THE

ROYAL ASTRONOMICAL SOCIETY

OF

CANADA

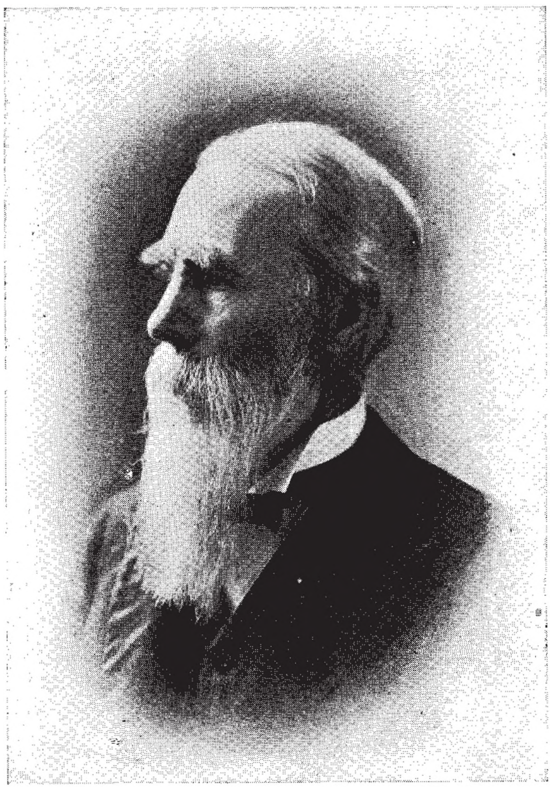
SELECTED PAPERS AND PROCEEDINGS

1904

EDITED BY C. A. CHANT.

TORONTO:
ROYAL ASTRONOMICAL PRINT,
1905.

The
Royal Astronomical Society
of Canada.



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ERRATA.

Page 19, footnote; for *Sept.* read *Aug.*
for *a mile and a half* read *half a mile.*
Page 29, line 9 from top; for 68·17 read 69·17.
" " 6 " bottom; for 64·34 read 65·34.
" 36, " 9 " " for *marking* read *masking*.

Any member whose name or address is incorrectly given will please report to the Secretary.

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PRESIDENT'S ADDRESS

AND

SUMMARY OF PROCEEDINGS

FOR 1904.

PRESIDENT'S ADDRESS,

JAN. 10, 1905.

ASTRONOMICAL AND ASTROPHYSICAL PROGRESS DURING 1904.

AFTER referring to changes in the list of officers; to the death of Mr. John Bertram, a distinguished Canadian citizen and a member of our Society; and to the loss sustained by the astronomical world in the death of Isaac Roberts, William Noble, Frank McClean and Theodor Bredichin*; a brief account of a few of the principal objects of research was given.

THE SUN.

The sun is our great source of light, heat and other forms of energy, and must ever be an object of profound study.

A close scrutiny of his face for spots and faculæ has been continued in observatories modest and pretentious all over the world, in order that these results may be correlated with other cosmic phenomena, such as rainfall, temperature, disturbances of the magnetic needle, etc.

In the "Monthly Notices" for November, which we have just received, is a long and valuable paper on "Magnetic Disturbances . . . . and their Association with Sun-spots", by Mr. E. Walter Maunder, of the Royal Observatory, Greenwich. In

* To these must be added the name of our distinguished Corresponding Fellow, M. Paul Henry, who died at Paris, on Jan. 3, 1905.

this paper the magnetic disturbances, 1882 to 1903, as recorded at Greenwich, are analyzed, and the manner of their occurrence compared with the record of sun-spots, faculæ and prominences. The author certainly demonstrates that the magnetic disturbances have their origin in the sun, the period of their recurrence being a synodic rotation of the sun. He points out, also, that the areas of the sun giving rise to our magnetic disturbances are definite and restricted, and that the energy radiates from these areas in definite, limited streams; that the region of the sun wherein these magnetically active areas are situated rotates with the speed of the chief spot-bearing zones, viz., latitudes 0° to 30°; and that these areas of the sun can be magnetically active before the visible formation of a spot-group, and can continue to be active after the spot-group has disappeared. In addition, he discusses the probable magnitude of the energy-streams and other minor matters.*

Though we recognize the value of Mr. Maunder's paper I must point out that in every important particular he has been anticipated by a member of our Society, Mr. Arthur Harvey. Indeed the connexion between sun-spots, auroræ and magnetic disturbances has been a favorite subject for discussion by our members from the very beginning of the Society, but Mr. Harvey has, with unwearied patience, searched through the records of our own Observatory, as well as those made at other points of the earth, and has published many papers on the subject. I might refer especially to one in the Trans. of Can. Inst., 1898–9, p. 345, and to another in our last volume of Transactions, p. 71.

Mr. Harvey has carefully plotted curves to show variation of horizontal force from the establishment of our observatory, and in the former paper he gives some of the curves; and referring to a magnetic storm which was very violent on the 22nd and 23rd of February, 1894, at which time a great sun-spot was central, he notes that there was a repetition of the disturbance on March 22, April 18, May 14, June 10 to 13,—at intervals corresponding to a rotation of the sun.

Mr. Harvey's remarks, (p. 349), are so explicit that I cannot do better than quote them:—

* See a portion of Mr. Maunder's paper at the end of this volume.

"The *cause* of the great spot was influencing the magnet every time its position faced the earth, long after the spot itself had been absorbed or filled up.

"Almost every magnetic storm, sun-spot-attended or not, repeats thus regularly, often to the day, for several months. It may be asked, why not invariably and to the precise hour? The disturbed area of the sun is usually so large, as we may fairly argue from the great size of the spots it causes, that parts of it face the earth for more than a day, and the eruption which causes the storm may be in a different part of that area at each successive rotation.

"Nor am I yet prepared to admit that the radiation of force is in a single direct line from sun to earth. It seems to be cone-shaped, driven outward from the sun, not drawn like a beam to the earth. And the intensity of the action may vary in the different parts of the conical pencil.

"By taking the average interval between magnetic storm repeats, the rotation of the sun ought to be accurately ascertainable, even better than by observing sun-spots, which are constantly changing their form and often their position, being so controlled by movements in the solar atmosphere that they go round in different times in different latitudes."

It will thus be seen that Mr. Harvey realized that the source of the disturbance was not located in the somewhat variable atmosphere of the sun, but below it in the more stable portion of his great body; and by going back to the early days of our observatory Mr. Harvey thought he could connect our present disturbances with those which had occurred 679 rotations earlier. In this way he deduced 27·24575 days as the synodic rotation of the sun.

In the paper which appears in the last volume of our Transactions the results are brought up to date, and diagrams are given to illustrate the various solar and terrestrial phenomena.

It must not be supposed that I in any way underrate Mr. Maunder's work, but in simple justice to ourselves I make these remarks. The work of the Italian spectroscopists on which he relies for much information have been regularly studied by us, and Mascari, Ricco and Father Fengi are names not unfamiliar to our meetings. The record of magnetic observations made at our Observatory is almost unrivalled, and though the results were formerly published somewhat irregularly, now the energetic Director has them promptly printed for public use.

Having the necessary data for such investigations directly at our disposal, we should be ashamed if we did not utilize them;

and our Society would have little reason for its existence if we did not reach sound conclusions.

But the most important investigations on the sun are now being made by means of an instrument known as the spectroheliograph. The object of this instrument is to allow a photograph of the sun to be taken by light belonging to some particular portion of the sun's spectrum, and it is believed that we thus obtain images of the sun at various depths of his outer envelope. The photographs of the sun's surface generally and of the spots on it are very striking and must lead to important results.

The spectroheliograph was first suggested by Janssen in 1869, but the first successful instrument was brought into use at the Kenwood Observatory by George E. Hale. Here it was used for three years, when Hale and his instruments were removed to the great Yerkes Observatory. At this place it has been greatly improved and adapted to the 40-inch refractor, and some wonderful pictures made by means of it have recently been published. Valuable results have also been obtained at other places, notably by Janssen and Deslandres at Paris, Lockyer at South Kensington, London; enough, indeed, to show that the spectroheliograph is a more powerful means of investigating solar phenomena than any heretofore devised. Through its revelations it is believed that we shall gain information on general physical problems, obtained from observing the behavior of matter in a monster laboratory whose conditions can never be duplicated on the earth.

Of course to secure these pictures it is necessary to have a clear atmosphere. Careful search has shown that a suitable place for such work is on the summit of Mt. Wilson, near Passadena, in California; and at the present time Dr. Hale, aided by a grant of $10,000 from the Carnegie Institution, is personally superintending the mounting of a new horizontal telescope, 140 feet in length, generously supplied by Miss Helen Snow, of Chicago. A large coelostat will direct the sunlight into this stationary telescope, to which will be attached a spectroheliograph of the latest type. It is proposed to continue the work at this station for at least a sun-spot period, *i.e.* 11 or 12 years, and it is confidently expected that the next decade will see unparalleled progress in the field of solar physics.

An extraordinary fact which has been established by records made by Dufour, of Lausanne; Kimball, Langley and Abbot, of Washington; Wolf of Heidelberg; and Gorczynski of Warsaw; is that there was an appreciable diminution of the transparency of the earth's atmosphere sometime during 1902, a normal state of affairs being reached again in 1903. Some have ascribed this effect to the dust projected into the atmosphere by Mont Pelée, but this view has not found wide acceptance.

On the 29th of August next there will be a total eclipse of the sun, the belt of totality running from James Bay, across Labrador, Spain, Tunis, Egypt and ending in Arabia. Now there are many problems,—such as the nature of the sun's corona, his action on the magnetic needle, the existence of new planets near the sun, etc., which can be best studied during an eclipse; and hence these occasions are considered very important by astronomers.

Acting on the view that Canada should not be behind in scientific matters, the Council of this Society, at a meeting held on Nov. 11 last, passed the following resolution:—

"In view of the fact that on August 30, 1905, there will be a total eclipse of the sun, first visible on the shore of James Bay; and that it is in the interests of physical and astronomical science that the phenomenon be observed as fully as possible and reported upon; and the further fact that already the Government of the United States, and the governing bodies of the Lick Observatory and the Carnegie Institution have determined to send parties of observers to different parts of Canada;

"Be it requested of the Government of Canada that steps be taken to organize an expedition, under its control, to proceed to the neighborhood of James Bay, the coast of Labrador, or other suitable place to observe and report upon this eclipse;

"And be it further requested that a limited number of members of the Royal Astronomical Society of Canada who are qualified observers shall be granted the privilege of accompanying the expedition, free of expense to themselves, the extension of such a privilege to a national astronomical society being entirely in accord with the custom which has obtained in all previous eclipse expeditions despatched by Great Britain and other countries to foreign parts."

This was at once transmitted to the Premier, Sir Wilfrid Laurier, who, in acknowledging the receipt of the communication, intimated that he had handed it to his colleague, the Hon. Mr. Sifton for his consideration. Apparently his report was in approval of the project, as in yesterday's newspapers we were informed that the Government would send out an expedition.

This announcement is highly pleasing to us; but knowing the enlightened views on scientific matters held by the Premier, the Minister of the Interior and other members of the Cabinet, and their anxiety that Canada should not be behind in the march of progress, we confidently expected that the expedition would be despatched.

The work will be under the direction of Dr. W. F. King, the Government's Chief Astronomer, and will be largely photographic. It is intended to use four cameras of focal lengths, 7, 10, 10 and 40 feet, respectively, to take photographs of the corona, as well as three spectrographs for photographs of flash and corona spectra. But besides this photographic work there will be eye-observations, and a careful attention to any variations in the magnetic elements.

We are all delighted that Canada has made a start in this important and advanced work.

THE MOON.

The magnificent photographs of the moon made at the Paris Observatory continue to come to us, while Ritchey of the Yerkes Observatory, with the 24-inch reflector made by himself, has given us some plates which for clearness of definition and wealth of detail have never been equalled. Ritchey has also ground a mirror 5 feet in diameter, and he is now assisting Dr. Hale on Mount Wilson.

But perhaps the most interesting and important publication has been Prof. W. H. Pickering's book on "The Moon". Finding the climate of Jamaica suitable for lunar work, he installed on the island on the side of a hill, a telescope 135 ft. long, having its axis parallel to that of the earth, and began his work. Dividing the surface of the moon into sixteen parts, easily remembered, he photographed each five times under different illuminations, and the result is 80 photographs forming the only complete atlas of the moon published. All the negatives of these photographs were secured in seven months.

Pickering has continued his lunar work at the Lowe Observatory in California, and it was at this place that his recently reported observations of variations in the lunar landscape were made. On July 31 he saw a small bright object on the floor of the crater Plato, which observations on six days just before this had not revealed. On Aug. 2 a black elliptical shadow two miles in diameter was seen in the place of the previously observed bright spot, whilst to the N.E. and N. there was found a new large white area. On Aug. 22 further conspicuous changes were reported ; while it was also seen that a white area, which had been observed and reported on years before, had disappeared.

In a recent issue of the Harvard Annals W. H. Pickering gives a detailed study of the crater Eratosthenes, in which a complete set of 12 photographs illustrate the changes which occur in the aspect of this lunar feature as the moon grows older. These changes he ascribes to ice, snow and vegetation. Similar results had been observed visually and reported in the same publication.

A report was received just a short time ago that at the Lick Observatory there had been observed a new crack, apparently a fissure in a river-bed in the Alps. We await with interest further details of this observation.

THE PLANETS.

SATURN.

For more than fifty years it was known that Saturn had eight moons , and on March 17, 1899, the announcement was sent out from Harvard Observatory that a ninth satellite had been discovered by W. H. Pickering ; but it was only recently that full information regarding the body was published.

The original discovery was made from photographs taken in Aug. 1898 by the 24-inch Bruce telescope ; and when a careful scrutiny of some plates taken at Arequipa in Peru failed to show the little image, it was wondered if there had not been a mistake after all. But the search was continued, and finally a set of photographs was secured which confirmed the original announcement and which allowed the accurate calculation of the elements of the moon's orbit.

The eccentricity of its orbit is high, the distance of the satellite from Saturn varying from 6,210,000 to 9,740,000 miles. The period is 546·5 days, just one day short of a year and a half. Its diameter is thought to be about 200 miles. Viewed from the earth it appears to be of about the 16th magnitude, and seen from Saturn it would be about the 6th.

Excluding comets it is the largest body discovered in the solar system since the inner moons of Uranus were found by Lassell in 1851. It is also the faintest object.

But the greatest surprise is in the fact that the motion is retrograde, while all the other eight moons revolve in the direct sense. The satellites of Uranus and Neptune also move in the retrograde manner, but (with the possible exception of Jupiter, to which reference will presently be made), the system of Saturn is the only example of a celestial family being divided against itself. It is surmised by some that this newcomer did not originally belong to the system, but is some extraneous body such as a comet, captured by the superior attractions of Saturn and held in bondage as a satellite.

The name Phœbe has been given to the satellite.

MARS.

The discussion regarding the form of the markings on Mars and their physical significance goes merrily on, Percival Lowell being the leading speaker. This indefatigable observer, on carefully analyzing several hundred drawings which he had made, concluded that there was a complete network of canals over the planet's surface. He argues that the normal blue-green color is due to vegetation, which, owing to the absence of large bodies of water on the planet can thrive only when fed by the water which fills the canals at the melting of the polar snows. He also suggests that the brown color, which accompanies the minimum visibility of the canals, is due to the exposure of the bare soil which probably covers the beds of such "seas" as the Mare Erythræum. But the small amount of water on Mars would necessitate prodigious irrigation schemes, for which, however, Mr. Lowell considers the Martians quite capable.

Antoniadi, Maunder and others consider the "doubling" of the lines, and the canals so indicated, and perhaps some of the lines themselves, as the physiological effect of contrast.

Undoubtedly many of the appearances are optical illusions, yet there is sufficient agreement amongst observers for us to conclude that the streaked and striated marking of the northern hemisphere is an objective reality. Denning believes that there are real variations on Mars, due probably to atmospheric causes; and that the dark markings have a natural rather than an artificial origin.

W. H. Pickering agrees with Lowell as to the vegetation, but cannot accept the irrigation proposals. He thinks the canals, both on the moon and on Mars are of volcanic origin, the cracking of the crust being due to internal stresses. From the fissures so caused the water-vapor and the carbon dioxide issue directly, and these along with the sunlight produce the vegetation.

VENUS.

