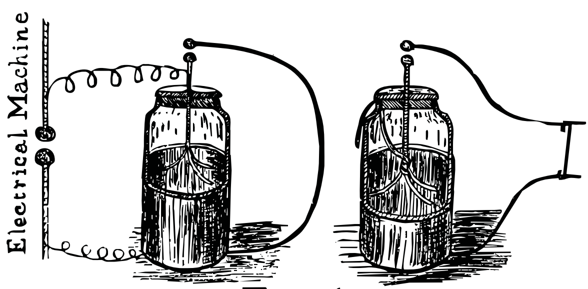


Fig 4.
Lodge's Synchronized Leyden Jars.

has its coatings joined by a wire, whose length can be varied by means of a slider. If this is moved so that the jars are synchronized, when a spark is produced in the first there is a strong oscillation in the second; its electricity tends to overflow, but of course in ordinary cases cannot do so. Now let a piece of tin-foil lead from the inside almost to the outside; the overflow at once occurs, and the spark is seen. Some of you might suspect that the action here is due to ordinary electrostatic or electromagnetic induction, but this cannot be so, as a slight motion of the slider either way destroys the action. By doing this the jars are thrown out of tune.

The only method of detecting these electrical surgings which I have so far mentioned is that of observing a spark; but in various ways has this same result been reached. Vacuum tubes can be illuminated; wires raised almost infinitesimally, though measurably, in temperature; an exceedingly sensitive galvanometer can be affected; thermo-electric phenomena have been shown; a suitable electrometer has been used; but the most sensitive and most convenient of all is the simplest as well. This is also due to Lodge. He discovered that electrical waves have a peculiar effect on two conductors touching loosely; these are made to cohere slightly. As an experiment he cut the wire connecting an electric bell with its battery, and then put a knob on each end of the severed wire. These were then placed so close together that a very slight current would flow, but not enough to ring the bell. Then on placing them in the path of electric waves, they were found to cohere so distinctly that enough current would flow to work the bell. The waves improved the bad contact. Now a tube of iron borings or turnings contains a great many bad contacts, and, as in the other case, the waves cause a minute coherence, and the electrical resistance of the tube is decidedly lowered. The best way to show this is to join in circuit a tube of turnings, a voltaic cell and a galvanometer of moderate sensibility. (See Fig. 8.) The resistance of the tube is so high that in its

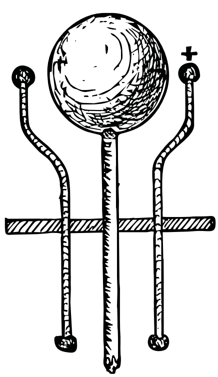


Fig 5.

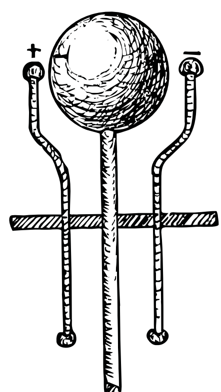


Fig 6.

Distribution of Electricity on the Spherical Radiator before and after spark.

ordinary condition very little current can pass and the galvanometer needle is but slightly deflected; but when the coherence takes place the current increases and deflects the galvanometer much more. By simply tapping the tube the turnings are reduced to their original condition, again ready for action. This method of detecting the presence of electric waves is the one I propose to show you to-night.

Let me describe my apparatus. First of all take the radiator. It consists of a 5-inch brass sphere, with a small knob on each side of it. (Figs. 5, 9.) These latter are attached to the secondary of an induction coil, which will cause sparks to pass between the knobs and the sphere. Perhaps I had better explain more fully the action of this part of the apparatus. I have several times stated my belief that electrical action is due rather to strains and stresses of the medium than to the presence of anything in or on the conductor, but as this view is so new, and also so difficult to picture to the mind, I shall explain the action on the old two-fluid hypothesis.

When the primary of the induction coil is interrupted positive electricity is gathered on one knob, negative on the other; or the potential of one knob is raised, that of the other is lowered. At once, by electrostatic induction the distribution on the sphere is altered, positive rushing to the left (Fig. 5), negative to the right. Then the spark passes. The charges rush back to produce equilibrium, but go past their original position, and the distribution is reversed. (Fig. 6.) Then another back rush ensues and the first distribution is produced. This rapid motion continues until the energy is dissipated. By this means you see the electricity on the sphere rushes to and fro from pole to pole. These oscillations are sufficiently quick to set in motion the surrounding ether, which at once sends out the energy in every direction. If we wish to make this radiation more definite in direction and increase its power, we inclose it in a copper hat and thus project the beam out from the opening. For most experiments it is more convenient to put the coil and its battery in a metal-lined box, screw the copper hat on the front of it, and lead the wires into the latter through glass tubes. (Fig. 7.)

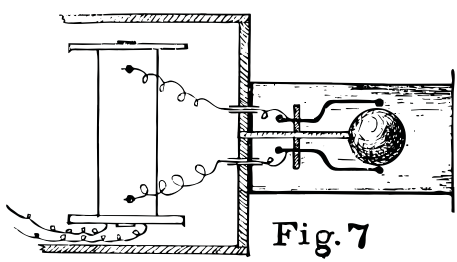


Fig. 7

Radiator within copper hat on outside of tin-lined box; coil within the box.

The smaller the sphere the quicker will be the oscillations, and hence, the shorter the wave-length. The wave-length is approximately 3·6 times the diameter, and hence for this five-inch sphere the waves are about 18 inches long, and their frequency is over 600 millions per second. It will he seen how great is the discrepancy between these waves and those of light. To affect the retina the latter must have a frequency of at least 400 *millions* of millions. To produce these the sphere would have to be of atomic dimensions—and yet all these waves have been shown by actual experiment to travel at the same rate!

To receive the radiation, I use a tube almost filled with iron turnings, placed at the back of another copper hat (Figs. 8, 9). It will be seen that these copper hats take the place of parabolic mirrors in ordinary light experiments. My most sensitive tube is about seven inches long and half an inch in diameter. It is filled with turnings made from cast iron, the shavings being rather thin and curled up. One filled with fine filings was quite unsuccessful.

The galvanometer consists of a flat coil, *C* (Fig. 8), of fine wire, with a magnetized needle suspended above it by a silk fibre. The coil is wound with three ounces of number 32 (B. & S.) copper wire, there being about 2,500 turns on it. When a current traverses the coil the needle above is deflected, and to render these deflexions visible to you all, a small mirror, *M*, is fastened above the needle, and a beam of light, projected by the electric lantern against it, is reflected upon the wall

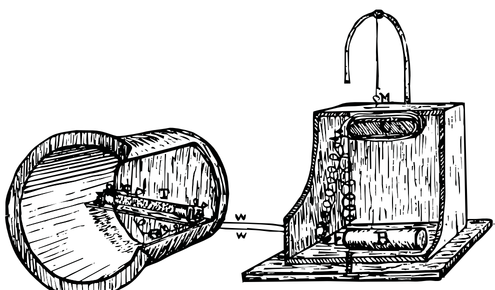


Fig. 8

Arrangement of various parts of Receiver and Galvanometer, and connections.

before you. When the needle is deflected, the light-spot will travel on the wall. The galvanometer coil is screwed to the top of a box, on the inside, and below it in the box is a single voltaic cell, *B*—in this case a dry cell. The coil, the cell, and the tube of turnings, *T*, are joined in a single circuit. As the electric waves will run along a wire whenever they can (the wire behaving towards them like a speaking-tube to sound-waves), and as they cannot pass through a metallic conductor, the connecting wires *W*, *W*, and also the coil of the galvanometer, are all protected by metal coverings. The coil and battery are in a copper box; the connecting wires are simply incandescent lamp cord wrapped with tin-foil, and then over-wound with rubber tape to keep the foil in place. With this arrangement, if the galvanometer is affected, this disturbance must come from within the receiving copper hat.

Let us now try some experiments. First, I shall use the smaller of the two coils I have and allow the energy to radiate freely in every direction. The receiver being placed a few feet away, the galvanometer responds readily. (Fig. 9.) On tapping the tube, the galvanometer needle goes back to its former position. For more powerful effects I shall use the larger coil which is within the tin-lined box, and whose secondary is connected to the radiator in the copper hat. (Figs. 7, 10.) Now the effect is decided even when the radiator and receiver are at opposite sides of the room. Prof Lodge obtained marked effects at a distance of forty yards. If I hold a sheet of metal over the radiator or receiver the energy is stopped; it cannot go through a conductor. An ordinary electrical gas-lighter (which is simply a small static machine) can drive the spot across the scale. But observe that the waves escape through the ebonite handle; this end, as you see, must be held towards the receiver.

Now turn the receiver away so that direct waves cannot enter it. When I hold a metallic sheet (in this case brass, though copper is a better reflector), at the proper angle, the waves are reflected into the receiver, upon the turnings, and at once drive the light spot across the room. Almost any angle of incidence is successful in this experiment. A glass plate or a board has no appreciable effect as a reflector.

Refraction experiments are interesting. In them, however, the effect is not so marked as in direct transmission or reflection. My refracting prism is of roofing pitch. The angles are 45°, 60°, 75°, and the largest side is about eighteen inches square. The pitch was cast in a wooden box and the latter then removed. I shut off part of the radiation by putting a diaphragm with a 6-inch hole over the radiating hat, and turn the receiver until the light-spot moves only a short distance. Now, putting the prism in the path of the rays, so as to use the largest angle, the galvanometer responds and drives the spot almost across the room. (Fig. 10.) This effect is not so easily exhibited as in optics, owing to the greater wave-length.

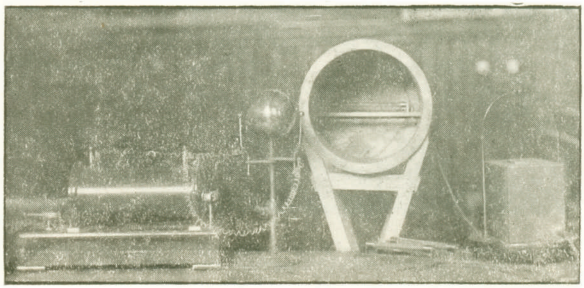


FIG. 9.—Actual apparatus used. Radiator connected to induction coil. Tube of turnings at back of copper hat, from which the covered wires lead to the galvanometer on the right. Two additional tubes on the table.

Let us now try polarization. A ray is plane-polarized when the vibrations are continually in a single plane. In the present case, from the nature of the source, namely, a row of sparks, you would expect to find the emitted rays polarized. Such they are. If I hold a grid of copper wires (see Fig. 10) so that the wires are parallel to the spark-line, the spot moves but little, the waves cannot get through. But as I rotate the grid in its own plane the effect is wore noticeable until when the wires are vertical the spot moves farthest. If I take two grids and cross them the screening is almost perfect. I turn them until the wires make an angle of 45° with the spark-line and the tube in the receiver; on removing one grid the spot moves some distance; on taking away the other the spot goes clear across the room. This experiment is precisely similar to the one in optics in which two crossed tourmaline plates extinguish the light. In my experiment the grids are simply square wooden frames, 14 inches across, with parallel copper wires about ¾ inch apart strung on them.

Many other researches and experiments have been made in this fascinating branch of science, but I have not time at present to refer to them.

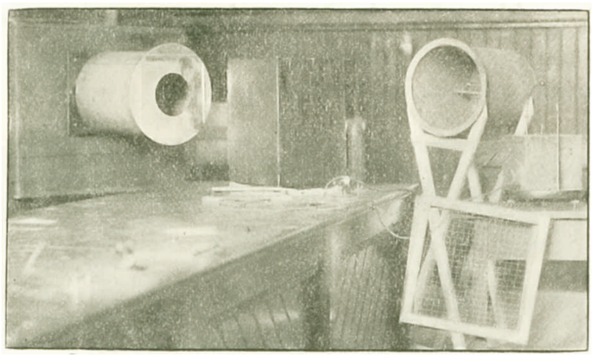


Fig. 10.—Apparatus arranged to show refraction through pitch prism.
Polarizing grids hanging on galvanometer table in front.

I hope I have in a general way shown that electrical radiation is a reality, and that it follows laws similar to those in optics. I hope, too, that I have in some imperfect manner shown the probability that all those so-called forces acting at a distance are really some peculiar action handed on by that invisible and unweighable, ever-present substance which fills

"The lucid interspace of world and world,"

and which is called the *ether*. The marvellous properties of this remarkable medium are now receiving the most careful attention of workers all over the globe, and when the man of science contemplates how the forces of heat, light, electricity, magnetism, gravitation, and the rest are all due to its mysterious behaviour, the stupendousness and the intricacy of the mechanism of the universe almost overawe him. With the late Laureate he will conclude:

"To those still-working energies
I spy nor term nor bound."

Mr. Chant concluded amid enthusiastic applause. All present had entered most heartily and sympathetically into the spirit of the work, while experiment after experiment successfully illustrated the subject. The apparatus proved to be most complete, reflecting the highest credit upon the lecturer and his assistant, Mr. Plaskett. Mr. W. A. Douglas, B.A., moved a cordial vote of thanks to Mr. Chant and Mr. Plaskett. He referred to the enormous strides which electrical science had made during the past score of years, and voiced the opinion of the audience when he stated that he was proud to have had an opportunity of hearing, in Toronto such a lucid exposition of a most difficult subject. Mr. Thomas Davies, member of the City Council of Toronto, seconded the vote of thanks, and added on behalf of the visitors of the evening that the Society's efforts to popularize scientific study were most thoroughly appreciated.

The thanks of the audience were tendered by the Chairman, who added his own personal acknowledgment of the pleasure he had derived from the lecture. In reply, Mr. Chant stated that he appreciated the good will of the ladies and gentlemen present, and would be glad to prepare on some future date a lecture on similar lines.

TWENTY-FOURTH MEETING.

December 10th; the Vice-President, Mr. John A. Paterson, M.A., occupied the chair.

A letter was read from the Secretary of The Astronomical Society of Wales, cordially inviting correspondence on subjects relating to astronomy; Dr. M. A. Veeder wrote to thank the Society for the good wishes expressed, but stating that he would be unable to change his decision in regard to giving up his auroral researches.

It had been announced that members interested in lunar observations would bring to the meeting any drawings that had been recently made. Several were received. Miss Eva M. Brook, of Simcoe, forwarded two sketches of the Mare Crisium, made at Dr. Wadsworth's large reflector. The Doctor, who was present, read some interesting notes on lunar study, and emphasized what had been frequently pointed out, that the skilful artist can represent what the photographic plate cannot show, for various reasons. Particular attention was also called to the fact that neither sensitized plate nor artist's pencil can show what the eye sees, even in a small telescope, when directed to the Moon. Some very valuable hints to the amateur were given, after which the lantern was used to project on the screen a series of photographic slides, illustrating the lunar surface. An interesting letter was also received from Mr. H. B. Witton, ex-M.P., of Hamilton, who had long taken a very active interest in telescopic observation, as well as drawing, in connection with the Moon. He had very kindly forwarded a set of the Rutherford photos, taken as early as in the fifties, showing the Moon at various ages in the lunation; these were handed round for inspection, and were much admired. A copy of the first map made by Beer and Maëdler was also received from Mr. Witton, and a photograph of a map he had himself drawn. This latter was projected on the screen and comparisons made between recent drawings and the earlier sketches. Referring to differences such as were observed, Mr. Elvins stated that it was always necessary to consider just how the sunlight is falling on the Moon when drawings are compared; under different illumination an object will appear surprisingly different in aspect. The thanks of the Society were due to Mr. Witton for the pains he had taken to make the subject interesting.

Mr. J. A. Copland then presented the following notes on

THE CANALS OF MARS.

Some time ago astronomers were astonished, and disappointed it may be added, when they heard Prof. W. W. Campbell's announcement that he had demonstrated by careful study with the spectroscope at the Lick Observatory that no water-vapour exists in the atmosphere of the planet Mars. "Surely there is some mistake!" was the common ejaculation in astronomical circles; but as the statement comes from what is supposed to be the first observatory in the world, it was deemed in some quarters that this decision should be taken as conclusive. Artist-astronomer Brett had maintained that Mars appeared to him as an arid ball, and Prof. Campbell's announcement seemed to confirm that hypothesis.

Long has it been axiomatic that "doctors differ"; so may it be said of astronomers, for now M. Janssen has informed the French Academy of Sciences that, through the spectroscope's aid, he has determined conclusively that there is water-vapour in the Martian atmosphere. Observations made by other astronomers point to the same conclusion. Thus it is in order for Prof. W. W. Campbell to explain his position; he is a clever and pains-taking scientist, therefore is his deduction the more remarkable. There apparently is one thing clear, and that is, that moisture is not distributed over the surface of the planet Mars in a manner similar to that which rules on our Earth. Large portions of the Martian world are arid; and more would be so were it not for the artificial means of irrigation employed by the inhabitants of the fiery little orb with the two small fleet moons. Here we have the explanation of the canals of Schiaparelli, even to their doubling. Generally those striations of verdure will loom up as but single lines, because of the vast distance to be spanned by our sight. Then again, two of these canals may be running parallel, though miles apart, thus giving the doubling impression. One gentleman who is taking great interest in the planet Mars is Mr. Percival Lowell. It was he who recently endowed and has had erected in Arizona an observatory, under the management of competent astronomers, purposely to observe the planet namesake of the god of war. He argues convincingly in several articles he has written that there are evidences of intelligent life upon yonder other world; that the broad physical conditions of Mars are not by any means antagonistic to life in some form; that, there being an apparent dearth of water upon the planet's surface, if beings of sufficient intelligence inhabit it, they would have to resort to irrigation to support life; that there turns out to be a net-work of markings covering the disc precisely counterparting what a system of irrigation would look like; that there are spots placed where we should expect to find the land thus artificially fertilized, and behaving as such constructed oases should.

All these phenomena, so concisely set forth by Mr. Lowell, can be anything but mere coincidences, signifying nothing. Undoubtedly, under the conditions, any Martian life must be markedly different from what we here have. Considered from a Martian view point, there may be nothing extraordinary or in any wise malformed or incongruous. We recollect that life suits itself to conditions; never conditions to life. What are the Martian conditions? Regularly every season the polar ice and snow caps there melt under the Sun's rays, and the water begins to flow toward the equator, so rapidly that in a few days there is a complete metamorphosis of the planet's face. Much of the inter-polar region is red coloured and arid; but bye-and-bye those "canals" and "lakes" show out, appearing more and more prominent lines and dots to our vision, because they are bordered and surrounded with growing verdure. Scarcity of water, or lack of proper natural distribution of that prerequisite to life, is a condition which must be sedulously kept in mind as a fundamental truth when an analysis of Martian phenomena is sought. Those canals and storage basins or reservoirs, as we see them, and measured by terrestrial standards, are stupendous affairs, and would involve herculean, almost superhuman, labour in their construction; but when we admit that those so-called narrow water-ways and artificial lakes have been constructed for irrigation purposes, and when we take into account the wide difference between Martian and terrestrial gravitation, the task of digging those canals and of constructing those reservoirs cannot seem so disproportionate. Of course, if the systems of canals have been contrived for irrigating the country through which they pass, it is plain that those lines which we see and call canals are really not the canals at all, but the fertile strips of land concurring with them. Safely may it be surmised that the canals proper are never seen in the telescope, so hair-like must they appear in contrast to the green-clad land through the middle of which they pass. There are plenty of analogies on Earth to prove that such must be the case. Looking from a height over irrigated land we see the verdure, but not the canals which cause it to flourish.

Examining further, we must fail to discover what manner of inhabitants there are on Mars, where the conditions are so different from ours. That very fact of so wide a variance existing between Martian and terrestrial gravity reaches farther in its effect than might be at once supposed. As Mr. Lowell recalls in one of his articles, gravity on the surface of Mars is only a little more than one-third what it is on the surface of the Earth. This would work in two ways to very different conditions of existence from those to which we are accustomed. To begin with, three times as much work, as for example in digging a canal, could be done with the same expenditure of muscular force. If we were transported to Mars we should be pleasingly surprised to find all our manual labour suddenly lightened three-fold. But, indirectly, there might result a yet greater gain to our capabilities; for if nature those she could there afford to build her inhabitants on three times the scale she does on Earth without their ever finding it out except by interplanetary comparison. There may be nothing new in these thoughts, but at all events the subject is an enticing one to all of us.

The Assistant-Secretary then announced that he had received from Mr. W. N. Greenwood, of Lancaster, Eng., an exhaustive paper on the subject of

THE UNIFICATION OF TIME,*

extracts from which were read.

Mr. Greenwood had addressed certain questions to British shipmasters and others, asking their opinions regarding methods of reckoning time and longitudes. Question No. 3 of the circular read as follows :—

3. Are you in favour of the Unification of Time as applied to the Civil, Nautical, and Astronomical Days, and is it desirable in the interest of all concerned that such days should each commence at Greenwich mean midnight?

A large number of the circulars had been distributed, and replies had been received from two societies and 243 individuals.

The Shipmasters' Society, London, 1,033 members and 200 associates, had replied "Yes." The Manchester Geographical Society, 850 members, had reserved opinion, but added a note to the effect that the Society would most probably be favourable to the proposition.

The following table exhibits the replies received from individuals :—

Replies Received From				Yeas.	Nays.	Opinion Reserved.
42	British shipmasters not following their profession		....	37	4	1
111	British shipmasters,		steam (186,252 tons)	106	3	2
32	"	"	sailing (36,467 tons).....	32		
14	"	"	coasting (1,676 tons)	14		
44	Foreign	"	sailing & steam (25,117 t's)	44		
243				233	7	3

In concluding, Mr. Greenwood stated that the inquiry was still in progress; a large number was yet to be heard from, but the summary of replies already received undoubtedly conveyed the favourable opinion of the British shipmaster.

* Subsequently forwarded to Dr. Sandford Fleming, C.M.G., to be used in the preparation of a further report on the general subject.

TWENTY-FIFTH MEETING.

December 23rd; the Vice-President, Mr. John A. Paterson, M.A., occupied the chair.

Rev. Canon Macnab and Mr. L. Laurance, of Toronto, were duly elected active members of the Society.

The Corresponding Secretary read a letter received from Prof. E. C. Pickering, of Harvard Observatory, covering the transmission of two photographs of star clusters. These were a most valuable addition to the Society's collection.

The Chairman having called for nominations for the various offices to be held during the ensuing year, Mr. T. Lindsay stated that, at the Council meeting recently held, the President, Dr. Larratt W. Smith, Q.C., and the Vice-President, Dr. E. A. Meredith, had expressed their desire to withdraw from active official duties, and had requested that their names he not brought forward in nomination for 1896.

Mr. G. E. Lumsden, seconded by Mr. Elvins, moved the appointment of a Committee to prepare a resolution conveying the Society's appreciation of the services of the retiring President and Vice-President in the past.

Mr. Lindsay gave notice that he would move at the next regular meeting, an amendment to the constitution, enabling ex-Presidents and ex-Vice-Presidents of the Society to hold seats in council.

Nominations for the various offices were then received.

On the conclusion of the business of the meeting, Mr. M. Turnbull asked permission to address the Society on the subject of a popular observatory for Toronto, in which he had been always much interested. Mr. Turnbull having been requested to proceed, exhibited some diagrams illustrating in minute detail a method of mounting a large reflecting telescope which was a modification of Dr. Henry Draper's method. He gave estimates of the cost of constructing an observatory, exclusive of the site, which be thought might be granted by the city. It was decided, after hearing Mr. Turnbull, to refer his paper to the committee specially appointed to consider this question which had always been directly before the Society.

The following paper was then presented by Mr. J. R. Collins :—

THEORIES OF UNIVERSAL GRAVITATION.

Perhaps the greatest mystery within the field of physical science awaiting solution, is the problem of the cause of universal gravitation and the discovery of the nature of the mechanism by which, or through which it is enabled to leap the chasm separating satellite from primary, sun from sun, system from system, everywhere tightening atoms, molecules, masses and whirling worlds, in a grasp stronger than steel and as ceaseless as time.

The great Newton was enabled to prove that such a universal energy exists, and to discover the ratio of its action, as regards the distance separating bodies from each other; but gravity's cause, and mode of action aside from this, (except as being interpreted as the agent of the great first cause) were unknown to him, and still remain hidden in the depths of the mysterious; or perhaps still undulate unnoticed. Upon the expanse of the all-pervading ether. Since Newton drew attention to this question, physicists, mathematicians and philosophers in general, have given much time and thought to its investigation. Theories by the score have been advanced, but none seems to have yet met the requirements of the case. However, many of them are ingenious and instructive- and illustrate the different lines of thought pursued in the endeavour to discover these hidden springs of nature. It is the purpose of this paper to briefly review a few of the most important of the theories advanced, and endeavour to sum up a few of the salient points with which the question may be conditioned.