Lowell has also made a close study of Venus, and he concludes that there are real markings on this planet, but not of the canal form. They consist of irregular interlacing lines.

He gives the rotation period as 225 days.

JUPITER.

The planet Jupiter, one of the finest objects, especially for modest telescopes, though closely watched, still continues much of an enigma.

Stanley Williams reports that the red color has almost entirely disappeared from the southern belt, some parts actually appearing blue (except in the neighborhood immediately following the Great Red Spot); while the northern belt is of a bright, deep red color.

Denning finds that the rotation, as determined by the Great Red Spot, shows peculiar variations. It exhibited a slackening motion during the years 1878–1900, was accelerated in 1902, retarded in 1903 and is now accelerated again. The variation in the period of rotation (which has a mean value of about 9^h 55^m 40^s,) is less than two seconds, but that is large enough to be easily detected by astronomers. The same observer found that other portions of the planet show rotation periods smaller than than that of the Great Red Spot by as much as 20 sec.; but for all these irregularities no explanation is forthcoming.

But the newest surprise comes in the shape of a report, which appeared in the daily papers of Saturday last (Jan. 7), that a *sixth* moon to Jupiter has been discovered at Lick Observatory by Perrine. This moon is far distant from its primary, and its motion is believed to be retrograde.

THE STARS.

Burnham has continued his work on double-stars, and while measuring pairs previously known, has discovered 18 new ones. This brings the total number found by him up to 1308.

A very interesting study of the secular variation of starlight has been made by Gore, one of our Corresponding Fellows. He has compared the present magnitude of a number of stars with their respective magnitudes as recorded by Al-Sufi and Ptolemy. He gives two lists. In the first are 26 stars whose magnitudes have apparently increased; in the second, 20 stars with apparently decreasing magnitude. In many cases those stars which have decreased in magnitude have spectra which indicate that the temperature is diminishing. For instance the star β Leonis was of the first magnitude in Al-Sufi's time, but now its magnitude is 2·2.

An interesting computation of the number of the stars has been made by Gavin J. Burns. Assuming that the stars are evenly distributed, and using the plates for the Greenwich Zone of the Astrographic Chart, he deduces that there are:

38	stars	brighter	than	the	second	magnitude;
13,421	"	"	"	"	seventh	"
8,325,000	"	"	"	"	fifteenth	"

He finds also that the stars thin out as their distances from the solar system increase.

COMETS.

On April 16, Brooks of Geneva, N. Y., announced the discovery of a new comet. Then there was a decided scarcity of these strange objects, but the year ended well. Encke's comet, which has a period of 1206·8 days, appeared at its proper time and was seen by many observers. It was nearest the earth on Nov. 23 and was brightest on Dec. 26, but as the moon was very bright at this time it could not be seen by the eye. Tempel's second comet was re-discovered by St. Javelle at Nice on Nov. 30 ; Giacobini at Nice discovered another on Dec. 17 ; and I see by the papers that Borrelley of Marseilles has discovered still another.

THE YEAR'S WORK OF THE SOCIETY.

During the year there were twenty-four meetings of the Society and the subjects discussed ranged over a wide field.

One of the features of the work was the course of four public lectures given during the month of March. Professor DeLury delivered three lectures on "The Rise and Progress of Physical Astronomy", and Prof. John Watson of Queen's University gave an able paper on "The Relation of Philosophy to Ancient and Modern Theories of Cosmogony". An extended digest of Dr. Watson's lecture will be found in our Transactions.

The paper by Dr. C. I. Kelly, of the Hamilton Association, on "Electricity and Magnetism" was unfortunately accompanied by an exceptionally heavy snow-storm ; but the paper was heard with great interest, and the experiments shown in illustration were very suggestive and some of them very beautiful.

The simply-constructed sun-dial which Mr. J. E. Maybee exhibited and the paper he read were highly appreciated, and excited interest and inquiries beyond the Society.

Another enjoyable part of the work was the papers by Mr. D. J. Howell on Lunar Photography, and the exhibition on two evenings of exceptionally fine lantern slides which he had prepared from the latest plates made at the Paris and Yerkes Observatories. Mr. Howell contributes a short paper to the Transactions.

One of the largest meetings of the year was that of Oct. 4, on which occasion the two meteorites which fell at Shelburne on the evening of Aug. 13, were on exhibition. Prof. DeLury dwelt briefly on the astronomical aspect of these strange bodies, and Prof. T. L. Walker gave an excellent statement of the mineralological side of the subject. The smaller meteorite, 12½ lbs. in weight, is still, I believe, in the possession of Mr. John Shields, by whose house it fell. The other specimen, 28 lbs. in weight, is now at the School of Mines, Kingston, where it has been the subject of a minute investigation by Dr. L. H. Borgström, who furnishes us with a full account of it. In recognition of assistance rendered by our Society, the School of Mines has presented us with a full-size model of the meteorite. This model is on exhibition this evening.

Prof. Baker's lecture on the "Beginnings of Astronomy" was illustrated by numerous appropriate charts; and Mr. John A. Paterson treated the Society to a lucid "Chalk-talk" on some of the fundamental laws of astronomy.

In his paper on "Stellar Motions" Mr. A. F. Miller continued his treatment of a subject which he has made his own, and on which we would be glad to hear him again.

Another useful and very interesting paper was that by Miss Dent, giving brief biographies of our Honorary and Corresponding Fellows. The subject was too wide for proper treatment in one evening and we hope to have it continued in the near future.

The recently-advanced theory of the formation of the universe known as the Planetesimal Theory was explained by Prof. A. P. Coleman. In this hypothesis the requirements of geological time and stratification are met satisfactorily, and in many other respects the theory is quite as acceptable as the nebular hypothesis. A brief statement of the theory appears in the Transactions.

Another paper which led to some discussion was the review by the Secretary of Wallace's book, "Man's Place in the Universe". Mr. Collins has had interesting correspondence with the distinguished author, and in a brief paper presents his views on the subject.

A review of some of the latest results in astrophysical work was presented by Vice-president W. B. Musson, and a condensed account will be found in the Transactions.

Mr. J. Miller Barr, of St. Catharines, has communicated to the Society some very interesting observations of variable stars. Mr. Barr uses only an ordinary field-glass, but by skilfully choosing his stars he has secured some really important results. We congratulate him on his success, and we are glad to publish his work.

Mr. W. H. S. Monck, of Dublin, Ireland, has continued his studies of meteorites, and we publish a paper by him in which he endeavors to show that there is a periodicity in their falls.

During the year we received some communications from Professor A. W. Bickerton, Christchurch, New Zealand, dealing with the theory of stellar impact, and we publish a short statement of the subject by him.

At the last meeting of the year your President gave the results of an investigation into the reflecting power of glass and some mirrors. This paper appears in the Transactions, and the editor of the Astrophysical Journal has expressed a desire to publish it in the next number.

But though our programmes have been interesting, the attendance at some of the meetings has not been as large as we would wish. This has been in part due to the loss of some of our most active and valued members by death or removal, and the inability of others, through advancing age, to attend the evening meetings. I would appeal to those present, who are not members, to unite with us and help on the cause we have at heart.

In conclusion I wish to refer to another matter of interest to us all. A proposition is on foot which has for its object to extend and popularise the study of astronomy at the University, and it is hoped that an arrangement between the University and the Royal Astronomical Society of Canada will be reached, by which the Society will be given accommodation for our meetings and our library, the University to receive in return the use of the library and of our instruments. Every one I have spoken to about the matter has expressed hearty approval of the proposal. At present fuller details cannot be given, but I venture to believe that an arrangement will be made which will be of great advantage to the Society, to the University and to the people generally.

PAPERS AND LECTURES, 1904.

Jan. 12.—Society's "At Home"—Retiring President's Address on "Astronomy and Physics of 1903."

Jan. 19—"The Beginnings of Astronomy." Prof. Alfred Baker, M.A.

Feb. 2—"Electricity and Magnetism" Dr. C. I. Kelly, Hamilton.

Feb. 16—Astronomical Chalk Talk—Stars' Apparent Motion—Day and Night—Latitude and Longitude—Kepler's Laws—Bode's Law—Calculation of a Planet's Distance—Newton's Illustration of Law of Gravity from the Moon's Motion. John A. Paterson, M.A., K.C.

Mar. 1—"The Sun-dial and its Lessons—How to Construct and Use One." J. E. Maybee, M.E.

THREE LECTURES ON "THE RISE AND PROGRESS OF PHYSICAL ASTRONOMY" BY PROF. A. T. DE LURY, M.A.

Mar. 15—I. "The Work of Newton."

" 22—II. "The Sequel to Newton's Discoveries."

" 29—III. "Speculations on the Evolution of Solar and Other Stellar Systems."

March 18—"The Relation of Philosophy to Ancient and Modern Theories of Cosmogony." Prof. John Watson, M.A., LL.D., Kingston.

April 5—"The Planetesimal Hypothesis." Prof. A. P. Coleman, Ph.D.

April 19—"Stellar Motions." A. F. Miller.

May 3—"Man's Place in the Universe,—a Review of Alfred Russell Wallace's Recent Book." J. R. Collins.

May 17—"Solar Activity." Prof. Louis León, Mexico City, Mexico.

May 31—"The Paris Lunar Photographs," with Lantern Slides from the latest Plates. D. J. Howell.

June 14—"Some Late Results in Astrophysical Research." W. Balfour Musson.

June 28—Evening at the Observatory.

Sept. 20—Review of Summer's Work and General Discussion.

Oct. 4—"The Shelburne Meteorites." Prof. A. T. De Lury; Prof. T. L. Walker, Ph. D.

Oct. 18—"Review of Some Recent Observations of the Surface Markings of Mars and other Planets." J. R. Collins.

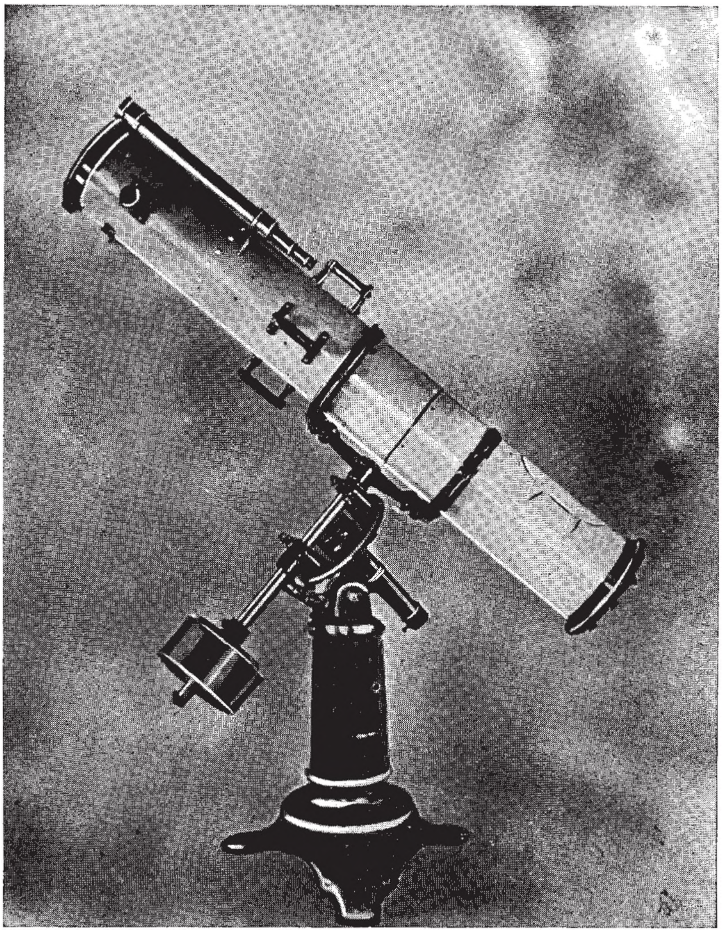
Nov. 1—"The Diffraction Spectrum" (with experiments.) C. A. Chant.

Nov. 15—"Eclipses." Prof. DeLury.

Nov. 29—"Biographical Sketches of our Honorary and Corresponding Fellows." Miss Elsie A. Dent.

Dec. 13.—"Recent Lunar Photography." D. J. Howell.

Dec. 27—"Some Recent Experiments with Reflected Light." C. A. Chant.



8½ INCH BRASHEAR REFLECTOR,
PRESENTED TO THE SOCIETY BY WESTON WETHERBEE, F.R.A.S.C.,
ALBION, N.Y., U.S.A., 1904.

THE

ROYAL ASTRONOMICAL SOCIETY

OF

CANADA.

SELECTED PAPERS.

THE RELATION OF PHILOSOPHY TO ANCIENT AND MODERN COSMOGONIES.

BY

JOHN WATSON, M.A., LL.D.,
PROFESSOR OF PHILOSOPHY, QUEEN'S UNIVERSITY, KINGSTON, ONT.

THE Greeks were the first people who rose above a half-imaginative view of things and constructed a philosophy. At first there was no explicit distinction between philosophy and cosmogony. Thales, the first Greek philosopher, inferred that the moon received her light from the sun and that the earth was spherical, and he is credited with having predicted an eclipse, but he lives in the memory of men mainly because he was the first to raise the question as to the principle which unites all that is into the unity of a single whole. It was, however, Xenophanes who clearly opposed the unity of the cosmos to its diversity. Though he made no contribution to astronomy, he prepared the way for a scientific view of the world by making a clean sweep of the prevalent mythological conceptions. We have in him an instance of the general relation of philosophy and science; for while the former has always refused to admit that the visible universe is all, both have combined against the uncritical assumptions of common sense and of traditional beliefs. It was, however, the Aristotelian conception of the universe and the cosmical views of Eudoxus, as modified by his successors and formulated by Ptolemy, that ruled the minds of men for sixteen centuries; and therefore in considering the relation of Aristotle and Eudoxus we are in fact dealing with the fundamental contrast between ancient and modern thought.

It was assumed by Eudoxus that the whole of the heavenly bodies revolved around the earth in concentric spheres. Granting this assumption the obvious difficulty was to account for the apparently irregular movements and velocities of the planets. These movements, as it had been assumed, were due to the activity of the outermost sphere in which the stars were fixed, and therefore their velocity must increase in regular proportion the further they are away from the centre. Moreover the planets must, on this hypothesis, have a perfectly regular motion, and ought therefore to preserve the same position relatively to the stars. To account for the difference in the velocity and path of the planets, Eudoxus had recourse to the supposition that in addition to the motion from East to West, there was a second motion from West to East in a direction which is indicated by a great circle passing through the middle of the Zodiac. Finding that the phenomena could not be explained even by these two movements, Eudoxus added a third sphere, which also revolved from West to East and passed obliquely through the breadth of the Zodiac. In this way he seemed to account for the movements of the sun and moon, but he was forced to assume for the other five planets—Mercury, Venus, Mars, Jupiter and Saturn—a fourth sphere which revolved from East to West in a circle inclined obliquely to the centre of the third sphere. Even then the facts were not fully explained, and Callippus, a disciple of Eudoxus, found it necessary to assume five spheres for the sun and moon, five for Venus and Mars, and four for Jupiter and Saturn, making in all thirty-three spheres.