It seems questionable whether the ancients ever grasped the idea of gravity at all, certainly they did not as it is understood to-day. Aristotle held that the figure of the Earth must necessarily be spherical and pointed to the shape of the Earth's shadow upon the surface of the Moon during eclipse as proof of that form, but the reason given for bodies assuming this form was purely metaphysical, namely, because a sphere was the most perfect figure—reminding us of Pythagoras' reason for assuming the Sun to be the centre of the universe, because fire was the most important of the four elements; it was but natural and proper that as the Sun was composed of fire, all the rest should circle around him and do him becoming homage. Ptolemy supposed there were four regions in which each of the then elements tend to unite, and no where else, earth in the region of earth, air in the region of air, water in the region of water, and fire in the region of fire. The Stoics, Epicureans and Peripatetics, held that bodies tend towards the centre of the universe, the lighter giving place to the heavy. Archimedes' discovery that a body loses a portion of its weight, when submerged in water, equal to the weight of the water displaced, says Whewell, only added confusion into the opinions of philosophers. Plutarch endeavoured to introduce a little variety into the Ptolemaic theory, by pointing out that the Earth attracts earthly things, the Moon lunar things, the Sun sunly things, and so on. The motions of the heavenly bodies now assignable to the ceaseless tug of gravity, were then supposed to be due to the efforts of spirits whose several duties consisted in safely, regularly and steadily carrying the planets across the sky, or in some cases towing them across in row boats, or again driving them in chariots of gold, with fiery foaming steeds, across the azure blue, and in other ways, as imagination might suggest. At as late a date as A.D. 1510 even Copernicus, immortal founder of the system of astronomy that bears his name, attributed the motion of the planets to attending spirits, who, he supposed, guided them in their periodic rounds. The indefatigable Kepler in 1595 considered orbital motions due to a tremendous whirlpool of some sort of transparent fluid circling around the Sun, in whose current the planets became entangled and were carried or floated around the Sun, like a boat in a stream.

In 1600 Wm. Gilbert, the pioneer in the field of magnetic and electric research and the first to conceive and demonstrate the Earth to be itself a magnet, contended gravity between the Earth and the Moon, to be due to positive and negative magnetism residing in both, one force attracting and the other repelling; in this way reaching out great curved arms, which keep the distance constant.

Descartes, 1630, though he knew more of the laws of motion than most of his predecessors, supposed the planets to be carried around the Sun in his vortice, or whirlpool of ether, which ether consisted of fine particles worn from the corners of atoms of matter; these atoms being, as he considered, originally of all angular shapes, were deprived of their sharp corners and fine points by incessant collisions, as they clashed together in the vortices of the Sun, the Moon, or the Earth; all space being filled with such vortices or whirls of splintered atom dust, roiling and tumbling over each other, the greater carrying the less around with them, and in this way determining their motion in the great maelstrom of the universe, throwing heavy portions to the centre of the eddies, forming planets and holding them together. The pressure produced in this way wad, according to Descartes, the cause of gravity; reminding us of Addison's hues, wherein we may behold a picture of "the war of elements, the wreck of matter and the crash of worlds."

Jeremiah Horrocks, 1635, the first observer of the transit of Venus, seems to have been one of the first to recognize the necessity of some central force, continually urging bodies together, in order to explain orbital motions, but he had no opportunity of developing this idea, as he died at the early age of twenty-three.

In 1650 Gassendi, who had done much for astronomy and physics, while adopting the atomic theory of Democrites—viewing the universe as a shower of atoms moving in every direction—yet maintained the revolution of the planets to be due to certain fibres extending from the Sun, the action of which was similar to that of the muscles of animals. About the same time, 1650, Borelli, one of the first to recognize the parabolic paths of comets, expressed the conviction that planetary motions were due to "a fountain of virtue" springing in some way from the Sun, that compelled them to attend him, and likened this virtue to something resembling magnetism.

Dr. Robert Hook, 1670, who claimed priority to Newton, in the discovery and demonstration of universal gravitation, when experimenting with waves on the surface of water, noticed that light bodies floating thereon, approached and collected at the centre of disturbance, when the liquid was agitated with a small paddle; he suggested something like this as the cause of gravity, but did not follow up the experiments by investigating the effect of waves on submerged bodies.

Leibnitz, 1675, who also claimed some of Newton's work on the calculus, supposed the particles of the Earth to have separate motions, which produce collisions, propagating an agitation in the ether radiating in all directions and by the rotation of the Sun on its axis, with its rectilinear action on the Earth, arises and is maintained, the motion of the Earth around the Sun.

In 1692 Newton announced his discovery that every particle of matter in the universe attracts every other particle, with a force directly as the mass and inversely as the square of the distance between them. Though the attraction of the Sun, according to the square of the distance, was divined by Bullialdus, Hooke, Halley and others, Newton was the first to prove it. Newton speculated to some extent, as to the cause of gravity, but never formulated any theory on the subject, further than to make suggestions. One suggestion was that if the ether was very dense within and near bodies, and became less dense as it proceeded outward, such an effect as gravity might follow. And again, if the reverse was the case, and the ether in and near the surface of bodies was rare, and became dense as it proceeded outward, a pressure might result sufficient to produce the effects of gravity.

I quote here from Taylor's *Kinetic Theories of Gravitation:*—

"Fourteen years later—a decade after his culminating work—this topic was again incidentally touched upon by Newton in four letters addressed to Doctor Bentley, 'containing some arguments in proof of a Deity.' In his second letter, dated January 17, 1692-3, he says in reply to one from Bentley: 'You sometimes speak of gravity as essential and inherent to matter. Pray do not ascribe that notion to me, for the cause of gravity is what I do not pretend to know, and therefore would take more time to consider it.' *

"In his third letter, dated February 25, 1692-3, he expresses himself somewhat less guardedly, thus: 'It is inconceivable that inanimate brute matter should, without the mediation of something else which is not material operate upon and affect other matter, without mutual contact, as it must do if gravitation in the sense of Epicurus be essential and inherent in it. And this is one reason why I desired you would not ascribe "innate gravity" to me. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers.'†

"At the conclusion of the third book of his *Principia*, Newton remarks: 'Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypothesis; for

* Works, edited by Horsley, vol. iv., p. 437.

† Works, *ut supr*., vol. iv., p. 438.

whatever is not deduced from the phenomena is to be called an hypothesis. . . . To us it is enough that gravity does really exist, and act according to the laws which we have explained.'

" Afterward, as if driven back from every assault to the only retreat, which in earlier years he had stigmatized as ' so great an absurdity ' that no competent thinker could 'ever fall into it,' he despairingly asks 'Have not the small particles of bodies certain powers, virtues, or forces, by which *they act at a distance?* . . . What I call "attraction *may* be performed by impulse, or by some other means unknown to me. I use that word here to signify only in general any force by which bodies tend toward one another, whotsoever be the cause.' * And beyond this point, no human research has since been able to penetrate. This last and presumably deliberate judgment of Newton is a quarter of a century later than the inconsiderate utterances of his third 'Bentley letter,' which have been so eagerly seized upon by every speculative writer intent on propounding new theories of the universe."

Cotes, in the preface to the 2nd edition of the *Principia*, 1712, says gravity is unquestionably an attribute of matter.

In 1707 Villemot propounded a theory of gravity, founded upon Cartesian vortices; he claimed that there would be greater pressure on the outside of such vortices than towards the centre, and, as a consequence, all bodies floating therein would be urged to the several centres.

Bernoulli, 1734, maintained to the last the truth of the Cartesian hypothesis, with modifications of his own, and pretended to apply mathematical calculations to his principles, regarding gravity as due to a central torrent continually flung from the circumference of vortices towards the centre, and so urging bodies in the same direction. 1750—Le Sage, the eminent French-Swiss physicist and mathematician, put forth a theory of gravity that has been pronounced by Prof. Tait as the most plausible attempt hitherto made to solve this problem. Le Sage supposed that there exists throughout the universe an infinite number of small particles, moving through space in straight lines in every possible direction, with a velocity greater than light, perpetually colliding and rebounding, forming groups or bunches in places; such groups might become the basis of matter; these groups or other bodies immersed in this universal storm-cloud would receive a perpetual bombardment

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* *Loco citat.* Query 31.

from all sides by the little pellets which were supposed to swarm in from every direction in an infinite, never-ceasing shower. A single body immersed in such a sea of flying particles, receiving an equal number of impacts on every part of its surface, would remain stationary but if two or more bodies were to exist in such a sea, at a distance from each other, with missiles hurled at them from every possible direction, then each would shield the other from the blows falling upon the surfaces opposite to the sides facing each other, and as the bodies would receive the greater impact upon their unshielded sides, they would be driven in towards each other, the amount of shielding increasing as they approach —inversely as the square of the distance diminished—exactly the ratio with which gravity is known to act. For more than fifty years did Le Sage with unwavering faith proclaim his doctrine and urge upon his contemporaries its adoption, but without success. His hypothesis was really a further elaboration of the atomic theory of Leucippus, the Greek philosopher, in the 6th century, B.C., and adopted by Democrites, Epicurus and Lucretius, and in modern times by Gassendi. Le Sage developed the gravitational part of the theory which had never occurred to his predecessors. Clerk-Maxwell says of this view: "The rate at which the energy of the corpuscles is spent, in order to maintain the gravitating property of a single pound, is at least millions of millions of foot pounds per second; and if any appreciable fraction of this energy is communicated to the body against which they strike, in the form of heat, the amount of heat so generated would, in a few seconds, raise it, and in like manner the whole material universe, to a white heat." Furthermore, the atoms or bodies against which they strike would have to be porous or cage-like in structure to an *infinite* extent in order to allow sufficient quantities of these pellets to go through a second body, and influence a third, to the same extent as if the force did not penetrate the second at all. Further objections also exist.

Euler, 1760, considered gravity due to pressure of some kind in the ether. In 1816 John Herepath, of Bristol, Eng., accepted Newton's last suggestion of the ether becoming rare in and near bodies, and becoming more dense as it proceeded outward; giving heat as the cause of such expansion He also supposed that hard, inelastic atoms, after collision, would rebound without loss of energy.

In 1848 the great Faraday made experiments with a helix of copper wire and a galvanometer, for the purpose of testing whether gravity could or could not be transformed into electric energy, but with negative results.

In 1840 Mosotti supposed the particles of matter to float in an atmosphere of ether, each kind attracting its opposite and repelling its like. Mosotti has, in this way, deduced from this mutual action gravitation and the molecular forces. Clausius and Redthenbacher have since endeavoured to explain the other energies by supposing matter and matter to attract each other, also matter and ether, but ether and ether to repel their like. In 1858 J. J. Waterson, in the *Philosophical Magazine*, supposes all material atoms driven together by a perpetual inrush of energy from every direction, this energy is regarded as a function of space, is infinite in extent, producing gravity.

Prof. James Challis, of the University of Cambridge, and Director of Cambridge Observatory, till his death in 1882, devoted much of his time and untiring energy to the elucidation of this subject; he investigated the mathematical theory of the effect of waves of condensation and rarefaction in the ether, and concluded that the effect would be to attract or repel the body to the centre of disturbance, according as the wave's length is very great or very small, compared with the dimensions of the body submerged therein. Practical illustrations of the effects of waves have been given by Guyot, Schellback and Thomson. When a tuning fork is vibrated near a light body, the body is attracted towards the fork. Lord Kelvin has shown that this is explained upon the principle that, in fluid motion, the average pressure is least where the average energy of motion is greatest. J. S. Stewart Glennie, 1865, supposed gravity to be due to a mutual repulsion of atoms in the ether. In the same year, the brothers Keller propounded before the French Academy of Sciences, a wave theory differing but little from that presented by Dr. Hooke, nearly two centuries earlier. Kelvin showed, in 1878, that if space be supposed filled with an incompressible fluid, and all material bodies continually generating and emitting this fluid at a constant rate, the fluid flowing off to infinity, or if all matter be supposed to absorb such a fluid, flowing in continually from infinite space, attraction of gravity would result.

Dr. James Croll, author of *Stellar Evolution*, considers gravity in all probability, to be of the nature of an impact or pressure. Either supposition, he says is purely hypothetical, and he asks; why not assume it to be a force, without calling in the aid of corpuscles, or a medium filling space. In discussing the merits of pressure or attraction, Croll says " no principle will ever be generally received, that stands in opposition to the old adage—' a thing 'cannot act where it is not, any more than it can oppose that other adage' 'a thing cannot act before it is, or when it is not.' " "These venerable adages are, however," remarks Prof. W. B. Taylor, "about as valuable in directing us to the facts of nature, as that other celebrated adage of Zeno, 'a thing cannot move where it is not, and conversely, it cannot move where it is.'

Clerk-Maxwell observes that electric and magnetic action are explained by assuming tension in the ether along the lines of force, accompanied with pressure in all directions at right angles to those lines. Gravitation, he says, could be explained by assuming a reversed action; that is, *pressure* along the lines of force, and *tension* at right angles to them He was not, however, able to imagine any physical cause for such a state of stress. "It is easy," he remarks, "to calculate the amount of this stress that would be required to account for the actual effects of gravity at the surface of the earth. It would require a pressure of 37,000 tons weight on the square inch in a vertical direction, combined with a tension of the same numerical value in all horizontal directions; the state of stress, therefore, which we must suppose to exist in this invisible medium, is thousands of tons greater than the strongest steel can support.'

Prof. Wm. Crookes says: In the radiant energy of solar masses may at last be found that agent acting constantly according to fixed laws; he regards the action as action at a distance.

In John Hopkins University circular for 1894, A. S. McKenzie claims to have measured the gravitational attraction of a crystal along various axes, to determine whether it would vary, as do the velocity of light, conductivity and other physical quantities ; his results, however, show no such variation.

Mr. J. H. Kedzie, of Chicago, a corresponding member of our Society, has suggested that gravity may be due to vibrations in the ether caused by other forms of energy which have lost their motion across the line of propagation, and have been converted into direct backward and forward thrusts. Mr. Elvins also has propounded a theory somewhat similar to that of Le Sage, though differing in this respect, that he holds, we believe, that the collisions of the corpuscles would produce vibrations similar to that of a solid.

Prof. Thos. Preston, 1890, in *Theories of Heat,* suggests that ether vortices, not forming rings, or closed curves, may abut upon the surfaces of particles of matter, and form vortex filaments constituting Faraday tubes of force, connecting every particle reciprocally with each other; when the particles are far apart, the filaments would be drawn out and weakened, but when they are near each other, the filaments would thicken and exert a pressure—equivalent to gravity—inversely as the square of the distance. Dr. C. V. Burton, in *Philosophical Magazine*, February, 1892, contends that if the ether were subjected to an enormous strain, and the strain were to be then removed, it would take a "permanent set" forming "strain-figures." This theory supposes molecules not to be of definite size, but that in the ether surrounding each "molecular centre," there are stresses and strains which grow continually less, as distance increases, but which never absolutely vanish. Either gravitative attraction or repulsion might result from a general application of Dr. Burton's theory, but by making a plausible assumption in regard to the nature of the strain-figures, the force acting between particles of matter would always be attractive.

Prof. Tait in 1892, says: "Many attempts, often extremely ingenious, but all alike fruitless, have been made to explain gravitation; such failures, however, in the eyes of a genuine scientific man are only an encouragement to persevere; and the very remarkable success which has attended Clerk-Maxwell's attempt to explain electric and magnetic phenomena by means of the luminiferous medium, renders it at least probable, that the properties of the ether will some day explain gravitation, possibly inertia also. Mere speculation of course is of use in science, only in so far as it originates or directs enquiry. So that we must be content simply to express the idea that the ether may be the one material substance in the universe, gross matter simply being differentiated portions of it, denser or less dense than the rest; perhaps mere cavities or bubbles." This latter statement has reference to the views of Helmholtz and Lord Kelvin now generally accepted by physicists as the most plausible explanation of the nature of ether and matter that has yet been made, viz., that the universe consists of a single elastic, negatively compressible fluid, without grained structure, a single huge soft or elastic atom without parts, *one* in the same sense that the hard, indivisible, impenetrable, indestructible atom of Lucretius, is *one*. They claim that it is as easy to think of a single large thing as to think of a single small thing. If minute vortices be set up in the form of rings, in this single large soft atom, the rotary motion would stiffen them, and in a perfectly frictionless substance Helmholtz has mathematically shown, these minute vortex rings would be as indestructible as the hard Lucretian atom, and furthermore would possess the property of elasticity, which the former must lack; it was because of this lack chiefly, Kelvin informs us, that he was led to look for some other kind of atom. These vortices, it will be noticed, do not resemble the Cartesian vortice or whirlpool in any way, but consist of small closed rings, rotating in the manner that a rubber ring rolls as it is forced over an umbrella handle, continually rolling inside out.* If this huge universal atom be drawn or strained outward in every direction equally, a rigidity will be assumed analogous to a solid, directly as the strain. The dimensions of this system might be infinite or it might be finite, as we choose; but there is one thing there appears to be no escape from: whatever the nature and structure of the ether may be it is not perfectly frictionless, for although the truth of the conservation of energy cannot be questioned, yet the fact of the degradation of energy is equally plain. At every change of energy from one form to another, which is continually going on, a determinate percentage is converted into a form of heat that cannot be reconverted into any other form, and consequently unless the tendencies of the forces of nature be reversed, all other forms of energy must eventually assume this special form in final equilibrium, and end in universal night. Prof. Peddie says on this point: "Examples of the degradation of energy are everywhere seen in nature; the optical image of a body is less distinct than the object itself, because some of the so-called radiant energy is absorbed by the reflector, and takes the form of heat; the vibrations of a tuning fork die down because the energy is communicated to the surrounding air, but they also diminish because of the production of heat from molecular friction in the vibrating body itself; the stilling of storms is accompanied by dissipation of energy, and possibly starlight is weakened by its passage through the ether; indeed, no instance of transformation of energy can be pointed out, in which there is not also dissipation of energy." However, as our philosophers inform us, this state of things will not be brought about for a few million years yet to come, we need not be unduly worried about the outlook.

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* Mr. Collins here repeated the instructive "smoke-ring" experiment with perfect success.

That the ether exists, there can be no question, and that its structure displays the nature of a pseudo-solid, seems demonstrated beyond dispute. We endeavour to emphasize these facts as to the nature of matter and ether, because no theory of gravity can be tenable which ignores the nature and conditions of the medium through which or by which it acts. The vibrations of light, radiant heat and electricity are found to travel at the enormous rate of 186,000 miles per second, such vibrations being across the line of propagation, and of the nature of a solid. As no known form of matter can transmit vibrations at a fraction of such a speed, there must be a medium around and through all matter at sufficient tension to transmit these vibrations, necessitating a strain of thousands of tons to the square inch. But, it may be asked, what could be the physical cause of such a strain? It is well known that when a limp cotton disc is mounted on an axis and revolved rapidly it will stiffen out and become quite rigid, so much so that when the rotary motion is great the disc becomes rigid enough to cut into a plate of steel thrust against its edge. If a body resembling a Helmholtz-Thomson universe atom, of finite dimensions and possessing inertia, be rotated in a direction, say, towards the north and south, it would gradually assume the form of a disc, which would continue to thin and spread out indefinitely while the motion is continued. But suppose when it is given the rotary motion north and south, it be given also a rotary motion east and west at the same time, then the body will assume and maintain a spherical, lenticular or pseudo spherical form, depending upon the proportion the compounded angular rotary motions bear to each other. If both motions were equal a sphere would result; if unequal, some other form would be determined, and at the same time a continuous strain would be exerted and be distributed equally throughout the mass in every direction. If this rotary motion were great the strain would be in like proportion, and the media would assume the rigidity of a solid, or pseudo-solid. In this way any proportion of tension might be accounted for and be perpetuated in an ether of this description under such conditions. [Experiment—*]

Paradoxical though it may seem, bodies consisting of groups or tangles of vortex rings of this media, they themselves, stiffened by their own rotation into extreme rigidity of material atoms, could travel through

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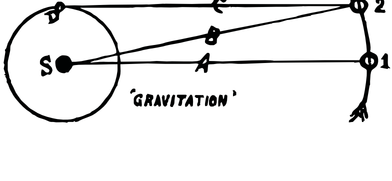
* An apparatus, consisting of a ball through which were thrust cotton fibres, was here introduced and set in rapid motion; the rigidity due to great velocity was thus demonstrated—compounded rotary motions producing a sphere.

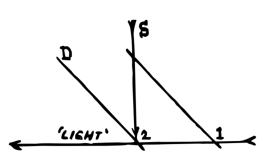
such a solid without any appreciable friction. Prof. Oliver Lodge continued his experiments in 1892, 1893 and 1894 to detect a possible drag exerted by matter as it passes through the ether. He has been able to rotate two discs of tough steel, a yard in diameter and one inch apart, on the same axes, 3,000 revolutions per minute, in the same and in opposite directions, and examined a beam of light passed between them, without obtaining any evidence of a drag in the ether, which should have shown itself if it existed. An oblate spheroid of wrought iron weighing a ton and magnetized by a current sent through a wire wound around it in a deep groove, was also rotated without effect. Lodge has also determined by experiment that moving bodies carry the ether along with them in their interstices, but that the ether outside their surfaces remains relatively stationary.

We usually speak of the material universe as being infinite. But why should it be so? If matter is finite it cannot be infinite; for one condition is the opposite of and denies the other. It is a contradiction of terms to speak of an infinite collection of finitudes. We use words and terms in an absolute and relative sense, and if this be marked there need be no confusion of thought. Space relative, is distance between extended points; space absolute is that which remains when everything (ether and all) is removed ; and what remains? why, nothing but void, absolute no-thing. Where does nothing end? It cannot end; having no parts it is simply absolute infinite Void. Space absolute cannot, therefore, be spoken of as finite, but space relative has a limit, and within a finite universe, without curved straight lines or any appeal to transcendental geometry whatever for its postulates. Lord Kelvin informs us that by three processes of determination he has been able to prove atoms and molecules to be of definite size and dimensions, in figures like these: "If a speck that floats in the sunbeam be divided under the microscope of highest power into 30,000 equal parts, then each of the parts will contain at least 60,000,000 molecules, and every molecule will contain from two to one hundred atoms." If this be so, what must be the number of atoms contained by this world of ours! The number undoubtedly must be great—past enumeration. But who will say for this reason that the world is infinite. And so apply this principle to the starry hosts ? They may number millions upon millions, and their exact enumeration may never be made, but vastness of measure and quantity do not necessarily indicate infinity.

If gravity be a form of energy in any way resembling the forms with which we are acquainted, it would seem that it would require time to be transmitted from one body to another; all the theories mentioned here, and any other attempting a physical explanation, require a "time propagation" for gravity. Most of our astronomical authorities have assumed, however, that the action must be absolutely instantaneous, and if it were otherwise, it would bring ruin to our solar system in short order. The reasons given for assuming this result are briefly, the aberration argument; the difference of effect when a body approaches and recedes from an attracting centre; and finally the fact that no allowance for time is made in the computations when gravity is reckoned with the usual mathematical formula.

We are told that; if gravitation took time to act, within the solar system, an aberration effect would ensue, the result of which—according to La Place—would ultimately be to draw a planet onto the surface of its primary; or, as is now generally stated, an aberration effect would throw it further and further away, as shown in the diagram below. Strange as it may seem, the mathematical calculations La Place applied in *Mecanique Celestè* to an approaching body are flow applied to a receding body. If a force emanate outward from the Sun at S with say the velocity of light, along the line A, towards the Earth in her orbit at position 1, the force acting along this line would not reach the Earth until she had arrived at position 2, eight minutes afterwards; consequently the pull of the Sun upon the Earth would be along the line C towards the aberration point D, instead of along the line B to the Sun's centre, and the continual addition to the tangential in this way, must throw the Earth further and further from the Sun; but as no such orbital change occurs, the force in action must, therefore, be of such a nature as to occupy no time in traversing the intervening space.





The above, with diagram, is the substance of the " aberration " argument as stated by Proctor in *Flowers of the Sky*, Herschel's *Familiar Lectures*, Arago's *Astronomy, Old and New Astronomy*, and other works of a more technical nature.

Now, it appears to me that a radiating energy (or force) acting from a centre, such as "S," must dart out rays (vibrations or stresses) equally in every direction at the same time—if gravity be considered as an emanating force it cannot do else than this—so that the energy, or force, must move outward along the lines "A" and "B" together, and along every possible radiant path also, with the same velocity; this being so, when the Earth is at "2" it will cross the line of force moving outward along the radiant "B," while the force moving along the line "A" will pass onward in its path without influencing the Earth, at that instant, in the least, because the Earth would not be there to receive it; but the force moving along "B" will meet and intercept the Earth as she crosses its path at "2," and so in like manner will every other ray crossed, in succession pull the Earth in a straight line for the Sun's centre, and not to the aberration point shown in the diagram. Furthermore, the aberration of light is simply and purely an optical illusion, the displacement of the path of the light ray is only an *apparent*, and not a real, displacement, and as gravity would not be subject to optical effects, even this apparent displacement could not occur. So that whether gravity be instantaneous or take time to leap across the chasm, the result would be the same.