The cosmogony of Eudoxus, as modified by Callippus, was accepted in principle by Aristotle with an unimportant change. In an early work George Henry Lewes argued from the astronomical mistakes of Aristotle that philosophy has never done anything to enrich the sum of human knowledge. But it was not metaphysical speculation which led to the mistakes of Aristotle but loyalty to the facts as far as these were known. In his logical treatises Aristotle assigns the main place in the discovery of truth to observation and induction, and he is not to be condemned because his mediæval followers, instead of being loyal to his method, were blind followers of his results. Another reason for Aristotle's acceptance of the astronomical views of Eudoxus was their agreement with the general principles of his philosophy. Inadequate as that philosophy necessarily was, it had one signal merit which is not found in the same degree in Plato: it showed the utmost respect for the facts of experience, and was thus to a great extent saved from the dangers of a too rapid synthesis. Aristotle insists that each department of knowledge must be carefully delimited and its appropriate principle applied to it. The ultimate aim of philosophy is the reduction of all orders of existence to system, but this system must be reached through the patient accumulation and interpretation of all the facts. In the attempt to construct such a system Aristotle employs the conception of organic development, in the sense that he found in all living beings a purposive activity analogous to the conscious purpose exhibited by man. For him the soul is not something which can exist apart from the body, but simply the principle of unity implied in the living being as a whole. This view leads Aristotle to see in the various orders of existence, beginning with the plant, passing on to the animal and becoming explicit in man, the effort of nature to secure a perfect form of being, although this effort is never completely realized. It is at this point that Aristotle sees in the cosmogony of his age a confirmation of his general conception of the universe. The conditions of life on the earth make the realization of perfection impossible, for nothing on this earth is eternal, the nearest approach to it being found in the perpetuity of the species. The reason of this incapacity in each finite thing to realize the end after which all are striving is due, Aristotle thinks, to its material substrate, which prevents the end from being completely realized, and compels the being to pass through a perpetual process of change. Hence Aristotle conceives of the Divine Being as free from all the limitations of matter, and even of practical activity. These considerations explain why Aristotle was led to solve particular questions in the way he did. Eudoxus had not asked whether the cosmos began to be or existed from all eternity. Aristotle raises the problem, and in contrast to the earlier philosophers he maintains that our cosmical system is eternal, having never come into being and being destined to continue forever in its present form. To this result he seems to have been led partly by his belief in the immutability of the æther composing the heavenly spheres, and partly by his conception of God as absolutely unchangeable in his nature. In his view the stars are God-like, because they present what in Plato's language may be called "a moving image of eternity." In contrast to the regular and unceasing motion of the heavens stands the sublunary region, which is characterized by incessant conflict and change. Here rectilinear motion is the rule, proceeding from the central point of the world outward and upward, or inward and downward towards the central point. These movements Aristotle, in common with his age, conceived of as properties belonging to the primary elements, and therefore he maintained that it was the nature of earth to strive after its appointed place in the centre of the universe, while fire ever strives aloft, and water and air seek to occupy the intermediate positions.

The Aristotelian philosophy gives a symmetrical and compact view of a limited universe, closed in by the sphere of the fixed stars beyond which there is nothing. This was the view which prevailed all through the middle ages. Why it should have done so it is not difficult to understand. It could apparently appeal to the testimony of facts; it agreed with the biblical cosmology; it cohered with the tendency of thought in all ages to represent the Divine Being as raised above the world with its perpetual birth and decay, its contest of evil and good, its strange mixture of beauty and ugliness. Moreover, the middle ages was a period when the barbarism of the Teutonic and the worldliness of the Latin races had to be modified by the higher impulse of religious faith, and it was therefore only natural that the antagonism between the secular and the sacred should be overemphasized. The very fact that the main interests of men were practical and religious tended to restrain the freedom of scientific inquiry, and in the absence of the spontaneous movement of thought it was natural to fall back on the authority of Aristotle, whose cosmology was easily fitted in to the dogmatic creed of the Church. Thus the philosopher who had most strongly insisted upon loyalty to facts was invoked in support of a system which turned away from facts and sheltered itself behind an external authority. Yet the scholastic philosophy, by accustoming the mind to see the objections which might be raised to what at first sight seemed beyond doubt, prepared the way for the overthrow of that sensuous view of the cosmos on which ancient and mediæval astronomy was based. And men's minds were more ready to accept the new cosmogony because such thinkers as Nicolaus Cusanus and Bernardino Telesio had already suggested doubts of the traditional view on general philosophical grounds. The former denied that the earth is the central point of the universe, or that it is at rest; the latter made a direct appeal to experience, maintaining that the quantity of matter is never increased or diminished and that there is no distinction between heavenly and terrestrial matter. By the labours of these two thinkers and others, the traditionary conception of the world was shaken, but it was only by the promulgation of the new cosmogony of Copernicus that it was completely overthrown.

The modern cosmogony rests upon a mechanical view of the world, and no philosophy can possibly obtain acceptance which ignores this fact. But if the whole cosmos rests upon a mechanical basis, what are we to say of those higher interests which concern us as men? If man like other beings is under the dominion of inviolable law, what becomes of his freedom and moral responsibility? All the great modern philosophers from Descartes downwards have found themselves compelled to face this problem, but it is in the philosophy of Kant that we first find it stated in all its force and clearness. The new cosmogony so entirely revolutionized men's ideas that every one of the propositions advanced by Aristotle and accepted for centuries was reversed. Our solar system cannot be regarded as having existed in its present form from all eternity, if with Kant we accept the nebular theory. The earth is not the central point of the universe, but a small and insignificant planet revolving around the sun, itself only a star of the fifth magnitude. There is no bounded sphere enclosing the world within fixed and narrow limits, but an illimitable space with world stretching beyond world and system blending into system. The perfect circular movement is a fiction, and the stars are composed of no ethereal element but of the same or similar constituents as those found in our earth, nor is there any more reason for calling them divine than for calling a stone divine. But if our earth is an infinitesimal speck in the universe, can we any longer attribute to man that superiority which the Hebrew psalmist expressed by saying that he "is made a little lower than God?" If all finite beings are continually losing their individuality, must we not conclude that the immortality of man is a fairy dream? Nay, is there any longer reason to suppose that the existence of God will stand the shock of modern mechanical explanations? Thus it would seem that the new conception of the universe, if it has, in Mr. Balfour's phrase, "glutted our imagination with material infinities", has at the same time given a tremendous shock to our religious beliefs.

It seems to me that the only adequate answer to these questions must follow the general lines indicated by Kant, though his answer cannot be regarded as entirely adequate. Kant accepted the new cosmogony in its entirety, and even made a further contribution to it by propounding the nebular hypothesis. He refuses to admit that there is any break in the continuity of natural processes. But, if all other beings are under the dominion of natural law, is it not an arbitrary proceeding to exempt man from law as if he were a sort of *lusus naturæ?* On the other hand, granting man to be under the dominion of natural law, how can we suppose that his acts proceed from himself in any other sense than that in which we speak of the movements of an animal or even the fall of a stone as spontaneous? Further, if there is no law but natural causation, i. e., if the whole sphere of reality is limited to particular phenomena and their connection with one another, we must conclude that the ideas of God or any other supersensible being, as well as the belief in immortality, are fictions. But, when by "victorious analysis" you have got rid of freedom, immortality and God, you will find that you have also to abolish the conceptions of duty, morality and responsibility.

Now, this apparent opposition between necessity and freedom has sometimes been sought to be solved by making the things of nature absolutely different in kind from spiritual beings. Thus it may be said that, while inorganic things and even the highest of animals, are absolutely subject to the law of mechanical causation, all their movements taking place solely in response to the action upon them of the environment, man on the other hand, as a spiritual, self-conscious, moral being, is the originator of his own acts. This solution Kant was unable to accept. It seemed to him that we cannot thus remove man from the sphere in which mechanical causation rules. When a man, e. g., seeks to satisfy any desire, is it not the case that that desire is excited by some object acting upon him? Given the man's natural susceptibility to certain objects rather than others, and the response which he makes when placed mentally in the presence of a certain object is just as fixed as the movement of a stone under the influence of external forces.

Is morality, then, a dream, or is it possible to defend at once the inviolability of natural law and the absolute obligation of morality? Kant answers that they can be reconciled, if we only reflect upon the meaning of natural law. In exact opposition to Aristotle he maintains that it is the will or practical reason which alone can be regarded as presenting a law for all rational beings, while natural law is merely the manner in which by the necessary character of our intellectual faculties we construct for ourselves a system of experience. But this system is never a completely rounded whole, and breaks down in contradiction the moment we assume it to be a determination of ultimate reality. If we are to preserve the unity of intelligence with itself, we must recognize that the world of experience—the system of sensible objects, to which we apply the principle of actual causation—is but an analogue of that ultimate reality which escapes from the frame-work within which our understanding seeks to confine it. Kant, it is to be observed, does not, like agnostic thinkers, maintain the impotence of reason to comprehend reality: what he argues is, that reason in its theoretical use is not supplied with the data necessary for an ultimate view of things, being tied down to sensible objects as presented in space and time. The ideas of reason are always larger than the sensible experience that constitutes our knowledge, and therefore the intellect can never pronounce against the reality of the supersensible. But it is different with the practical reason, which issues a moral law that admits of no limitation and demands that all rational beings should conform to it. Now, morality is impossible without freedom, and therefore we must refer our actions to ourselves as self-determining beings. There is no real contradiction between the inviolability of natural law and the absolute obligation of moral law : for, though our actions really proceed from ourselves, we have to represent them, so far as they fall within the phenomenal world of experience, as occurring in accordance with natural law.

In essence this doctrine of Kant means that in the self-conscious life of man, and above all in his moral life, we have the highest, and indeed the only real, revelation of the ultimate nature of things. Man, who physically is but a small and insignificant object, hardly visible in the immensity of the spatial universe, yet bears within him the consciousness of the ultimate principle of all things. If morality is the true nature of things, the universe, though it can never fall within the compass of our knowledge, *must* be such that the ideas of reason are capable of being realized ; and as such a universe is, in Kant's view, impossible without the immortality of the soul and the existence of an infinitely perfect God, the moral law carries with it the reality of these two ideas. God cannot be an object of knowledge, because he transcends the limits of space and time, but we have a rational faith in Him, since, if he does not exist, morality, which is bound up with our very nature as self-conscious beings, would be a fiction.

You will not expect me to do more than indicate what I regard as the truth, and what the inadequacy, of this noble philosophy. Its truth seems to me to lie in this, that the universe can reveal itself only to a rational being who, weak and limited as he is, yet contains within himself the principle of the whole. Its inadequacy lies in the assumption that the world of experience is at best a symbol, and an unreal symbol of reality as it truly is, and therefore so far from revealing, hides reality from us by an impenetrable veil. We may, and indeed we must, distinguish between the world as imperfectly conceived and the world as more adequately interpreted, but to speak of science as dealing only with appearances, and morality with the world of real being, is to do justice to neither. The conception of the cosmos as an assemblage of objects, rigidly bound together by mechanical law, is the first condition of a systematic view of things; but if it is supposed that this is the last word, we fall into the grievous mistake of taking the part for the whole. Not to mention that such a doctrine leads to the denial of all art, by abolishing its very source, the ideal interpretation of existence, it ultimately destroys, as Kant says, all the higher interests of man. Why should it be assumed that knowable reality is bounded by that which can be described in mechanical terms? If, as all modern philosophy assumes, knowledge must be an interpretation of experience, surely the experience we interpret must be taken in its totality, not arbitrarily limited to one aspect of it! Now, recent science has been forced to go beyond the Aristotelian view, that the conceptions of organism and evolution are limited to any special sphere, and above all to the transitory life of individuals. The facts of experience have compelled us to conceive of all orders of existence as bound together within a single system, which has developed, so to speak, entirely from within. We have, indeed, been forced to discard the fiction of an arbitrary creation of the world, and an arbitrary interference with it after its creation, but this has only revealed to us all the more clearly its all-pervading system and rationality. And if the universe, as we must believe, is rational through and through, there can be no absolute division between nature and spirit, any more than there can be any real antagonism between science and philosophy; on the contrary, just as in nature, there are indications of a tendency towards an ideal end, which is continued in the efforts of man to realize an absolute good, so the ordered system of the cosmos revealed by science is but the less explicit form of that spiritual unity which it is the work of philosophy to detect and articulate. We must, then, insist upon the equal importance of the work of science and the work of philosophy. Without the careful and laborious efforts of science, our modern cosmogony would have remained a thing of vague guesses and unverified hypotheses, each giving way to a new guess and a new hypothesis; and without the complementary work of philosophy, the higher interests of man, and the systematic unity of the whole, would have fallen into irretrievable confusion. By the co-operation of both it is possible, as I believe, to find satisfaction at once for the intellect and the heart: to bring to science the reverential feeling of one who is tracing out the ordered system of a rational principle, and to infuse into philosophy that scrupulous regard for facts without which it becomes the plaything of fancy or the arbitrary construction of a mind that refuses to be loyal to truth, and imagines that all theories are equally trne, and equally false ; in other words, that truth is a fiction woven from the groundless hopes of men. It is difficult to understand how any one, who has given the least attention to the immense progress from the ancient to the modern conception of the cosmos, can continue to say that nothing has been done to disclose "the open secret" of the universe ; and similarly, he has followed the history of philosophy to little purpose, who is not constrained to acknowledge that the reflective thought of man has not been in vain, but has afforded more and more a rational ground for regarding the universe in which we live, and the spiritual interests of men, as the ever-clearer revelation of that Divine Reason, which is the eternal and infinite principle of all that is. and has been, and will be.

FURTHER CONSIDERATIONS ON AEROLITES.

BY

W. H. S. MONCK, F.R.A.S., DUBLIN, IRELAND.

THE aerolites in my catalogue (see last vol.) were not numbered because I desired to leave space for the addition of new ones. The numbers in the principal catalogue (on which I chiefly rely) may now be given. They are

January, 21;	April, 28;	July, 23;	October, 21;
February, 24;	May, 43;	August, 31;	November, 25;
March, 20;	June, 39;	September, 30;	December, 22.

A mere glance at these figures shows that the distribution is not uniform, the preponderance in May and June being very marked. No doubt there appears to be a preponderance of falls in summer when compared with winter which might be explained on the principles suggested by Mr. Harvey, but no such explanation is possible of 43 falls in May and only 23 in July. In the two months of May and June—61 days— there are 82 falls. In the rest of the year, 304 days, there are 246. Roughly speaking the number *per diem* for May and June is 1.34, while for the rest of the year it is only 0.81. Moreover instead of distinguishing between Summer and Winter I am inclined to adopt a first maximum in May and June and a second in August and September. In these latter months the average fall is one aerolite *per diem*. It is however perhaps more material to observe that the maximum aerolite-fall does not coincide with the maximum of meteor-showers. Indeed as regards the principal maximum the reversal is complete. May and June are the two months in which the smallest number of shooting-stars have been observed and recorded. It would be too hasty to conclude that there is any antagonism between the two kinds of phenomena, but the figures seem to establish their complete independence. August, in which more shooting-stars have been observed than in any other month, no doubt stands high in the aerolite catalogue, but not higher than September and hardly higher than April, in neither of which months is there any unusual display of shooting-stars. In November—the month of the Leonids and Andromedids—we have only the average amount of aerolite-falls.

Mr. Harvey examines the arguments of Dr. Bornitz in favor of his theory that all the great shooting-star showers produce aerolites, and arrives at a contrary conclusion with, I think, good reason. But Bornitz's catalogue is by no means a reliable one and I think the mere substitution of a more reliable catalogue (which I hope mine is) will suffice to displace his theory. Taking the Perseid radiant, we have one aerolite-fall on August 8, one on August 10, three on August 11, one on August 12, and none on August 9 or August 13. For the six days, August 8–13 inclusive, we have thus seven falls, the average for the month being one fall *per diem*. For the Leonids we have one aerolite-fall on Nov. 10, another on Nov. 11, two on Nov. 12, two on Nov. 15 and one on Nov. 16—in all seven falls: but the last of these took place in A. D. 1492, when the Leonid display occurred at a considerably earlier date, and omitting it we have six falls in seven years which is just equal to the monthly average. In the eight days, Nov. 23-30, both inclusive, there were ten aerolite falls which is no doubt above the monthly average, but three of these occurred when the Andromedid shower must have occurred at a later date. I need not here enter into the question of the Mazapil aerolite of Nov. 27, 1885, but I may remark that the position which these Andromedids occupy is a peculiar one. I hold that the reason why aerolites fall in the solid form to the earth is because they entered the atmosphere with a slow relative velocity, which implies that they were moving round the sun in direct orbits whose planes were inclined at a small angle to the ecliptic (a result previously arrived at by the late Prof. H. A. Newton). But the comet of Biela, whose track these shooting-stars follow, moves round the sun with direct motion in an orbit inclined at rather a small angle to the ecliptic, though owing to the eccentricity of its orbit the relative velocity amounts to about 14 miles per second. Is 14 miles per second so great a velocity that it is impossible for an aerolite travelling at that speed to reach the earth in the solid form? And when the comet burst into fragments, may not the motion of these fragments have been so retarded that when they reached the atmosphere their velocity may have been less than 10 miles per second? I neither affirm nor deny. I do not think a Perseid or a Leonid (unless the mass were enormous) could reach the earth as a stone. The heat generated by its velocity would be certain to dissipate it. But as regards an Andromedid I should be slow to give a positive opinion. My catalogue affords no argument in favor of aerolite-falls connected with the Lyrids—rather the reverse. I may add that in the case of showers like the Leonids and the Andromedids, which are not perennial but recur at certain intervals, I find no trace of the same periodicity in the aerolite falls about the same date.