As regards the reaction of the Earth upon the Sun the same would apply; the Sun would be drawn towards position "1" instead of to position "2," but the lag in that case being a constant "2" might be blotted out and treated as if it were at "1."

As regards the second argument against a time propagation of gravity, namely, the difference of effect experienced during the approach and recession of a gravitating body, such as a comet, to and from the centre of attraction :—

If the force exerted upon the body were of the nature of an impact or mechanical pressure from without, this argument might apply; but if the force be of the nature of a strain or stress, penetrating the body in question, without friction, it is difficult to see how the motion of the body can modify or intensify the effect of such a strain one way or the other; whether it crossed many vibrations (or waves), or as it were, rode on the crest of but one wave or vibration, the stress exerted upon it would be the same, depending solely upon the distance separating the attracting centres, which may be considered as stationery or in motion through a relatively stationery ether. As to no allowance being made for time, none is needed when the result of a "time" or "instantaneous" action is *practically the same*. This does not prove that gravity takes time to act, but it does show that any argument alleging that such an assumption necessarily introduces an element of instability into the system, is fallacious and if persisted in, must remain a stumbling block that effectually stops all investigation at the very threshold of the subject.

That gravity can be of the nature of a mechanical pressure seems improbable when it is noticed that if a plastic sphere, when bulged at the equator, as Jupiter is, were subjected to equal pressure from every side, it would, after passing from the form of a sphere, continue to flatten out into a disc one molecule in thickness, on account of greater surface presented to pressure on the sides than the edge. Attraction, though it would be satisfactory here, on the other hand, implies an action not easy to explain in other respects.

To sum up, the different theories of gravitation may be classed as (1) due to magnetic or electric action; (2) impact of particles, as of a gas; (3) pressure in a liquid (negative or positive) and Cartesian vortices ; (4) waves in a liquid ; (5) vibrations in a solid ; (6) action from centres of force, and between matter and ether ; (7) attraction, necessary property of matter—action at a distance; (8) pressure and tension in a practical solid, or pseudo-solid.

Some of our foremost modern philosophers have endeavoured, in the name of science, to fence off a portion of nature's broad expanse into a little domain of their own, and have placarded its every possible approach, with conspicuous and forbidding letters of black, "this is the domain of the unknowable—none may enter here," and have consigned this question, as well as every other knotty question of like nature, into this handy though misty limbo of oblivion. But that this subject is, however, within the legitimate field of investigation, it is only necessary to point out, that to be aware of the existence of anything, objective or subjective, we must have some knowledge of that thing; and no matter how slight that knowledge may be, if we know anything about it, the thing must be knowable. Consequently, as we have some knowledge of gravity, it must be a subject or object of possible knowledge. Want of progress in the investigation, is no valid argument against its possible solution.

The words of Prof. Tait are again appropriate here, "Failure in the eyes of a genuine scientific man, is only an encouragement to persevere."

Any satisfactory theory must, however, successfully meet the requirements with which the subject is conditioned, namely, (*a*) the energy must be such as can be transmitted by a solid, or pseudo-solid, and (*b*) penetrate all bodies without dimunition of force, (*c*) must be a radiant and continuous (*d*) diminishing in intensity as the square of the distance. Though the *cause* of gravitation remains still unknown, it may not always be so. Of the great generalization itself, I cannot do better than close with the words of Prof. Taylor, "But as yet, this grand generalization of universal gravitation, after two centuries of busy thought and daring speculation, still remains the largest, clearest, surest, yet attained by man ; and with each revolving year, new demonstrations of its absolute precision, and of its universal dominion, serve only to fill the mind with added wonder, and with added confidence in the stability, and the supremacy of the power, in which has been found, 'no variableness, neither shadow of turning, but which, the same yesterday, to-day and forever.'"

SIXTH ANNUAL MEETING.

January 7th, 1896 ; the Vice-President, Mr. John A. Paterson, M.A., occupied the chair.

After the reading of the minutes of the previous meeting, reports from the various officers were received.

Mr. Chas. P. Spaling, Recording Secretary, reported that there had been held during 1895 twenty-four regular meetings of the Society, with an average attendance of about thirty members, and two open meetings, to which the public had been invited. All arrangements in regard to the removal of the Society's effects to the Technical School building had been satisfactorily completed. Mr. G. G. Pursey, Librarian, presented a list of donations to the Library dining 1895. This included a number of valuable works on astronomy presented by the retiring President, Dr. Larratt W. Smith, Q.C., to whom the thanks of the Society were specially due The various observatories and scientific bodies which in the past had favoured the Society by forwarding copies of their publications, had continued to do so in 1895 ; while several, making donations for the first time, had kindly intimated that the Society would be placed upon their regular exchange lists.

Mr. J. Todhunter, Treasurer, reported that the balance of cash on hand would be sufficient to meet the expenses now being incurred in connection with the publication of the *Transactions* for 1895; the income of the Society was, however, still too small to warrant certain expenditure which had been recommended by the Council. It was with the strictest economy only that all necessary expenses could be met. On motion, the Treasurer's report, being duly certified, was adopted.

The election of officers for 1896 was then proceeded with, and resulted as follows—Hon. President, Hon. G. W. Ross, LL.D.; President, Mr. John A. Paterson, M.A.; Vice-Presidents, Mr. Arthur Harvey and Mr. R. F. Stupart; Corresponding Secretary, Mr. G. E. Lumsden; Recording Secretary, Mr. Charles P. Sparling; Treasurer, Mr J. Todhunter; Librarian, Mr. W. B. Musson; Assistant Librarian, Miss Jeane Pursey; Assistant Secretary and Editor, Mr. Thos. Lindsay; Members of Council—Messrs. C. A. Chant, B.A., J. R. Collins, A. DeLury, B.A., G. G. Pursey, ;and Rev. C. H. Shortt, M.A.

It was moved by Mr. T. Lindsey, seconded by Mr. A. Elvins,

Resolved, That the Society place on record its high appreciation of the services of Mr. G. G. Pursey, who had voluntarily retired from the office of Librarian; one who had been among the first in Toronto to take active steps towards the founding of the Astronomical and Physical Society, and who had indeed, during the whole of his life since early manhood, been actively interested in scientific pursuits.

This was carried unanimously.

It was moved by Mr. G. E. Lumsden, seconded by Mr. T. Lindsay, and

Resolved, That section 1 of Article III. of the Constitution be amended by adding the following words after the word "officers" in the fourth line, viz., "and every ex-President and ex-Vice-President"

This was carried.

It was then moved by Mr. G. E. Lumsden, seconded by Mr. W. B. Musson,

Resolved, That we, the officers and members of The Astronomical and Physical society of Toronto, hereby tender our thanks to our retiring President, Larratt W. Smith, D.C.L., Q.C., who has for the past two years so capably filled the chair. We owe to him a debt of gratitude, not only for the uniform interest he has exhibited in our pursuits, but, moreover, a special meed of praise for the munificence of his gift to our Library, thus giving tangible proof of an earnest devotion to the cause of science and an earnest desire for the advancement of our knowledge therein. It has been our loss that the infirmities of years have prevented him from more frequently discharging his duties among us as our presiding officer. We, however, confidently expect to retain the benefit of Dr. Smith's counsel in the direction of our affairs, and it is our hope that He that doeth all things well will so tenderly guide our retiring President that his life will yet grow richer in happy years and brighter yet as it moves onward from the mere discoveries of human experience to that revelation of divine good that crowns every useful life. May it be long ere with him it grows late and dark.

The resolution was adopted and the Recording Secretary instructed to inscribe the same upon the minutes of the Society.

It was moved by Mr. Chas. P. Sparling, seconded by Mr. A. Elvins,

Resolved, That we, the officers and members of The Astronomical and Physical Society, of Toronto, hereby offer our thanks to our retiring Vice-President, E. A. Meredith, LL. D., for the uniform and kindly interest he has always exhibited to us in our meetings and pursuits. It has been most gratifying to us all to have enjoyed the distinction of ranking amongst our officers the name of Dr. Meredith. Although he retires from our list as Vice-President, we yet confidently trust to retain the benefit of his counsel in the direction of our affairs. If a man will but keep his light shining, the Almighty will put it where it can be seen. We earnestly hope that our retiring Vice-President, full of years and rich in life's experience and of mature scholarship as he is, will be helped to keep his light shining, and we know that it will be seen ever lustrous and clear for the good and the right and the true.

The resolution was adopted and the Recording Secretary instructed to inscribe the same upon the minutes of the Society.

Mr. R. F. Stupart, Director of the Toronto Magnetic Observatory, presented the following

REPORT OF THE EARTH CURRENT COMMITTEE.

During the past year, through the courtesy of Mr. S. S. Dickenson, Superintendent of the Commercial Cable Company at Canso, N. S., returns showing the direction and voltage of Earth Currents measured at 5 a.m. each day on the Company's cables, have been received at Toronto. These readings are likely to prove most useful and valuable; they have been charted, the ordinates used being the current expressed in volts and time. On the same paper have been charted curves showing the daily mean values of the vertical and horizontal magnetic components at Toronto Observatory. A comparison of the curve formed from the Earth Currents, measured on a cable running eastward from Canso to

Ireland, with the magnetic components, is especially interesting, as there is often a marked similarity in the curves, indicating pretty clearly that there is a close connection between the two phenomena; doubtless had we a continuous record of the Earth Currents the comparison would be still more interesting, and the connection more evident.

In the accompanying table are given the values of the E. C. readings for the month of April, together with the absolute values of the magnetic elements :—

APRIL, 1895.

DATE.	EARTH CURRENTS IN VOLTS. CANSO, N.S. CABLE NO. 3	HORIZONTAL FORCE, TORONTO.	VERTICAL FORCE, TORONTO.	TOTAL FORCE, TORONTO.	DECLINATION, TORONTO.
1	+ 3·28	0·16645	0·60271	0·62527	284·9 West
2	+ 8·82	0·16657	0·60283	0·62541	284·3 "
3	– 2·85	0·16662	0·60252	0·62514	284·0 "
4	+ 0·20	0·16670	0·60276	0·62539	283·9 "
5	+ 4·02	0·16647	0·60285	0·62540	285·9 "
6	+ 1·62	0·16651	0·60267	0·62525	285·4 "
7	+ 1·20	0·16652	0·60313	0·62581	285·7 "
8	+ 0·62	0·16663	0·60282	0·62542	285·3 "
9	+ 0·38	0·16653	0·61262	0·62520	284·0 "
10	– 0·45	0·16650	0·60304	0·62560	285·8 "
11	+ 9·76	0·16635	0·60257	0·62511	281·6 "
12	+ 20·32	0·16623	0·60321	0·62570	285·2 "
13	– 1·36	0·16643	0·60300	0·62554	283·6 "
14	+ 1·83	0·16647	0·60263	0·62532	282·9 "
15	+ 2·43	0·16650	0·60273	0·62530	285·3 "
16	+ 0·55	0·16659	0·60311	0·62569	288·8 "
17	+ 6·10	0·16649	0·60256	0·62514	284·5 "
18	+ 4·83	0·16649	0·60285	0·62542	283·7 "
19	+ 0·22	0·16651	0·60288	0·62545	283·3 "
20	– 0·48	0·16648	0·60290	0·62546	283·6 "
21	+ 1·54	0·16660	0·60304	0·62564	285·6 "
22	– 0·37	0·16667	0·60251	0·62540	284·6 "
23	– 3·15	0·16669	0·60274	0·62537	283·6 "
24	+ 1·22	0·16653	0·60296	0·62554	284·7 "
25	+ 1·02	0·16658	0·60277	0·62537	282·2 "
26	+ 9·80	0·16647	0·60275	0·62531	286·1 "
27	+ 0·50	0·16657	0·60261	0·62534	284·9 "
28	+ 0·20	0·16660	0·60311	0·62571	285·4 "
29	+ 2·23	0·16663	0·60289	0·62549	284·9 "
30	+ 1·50	0·16670	0·60280	0·62543	284·9 "

It was then announced that at a special meeting, to be held Jan. 21st, the President would read a review of astronomical progress during the past year, and the proceedings were brought to a close by adjournment.

[Address delivered before The Astronomical and Physical Society of Toronto in the Lecture Room of the Technical School, January 21st, 1896, by the President, Mr. John A. Paterson, M.A.]

THE PROGRESS OF ASTRONOMY IN 1895.

I wish to thank you very earnestly for the distinguished honour you have conferred upon me in placing me in the Presidential chair. With much diffidence, I accept the distinction, for it is one worthier of an abler man, but the hesitancy which almost overpowers me is counteracted by the conviction that you do not expect too much from me, and I know that the members of the Council who surround me are so earnest and so capable, that I am assured of a most vigorous support in the discharge of my duties. But I wish you to know that although my heart is stirred with that conviction, yet I purpose to do all that lies within the circumference of my power to cope with all the responsibilities involved in and chargeable upon the office. You are all searching for Truth—I trust your search for her will meet with more success than you have met with in your search for a President.