Ordinary shooting-star showers may be divided into two classes, perennial and periodic. But there is no clearly marked line of distinction between them. Some writers have attempted to assign a period to the Perseid shower which produces hundreds of meteors every year, and on the other hand a few meteors from the Leonid and Andromedid radiants have been noticed in years far remote from the date of the periodic display. But periodic displays are seldom limited to a single year. Even with the Leonids the shower is much more conspicuous than usual for some time after (if not before) the grand display. There were good displays in 1867 and 1868, following the grand one in 1866. If there is a good display in one year, we are apt to find the number also over the average for two or three years afterwards. Again, meteor showers are never confined to a single night. They last for several days—on the average I think for at least a week—though there may be only one night at which they reach their maximum.

Bearing in mind these characteristics of shooting-star showers, I ask whether there is any indication that aerolites occur in showers which exhibit similar characteristics? I think there is. It is of course a question whether the coincidences are more numerous than chance will account for, and this is sometimes a mathematical question of considerable difficulty. But I think the coincidences which I am about to mention are too numerous to be explained as casual.

I. Aerolites which fell in the same year within a few days of each other.—I should refer in the first place to falls to which Mr. Harvey has called attention, in which hundreds or even thousands of stones fell at the same time, though there seems no reason to regard them as disrupted members of the same original mass. In addition to these I may mention two falls on March 6, 1853 (the date of one of which, however, seems open to doubt); one on the 11th, another on the 14th, and a third on the 20th of May, 1874; two on the 25th of August, 1865; one on August 1 and another on August 4, 1835; one on April 10 and another on April 15, 1812; one on August 1 and another on August 5, 1898 (indicating a repetition of the shower of 1835); one on the 12th and another on the 14th of May, 1861 (apparently a previous appearance of the shower of 1874 already referred to); one on the 27th and another on the 30th of May, 1866; one on the 24th and another on the 28th of May, 1886, which seem to belong to the same shower as the foregoing; and one on the 22nd and another on the 26th of September, 1893. I think these instances are too numerous to be explained by the doctrine of chance, and the difficulty involved in that explanation is increased when we find duplicate falls occurring in different years at almost the same dates. But those who hesitate about accepting this conclusion may perhaps be convinced on reading what follows.

II. Aerolite-falls occurring at almost the same date in two consecutive years.—These include falls on March 28, 1859 and 1860; on April 5, 1804, and April 6, 1805; May 9, 1894 and 1895; on May 22, 1868 and 1869; on June 19, 1876 and June 17, 1877; on June 12, 1840 and 1841; on July 24, 1837 and July 22, 1838; on August 1, 1897 and 1898; on August 5, 1855 and 1856; on

August 7, 1822 and 1823 (the last three pairs probably belong to the same shower) ; on September 5, 1813 and 1814 ; on Dec. 7, 1863 on Dec. 4, 1864 ; and on Dec. 27, 1857 and Dec. 24, 1858. It will be recollected that the total number of falls in the catalogue comes short of one for each day. The chances are therefore against two falls on the same day. What then are the chances against two falls on the same day *occurring in two successive years?* But the above list contains eight such pairs. Can this be the result of chance? Is it an incident that might be expected to occur with one out of every twenty aerolites in the catalogue?

III. Aerolite-falls occurring at nearly the same date with an interval of two or three years.—These are, I think, also too numerous to be explained by chance. There is one on Jan. 19, 1865, another on the same day, 1867, and a third on Jan. 20, 1869—to which may perhaps be added a fourth on Jan. 23, 1870 ; one on Jan. 28, 1883 and another on Jan. 27, 1886 ; one on Jan. 31, 1835 and another on Jan. 29, 1838 (the last four probably belong to the same shower) ; one on Feb. 13, 1893, a second on Feb. 10, 1896 and a third on Feb. 12, 1899 ; one on Feb. 18, 1824, a second on Feb. 16, 1827 and a third on Feb. 15, 1830 (these figures rather suggest a shower becoming earlier each year which might possibly be identical with the preceding trio) ; one on March 19, 1882, and another on the same day in 1884 ; one on April 17, 1877, and another on the same day in 1879 ; one on May 23, 1865, which seems referable to the same shower as those of May 22, 1868, and May 22, 1869 ; one on May 26, 1893, and another on May 27, 1895 ; one on July 14, 1845, and another on the same day in 1847 ; one on Sept. 9, 1829, and another on the same day in 1831 ; one on Nov. 26, 1874, and another on Nov. 27, 1877, (in neither of which years was there any great display of Andromedids) ; one on Dec. 6, 1866, and another on Dec. 5, 1868, (which are clearly connected with a pair on Dec. 7, 1863, and Dec. 4, 1864, mentioned under the last head) ; a pair on Dec. 25, 1846, and Dec. 27, 1848, which seem to be connected with those of Dec. 27, 1857, and Dec. 24, 1858, already referred to (suggesting a period of 10 or 11 years). There seems to be a complete family of them on Oct. 7, 1861, Oct. 1, 1862, Oct. 5, 1866, Oct. 1, 1868, and Oct. 6, 1869. I think the following may be regarded as a similar family: April 6, 1885, April 7, 1887, April 7, 1891, April 9, 1894, and April 9, 1896. Here are six falls occurring within five days during a period of twelve years only, but six falls in five years is above the general average for the whole period embraced in the catalogue.

IV. But independently of these considerations we find in many cases *a clustering of aerolite falls about particular dates* (months and days of the month) which cannot be ascribed to chance. The average number of falls *per diem* is less than 1 for the entire year ; but on Oct. 13 (not a very prolific month) there are 5 falls in the catalogue and on the following dates the number is 3 or upwards—Jan. 23, Feb. 10, Feb. 16, Feb. 18 (the last two probably representing the same shower), March 25, April 10, April 26, May 8, May 9 (four aerolites, doubtless belonging to the same shower which produced three on the 8th); May 14, May 17 (four aerolites), May 19, May 20, May 22 (the last three if not four probably belong to the same shower), June 12, June 21, June 28, July 14 (four aerolites—and the total number for this month is only 23), Aug. 1, Aug. 5 (four aerolites : this date is earlier than the Perseid maximum); Aug. 11, Aug. 29, Sept. 5, Sept. 13, Sept. 14 (if we include the Crema aerolite); Sept. 23, Oct. 1, Nov. 27, and Dec. 13. In the majority of cases it will be found that the number of aerolite falls immediately before and after the dates mentioned, are also above the average. There are, I believe, aerolite showers at all the above-mentioned dates (save that in some instances two or more of them belong to the same shower) which usually last for some days both before and after, bnt we are hardly in a position to fix exact dates for their beginning and end. For instance, after two aerolite falls on one day, there is often a day on which none are recorded and then two more fall. Here it seems reasonable to conclude that the same shower extends over the three days. But if the break was one of two days we might suspect two showers separated by a short interval. We can hardly decide at present whether the great aerolite fall in May consists of a continuous shower or of two or more successive showers, with a short interval between them.

Many showers of shooting-stars are periodic. Do aerolite showers possess this property ? We shall require further observations before answering this question positively, but I think there are strong grounds for suspecting it. We cannot expect to find exact periodicity for the reason (already indicated) that the shower probably lasts for three or four years at each return, and the recorded fall of an aerolite belonging to it does not indicate that the shower was at its maximum when this aerolite fell. Therefore a seven years' period seems to me to be suggested by the following falls: Feb. 10, 1825; Feb. 13, 1839; Feb. 10, 1853; Feb. 12, 1875; Feb. 10, 1896. Again, the following suggest a period of 10 or 11 years, the date of the shower becoming a little later at each return: Feb. 19, 1785; Feb. 19, 1796; Feb. 25, 1847; Feb. 28, 1857; Feb. 29, 1868. We cannot expect an aerolite to be seen falling at every return. A period of 21 or 22 years would explain March 25, 1843; March 25, 1865; and March 27, 1886. It would perhaps be a little fanciful to deduce a period of 19 years from March 15, 1806; March 16, 1863; and March 19, 1882; but periods of about 20 years are frequently suggested. The aerolites of April 26, 1842; April 29, 1844; April 28, 1893; and April 26, 1895 would suit a period of 51 years. A period of 22 years would fit in with May 9, 1827; May 8, 1829; May 8, 1872; May 9, 1894; and May 9, 1895: a 10 years' period with May 22, 1808, May 20, 1848; May 19, 1858: and May 22, 1868: a period of 20 years with June 3, 1822; June 3, 1842; June 2, 1843; and June 2, 1863; to which might be added June 1, 1902: a similar period with June 13, 1819; June 15, 1821; June 12, 1840; June 12, 1841; June 15, 1900; and June 10, 1901: a 10 years' period, becoming a little later each year, with June 12, 1840; June 13, 1850; June 16, 1860; June 17, 1870; and perhaps June 25, 1890. (It will be seen that the aerolite of June 12, 1840, appears in both these series. It, in fact, fits into both, though, of course, it can only belong to one). July 12, 1820; July 17, 1840; and July 14, 1860 may also indicate a recurrence. The following series is perhaps one of the best marked: Aug. 7, 1822; Aug. 7, 1823; Aug. 5, 1855; Aug. 5, 1856; Aug. 1, 1897; Aug. 1, 1898. The period is 32 or 33 years, becoming a little earlier on each return. Sept. 14, 1825; Sept. 16, 1843; Sept. 17, 1879; and Sept. 15, 1897 indicate an 18 years' period with one return unobserved (as often happens with the return of a comet). Oct. 14, 1824; Oct. 13, 1838, and Oct. 13, 1852 look like a period of 14 years. These instances may suffice. I think there are more of them than chance will account for, but on the other hand I cannot say that a single periodicity has been clearly established as a scientific fact. They can only be regarded as "suspected" periodicities, to be closely watched hereafter. Some are no doubt due to chance. It should be noted, however, that a period of 20 years does not imply that the aerolite is moving round the sun in an orbit having that period. Such a meteor would probably enter the air with too great a relative velocity to reach us in the solid form. Twenty years may be equal not to one but to nineteen revolutions of the aerolite in its orbit. What is necessary for periodicity is only that some number of revolutions of the aerolite should be almost equal to an integer number of years.

As there is a clustering of aerolite-falls about particular dates in a year, there is a marked deficiency about other dates. In this respect they also resemble shooting-stars. It is doubtful whether there is any such thing as a solitary, detached shooting-star. They seem all to belong to showers, though these showers are sometimes very attenuated; and there is not a night in the year in which more than one meteor-radiant is not active. Some meteors or shooting-stars fall every night. But sometimes the active radiants are fewer and more attenuated than usual while at other times they are more numerous as well as richer. On the nights of the great showers—Perseids and Leonids—meteors from other radiants will always be noticed during a watch of considerable duration. In like manner there is no time of the year when aerolite-falls entirely cease, but there are times when they are much below the average, as well as times when they are much above it. In the first 14 days of January my catalogue only gives 4 falls, yet it is quite possible that each of these four belonged to a shower. There are other marked deficiencies at the end of September and the end of October, and others still will be noticed on examination. It is not to be supposed that when we reach a great display like that in May all the aerolites belong to the same shower, but I think we may conclude that at least one rich shower is active on these occasions—which is all that we could conclude with regard to the Perseids and Leonids if we knew as little about their radiant-points as we know about the radiant-points of aerolites. Indeed the latter often fall nearly perpendicularly, which does not, I think, imply that the radiant is near the zenith but only that their relative motion has been destroyed by the resistance of the air and that the fall is due to gravitation. The motion previous to the fall may have been nearly horizontal instead of nearly vertical.

The very small velocity with which aerolites reach the earth has been noticed on many occasions, and I do not recollect any instance in which the velocity has been shown to have been high. I am using the words "high" and "low" with reference to the velocity of the earth in its orbit. They may travel faster than a cannon-ball and kill any person whom they strike ; but has one of them ever struck the earth with a velocity of one mile per second? The small depth to which they penetrate into the soil —which is often quite as remarkable in cases where the aerolite is "found" as in those where it is seen to fall—goes far to establish the contrary. And I know of no instance in which the aerolite when first seen after the fall was at an intense white heat, although if it fell with considerable velocity it must have been highly heated by the destruction of this velocity. A high velocity cannot be destroyed without developing a large amount of heat.

A word may here be added as to the recent fall at Shelburne, Ontario, Canada. Though nearly coincident in date with the Perseids I do not think this pair of aerolites can be referred to that head. There is no night during the Perseid display on which Mr. Denning and other observers have not noticed some meteors coming from radiants at a considerable distance from that of the Perseids: and some are of the slow-moving, long-pathed kind which seem much better suited for producing aerolites than the Perseids are. It will be noticed too, on looking at the catalogue, that while the Perseids rise to a very marked maximum on August 9–11, the aerolite-falls are pretty equally distributed over the first half of the month and exhibit no distinct maximum about the date in question : while it is difficult to believe that a meteor entering the atmosphere with the great computed velocity of the Perseids could escape dissipation before reaching the earth. Several Perseids have had their paths through the air computed by means of simultaneous observations made at different places. In all instances, as far as I know, the meteor was computed to have disappeared—presumably by dissipation—at a great height above the earth. On the other hand, the one of the Shelburne aerolites which was best reported on was not very highly heated when it fell to the earth, and that it did not fall with any very great velocity is evident from the comparatively small depth to which it penetrated the soil. The amount of light which it gave is indeed somewhat difficult to reconcile with the other facts of the case. A very rapid cooling, as the aerolite moved through the lower regions of the atmosphere, is suggested, but there are difficulties in this supposition. At all events, a rapid diminution of velocity is hardly consistent with such rapid cooling, for diminished velocity means motion converted into heat. The Perseid shower, moreover, is usually almost over by the 13th of August, when the Shelburne fall took place, and my catalogue contains two falls on the 14th, in the United States, which had probably the same origin as the Canadian fall. With regard to rapid cooling I may point out that moist air would absorb heat much more rapidly than dry air, and the upper regions of the atmosphere are usually very dry. Red-hot shot, I believe, has often been fired in war-time, but I am not aware whether observations have been made as to the rate at which it cools during its aerial flight. I may also refer to the rushing sound heard just before, or simultaneously with the aerolite fall at Shelburne. If the aerolite had been moving with a high velocity the sound would have been heard subsequent to the fall. The same feature has occurred in the case of several other aerolites.

A total of 329 aerolites*—of which the dates of not more than 300 can be regarded as absolutely certain—is too small to enable many conclusions regarding them to be drawn with scientific certainty. That they occur in showers seems certain and if we cannot fix the exact duration of each shower, we can, I think, in almost all instances, name one or more of the days included in it. It is also clear that there is no marked connection between them and the principal showers of shooting-stars—indeed there is not one of these latter showers that can be proved to have produced aerolites. The comparatively small velocity with which these aerolites fall to the earth is, I think, also an established fact. There seems to be a wide gap between the aerolites and the shooting stars which are converted into vapour at the height of 30 or 40 miles—often more : and it is not unreasonable to con-

* CORRECTION TO CATALOGUE.—One meteor has been added : Sept. 13, 1904, 8 p.m., Shelburne, Co. Dufferin, Ontario, Canada ; (two stones, a mile and a half apart). One has been struck out ; viz., a Kansas meteorite, twice inserted in April.

clude that all meteors, which move round the sun with retrograde motion and consequently encounter the earth with a velocity of more than 18 miles per second, are dissipated in the air. But direct motion may be insufficient to produce even an occasional aerolitic fall if the orbit is a very eccentric ellipse, or its plane is inclined at a high angle to the ecliptic. Our atmosphere is a kind of trap which may catch members of the solar system alive but is pretty certain to kill intruders from without. Should these last remarks prove well-founded it will be seen that an attempt to deduce the nature of the universe, outside of the solar system, from the spectra of aerolites rests on no satisfactory basis, for these aerolites are members of the solar system, not visitors from space. If we could obtain good observations of the spectra of shooting-stars during a rich shower in which none fell to the earth, we might have more reliable grounds for drawing conclusions with regard to the external universe.

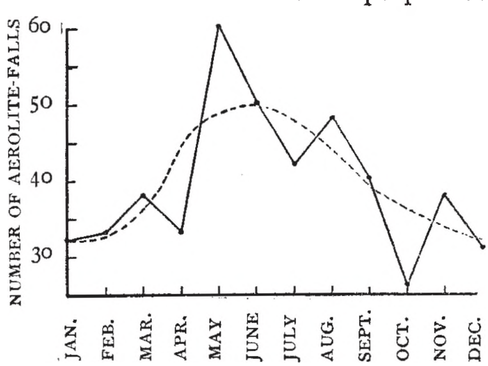
ADDITION BY MR. ARTHUR HARVEY.