This country is not old enough nor large enough yet, to possess any number of men of leisure possessing cultured surroundings and scientific aspirations. We are all busy men and women, and although one must, on the one hand, avoid frittering away energies in too many pursuits, and thus, by too great expansion and consequent rarefaction, diminish kinetic energy according to a well understood physical law which has also its counterpart in the domain of intellect, yet, on the other hand, a change of pursuit is beneficial, and tends to develop a fully rounded man and make his nature, so to speak, spherical and not eccentrical. As Longfellow, in his Table Talk, puts it: "As turning the logs will make a dull fire burn, so changes of study a dull brain."

To wrest from the book and volume of the sky its secret and to read its mystery, is a problem that for centuries has strained the highest powers of human intelligence. The solution of that problem has been in progress since our great ancestor gazed for the first time on the gloom that deepened from the purple twilight and in the mellowing shade he first beheld the radiant rulers of the night.

Discovery commenced as a very feeble rill, and as centuries rolled on swelled into a mightier and swifter current, although the silver purity of Truth has been fouled many times by muddy torrents of error, which have often blotted out the efforts of nature to reveal to man the teachings she would have him learn. There have been in its progress periods of hybernation, during which the astronomic mind rested from its labours to gather strength for some greater achievement. Kepler, toiling with laborious induction, died twelve years before Newton was born, and reached the meridian of his glory well nigh three-quarters of a century before the great English philosopher invented his calculus and taught the ages the secret of the universe by a rigid deduction from the one great general principle of gravitation. Nature, ever since primitive man had first hurled a stone, had been striving to reveal the secret which holds the universe in its power, and to teach the law which has in its keeping the earthward fluttering of the linnet's feather, as well as the ceaseless swing through the infinitude of space of the mighty Sirius. Newton first interpreted the strivings that Nature was making to teach and read her symbols to the world of philosophy, and the era of Physical Astronomy dawned. And then followed a long quiescence. The Sun since first he had shone through his nebulous shroud had been flashing signals to the Earth telling her of his nature and of her genesis. Every ray of light was a messenger bearing symbols of scientific truth ; but man would not, or could not, read the message until Fraunhoffer, and Kirschoff and Huggins, and Doppler, and Young, received the beam through the spectroscope and read the story the Sun wished to tell. And soon, too, Arcturus and the rest of the starry host gave up their secrets; their structure was examined, and as a musician by his tuning-fork determines the pitch of a note, so the spectroscopist by the dark line or the bright-line analyses the constitution of the most distant wanderer that hovers on the very confines of visibility. And so the work goes on, and Science plants her foot still further and further onward, ever onward, and slowly conquers the desert of the unknown but the knowable, and transforms it by her wondrous culture into smiling fertility—a hand-breadth only it may be in a whole century—but many hand-breadths make a space which lessens the vast unknown. It has been my great privilege on two previous occasions to review shortly the advancement of astronomic science in 1893 and 1894. The year 1895 is behind us—it is a thing of old, but its achievements in Astronomy and Physics will live with those of previous years, and so we push out into the dim twilight, to be succeeded by the clearer day. What, then, has 1895 accomplished for us in our pusuits ? What pæan of victory can it sound ? What conquest has it made?

The past year has added a more than ordinary share to the world's observatories. The Universities of Pennsylvania, Minnesota and Ohio, and the Case School of Applied Science (Cleveland), are having huge telescopes built. At Flower Farm, near Philadelphia, the first of the new buildings to be erected at a cost of $50,000, has been commenced; this is to be equipped with an 18-inch refractor, with an objective by Brashear. Dr. Wolf, at the University of Heidelberg, has mounted two astrophotographic doublets made also by Brashear, each sixteen inches clear aperture. The Willard lens of the Lick Observatory, which has produced such matchless photographs of nebulæ and comets and the Milky Way, is only six inches in diameter. We all expect great results from the new Heidelberg doubtless. A Solar Physics Observatory has in 1895 been even built on "India's coral strand" (if, indeed, that is a correct expression at 7,700 feet above the sea level), in the Madras Presidency, under the best atmospheric conditions beneath that star-gemmed sky, whereto the old Fire Worshippers centuries ago lifted devout eyes from their Zend Avesta, and there traced the footsteps of their deity in the ever circling stars. The superb gift of Mr. Edward Crossley, F.R.A.S., and M.P. for Halifax, England, to the Lick Observatory has arrived, and is being placed in position. This giant reflector is in diameter thirty-six inches, its mirrors and finer mechanical parts weigh more than one and a-half tons, the mounting seven and and a-half tons, and the larger part of the dome twenty-three and a half tons. This giant light gatherer will make Mount Hamilton the most famous spot in the world for its aggregation of magnificent instruments. This is truly a great international courtesy, and proves surely the uplifting power of science. When England gives one of her greatest instruments to help America conquer the depths of space and pluck from the heart of the far, far distant nebula that faintly gleams on the confines of space the secret of her nature, let not the nations wrangle over a few miserable acres of swamp land in South America. The aspirations of science are loftier than those of politics. Were those who guide the destinies of nations to sit at the feet of Philosophy and become scientific, they might become high-minded. But when I read the triumphs of other Universities in procuring magnificent astronomical equipment, I bethink myself of that University that lies nearer our hearts and cannot yet for want of money and public spirit take her true place among her sisters on this continent, share in the triumphs of astronomic progress, help to bend down lower the branches of that tree of knowledge that hides its top in the firmament, and thus write her own page in the ever multiplying books of discovery and observation. Before I close this part of my review, let me tell you of the leviathan that is expected at the Paris Exposition of 1900 to gaze into the abyss of space with a 48 inch objective. It is to be so arranged that images of objects under view will fall on a screen, so that a number of observers will see at the same time. What this will cost is not yet recorded, but nothing is too costly in the search for truth. I notice in passing a new instrument invented by M. Lippman, called the cœlostat, whereby a star and the whole stellar vault presents a steady image for the purposes of observation. A plain mirror is mounted on an axis parallel to a line joining the celestial poles, and by clock-work the whole apparatus is rotated in forty-eight hours.

There is a very popular impression that astronomers at large observatories spend their time in sitting in turn at the eye end of their instruments, and live laborious night in seeking to discover some new thing, like the ancient Athenians in St. Paul's time; as if indeed the be-all and the end-all of astronomic effort was to ferret about in the sky and find out some new cornet, or planet, or satellite, or star. But the fact is that very few professional astronomers engage in that work, and such a pursuit is left to amateur astronomers and the smaller observatories. The Lick Observatory, for instance, devotes about one twenty-fifth of its energies, and much less than one twenty-fifth of its equipment in looking for new objects. Such an expensive equipment as it possesses could not be perpetually employed in such uncertain work. Nearly all the unexpected comets have been discovered by private astronomers. Of the five new stars discovered since 1866, three are to be credited to amateurs ; of the seven satellites of our solar system discovered since 1840, four were discovered by a private observer at Liverpool, although indeed for such research a large telescope is peculiarly fitted. The sweeping for variable stars is nearly all done by amateurs. The inquiry then is pertinent—if the greater part of this success is due to private enterprise, what is left for professionals with their splendid observatories equipped with the finest and mightiest instruments endowed by princely munificence or under government patronage ? Prof. W. W. Campbell tells us what such scientific work really is. It begins just when the new object is discovered ; it consists in the collection of facts and numerical results, and their arrangement in such a form that the general principle and laws of the science become apparent. The nucleus of their work is not " discovery," but it is " investigation." As Darwin laboured to construct a theory of organic life from a vast inductive research of every form of life that existed, and by comparison of all grades and varieties sought to evolve some great and universal law, so the astronomer seeks from a laborious research into the inorganic life of the universe, tracing the flight and watching the movements of sun, planet and satellite, comet, meteor, nebula and cluster, to construct one vast and pervading theory that will dominate as a mighty law of which even that of gravitation may be merely a sub-section. All the facts of their nature, their whence and their whither, their present and their future, all the essentials of their being the professional astronomer, with unstinted struggle and unbaffled skill, labours to conquer. Nor is it to be supposed that a big telescope is all that is wanted. It is, no doubt, an important matter to use a large objective, but it is of more importance to have a keen eye at the eyepiece, and behind that again a large brain that can interpret what the eye sees. Great results can flow from the little end of even a little telescope, provided a mathematical astronomer is there, and not a mere observer. La Places, Herchells, Struves and Bessels are wanted more in the world of astronomic science, very much, indeed, more than Lick or Yerkes, or the most renowned artificers, although all these have built the ladder by which the scientific astronomer has scaled the heights of success. God alone makes the astronomer; man may make an observer. Bessel said, "a practical astronomer could make observations of value if he had only a cart wheel and a gun barrel." The profoundest discoveries excite little popular attention, but if a new comet is discovered, then every newspaper heralds it, and the world ruled by reporterdom is all agog with amaze, and the fortunate discoverer becomes *digito monstratus*, "pointed at with the finger." There is, indeed, a popular superstition about the superiority of large instruments, but there is at hand an effective commentary on that for Schiaparelli discovered the peculiar configurations on Mars which he calls canals with eight and one-third inches, while the great Washington refractor of twenty-six inches had failed to reveal them. It is one thing to see and another thing to perceive what you see. Furthermore, there is a limit to telescopic power even were there no limit to the size of an objective or the strength and steadiness of its mounting, because we live at the bottom of an ocean of atmosphere and look through what is often a heaving, whirling mass of disturbing billows. As we increase the magnifying power, we proportionately increase the distortive power of the boiling, quivering fluid in which we are immersed, and through which we have to look. When the atmosphere is disturbed in this way, which may, indeed, happen apparently in the clearest and calmest night, scrutinizing planets in detail is like trying to read a page of fine print through an opera glass, when the print is kept dancing up and down rapidly and irregularly. The *ne plus ultra* of telescopic work would be in a condition where we could set it up *in vacuo* and dispense with the embarrassment of lungs; then what an Elysium of telescopic joy we would have, because indeed we would be then in the Elysian fields. The result of this terrestrial atmospheric inconvenience is that the giant telescopes can give their best results in searching out minute configuration of planetary or lunar detail only at rare intervals of time, and so the Lick observers are forced to wander from instrument to instrument and suit their observations to the atmospheric disturbance.

THE SUN.

The problem of the Sun and its nature has made progress during the last year. This globe of flaming gas has had volumes written on it, and theories of its nature are in number “thick as autumnal leaves that strow the brooks in Vallombrosa,” until one would almost re-echo Macbeth, who, when oppressed with dismay at the approach of Birnam wood, said, “I ’gin to be aweary of the sun.” The period of the Sun’s rotation has been variously estimated. Prof. F. W. G. Spörer, of the Astro-Physical Observatory at Potsdam, attained great celebrity by his determination of the rotation period, and by his investigations relating to the law of the retardation of this rotation dependent on latitude. Duner solved the same problem from spectroscopic observations. But it has been reserved for M. Von Stratonoff to demonstrate in 1895 a shorter period of the solar rotation by observations of the faculæ. It was once a favourite theory that terrestrial phenomena and sun-spots were closely connected, but now

Dr. Lewis Swift has suggested that the true connection is between these phenomena and the faculæ. Mr. H. Ebart has sunk his thermometer, so to speak, below the photosphere and finds that it records 40,000° C. The scientific World is now making preparation for that sublimest and rarest of astronomical spectacles, the total eclipse of the Sun, which is timed for August 9th, 1896. The line of totality crosses the coast of Norway and the region of the Varanger Fjord is the objective point of the eclipse expedition organized by the British Astronomical Association, and there, right in the home of the north wind, scientists will establish themselves and during that critical period of totality, with telescope and spectroscope and every available weapon from the arsenal of science, will attack the mystery of the corona and compel her to let go her secret. Already observers are rehearsing for this great event, dividing an imaginary corona amongst themselves, and testing their capacity at rapid draughting, for Dame Nature is not, in this regard, generous, and offers her students a fleeting glimpse of this most wondrous sight for only 106 seconds. Other observers are prepared to search yet again for Vulcan the supposed intra-Mercurial planet and others yet for a comet which may be in conjunction.