In reference to the first paragraph of the above paper, Mr. Harvey thinks the "Supplementary Lists" in Mr. Monck's catalogue should be taken into account in reckoning monthly averages. There are perhaps inaccuracies in them as to days and hours of fall, and so Mr. Monck properly rejects them in calculating exact periodicities.

Including them we have the following numbers:—

Jan. 32,	May 60,	Sept. 40,
Feb. 33,	June 50,	Oct. 26,
March 38,	July 42,	Nov. 36,
April 33,	Aug. 48,	Dec. 31.

Co-ordinating these numbers and the months, and smoothing the curve, we have the annexed figure, which shows the excess of aerolites falling in May and June pointed out by Mr. Monck, and illustrates Mr. Harvey's argument that more are seen in the fine summer months than in those when people live indoors.



SOME NEW DETERMINATIONS OF THE REFLECTING POWERS OF GLASS AND SILVERED-GLASS MIRRORS.

BY

C. A. CHANT.

I. REFLEXION FROM METALS.

THE question of reflecting-power is of great importance both from the theoretical and the practical point of view; and it is not surprising, therefore, that numerous investigations have been made upon it. Lord Kelvin has proposed the word *reflectivity* to designate the ratio of the whole reflected to the whole incident light, and it will be used with that meaning in the present paper.

The first precise measurements were probably made by Herschel.*

He used a photometric method suggested by Bouguer,† and measured the reflectivity of one of his specula for nearly perpendicular incidence. He found that it returned 67·262% of the incident light.

About thirty years later Potter ‡ made similar experiments. Potter made his own mirrors, and becoming expert at polishing, he wished to compare his work with Herschel's. He noted, also, that Newton § had assumed that more light was reflected as the angle of incidence was increased, and he determined to test this statement.

*W. Herschel, "On the Power of Penetrating into Space by Telescopes, etc." Abridgment of Phil. Trans., vol. 18, p. 580, (1800).

†Bouguer, "Traité d'Optique." See Priestley's "History", p. 540.

‡R. Potter, Edin. Jour. Sci., N. S., vol. 3, p. 278, 1830.

§Newton, in a letter to the secretary of the Royal Society, dated May 4, 1672, discusses the advantages of his form of reflecting telescope over Cassegrain's, and remarks: "For it is an obvious observation that light is more copiously reflected from any substance when incident most obliquely." —Abridgment of Phil. Trans., vol. 1, p. 712.

Potter also used a modification of Bouguer's photometer, and he was astonished to find his experimental results at variance with Newton's "obvious observation." For a plane mirror of speculum metal the reflectivity at 10° of incidence was about 68 % and this gradually fell for increasing incidence, becoming about 64% at 60°. For a mirror of cast steel the same behavior was observed, the reflectivity falling from 59% at 10° to 54% at 60°. He thus concluded that the metals reflect better at small incidences. Subsequent investigations fail to substantiate the general law enunciated by Potter.

Jamin * worked with polarised light and compared the amounts reflected from silver, steel and speculum metal with that from glass; and assuming the laws of reflexion from glass to be accurately represented by Fresnel's formulæ, he calculated the absolute amount of light reflected from the metals at various incidences. Jamin's results to a certain extent corroborate Potter's, but the agreement is not very good. Moreover Jamin's indirect method has been criticised by Verdet † as not susceptible of great accuracy. About the same time the reflectivities of metals for heat waves were studied by de la Provostaye and Desains ‡, and the values given by them for heat waves are approximately the same as Jamin's for light waves.

Conroy § made an extended series of experiments on metallic reflexion. He used a modified form of Ritchie's photometer, in which the light falls on the receiving screens obliquely, and thus polarisation effects may have influenced the results to some extent. His mirrors were of silver, steel, tin and speculum metal. With the first three the amount of reflected light gradually increased with increasing incidence, but with speculum metal it first increased, then diminished and then increased again. These results are at variance with Potter's, Jamin's and the theoretical formulæ deduced by Cauchy and MacCullagh. ||

* J. Jamin, "Ann. de Chim. et de Phys.", (3), 19, p. 296, 1847,

† Verdet, "Lecons d'Optique Phisique", t. 2, (Œuvres, t. 6), p. 546.

L. de la Provostaye and P. Desains, "Ann. de Chim.", (3), 27, p. 109, 1849; (3), 30, pp. 159, 276, 1850.

§ Sir J. Conroy, Proc. R. S., 28, p. 242, 1879; 31, p. 486, 1881; 35, p. 26, 1883; 36, p. 186, 1883; 37, p. 36, 1884.

|| See Verdet, l. c., p. 563 and fol.

Rayleigh* measured the reflectivity of silvered glass and mercury for almost perpendicular incidence. For plate glass silvered on the anterior surface, 93·9 %; when silvered on the posterior surface, 82·8 %; for mercury, 75·3 %.

The most extensive investigations on metallic reflexion, however have been recently made by Hagen and Rubens.† They measured the reflectivities for perpendicular incidence of numerous metals and alloys for wave-lengths ranging from 250 to 1500$\mu\mu$; and the general conclusion was that the reflectivity increased with the wave-length.

Work similar to this and leading to the same result has been done by Rubens, Langley, Nichols and Trowbridge.‡

In two still later researches Hagen and Rubens§ have shown that for long heat waves the amount of the radiation entering the reflecting metal, (i. e., the incident light less the reflected light), and the emissive power are inversely proportional to the square root of the electrical conductivity, and also inversely proportional to the square root of the wave-length of the incident radiation. This is in accord with Maxwell's electromagnetic theory.

II. REFLEXION FROM GLASS.

Potter ‖ was one of the earliest workers in this field also. He experimented with crown, plate and flint glass, determining the reflectivity of the front face and also of both faces of a plate for incidences ranging from 10° to 85°.

Since Potter's time measurements have been made by many experimenters, usually in verification of the theoretical formulæ given by Fresnel. ¶

* Rayleigh, Scientifiic Papers, vol. 2, p. 522, (1886); vol. 4, p. 3, (1892).

† E. Hagen and H. Rubens, "Ann. der Phys.", 1, p. 352, 1900; 8, p. 1, 1902.

‡ H. Rubens, Wied. Ann., 37, p. 249, 1889. E. F. Nichols, Wied. Ann., 60, p. 401, 1897; Phys. Rev., 4, p. 297, 1897. S. P. Langley, Phil. Mag., 27, p. 10, 1889; Am. Jour. Sci., Nov. 1888. Rubens and Nichols, Wied. Ann., 60 p. 413, 1895. A. Trowbridge, Wied. Ann., 65, p. 595, 1898.

‖ Hagen and Rubens, Ann. der Phys, 11, p. 873, 1903; Ann. de Chim. et de Phys. (8), 2, p. 441, 1904.

§ Potter, Edin. Jour. Sci., N. S.. 4, p. 53, 1831.

¶ See Verdet, l. c., pp. 395–518.

Rayleigh * measured the intensity of the light reflected at nearly perpendicular incidence from three specimens of glass, and concluded that recently-polished glass surfaces have a reflectivity agreeing closely with Fresnel's formula, but that after some months or years it may fall off as much as 30% without any apparent tarnish on the surface. Some years later he measured the reflectivity of water for nearly perpendicular incidence. He used a photographic method and found the value 2.076%.

Conroy† has also studied the amount of light reflected and transmitted by certain kinds of glass. He used three kinds having indices 1.5145, 1.5274, 1.6330, and worked with natural light. He showed that the amount of light reflected depends to some extent on the way the glass has been polished, and that the variation from the amounts calculated by Fresnel's formulæ is sometimes an excess, sometimes a defect. He found, too, that the surface of flint glass, after repolishing, seems to alter somewhat readily ; whilst with crown glass the change, if any, proceeds somewhat slowly.

A renewed interest has been given to the subject by the publication of Lord Kelvin's "Baltimore Lectures", an entire long lecture, pages 324–407, being devoted to reflection. He remarks that very few experimenters have determined the proportion of the whole reflected to the whole incident light, and says that it is greatly to be desired that thorough investigation of this kind should be made. The present writer hopes to continue his experiments and thus contribute to this desired result.

III. METHOD OF EXPERIMENTING.

The present investigation was undertaken through a suggestion made some time ago by Mr. J. R. Collins, secretary of the R. A. S. C. Mr. Collins and his brother, Mr. Z. M. Collins, designed and in the year 1896 constructed an achromatic telescope of 4·5-inches aperture, in which the light first passed through a double-convex objective of plate glass and then fell on a concavo-convex lens, also of plate glass, silvered on the back. Being reflected by this silvered surface the light was returned to a small reflector, or, preferably, a total-reflecting

* Rayleigh, Scientific Papers, vol. 2, p. 522, (1886).

† Sir J. Conroy, Phil. Trans., 180 A, p. 245, 1889.

lens-prism, near the posterior face of the objective, and thence into the eye-piece. (See fig. 1).

Fig. 1.

The focal length was 48 inches and the tube was 24 inches long.* It was in discussing the efficiency of such a combination, involving reflection internally at a silvered glass surface that the present investigation originated.

The method employed in it has, I believe, advantages over those previously used.

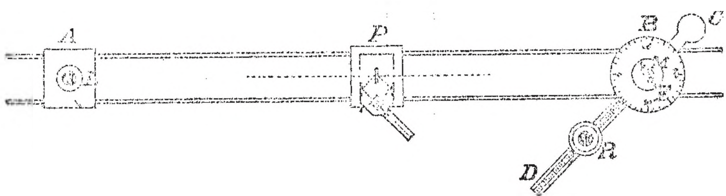


Fig. 2.

A (fig. 2) is a carriage moving on the ordinary photometric bench and bearing a Hefner standard lamp L. B is another carriage. On this is a wooden arm CD which can revolve about a vertical axis through the centre of the carriage. Just above this arm is a graduated brass disc E, rigidly fastened to the carriage. A pointer on the arm CD allows its position with respect to the disc to be read, and when the arm is parallel to the rails of the bench the reading is zero. A second Hefner lamp R held in a turned wooden block can be slid along the arm CD, and a scale on this arm allows the distance of the axis of the lamp from the centre of the graduated disc E to be read off directly.

* See Trans. Astron. and Phys. Soc., Toronto, 1897, p. 23; Toronto Astron. Soc., 1900, p. 30; "Knowledge", vol. 23, p. 252, 1900.

A somewhat similar arrangement is described by Schupmann in his work, "Die Medial Fernrohre", Leipzig, 1899, though all the figures in it call for two kinds of glass, crown and flint.

A small round table at the centre of the disc E carries the mirror to be tested. This table can turn about the axis of the disc, and the mirror M is held between two metal strips so that the reflecting surface is at the centre of the table. P is a Lummer-Brodhun photometer, the one used in these experiments being by Schmidt & Hænsch and arranged for equality of contrast.

It was necessary that the two lamps R, L should remain constant over an extended set of readings, and to this end cylindrical glass chimneys were placed over the flames, with a piece of fine wire gauze over the top and another over the bottom of the chimney. This answered admirably, the flame being entirely unaffected by air currents.* In order to render the law of inverse squares more rigourously applicable the glass chimneys

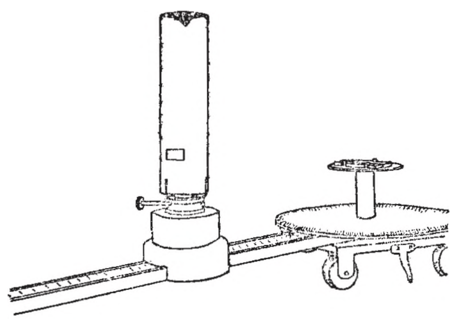


Fig. 3

were covered with black paper in which was made an aperture 1 cm. high, opposite the middle portion of the flame. (Fig. 3). This flame was approximately of the height required for the standard lamp, but no attempt was made to have it accurately so, nor to have the two lamps accurately equal to each other. The only requisite was that the ratio of one to the other should remain constant. When working with a glass surface, of which the reflectivity is small, the aperture in the black paper about the left lamp had to be reduced in size. It may be remarked, too, that the photometric bench had black velvet hangings all about it except where the observer was working.

* For this expedient, and also for some preliminary experiments I am indebted to Miss L. B. Johnson, B.A., and Mr. W. H. Day, B A., senior students at the time.

The method of operation was as follows. First the arm CD was turned to zero on the disc E, and the lamps R, L turned so that the photometer was exposed to their flames, the position of the carriages A and B and of the lamp R on the arm being carefully observed. The photometer P was then moved until equally illuminated by the two lamps and its position noted. This adjustment was usually repeated 5 or 6 times and the mean taken. The distances thus obtained give the ratio between the two lamps. Then the arm CD was turned through twice the desired angle of incidence and the lamp R turned about until its aperture was towards the mirror M. P was then moved until equilibrium was obtained between the light received directly from L and that received from R by reflection at M. This adjustment was made from 4 to 7 times and the mean taken.

In obtaining the ratio between the two lamps, let the distances of L and R from P be a, b respectively.

$$\text{Then } L/R = (a/b)^2.$$

Again, let equilibrium be obtained when the distances from L to P, P to M, M to R are c, e, f, respectively.

Then if x be the reflectivity, we have the relation

$$x = \left(\frac{a}{b} \times \frac{e + f}{c} \right)^2 \times 100\ \%.$$

The calculations were made by this formula.

The ratio between the two lamps was found at the beginning, the end, and usually also at the middle of a series of readings.

IV. RESULTS.

Table I illustrates the method. It was obtained with Mirror IIa, plate glass silvered on the front surface. The left lamp was at 70 cms. to the left of zero; the right carriage at 120 cms. to the right of zero; and the numbers in the table give the positions of the photometer to the right of zero. From these readings a, b, c, e, f are at once deduced.

TABLE I.

MIRROR II a, SILVER IN FRONT.

READINGS OF POSITION OF PHOTOMETER.

READING OF ARM.	ANGLE OF INCIDENCE. 5°	10°	20°	40°	60°	80°
20 = f	35.04	35.20	35.15	35.34	35.00	34.80
	35.25	35.15	35.26	35.50	35.10	35.04
	35.42	35.05	35.42	35.48	35.20	34.90
	35.30	35.24	35.50	35.46	35.32	34.80
Mean	35.252	35.160	35.332	35.445	35.155	34.885
Hence c =	105.252	105.160	105.332	105.445	105.155	104.885
e =	84.748	84.840	84.668	84.555	84.845	85.115
30 = f	40.30	40.62	40.52	40.12	40.34	39.80
	40.28	40.70	40.26	40·26	40.28	40.08
	40.40	40.70	40.32	40.36	40.30	40.22
	40.26	40.62	40.40	40.60	40.30	40.30
Mean	40.310	40.660	40.375	40.335	40.305	40.100
Hence c =	110.310	110.660	110.375	110.335	110.305	110.100
e =	79.690	79.340	79.625	79.665	79.695	79.900
	45.10	45.40	45.62	46.18	45.88	45.68
	45.60	45.62	45.78	45.94	45.90	45.60
	45.22	45.72	46.00	46.08	45.70	45.60
	45.32	45.52	45.80	46.18	45.60	45.54
Mean	45.310	45.565	45.800	46.095	45.770	45.605
Hence c =	115.310	115.565	115.800	116.095	115.770	115.605
e =	74.690	74.335	74.200	73.905	74.230	74,395

RATIO OF LAMPS.

At Beginning.	At End.	
14.72	14.25	Mean, 14.5125
14.72	14.25	f = 20
14.68	14.60	Hence
14.50	14.28	a = 84.5125
14.580	14.345	b = 83.4875

TABLE II.

REFLECTIVITIES OF MIRRORS FOR ANGLES OF INCIDENCE FROM 5° TO 80°.

Mirror	ANGLE OF INCIDENCE. 5°	10°	20°	40°	60°	80°	Remarks
Ia Silver before glass	95.62	95.84	95.45	96.07	96.44	95.94	Fresh mirror.
IIa " " "	96.71	96.13	95.99	96.57	96.32	97.08	" "
IIIa " " " thick film.	95.56	95.14	94.41	94.61	94.93	95.10	" "
Mean	95.96	95.70	95.30	95.75	95.90	96.04	
IIIa Silver before glass	68.39	69.34	69.66	68.17	68.11	...	3 months old.
Ib Silver behind glass	87.40	87.42	87.17	87.65	87.59	86.69	Fresh mirror.
IIb " " "	90.84	90.75	90.64	90.05	88.14	83.24	" "
IIIb " " "	90.88	90.77	90.58	90.03	88.41	80.76	" "
Mean	89.70	89.65	89.46	89.24	88.05	83.56	
IIIb Silver behind glass	88.19	87.35	86.35	87.33	87.35	...	3 months old.
IV Commercial mirror	86.75	85.99	85.62	85.69	86.16	92.39	3 years old.
V Plate glass, one face	3.98	4.08	4.20	4.70	9.33	40.90	Plate "backed."
	4.25	4.25	4.27	4.70	9.17	39.07	Calculated.
VI Flint glass	5.37	5.39	5.48	6.29	10.46	40.80	
	5.59	5.59	5.62	6.23	10.82	40.40	"
VII Dense flint.	6.82	7.02	7.11	7.79	12.40	42.21	
	7.10	7.10	7.13	7.77	12.42	41.38	"
VIII Glass plate, both faces.	7.68	7.66	7.70	8.82	15.76	59.84	Same plate as V.
IX Silver plate	66.09	64.34	65.28	65.25	66.01	72.34	Inferior polish.
		[70.05	70.06	70.87	74.19	81.19	Conroy.]
X Speculum metal	57.23	57.99	58.08	57.39	58.24	65.24	Inferior polish.
		[66.13	66.88	67.26	66.32	70.17	Conroy.]
		[67.26					Herschel.]
		[67.52					Potter.]

In Table II are given the final measurements made with the various mirrors.

I a, I b denote the same mirror with faces reversed ; similarly with II a, II b; III a, III b. These three mirrors were about 6mm. thick, and were silvered by Brashear. The following notes were made on them before testing their reflectivities.

I. Of ordinary density for a speculum, and with silvering as perfect as could be. Glass surface a little scratched, and not polished as highly as possible.

II. Not quite so dense as I, but silvering equally good. Reflected white light rather better, i.e., without the reddish tinge due to the rouge used in polishing. Glass side polished rather better than in I.

III. Doubly silvered ; film much thicker. Polish of silver surface not quite equal to the others. Showed slight rouge tint. Glass surface as in II.

The other reflectors were as follows :

IV. Commercial mirror, 2.8 mm. thick, and at least three years old. Before using, its face was cleaned and rubbed with rouge on chamois.

V. Plate glass 2.8 cm. thick and of refractive index 1.5193. Face rubbed with chamois. To avoid reflection from the posterior surface this surface was coated thickly with the preparation used for "backing" photographic plates. This answered admirably. When a candle flame was observed in the plate, not a trace of a second image could be seen.

VI. Flint glass, one face of a prism of refractive index 1.6194. The other faces were blackened.

VII. Dense flint, one face of a prism from a spectroscope by Lütz, and describded as very dense white flint. The face was tarnished and had to be repolished. The refractive index was 1.7265.

VIII. Glass plate ; the same as V with no backing on.

IX. Silver plate, of jeweller's "pure silver". The polish was not very good.

X. Speculum metal ; a flat mirror by Brashear, but having become tarnished, was repolished, though not very well, as the low reflectivity indicates. The measures taken, however, show what variation in the reflectivity there is with the incidence.

The calculated results given were obtained bv substituting in the Fresnel formula

$$\frac{1}{2}\left[\left(\frac{\sin(i-r)}{\sin(i+r)}\right)^2 + \left(\frac{\tan(i-r)}{\tan(i+r)}\right)\right]$$

The results in Table II are shown graphically in the curves of fig. 4.

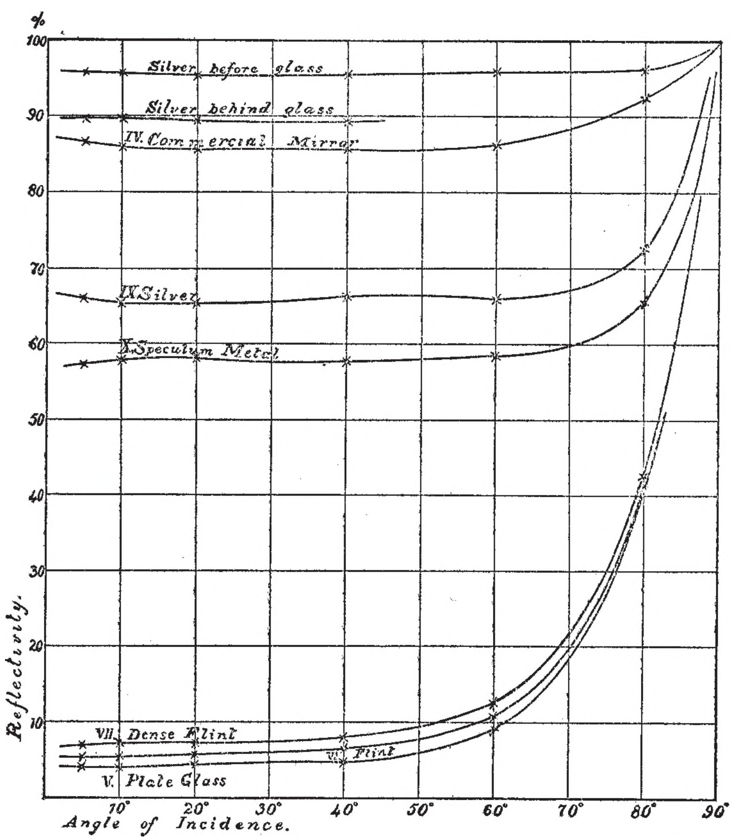


Fig. 4.

It would appear from the table that with the mirrors Ib, IIb, IIIb, (silver behind glass), the reflectivities are smaller for 60° and 80° than for the lower incidences. This anomaly is undoubtedly due to the multiple reflection within the thick plate, for which it is impossible properly to allow. With the commercial mirror, which was not half as thick, this effect is not observable.

Though the reflectivity of silver behind glass is about 6% smaller than that of silver before glass, this disadvantage is much more than balanced by the permanence of the former. After three months the mirror III had become so tarnished that as IIIa it fell from 96% to 68%, but as IIIb it fell only from 91% to 88%; and ordinary commercial mirror at least three years old was at 86.7%.

It will be interesting to determine the internal reflectivity of glass and of silver-on-glass. This may be calculated in the following way.

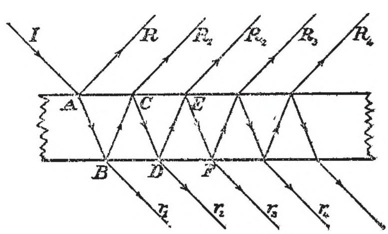


Fig. 5.

Let I (fig. 5) be the intensity of light incident on a glass plate at A. A portion R is reflected and (I–R) enters. Suppose that while travelling from A to B this is reduced by absorption to $(I-R)s$. If now the internal reflectivity be r the part reflected at B is equal to $(I-R)sr$. By the time this arrives at C it has been reduced by absorption to $(I-R)s^2r$. The portion of this which is reflected at C is $(I-R)s^2r^2$; and so on.

Then $R_1 = (I-R)\, rs^2\, (1-r)$,

$R_2 = (I-R)\, r^3s^4\, (1-r)$; &c.

Hence $R_1 + R_2 + R_3 + \dots = (I-R)\, rs^2\, (1-r)\, [1 + r^2s^2 + r^4s^4 + \dots]$

$$= (I-R)\, r\, s^2\, (1-r)\, \frac{1}{1-r^2s^2}$$

Now for a plate 2.8 mm. thick

$$I = 100\ \%$$

$$\left.\begin{array}{r} R = 4\% \\ R_1+R_2+R_3 \ldots = 7.7\% \end{array}\right\} \text{ (See Table II).}$$

$$\text{Hence} \quad \frac{r\, s^2(1-r)}{1-r^2s^2} = \frac{3.7}{96} \ldots\ldots\ldots\ldots\ldots (1)$$

Again, Conroy found that light of mean refrangibility on traversing 1 cm. of crown glass has its intensity reduced by 2.62 %.

Here the reduction by 1 cm. of glass is from 1 to $\frac{97.38}{100}$

and " " " 0.28 cm. " " 1 " $\left\{\frac{97.38}{100}\right\}^{0.28}$

$$\text{i. e.} \quad s = \left\{\frac{97.38}{100}\right\}^{0.28} = \frac{99.26}{100}$$

Substituting this value of s in equation (1), and solving the quadratic in r thus obtained we find

$$r = 4.07\%,$$

the internal reflectivity of glass.

For a plate silvered on the back, let p be the internal reflectivity for silver-glass. Then, as before,

$$R_1 = (I-R)\, s^2p\, (1-r),$$
$$R_2 = (I-R)\, s^4p^2r\, (1-r),$$
$$R_3 = (I-R)\, s^6p^3r^2\, (1-r), \text{ \&c.}$$
$$R_1+R_2+R_3+\ldots = (I-R)(1-r)\, s^2p(1+s^2\, p\, r+s^4p^2r^2+\ldots)$$

$$= (I-R)\, (1-r)\, s^2p\, \frac{1}{1-s^2p\, r.}$$

Now $R+R_1+R_2+R_3+\ldots = 90\%$ and $R=4,$ } (Table I).

hence
$$\frac{(1-r)\, s^2 p}{1-s^2 p\, r} = \frac{86}{96} \dots\dots\dots (2).$$

Using the values deduced above, namely,

$$s = \frac{99.26}{100}, \qquad r = \frac{4.07}{100},$$

we find $p = 0.913$, or 91.3%,

i. e., the internal reflectivity of silver-glass is 91.3%.

A formula similar to that for the reflected light can be found for the transmitted portion,

$$r_1 + r_2 + r_3 + \dots = (I - R)\, s\, (1 - r)\, (1 + r^2 s^2 + r^4 s^4 \dots)$$

$$= (I - R)\, s\, (1 - r) \frac{1}{1 - r^2 s^2},$$

and on substituing in this expression the values for r and s calculated above, we find

Transmitted light = 91.5%

ASTROPHYSICAL RESEARCH.

BY

W. BALFOUR MUSSON.

THE interpretation of the solar, and stellar spectra is a problem of supreme importance in modern astronomy, but unfortunately, is one surrounded by many difficulties and complications.

The perfecting of the instruments of research, the absolute measurement of wave length, and the nature of the displacement and modification of the spectral lines require skill of the highest order ; various conditions of temperature, density, and electrical condition are also disturbing factors, while a perfect analogy between results obtained in the laboratory, and apparently similar phenomena observed in stellar regions cannot be relied upon.

In a consideration of the chemical and physical condition of the stars, it is evident that the sun, as the nearest of these bodies, must be an object of the greatest interest. Among the many solar problems awaiting solution are, the true nature of sunspots and of the sun's rotation, confirmation or rejection of the suspected variability of the spectrum, and the degree and stability of the temperature of the photosphere. The existence of carbon in combination with nitrogen, at a temperature usually regarded as destructive of compounds, requires consideration. The presence of carbon in the sun was long disputed, but reliable authority now exists in favor of a permanent carbon envelope near its surface.

Prof. Trowbridge in 1896 photographed the spectrum of pure carbon and of an electric arc, whose carbons contained 28% of iron, and found that the iron nearly obliterated the carbon bands in the arc. The presence of iron, therefore, to say nothing of that of other metals, may suffice to mask the influence of carbon in the sun.

Is the temperature of the sun stable? The opinion is growing that it is not, and that it is subject to a periodic variation. In this connection the summary of Prof. Langley of a series of observations of the solar constant, which were conducted under his direction by Mr. Abbot, is of interest.*

The investigation pointed to a falling off in the solar radiation to the extent of 10%, beginning with the close of March, 1902. This conclusion is given with reservation, as the investigation is a difficult one, but assuming it to be correct, it should represent a decrease of temperature on the earth of something less than 7.5° C. A comparison of the temperature of 89 stations in the north temperate zone with the mean temperatures of the same stations for a number of previous years, shows that an average decrease of over 2° C. did actually occur.

An attempt to correlate such a variation with the sunspot cycle might yield interesting results. The latter is now known to be variable within itself, the recurring maxima and minima being subject to considerable fluctuations. The minimum just past, for example, was unusually prolonged.

The possible bearing of this fact upon the irregularity of long period variables, and the coincidence of the bright lines of the faculæ with the appearance of similar lines during the maxima of these stars cannot be overlooked, and we are tempted to ask whether the sun may not indeed be a true variable star. If so, its fluctuations are not at present sufficiently great to exhibit any marked augmentation or diminution of brightness, and the question arises whether this instability is not common to all stars at certain stages of their existence, and is, in fact, an inherent peculiarity of constitution.

Miss Clerke points out† that the marking of a bright hydrogen line by calcium absorption in the maximum spectrum of Mira is evidence that the region of augmented activity lies low in the star, while the outer layer remains relatively cool—strong evidence that the source of disturbance is internal not external.

When a certain stage of development is reached, however, the loss of heat by radiation is balanced by the gain of heat from the interior, and a period of comparative stability ensues.

Chandler found that redness of color appeared to be a

* Astrophysical Journal, vol. 19, No. 5.
† "Problems of Astrophysics," p. 350.

function of variability. Now a red color has been associated with a thickening of the absorbing layer of a star's atmosphere, and this condition would appear to be accompanied by greater amplitude and irregularity of the light curve. The whole question, however, is at present in a state of uncertainty and it would be unsafe to speculate further. Whether the slight traces of carbon now believed to exist in the sun will ever develop into the dense outlying strata giving rise to the bands in Secchi's 4th type can only be determined by the astronomer of many ages hence.

Approaching the great question of the evolutional development of the stars the investigator is confronted by many problems, important among which are, the order of progression from one type to another, and their relative temperatures at different stages of development.

It may be well at this point to briefly outline the characteristic differences of stellar types. In the light of later research Secchi's grouping has been considerably enlarged, Miss Mauray having divided the spectra photographed at Harvard observatory into no less than twenty-two different types. The main features of classification, however, may be comprised under the following heads:—

1st. Helium Stars. Color white; helium and hydrogen absorption predominant; contain complete Huggins hydrogen series, from *C* to the end in the ultra-violet, and, in a few examples, the Pickering series; also 26 lines of helium from the entire six series. The metallic lines are scarce and faint, and the magnesium line 4481 comparatively prominent, to the exclusion of *b*. They show little general absorption. The Pleiades and Orion stars are good examples.

2nd. Hydrogen Stars. (Secchi's Type I). Distinguished by intense hydrogen absorption, but showing no evidence of helium. The *H* and *K* lines are thin but distinct, and the lines of iron feeble. General absorption slight, and the ultra-violet end of the spectrum pronounced, thus accounting for the blue color. Example Vega.

3rd. Solar Stars. (Secchi's Type II). Color yellow; leading feature the development of *H* and *K* lines of calcium and the appearance of a great number of metallic lines; absorption, pronounced. Capella is a typical star of this class; Procyon may be regarded as intermediate between this and the preceding type.

4TH. STARS WITH FLUTED SPECTRA. (SECCHI'S TYPE III). A set of flutings, shading off towards the red, about ten in number, being superimposed upon the linear spectrum. These bands are of undetermined chemical origin, but strongly suggest the presence of oxides, and consequently a reduced temperature. There is no abrupt line of demarcation between this and the solar type, but a gradual progression from Capella, through Arcturus and Aldebaran to Alpha Orionis. They are subject to strong fluctuations of light.

5TH. CARBON STARS. (SECCHI'S TYPE IV). Noted for three prominent bands, shading towards the blue, due to carbon absorption; hydrogen being faint. Variable.

6TH. Includes stars with fluted spectra showing bright lines of hydrogen, Mira being a typical example. They are inconstant in light.

7TH. Helium stars with bright lines, especially *C* the line at wave length 4481 also being present.

8TH. WOLF-RAYET STARS—containing the Pickering series. Bright and dark helium lines. No metallic absorption, but showing a continuous spectrum. They are never or rarely variable.

Regarding the question of temperature, Prof. Schuster favors a higher temperature for helium and hydrogen stars than for those supposed to represent a later stage in their life history*, in this respect coming to a different conclusion to that of Sir Wm. and Lady Huggins who give the greatest heat to the solar stage†.

Schuster bases his conclusion mainly upon the researches of Prof. E. F. Nichols upon the heat radiations of Vega and Arcturus, the strength of the ultra-violet region in the spectra of the 1st type stars, and upon a certain similarity of these spectra to the spectrum of the electric spark.