In 1868, when the spectroscope was first directed upon a solar eclipse, the famous D-3 line, the bright yellow line near the D lines of sodium, was first seen in the prominences. It has no corresponding dark line in the ordinary solar spectrum. Frankland gave to the unknown substance to which this line was ascribed the name of *helium*, as if it were indigenous, so to speak, to the Sun. After this, D-3 was detected in the stellar spectra, but it was not found in any part of the Earth, which was strange, and could not be accounted for if indeed the Earth is a child of the Sun, and not an aggregation of independent materials. If the nebular hypothesis is true, the Earth should show its kinship to the Sun by distinct marks of inheritance, and amongst others *helium*, unless, indeed, all the *helium* that ever was in the Earth became free, and its molecules having a velocity of more than seven miles a second, had left the Earth to seek some body of greater gravitating power. And thus the inquiry rested until Lord Rayleigh and Prof. Ramsay announced the discovery of a new substance in our atmosphere. They had noticed that the density of nitrogen taken from the air differs about one-half per cent. from the density of nitrogen obtained in any other way. This element, that so strangely affected atmospheric nitrogen and gave it a distinctive character, they separated by the action of magnesium, and a new gas was evolved whose density was fifty per cent. greater than nitrogen. This they called *argon*, because "it did no work," although indeed, that negative character could not have been attached to these indefatigable chemists. Last March, Prof. Ramsay, seeking to ascertain if this youngest born of scientific discovery could combine with anything else, was examining the rare earth found in Norway known as cléveite. When treated with weak sulphuric acid it gave off *argon*, associated with something else, which he described as "a gas which has not yet been separated." It was submitted to Prof. Crookes, and it was proved to be *helium* imprisoned in the cléveite, and thus *helium* is now a misnomer, and the Earth bears another possession from her great Sun-mother, although it is of such a light and frivolous character that if released from its rocky prison it will fly Sun-wards and seek once again to nestle in the bosom of that fiery power that gave it birth.

MERCURY.

This nearest attendant on the monarch of our system has had his density measured anew during 1895. He is neither too swift nor too small to escape the grasp of the calculus and the probing of the astronomer's staff. Through a rigid demonstration, based on the perturbations of Encke's cornet, the density of Mercury has been found to be 3·7 (water being 1). The Earth is the densest body in our system.

VENUS

Herr Bremner has been at work on the rotation period of our evening and morning star, and has evolved a new result of 23h. 57′ 8″, not differing much from that of our own daily movement; but this is several minutes slower in time than the formerly accepted results.

MARS.

When Mars comes to be reviewed we do it with hesitancy, knowing how easily we turn from the firm ground of scientific investigation to the slippery path of romantic story. The popular humour delights in philosophy decked with the charm of conjecture. Anything which is conceivable may be interesting, but science is founded upon the rock of evidence. Even an astronomer may say "it can be stated without fear of refutation that such and such a proposition is true." A new theory of cosmogony might he built upon such a basis, and lecture halls would ring with plaudits at the unfolding thereof notwithstanding the warning " that the assertion which outstrips evidence is not only a blunder but a crime." Bacon truly said in his essay on Truth, "The mixture of a lie doth ever add pleasure," by which he meant not a lie in malice, but any bold statement that was fleeter of foot than evidence. Rigid demonstration is unpopular, but any astronomer of reputation and of ready wit can fill a lecture hall if he proposes to discuss the altitude of the Martian inhabitant; or if he will tell us how to construct some mighty triangle or pentagon on the Desert of Sahara, light it with miles of electric lights, and then watch for results from our neighbour Mars. This was exactly what happened with Mr. Lowell, of Flagstaff Observatory. Huntington Hall, at Boston, was filled, every seat and all the standing room, when he gave his four lectures on the planet Mars. He is a very famous astronomer, and writes most charmingly in the *Atlantic Monthly*, and when he speaks he will be listened to eagerly, and what he writes will be read by the magazine-loving public from cottage to boudoir. We have all beard the story of the canals, and they are now accepted as indubitable by the scientific world. Much more is now added. The great and absorbing question with the Martian people, it seems, is the water question. Worlds, like individuals, are not gifted with perpetual youth; they are born, spend a hot and feverish infancy, grow cooler with advancing years, attain an early, youthful vigour, and are fitted for the abode of life. As they advance in life, higher types of inhabitants are evolved; they then grow old and commence to droop with the cold, until they reach senility, and then come decay and death, after millions of revolving ages. Mars, it seems, is far advanced in its life; its mountains are all levelled, its water has nearly all evaporated, its inhabitants are driven to protect themselves by a gigantic international system of irrigation. They have dug a network of canals, and to catch the annual meltings of the polar ice-cap; oases are formed at the junctions, and there the strong-minded and mighty limbed Martian most do congregate, and admire their ingenious hydrographic system. The silver thread of the actual aqueduct is not visible, but what are seen are two broad strips of vegetation growing on the banks. It is proved by strict mathematical reasoning that on account of the small gravitating power of Mars, its men are giants and are fifty times as effective, and can do fifty times as much work, so that the task of excavating these wonderful ditches is easy. One Martian is as good as fifty Chinamen. Life is, moreover, further advanced: the arts and sciences are thousands of years older than here on Earth, and the powers of nature being better understood, more gigantic results can be produced. Steam and electrical machinery are long out of date, and are kept in museums as relics of a bygone civilization, and so the rein is given to the most fervid imagination, and the grandest results flow easily. Even good Schiaparelli is quoted, speaking on the idea that the "canals" are the work of intelligent beings: "I should carefully refrain," he says, "from combating this supposition, which involves no impossibility." But Schiaparelli was speaking as a philosopher, and was not lecturing to a Boston audience, or writing for the *Atlantic Monthly*, or he would have put it positively and not negatively. The Lick Observatory people are singularly unappreciative; they exhibit little real enterprise, or the air at Mount Hamilton is not so exciting as that at Flagstaff. This is what Prof. Edward S. Holden coldly writes: "Something is seen, no doubt, but I may say that nothing has been observed at the Lick Observatory during the years 1888 to 1895, so far as I know, which goes to confirm the very positive and strong conclusions which are described. It is a point to be noted, that the conclusion reached by Mr. Lowell at the end of his work, agree remarkably with the facts he set out to prove before his observatory was established at all." Conjecture, however, is often the pilot of discovery. Let us suspend judgment until we hear from the Yerkes' telescope at Lake Geneva, near Chicago, working under the best atmospheric conditions next spring, or until the projected monster at Paris in 1900 verifies, if it will, Mr. Lowell's anticipations. We may add that to explain the mysterious gemination, or doubling of the canals, so far has defied the most laboured efforts of the Flagstaff Observatory. It is from no desire to depreciate Mr. Lowell's delightful conjectures that I call to mind that most Munchansen-like story which an American newspaper scribe boldly fabricated, that the network on the Martian disc was fashioned in the form of the Hebrew letters signifying "Shaddai" or God; and so the Martian people were signalling to us the name that was known throughout the universe, and were calling for a similar sign of recognition. Among many other startling hypotheses which are thus aroused, it must be that the Hebrew characters and language are well-known to the inhabitants of our fiery neighbour. But this out-herods Herod ; even Boston has not been able to reach this wondrous height, and we feel oppressed with the most poignant grief when we consider how many men have lived in past ages and died at last in ignorance of this great discovery.

THE MOON.

Our satellite was twice totally eclipsed in 1895—in March and September. There is little real scientific value in lunar eclipses. They are extremely interesting and attract popular attention to what has been justly called the " Queen of Sciences." The shifting shades of colour were a beautiful object of study. The theory has been advanced that the Moon is self-luminous from the intense and constant heat that is being poured upon it, and that it has risen in colour to a dull red and so the disc is not absolutely blotted out by the Earth's shadow. But on the other band the delicate observations made by Lord Rosse and Langley with the thermopyle show that the heat diminishes in an eclipse almost as rapidly as the light does, and so the heat we receive is almost entirely reflected.

THE PLANETOIDS.

The roll-call of these pygmy children of the Sun continues to grow. Up to the end of 1895 about 420 were discovered, but they do not always answer obediently to their names or to their distinctive numbers (for they have not all been honoured by names), as if the belt they move in were a sort of celestial penitentiary. One named Œthra, discovered in 1875, has never been seen since, and seven more of the group are at present lost. Re-discovery would be interesting if they still exist in space; the photographic plate will be able to execute a writ of *capias ad respondendum* upon them. It is worthy of note that the greatest density of the system of planetoids occurs just where Bode's law fixes the missing planet between Mars and Jupiter. One of those pocket world discoveries of 1895, Ottilia by name, has a period just one-half that of Jupiter, so that by Kepler's third law Ottilia's mean distance from the Sun must be Jupiter's mean distance divided by the $\sqrt[3]{4}$. Barnard has been measuring the diameter of the four first discovered of this family, viz. :—Ceres, Pallas, Juno, and Vesta. One finds no relation between their size and their brightness, as, for instance, Vesta is only half of Ceres in diameter but has an albedo four times as great.

SATURN.

This planet has been much studied since Clerk-Maxwell in 1856 gained the Adams prize for his proof that Saturn's rings were loosely revolving meteors which was confirmed by the spectrographic observations of Prof Keeler, of the Allegheny Observatory, who reached his conclusions by an application of Doppler's law. Mr. Stanley Williams read an important paper before the Royal Astronomical Society last April, by which he proved that its rotation period is different in different latitudes. Observations based on different groups of spots give different results, which thus show that the surface must be in violent agitation and it is almost certain that its rotation period increase from year to year. In 1891 the mean observations gave as a result 10h. 14′ 21″·8, while in 1894 the result of 10h. 12′ 35″·8 was reached.

COMETS.

To 1895 is due the discovery of three new comets, and the re-discovery of an old friend, Faye's periodic comet, by the Nice observers on the 25th September. Its period is about seven and a-half years, and it belongs to the Jupiter family. This comet is known as B (1895). A faint comet, A (1895), was discovered by Dr. Swift, of Lowe Observatory, on August 20th in the constellation Pisces. Its orbit is elliptical and its period seven and a-quarter years. To C. D. Perrine, of the Lick Observatory, is due C (1895), picked up by him at 5.30 a.m. November 17th, in the constellation of the Virgin, which fact may account for its contrary temperament, for it has a retrograde movement. Brook's comet D (1895), was discovered on the 21st of November ; a very swiftly moving and faint comet, but large and round with ill-defined edges. Two new comets in November and then the President's message, and one William Hohenzollern, of Berlin, filling up blank telegraph slips, and laying his hand on his sword. Do the modern comets still shake from their horrent hair fell pestilence and war ? In 1894 we had Salisbury, the President of the British Association, struggling with the unsolved problems of science. In 1895 a change comes o'er the spirit of our dream, and we have Salisbury, the British Premier, struggling with at least one unsolved problem of politics—the Venezuela boundary. The nature of the atom, or the mystery of the ether, or whether the force of gravity acts instantaneously or not is as nothing to that. It has been made sufficiently clear, however, to the political scientists that Cleveland's message acted with sufficient responsive instantaneousness.

NOVA CARINÆ.

A new star flashed her glory in the firmament in 1895; not one that had always existed and has just been observed, but one of the class that comes into existence suddenly, increases in radiance, and then melts into obscurity. To Mrs. Fleming, of Harvard Observatory, is due this discovery. Photographic plates (of which there are two kinds, spectrum and chart plates) of the heavens are taken by the large photographing telescope at the Harvard Annex Observatory in Arequipa, Peru, and despatched to Prof. Pickering at Cambridge, who deputes the task of their careful examination to Mrs. Fleming, assisted by a corps of ten or twelve young ladies, working in a small brick building, where already have been stored tons of plates. Thus a new star was discovered in the constellation Carina, which is above the horizon at Arequipa, whose latitude is about 17° south. The Nova was first seen at Cambridge on the photographic plate, where Carina is never visible, and thus a ray of light wrote upon the photographic film the record of either the collision of two bodies or of two meteoric swarms rushing through a nebula or of some magnificent volcanic outburst; and so the most mighty and most subtle power of nature became vassal to the human intellect, and, at the bidding of the scientist, at last reveals secrets that long have eluded the keenest intelligence. The spectra of these Novæ of Carina, Norma and Auriga are remarkably alike. Nova Carina is the fifteenth Nova that has been discovered in the last two thousand years.

The international photographic charting of the heavens goes on apace. Three or four observatories have already furnished their share.

The Royal Astronomical Society held a meeting on the 14th of June last at London, and there Miss Alice Everett, M.A., one of the Secretaries of the British Astronomical Association, read a paper on the binary star ι Leonis. This binary has a period of about one and three-quarters centuries. I note this as being the first paper read by a woman before the Royal Astronomical Society. The British Astronomical Association. has not only a lady Secretary, but also a lady Editor, Miss Russell, and another lady, Miss Brown, who is Director cf the Sun observing section. That distinguished astronomer, Miss Agnes M. Clerke, is also a member of its Council.

Chandler's discovery of variation in latitude of points on the Earth's surface continues to attract investigation. Until the year 1888 latitude was considered invariable, but now it is shown beyond doubt that it changes and the hypothesis grows into a proposition that the Earth is a body with variable form and some portions of its mass change positions relatively. Thus results a shifting back and forward of boundaries between states and countries, so that on exact limits a man by this curious wobbling of the Earth's axis of rotation may during the cycle of change find himself and his house under different flags. A Canadian Sheriff, learned in astronomy, and being advised by a solicitor who has made a special study of Dr. Chandler's discoveries, may yet, in a doubtful case of extradition, be able to arrest his man on Canadian territory where the latitude of his quarry has shifted a fraction of a second of arc northward. It were best, however, not to consider this question too curiously. International complications might be aroused and custom-house officers are sensitive.

As for the Unification of Time Reckoning, the goal is not far hence for which our Toronto Society has worked under the able guidance of Dr. Sandford Fleming. Difficulties are disappearing, and Civil, Astronomical and Nautical Day will ere long be coterminous.

Scientific research had during last year its triumphs over many problems, but there is with us all a problem over which no man can triumph. Death reached out his fell hand and gathered in more than one of the noblest of the sons of science. Dr. Arthur Cayley, once an English barrister, and a most distinguished mathematician, and who for thirty-two years occupied the Sadlerian chair of pure mathematics at Cambridge, passed away since our last annual meeting. He had been President of the Royal Astronomical Society and of the British Association; he contributed review to astronomical science concerning planetary and lunar theories, determination of orbits and solar eclipses. Dr. T. Brorsen died also in the past year; his name is well-known in connection with the discovery and literature of comets. Prof. F. W. G. Spörer, Chief Observer in the Astro-Physical Observatory at Potsdam, died last July. His chief work, which I have mentioned before, was the determination of the rotation period of the Sun and investigations relating to the law of the retardation of the rotation dependent on the latitude. The venerable Dr. Hind, once the supreme officer of the *Nautical Almanac* office, has also passed away. He discovered ten planetoids, sixteen variables, and three nebulæ. Prof. Daniel Kirkwood, the Dean of American astronomers, has also been added to the death roll; he is best known by his researches on the planetoids. He suggested the theory that meteor streams were the result of the breaking up of comets, and ranged himself under the banner of those who opposed La Place's nebular hypothesis. But before closing, I cannot forbear to speak of Thomas Henry Huxley, the great English apostle of evolution. He was not an astronomer in the narrow sense, but a physical philosopher of the highest type as a physicist; time forbids me to speak of his greatness as a scholar, scientist, and man of culture. He was, moreover,—what scientific men are not always, yea, indeed, very seldom are—a master hand in the use of the English language, a forceful platform speaker, gifted with an elegant diction. His thoroughness as a student is illustrated by one of his maxims: "Know a thing directly, and do not assume that you know more of it by knowing around it." But a strange sadness oppresses one reflecting upon his death, not only because he died, but because he died as he did. Those who wrote his epitaph understood him best, or at least thought they did, and this is what they wrote :—

> "And if there be no meeting past the grave,
> If all is silence, darkness, yet 'tis rest;
> Be not afraid ye waiting hearts that weep,
> For God still giveth His beloved sleep,
> And if an endless sleep He wills,—so best."

"And if an endless sleep," why "endless" ? Is this the end of all evolutionary philosophy? Does the summit of human glory and scholarly renown wide as the world, crash into such a pit of dark despair ? I stand appalled! Let me turn from this sad, sad dirge, to the glorious requiem sung by the last Laureate for himself :—

> "Sunset and evening star,
> And one clear call for me,
> And may there be no moaning of the bar
> When I put out to sea.
>
> Twilight and evening bell,
> And after that the dark,
> And may there be no sadness of farewell,
> When I embark.
>
> For though from out this bourne of time and place
> The flood may bear me far,
> I hope to see my Pilot face to face,
> When I have crost the bar."

And here is another yet clearer evangel :—

> Life is real, life is earnest,
> And the grave is not its goal;
> Dust thou art, to dust returnest,
> Was not spoken of the soul."

But let me not judge. I may not understand the epitaph, or the script on his tomb may belie the man. From a scientific point of view, it certainly is not evolution, for evolution is not endless, but glows from more to more. Can it be that it was an eclipse of faith, or an echo of that great soul cry that went forth centuries ago, "Though He slay me, yet will I trust in Him."

A list of the losses which the Royal Society of England has suffered by death during the past year shows that the Goddess of Science well guards her sons. The list comprehends nineteen Fellows and seven foreign members, and the average lifetime of these twenty-six members was over seventy-six years and a-half.

We are now in the early days of 1896 ; the future is rich in promise; it is already flushed with a coming glory; the golden age is not behind us, it is before us. To read the book and volume of the sky more correctly, and to see therein and on every page of it emblazoned the name of the Creator, yea, every letter of it a glistening sun, is the lesson for every astronomer, whether amateur or professional, to learn. Lelande impiously said: "I have swept the heavens and searched the universe, and found no God." Let every man search his own heart; if it is rightly attuned, it will respond to the thrilling chorus of the morning stars when they sang together and rejoiced. Nature never says one thing and wisdom another. The universe sings one universal psalm of praise, and the more we know of the universe the more clearly and harmoniously that psalm strikes our dull ears : —

> "The Sun and every vassal star,
> All space, beyond the soar of angel wings
> Wait on His word: and yet He stays His ear,
> For every sigh a contrite suppliant brings."

THE MEAFORD ASTRONOMICAL SOCIETY.

(INSTITUTED NOVEMBER, 1893.)

Affiliated with The Astronomical and Physical Society of Toronto.

Officers:

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Vice-President .... Mrs. H. Manley.
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Mrs. J. C. Saunders .... Ottawa.
Dr. M. A. Veeder .... Lyons, N.Y.

The Meaford Astronomical Society.

The Meaford Astronomical Society began from an informal gathering of friends interested in the study of astronomy at the house of Mrs. H. Manley the present Vice-President. This meeting was held on the 10th November, 1893. The better part of the evening was taken up in discussing the subject of astronomy, remarking on its gigantic outlines, etc. Officers were then chosen, and a rough programme of the future lines of study sketched in, the President volunteering to give a series of notes on the fundamental principles of the science. It may not be out of place to mention here that, during more than two years of this Society's life, with one exception, (caused by removal from the town), the officers remain unchanged, and that the interest of the members has increased instead of abating, and despite the inclemency of the weather last winter the meetings were well attended.

The members were encouraged to attempt writing short accounts of the planets and various heavenly bodies, these papers being read at the semi-monthly meetings. Others again undertook to collect newspaper clippings on the same interesting subject; several of the members kept a daily account of the temperature, rainfall (if any), noticed any unusual appearance in the heavens, such as meteors, northern lights, etc., thus creating great general interest from the records of their note books. Discussion was freely indulged in, numerous questions asked and answered by the President. These meetings proving so enjoyable, a good deal of curiosity (on what was an entirely new subject to most persons in this little town) was aroused; this led to the members being allowed to bring any friend or friends they chose. This privilege has never been abused, and the meetings though rather more formal than the first gathering have been without a single exception remarkable for their pleasantness and the very friendly feeling existing among the members.

The next step was to subscribe to some good serial, and on the 5th February it was decided to select *Popular Astronomy* for that purpose. This periodical has afforded so much satisfaction that its advent is eagerly looked forward to and it is carefully read and preserved.

On March 2nd, a short address was read to the President and a small telescope presented to him as a memento and token of the Society's appreciation of his kindness. It was also mooted for the first time that it might be possible to have our Society affiliated with the Astronomical and Physical Society of Toronto. This idea took root, and it was a proud day for us when the affiliation was a *fait accompli*.

The benefits arising from this affiliation have far exceeded our expectations, and to members of the Toronto Society we have been indebted for numerous kindnesses and many most interesting letters and papers; there is a general feeling of disappointment if we have no communications from them for several weeks.

On fine nights, when the sky was clear, the Society spent some time out of doors, and learned, under the direction of the President, the various constellations. The President having discontinued his notes, substituted a series of short lectures, frequently accompanied by charts illustrating the subject under discussion. These were found very interesting, particularly one on sun-spots, which excited a good deal of attention.

At a meeting on the 10th August, 1894, a Committee appointed at a former meeting brought forward a form of Constitution and By-laws, which are similar to those of the Toronto Society, with one or two modifications, and which were adopted and ordered to be printed.

An eclipse of the Moon being expected on September 14th, there was an informal gathering of members and friends on the wharf, where an excellent view of her Lunar Majesty was obtained. This was quite an event in the annals of the Society, and led to a great wish of the members to own a medium-sized telescope.

From time to time the Meaford members have taken advantage of their affiliation and visited the rooms of the Toronto Astronomical Society and have always been most kindly received. One of our members, Mr. S. D. Caswell, having removed to Toronto, often sends us accounts of the meetings in that city. These letters have been read with much pleasure. The Meaford Society resolved, on October 5th, 1894, to make a vigorous effort and raise funds to purchase a telescope. A committee with two ladies (and what can they not accomplish) was formed to interview the members, urging them to a special effort in that direction, and a fund was soon provided for the purpose. Through correspondence with Mr. A. Elvins, it was learned that object-glasses, by Vion Frères, of Paris, could be obtained from Messrs. Michael Bros., of Yonge street, Toronto. A selection was accordingly made and the lenses were mounted by Mr. J. R. Collins, of Toronto. About November 1st the telescope was received, and Mr. J. G. Sing, D.L.S., of Meaford, having kindly interested himself in preparing a tripod stand, the instrument was finally completed and ready for work. For almost two months afterwards, on account of cloudy or stormy weather, it was almost impossible to make any observations. However, during the winter, at intervals, observations were made and the telescope gave much interest and pleasure in revealing the system of Jupiter, features of the Moon, double stars, nebulæ, etc.

In December, Miss Paul kindly donated a copy of the *Marvels of Astronomy*. In January some correspondence was carried on regarding a suitable text book for beginners, and after some delay ten copies were ordered of Mrs. Giberne's *Sun, Moon and Stars*, which, being read from time to time at the meetings, led to discussions and gave greater interest to those who were new to the science. This text book is still in use, and is found most valuable because of its familiar illustrations rather than dry lists of statistics. The following papers have been prepared and read at our meetings :—" Saturn," by Mrs. Kirton; "Transits of Venus," with illustrations, by Mr. J. G. Sing, D.L.S.; "Spectrum Analysis" and "The New Element, Argon," by Mr. G. G. Albery. The President contributed several papers : "The Sun," "Sun-spots," "The Sun's Heat," "Meteors," "The Planet Mars," "Jupiter," "Astronomical Fictions," and "Retrograde Movements of the Planets," with diagrams.

Several papers by Dr. Veeder, of Lyons, N.Y., were read as received, and also some interesting papers from Mr. Thomas Lindsay, of Toronto, reviewing the astronomical work of the day, which were afterwards published in the Meaford *Mirror* under the head of "Scientific Notes." Our Society has been always kindly reported by the local press, and in the Meaford *Mirror*, besides the "Scientific Notes," published at considerable length, we have also been obliged by seeing the notices of our meetings, with date and place given. The meetings are held in the Town Clerk's office and in private residences alternately, invitations for the latter often coming in several weeks in advance. At the private residences our Society has always been made most welcome, and, after the business is completed, we have been most hospitably entertained and refreshments served, and the meetings have been of a social as well as of a scientific character.

One interesting meeting held at the Rectory, the residence of the President, will be one long to be remembered. It was held on June 7th, 1895, and when the members arrived the President had the telescope ready for observations. Attention was called to the remarkably favourable positions of the principal members of our solar system, and then one after another of the planets was carefully scrutinized, to the delight of all present. Mercury's crescent phase was distinctly seen, as well as that of Venus, in the western sky. Not far distant was observed the planet Mars. Jupiter and his moons were also visible, although too low in the West to be seen very favourably. And then, towards the South, we had the pleasure of seeing Saturn, with his rings glowing with yellow light. As these were observed for the first time by many of the members, one can well imagine the pleasure expressed at being at length able to verify what had been only heretofore seen in books on astronomy.

We are now entering on the third year of our organization as a society of amateurs in the science, and it may be truthfully said, that while some have become discouraged, and unable to read or keep up their attendance, the members generally are as enthusiastic as ever, and look forward to the coming year with the full purpose of pursuing the study of their favourite science with increased pleasure.