Nichols found that Arcturus had double the intensity of Vega in total radiation, while both stars are of about the same visual magnitude; and deduced a lower temperature for the former.

* Astrophysical Journal, vol. 17, No. 3.

† Atlas of Stellar Spectra.

Allowing such to be the case, however, it is quite possible that the temperature drops from the sun to Arcturus, and the argument would not necessarily hold good for solar stars.

The argument drawn from the strength of the ultra-violet spectrum is not conclusive. It should be borne in mind that the greater absorption in solar stars must materially modify their spectra in this particular, and as a matter of fact, the photographs of Sir Wm. Huggins distinctly show that the intensity of the spectrum between *K* and wave length 3400 is greater in Capella and Procyon than in Vega.

The relative intensity of the magnesium line at wave length 4481 over the *b* triplet has long been regarded as indicative of a very high temperature, as this condition was considered peculiar to the electric spark. Scheiner held the development of this line to be the criterion of intense heat, while Keeler considered the effacement of *b* to indicate a temperature beyond artificial production.

The writer is indebted to Miss Clerke for drawing attention to the experiments of Hartmann and Eberhardt, at Potsdam, who have recently succeeded in producing the effect in quest on under conditions which preclude its being considered alone an evidence of exalted temperature. One of the main arguments in favour of the high temperature of the reversing layer of white stars has thus broken down,

A strong argument in favour of a high temperature for the sun is drawn from Lane's law, but Schuster points out that when speaking of the temperature of the stars, as exhibited by their spectra, we are dealing only with the outer or radiating layers, and that Lane's reasoning applies to the condition of the centre and the gaseous region below the photosphere, the photosphere itself being outside such calculations. Prof. Young considered the proportion of true gases and liquids of the sun to be such as to keep the temperature nearly stationary, and Huggins argues that the only period at which the temperature can remain stationary is at or near the maximum, concluding his review of the question with the words, "It is not improbable that a star's highest temperature is not reached until its spectrum has become solar in character."

To explain the substitution of calcium for hydrogen as development advances Lockyer offered the suggestion that hydrogen stars were too hot to permit of the existence of the molecule responsible for the calcium lines, but as shown above, this assumption is open to grave objections.

Huggins found a satisfactory explanation in the existence of strong convection currents, as the stage (described by Lord Kelvin as that of convective equilibrium) was reached. Schuster accepted this argument in 1897, but has since concluded that the difference between the surface condition of such stars as Sirius and the sun is not sufficient to almost eliminate this cause in the one case and to render it predominant in the other.

What then is the cause of the weakening of the hydrogen lines in solar stars? Schuster's answer is—very probably absorption of the hydrogen by the body of the star.

In early agglomerations of matter hydrogen and helium would be left out owing to the gravitative power of the mass not being sufficient to retain them. These gases might then collect in neighboring regions of space and, condensing about fresh neuclei, form characteristic hydrogen and helium stars as the temperature rose. Helium being the denser of the two gases would be the first retained, to be replaced later by hydrogen as the mass increased and the former sought a lower level in the star's atmosphere. A sudden outburst from the interior would of course drive these gases outwards, thus accounting for well known spectral peculiarities.

A large mass would absorb the surface gases more quickly than a smaller mass and consequently run the gamut of spectral change more rapidly—a contention strongly maintained by Miss Clerke, from independent reasoning.

The peculiar grouping of certain types of stars, as well as a marked tendency to selective distribution in space, are undoubtedly facts demanding consideration.

The transition from the helium, through the hydrogen to to the solar type of star is well marked, but the position of Arcturian and Secchi's third and fifth types is a matter of much less certainty. According to Schuster's reasoning the amount of hydrogen a star might retain would depend upon its mass and the quantity of hydrogen in its neighborhood. If the mass were small the hydrogen might not be absorbed, in which case it would remain a hydrogen star to the end of its history.

The evidence as to the nature of the 3rd and 5th types is as yet too uncertain to warrant their assignment to a definite place in an evolutional order.

It has been suggested, however, that solar stars might develop either 3rd or 4th type spectra according to original differences of chemical constitution.

Carbon stars have been placed at the end of the spectroscopic series, while it is probable that the Wolf-Rayet stars will be assigned an earlier stage in development.

Prof. Schuster inclines to the belief that stellar spectra largely represent original differences of composition. Sir Wm. Huggins was impressed by the evidence in favor of an evolutional order.

The University of Chicago Press promises in the near future, a volume on "Stellar Evolution" by Prof. Hale. A new work on the subject by this high authority will be awaited with much interest.

OBSERVATIONS ON VARIABLE STARS.

BY

J. MILLER BARR.

I. SUPPLEMENTARY NOTE ON THE VARIABLE STAR B. D.—1°943 (ORION).

THE fluctuations in the light of this star have proved to be of a somewhat complex nature. A discussion of the early observations—up to April 14, 1904—had led me to infer that the interval between successive maxima and minima might be "quite short—possibly less than 12 hours"; and it was further noted that "the observations exhibit certain anomalies or irregularities which cannot be referred to errors of observation".

These early deductions have been fully confirmed by the observations of the present season. The records of Oct. 18–19, Nov. 6, 14–15 and 16–17 are especially instructive. The observations cover periods ranging from about five hours, on Nov. 6, to eight hours on Nov. 14–15 and 16–17. On the last-mentioned date two *maxima*, separated by an interval of four to five hours, were recorded. On Nov. 14–15 the following phases were observed: Principal *max.*, Nov. 14, 22^h; secondary *min.*, Nov. 15, 0^h; sec. *max.*, 1^h: *min.*, 2^h+; *max.*, 3^h+; prin. *min.* 5^h to 6^h *.

The observations made last spring, as well as those of the present season, point to the possible recurrence of similar phases at intervals of about three days or 72 hours. As yet, however, I have been unable to determine the period (or *mean* period) of this remarkable object, which is not surprising, in view of the intricate nature of the star's light-curve.

II. A NEW VARIABLE STAR OF SHORT PERIOD.

As the result of a series of observations made within the last three months, I am able to announce that the star 32 Cassiopeiæ = B. D. +64° 127 is a rapidly-changing variable. It is one of a pair

* The epochs here given are necessarily rough approximations to the truth. The observations were carefully made, under good conditions, and I have entire confidence in the general results.

of naked-eye stars—about 49′ apart—whose approximate places for 1900 are thus given in the Harvard *Photometric Durchmusterung:*

B. D. + 63° 149,	R. A. $1^h\ 5{\cdot}0^m$,	Dec. + 63° 40′.
32 Cassiopeiæ	1 5·1	+ 64 29 .

These stars are rated as equal (5·48 mag.) in the revised *Harvard Photometry.* Argelander makes 32 Cassiopeiæ the brighter by 0·1 mag., and a similar difference is found in the *Photometric Durchmusterung.* The photographic mags. and spectrum-class are thus given in the *Draper Catalogue:*—32 Cassiopeiæ—A, 5·34. B. D. + 63° 149—A, 5·40.

My observations of these stars were made with an ordinary binocular, by Argelander's method. Great care has been exercised in making the comparisons, and I have good reason to believe that the observations are practically free from systematic errors depending on the relative positions of the stars. Occasional comparisons have also been made with B. D. + 63° 99 (rated as 5·37 mag. in the revised *H. P.*) These show clearly that 32 Cassiopeiæ is the variable. I find that the latter is alternately brighter and fainter than B. D. + 63° 149, the extreme range being nearly seven grades, or *about* 0·4 mag. The period—or more exactly, the interval between successive maxima and minima—is very nearly *eight hours.* For stars visible to the naked eye this is the shortest period known. The star has been watched, at intervals, through a complete period on five nights, viz., Aug. 26–27, Sept. 10–11, 12–13, 13–14, and Oct. 30–31. Both the increase and decrease of light are very rapid. On Sept. 15 the variable *decreased* by about ⅓ mag. within twenty minutes, viz., between $20^h\ 30^m$ and $20^h\ 50^m$ Eastern Standard Time. On Oct. 2 a decrease of about 0·3 mag. in 10 or 12 minutes was recorded. The observations of Oct. 30–31 show that the *increase* takes place with about equal rapidity.

The rapid decrease in the light of this variable renders it possible to determine, with some accuracy, the epochs at which the star appears equal in brightness to B. D. + 63° 149. Up to the present I have secured fourteen observations of this phase*, as follows :

* It is believed that the maximum error in an observation of this kind will not exceed 15^m, while the probable error will fall below 5^m.

1904, Sept. 6,	$20^h\ 50^m$	Sept. 30,	$20^h\ 17^m$
" 15,	20 42	Oct. 2,	20 16 ±
" 17,	20 57	" 14,	19 35
" 18,	21 5	" 17,	19 29
" 19,	21 35	" 30,	18 46
" 22,	20 55	" 31,	2 53
" 29,	20 27	Nov. 9,	18 35

The phase $v = a$ was not directly observed on Oct. 2, but the following observations were recorded :—$20^h\ 10^m$, $v\,2\frac{1}{2}\,a$. 20^h $20\text{–}22^m$, $a\,2\,v$. [v = 32 Cassiopeiæ, a = B. D. + 63° 149]. The epochs are given in Eastern Standard Time. It will be seen that these observations point to an *oscillation* in the period—or possibly in the *form* of the light curve*. Combining the observations of Sept. 6 and Nov. 9, the mean period for the interval is found to be $7^h\ 59{\cdot}3^m$ †. The probable error of this result will not exceed one-tenth of a minute. For the interval Aug. 26–Nov. 9 a mean period of about $7^h\ 59\frac{1}{2}^m$ is indicated.‡ I hope, later on, to determine the period with some accuracy from a considerable number of observations.

Only one observation of the phase $v = a$, with *v increasing*, has as yet been secured. It was recorded on Nov. 9 at $21^h\ 50^m$ Eastern Standard Time—$3^h\ 15^m$ later than the corresponding phase with *v decreasing*. The time-interval between these phases is evidently variable. It was about *five hours* on Sept. 12–13 and 13-14, according to the records of those dates. I should add that the *mean* brightness of v is closely equal to that of a.

My first observations of 32 Cassiopeiæ and B. D. + 63° 149 were made on Aug. 20, the variation in relative brightness was detected a few days later. Up to date I have secured 187 complete observations of these stars, besides a number of comparisons with B. D. + 63° 99.

It may be desirable to indicate, in a few words, the writer's method of observation. No attempt is made to observe the stars

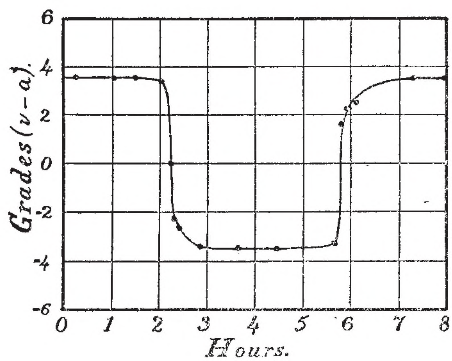
* The changes here referred to cannot be attributed to errors of observation, nor to variation in the light of the chief comparison-star, B.D. + 63°149.

† The correction for "light-equation", due to the earth's orbital motion, is here inappreciable.

‡ The observations of Aug. 26–27 afford only rough time-estimations for the phase $v = a$.

together, but each in turn is *quickly* brought to the centre of the field; the comparisons being repeated until the observer feels satisfied that the result which he writes down is substantially correct. I have found it advantageous to compare the stars both *directly* and by slightly averted vision; the binocular, in each case, being carefully focused. It seems evident that this procedure must yield results sensibly free from systematic errors, other than those due to the varying value of a *grade* for different intervals and magnitudes.

A first approximation to the light-curve of 32 Cassiopeiæ is shown in the annexed diagram. It is based mainly on the records



LIGHT-CURVE OF 32 CASSIOPEIÆ.

of a single night (Oct. 30–31) though some addititonal data have been utilized in drawing the steeper parts of the curve. The black dots represent the observations of Oct. 30–31. The abscissa of *four hours* corresponds to 20^h 28^m Eastern Standard Time on Oct. 30, which is nearly the time of central minimum of the variable.

The provisional light-curve suggests that 32 Cassiopeiæ may be an "eclipse variable". If so, we must assume that the component stars are nearly equal in size and brightness, and that they revolve, in relatively close proximity, in a period of about sixteen hours; the orbit-plane being inclined quite appreciably to the line of sight. To account for the rapid increase and decrease of light we must further suppose that the two bodies are considerably elongated in the direction of the line joining their centres*.

It is, however, open to question whether the very rapid changes already noted can be adequately explained on this hypothesis. Other objections—more or less obvious—are based on the general form of the light-curve, and the changes of form, as noted above. Any further remarks on this subject may with advantage be deferred until the light-curve becomes better known. We can scarcely doubt that spectrographic combined with photometric observations of this star will lead ultimately to a definite solution of the interesting problem which I have here ventured to touch upon.

Nov. 11, 1904.

ADDENDUM.

My recent observations of 32 Cassiopeiæ show clearly that the star's period is variable, quite apart from the remarkable changes in *form* of the light-curve. On Dec. 15 the central *maximum* occurred at about $20^h\ 20^m$ Eastern Standard Time—*i. e.*, $2^h\ 35^m$ earlier than the computed time, as based on the data given above. The corresponding mean period for the interval Oct. 30–Dec. 15 is about $7^h\ 58{\cdot}2^m$. For the interval Aug. 26–Dec. 15 the approximate mean period, as derived from the epochs of central *minimum*, is $7^h\ 59{\cdot}0^m$. On Dec. 15 the star remained near *maximum* during less than $1^h\ 33^m$; while the corresponding interval on Oct. 30-31 was nearly $4^h\ 40^m$. It seems evident that no modification of the eclipse-theory will account for the striking change thus indicated.

I have lately received from Director Campbell, of the Lick Observatory, a very encouraging letter, in which he states that spectrograms of 32 Cassiopeiæ would be secured at once with the Mills spectrograph. We shall, therefore, soon be in possession of important data bearing on the problem offered by this star's light-changes. In this connection I may note that the star has an unmistakable yellow or yellowish tint. The fact is of some interest, since all known variables of the δ Cephei type are *yellow*,

* Cf. Myers, "The System of β Lyræ", Astrophysical Journal, Jan. 1898, p. 1.

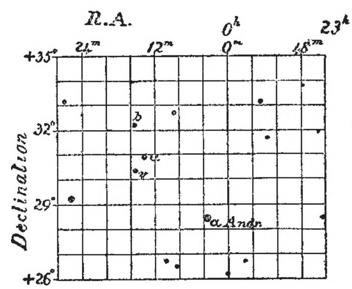
while the "eclipse-variables" are, I believe, always white or bluish-white.

Dec. 23, 1904.

III. A REMARKABLE STAR IN ANDROMEDA.

AMONG known variables the star *W* Ursæ Majoris is distinguished for the shortness of its period, which is only about $4^h\ 0^m\ 13^s$. This wonderful object was brought to light in 1903, from photometric observations made at the Potsdam Observatory.*

It has been the writer's good fortune to meet recently with a yet more striking instance of rapid change in a comparatively bright star, viz., B. D. + 30° 42 (Andromeda). This star apparently completes its cycle of light-changes in about *two hours and forty-one minutes.* It is about 3° distant from α Andromedæ, in the approximate position $\alpha = 0^h\ 15{\cdot}2^m$, $\delta = +30°\ 23'$ (1900). The star has been compared chiefly with B. D. + 30° 35, and occasionally with B. D. + 32° 45. These stars are shown in the accompanying chart, and their magnitudes, according to Argelander and Pickering, are given below :



	B.D.	H. P. REV.	DRAPER CAT.	SPECTR.
a = B.D. + 30° 35	6·0	5·70	5·78	*A*
v = B.D. + 30° 42	6·4	5·83	5·57	*A*
b = B.D. + 32° 45	6·0	5·93	6·39	*H*

The *Draper Catalogue* magnitudes are *photographic.* Star *v* is bluish-white, *a* is yellowish, and *b* of a distinct yellow tint.

* Müller and Kempf, "A New Variable Star of Unusually Short Period," Astrophysical Journal, April, 1903, p. 201.

A first approximation to the period of this new variable, viz., $2^h\ 41.1^m$, has been derived chiefly from the observations of Dec. 4, 15 and 16, 1904. * The star attained its maximum brightness at about 19^h Eastern Standard Time on Dec. 15 ; at $21^h\ 29^m$ it was rated as equal to star *a*, and was then rapidly *increasing*. On Jan. 14, 1905, the variable was near its maximum brightness at $21^h\ 30^m$. It was then about two grades brighter than star *a*.

The range of variation is about 5 to 6 grades, or probably somewhat less than 0·4 mag. There is some evidence of change both in the range and the form of the light-curve. Several observations point to the occurrence of subordinate maxima and minima of very brief duration ; but these features require confirmation.

It is possible that the period of B. D. + 30° 42, as given above, will have to be doubled. If so, the resulting period of about $5^h\ 22^m$ will be the shortest known with one exception—that of *W* Ursæ Majoris, as already noted.

As the new variable now culminates early in the evening, it is hoped that observers will not neglect their present opportunities for studying this remarkable object.

Jan. 17, 1905.

*It should be noted that a shorter period of about $2^h\ 25^m$ is not absolutely precluded ; but the evidence is in favor of the longer interval, as given above.

NOTE.—No. I of these observations is supplementary to a paper appearing in the Society's Transactions for 1903.

RECENT LUNAR PHOTOGRAPHS.

BY

DAVID J. HOWELL.

OF lunar photographs made in recent years several series, which are the result of careful plans and systematic work, claim special attention. Most of these have been issued in the form of very fine photogravure plates, by the observatories where they were taken.

They may be enumerated, in the order of issue, as follows:—The Lick Photographic Lunar Atlas.—The Paris Photographic Atlas of the Moon.—Prof. W. H. Pickering's Lunar Atlas, issued by the Harvard College Observatory.—Lunar Plates from negatives made at the Yerkes Observatory by Mr. G. W. Ritchey. —Seven plates on a larger scale than their Atlas, issued by the Paris Observatory.

One of the first attempts to photograph the moon with any degree of thoroughness, if not the first effort to produce a photographic atlas of the moon, was begun at the Lick Observatory shortly after their large telescope was completed. A number of excellent plates were obtained and reproduced in photogravure, most of which are in the Society's library. It is to be regretted that a lack of funds prevented the carrying out of the plans of the observatory in their entirety.

In a former paper an account was given of the lunar atlas of the Paris Observatory, a copy of which we are fortunate in having in our library.

It consists of some fifty plates, seven of them being reproductions of the same size as the original negatives, the others are enlargements made from these negatives on a varying scale of from five to eight diameters.

The resulting plates are sections of these original plates and measure with slight variations eighteen by twenty-two inches, the whole making the finest photographic atlas, in technical excellence and in the size of the plates, that has yet been issued.

The most recent photographic atlas is that of Prof. W. H. Pickering of the Harvard College Observatory. Two copies of this are in the library of the Society.

It is planned in perhaps a more systematic manner than the Paris atlas, and consists of over eighty plates, all of which were taken during the first eight months of 1901, and which are therefore more recent than any of the Paris plates.

The photographs were made in Jamaica with a telescope whose objective had an aperture of twelve inches and a focal length of one hundred and thirty-five feet. The latitude of this island, which is about 18° N., permitted the telescope to be mounted in the form of the Fraunhofer heliostat, the axis of the lens being placed parallel to that of the earth, the tube being mounted on the side of a hill with the mirror and objective at the lower end of the tube. A single revolution of the mirror about its polar axis in twenty-four hours, enabled the driving mechanism to be reduced to the simplest possible form. A similar revolution was given to the photographic plate. The mirror was silver on glass, eighteen inches in diameter, mounted in a short fork. The instrument was very successfully driven by electric motors controlled at the eye end, instead of by clockwork. A serious fault in the mirror made it necessary to cut down the aperture of the objective on most of the work to six inches, thus greatly increasing the time of exposure. With the full aperture an exposure of from 15 to 30 seconds was given, and with the reduced aperture the exposure varied from 60 to 480 seconds according to the plate used and the illumination of the moon. Seed 23*X* and 27*X*, Cramer Contrast and Cramer Isochromatic plates were used. For most of the work, the Cramer Contrast plates which are one-fourth as rapid as the Seed 27*X*, seemed to give the best results.

I have used Seed plates very extensively and find that while it is easier to get good negatives on slower and more contrasty plates, it is possible, with proper exposure and development, to get good contrast and brilliant negatives, even of difficult subjects, with the Seed 27*X* plates.

With the great number of negatives obtained in such a comparatively short time it could hardly be expected that the resulting atlas would attain the high excellence of the Paris plates, where only the best seven obtained during four years were utilized. A consciousness of this shortcoming evidently was in Prof. Pickering's mind when he enumerated the advantages his atlas possesses over any before issued. But though it has not the beauty and magnificent scale of the Paris Atlas, in thoroughness of plan and completeness of execution it far surpasses its impressive rival.

By a division of the lunar surface into sixteen parts, and photographing each part at five different phases, the lunar surface is depicted in a most satisfactory manner. It is possible to follow a certain region of the moon through these five phases and note the changes which different illumination produces.

There are many other good features in this atlas which will appeal to all who have made a study of the moon.

Prof. Pickering does not think that shading or screening of portions of the moon's image on the photographic plate is justifiable; he says that "no shading of the limb was permitted, therefore every region appears in its true photographic relation of light and shade." In another place he claims that "the plates are of such a shape that all the objects shown are similarly illuminated. Had fewer and broader plates been used many objects could not have been shown near the terminator. The similar illumination also permits the most suitable plate and exposure to be used for the whole region under consideration."

Again he says: "Sometimes the same region contains some very bright and some very dark areas, such as a bright mountain mass and a dark *mare*. In such cases, both cannot be shown to advantage on the same photograph, however the exposures, and printing can be so adjusted that every object will be shown with a suitable exposure upon at least one plate."

In the Paris Atlas broader plates are used, and shading of the limb introduced whenever a more satisfactory negative could be obtained in this way. It seems to me that the aim should be to depict the lunar surface with the accuracy of photography and as nearly as possible as seen with the eye under the best condition through the telescope.

The Paris plates are certainly excellent in these particulars and I cannot see that there is any real difference between using a number of plates with varying exposures etc., and of modifying the light so as to equalize the illumination on the one plate.

There is another difference between the Harvard and the Paris Atlas. Whereas the Harvard plates are all on the same scale, and it is almost necessary to have them this way for comparison of the five different phases; the Paris plates are on as large a scale as the negative will permit, and while the grain is rather strongly evident in many of the Paris plates, the effect at a proper distance is very good. It is doubtful if many of Prof. Pickering's negatives would bear the enlargement to which those from Paris have been subjected.

If we could have photographs as good as those of the Paris Observatory on Pickering's plan we would have the ideal lunar atlas.

The lunar photographic work of Yerkes Observatory has been in the hands of Mr. G. W. Ritchey, who has produced some of the finest lunar plates that have yet been made. In the opinion of some lunar workers of the Society they excel the best work of the Paris Observatory. Mr. Ritchey's work is important enough to devote an evening to, and at a later date I hope to present a review of it.

The Society has very recently received from the Paris Observatory seven large plates of the moon on a scale much larger than that of the former atlas. Four of these seven plates form a complete picture of the moon, each plate showing one of the quadrants. They are enlargements of 8·7 diameter from two negatives—one made at the moon's first quarter and the other at the last quarter. They are on a scale of 1·38 m. to the lunar diameter.

The other three are on a still larger scale, two being 2·77 m. and one 2·63 m. to the lunar diameter, being enlarged 16·16 diameters from the original negatives. They show the regions of the Apennines, Alps and Sea of Serenity; Maginus and Arzachel; and that of Piccolomini, Sea of Nectar and Theophilus.

On displaying these magnificent plates in the light from the arc lamp the wonderful detail is seen, and lantern slides made from them exhibit their beauties with success.

THEORIES OF WORLD BUILDING.

BY
A. P. COLEMAN, PH.D., PROFESSOR OF GEOLOGY,
UNIVERSITY OF TORONTO.

THE nebular hypothesis of Kant, LaPlace and others has long been accepted as satisfactorily explaining the origin of the solar system and of our world, starting with a "fire mist" intensely hot, in rotation and acted on by gravitation. Contraction by loss of heat is supposed to have caused annulation, and the rings to have collected into planets and satellites. The hypothesis is beautiful and comprehensive, and accounts very well for many relationships between the sun and planets, such as the plane of their orbits and also of most of the moons. From the geological side it explains the earth's internal heat and its oblate spheriodal form.

There are however numerous and serious objections to the hypothesis, both astronomical and geological. There should be, among the nebulæ, examples of systems in all stages of formation, including annulation on a general scale, but the stages are infrequent and of doubtful significance. Among the 5000 nebulæ few instances of the annular form are known, and, I believe, still fewer of a great nucleus with rings about it. Saturn's rings are known to consist of discrete particles.

The original heat of the nebula must have been intense to keep all the substances of the solar system volatile, and this implies exceedingly high molecular velocities, *e.g.*, the average velocity in feet per second, with the barometer at 760 mm., is as follows for certain gases :

	0° C.	1000° C.	4000° C.
H_2	5,653	12,240	22,363,
H_2O	1,883	4,064	7,453,
O_2	1,306	2,823	5,163.

Individual velocities may be much higher. Under these conditions, even when cold, the smallest planets and satellites have not attractive power enough to retain an atmosphere owing to the high molecular velocities. Any body has power to control molecules shot away from it only at velocities below a certain limit. Up to this point the paths are elliptic, but beyond it parabolic. On the earth's surface the parabolic velocity is 6·9 miles per second ; and it diminishes with the height and consequent increase of centrifugal force. The velocities increase of course with the temperature.

The temperature of the supposed nebula when it became liquid was probably over 4000° C., which was high enough to dissociate water. Hydrogen has so high a parabolic velocity that the present cold earth hardly retains any of it. At nebular temperatures the earth would be incompetent to retain any of its gases or of its water.

The momenta of the solar system do not correspond to the nebular hypothesis. With the rate of motion of Neptune the momentum of the nebula would be 213 times the total momenta of the solar system ; and similar calculation for a nebula extending to other planets gives quite discordant results. What has become of the lost momentum?

The geologist objects to the nebular hypothesis because it cuts down the amount of time since water could work upon the earth, giving far too short a space for the phenomena recorded in the rocks. For these and other reasons geologists require a more satisfactory theory, and hope that the new one presented by Prof. Chamberlin as the Planetesimal Hypothesis will correspond better to the facts. This is in a sense a modification of Lockyer's meteoritic hypothesis, and supposes that small particles fell together at small velocities, not causing a high surface temperature. Each particle carried its small quantum of gases as meteorites do, and these gases were entrapped in the porous mass. Until the size of the moon was reached no atmosphere could be retained.

Gravitation gave self-condensation and thus generated heat in the interior, aided by tidal kneading and chemical action ; so that at lunar size there was : 1, a dense, central, hot sphere ; 2, a zone of declining temperature ; 3, an unconsolidated, porous, cold surface. The central heat tended to drive out the gases, and gravitational pressure aided in this. The gases condensed in the outer, porous layer in the intense cold of space, as possibly they do now in the moon. The gases may have been expelled in explosive volcanic activity, producing, perhaps, the lunar craters.

As the planet grew it would hold gases better and better, and become enclosed in an atmosphere retaining solar heat, until the surface temperature permitted water to exist as a liquid, when rivers and the ocean would begin their work. On this theory the atmosphere is derived from the interior of the earth, but part of it may have been collected from wandering gases. The present atmosphere may reach outward 620,000 miles, but beyond that it would be drawn to the sun. That the original particles might be the source of our atmosphere is proved by the presence of gases in meteorites.

The heat produced by the falling together of matter of specific gravity ranging from 3·5 to 5·6 (the average of the earth) is sufficient to raise the whole mass of the earth to 6500° C., which is four times the average melting point of ordinary rocks.

The shrinkage of the earth by self-condensation would be far greater than by loss of heat, the latter providing only 600 miles of shrinkage in circumference, and that mainly in early times. This amount is far too little to account for all the mountain ranges ; hence the inequalities of the surface, such as sea bottoms, continents and mountains are better accounted for by the planetesimal hypothesis. The length of geological time also is greatly increased, which accords better with geological requirements.

There are many other points in which this theory harmonizes best with the results of geological investigation, such as the fact that the earth has always been cold on the surface since our record begins, and by the planetesimal hypothesis the internal heat may be still increasing instead of diminishing. Volcanic activity with its immense outpourings of steam and gases suggests that the atmosphere and ocean are still growing, which would be impossible under the nebular hypothesis.

The old theory made all the carbon of the earth come from the atmosphere, which must in early times have contained at least 20,000 times as much carbonic acid as now. Animal life would be inconceivable under those conditions.

The earliest known rocks are sedimentary and not igneous as the old theory demanded. Volcanic phenomena are easily explained if water and gases already exist in the rocks, and the fluid and gaseous inclusions in igneous rocks are accounted for. It is possible also to explain volcanic activity at a distance from the sea coast. The amount of water given off as steam by one parasitic cone on Mt. Etna has been estimated at 462,000,000 gallons in 100 days and the whole volcano may have given off 1000 times as much in the time, and this is explainable if the volcano is an outlet for the earth's original gases, which could never have remained there in a molten earth. The hypothesis simplifies the explanation of geological climates by furnishing a continuous source of carbonic acid to the atmosphere.

The brief comparison of the nebular and planetesimal hypothesis given above is taken mainly from the writings of Professors Chamberlin of Chicago and Fairchild of Rochester, and supplies the reasons why geologists prefer the more recent theory.

CONFIRMATION OF MR. BARR'S OBSERVATION.*

MR. Paul S. Yendell, of Dorchester, Mass., one of the best-known observers of variable stars, writes as follows:—

"It gives me pleasure to enclose the accompanying confirmatory note on the variability of Mr. Barr's star 32 Cassiopeæ, and I hope it is not too late for publication. I could not send it sooner as the evidence was not complete.

'NOTE ON F 32 CASSIOPEÆ.

'I have observed this star on sixteen evenings, my observations numbering in all one hundred and fifteen. I find the star to be variable to the extent of four-tenths of a magnitude, in the period of $7^h\ 58{\cdot}1^m$. These results are in good accord with Mr. Barr's.

'For more extended particulars the reader is referred to my forthcoming paper on the star in the Astronomical Journal.

Dorchester, Jan. 28, 1905.

PAUL S. YENDELL.' "

* Received after the previous form had been printed.

THE ENERGY OF STELLAR COLLISION.

DYNAMICAL DATA OF IMPACT.

BY

• PROF. A. W. BICKERTON, CHRISTCHURCH, NEW ZEALAND.

I HAVE thought it desirable to give a few preliminary scientific facts and principles at the basis of the new work. The gravitating energy of an infinitely diffused nebula, or of bodies at infinity, are not appreciably greater than if the dimensions of the Solar System be the basis of calculation. The difference is much less than 1%, hence when the term infinity is used, such distance may be thought of. The velocity due to the acceleration of a body falling on the sun is given in Tyndall's "Mode of Motion", and is 390 miles a second, hence it may be taken as a basis of calculation that the mean velocity acquired by the mutual fall together of stars or dead suns, will, at impact, be at least 200 miles a second. Dr. Roberts estimates the temperature produced by a collision of 100 miles a second to be 500,000° C. This is near enough for a basis of work. The temperature produced by the collision however, depends not merely on the velocity of fall, but also on the specific heat of the material, and is inversely as the atomic weight. It is also directly as the square of the velocity, hence a colliding velocity of one mile a second is $10^2 \times 60^2$ times less or a little over 1°C, while 200 miles a second is 20^2 greater, that is, 400 × 500,000°C., or 200,000,000° C. This then, may be used as the temperature of a stellar collision.

In addition to these notes on accepted data, a few preliminary notes especially connected with the theory are necessary.

Supposing a diffused nebula to have had two centres of condensation, and to have become a double star, each component being on opposite margins of the nebula, the potential energy of such a pair of stars, is obviously less than that of the diffused nebula, hence if the stars were to collide, the heat developed by gravitation would clearly not suffice to reconstruct the nebula.

Hence completely colliding stars have not gravitation energy sufficient to become a nebula. Really, were all the heat caused by a collision, used up in producing potential energy of expansion, the energy would exactly suffice to expand the colliding stars into a diameter twice that of each original star, hence it would be one-fourth the original density. This statement should have the calculation given to carry conviction, but as it is obvious that a complete collision would not be an explosion and be dissipated into a nebula, it could only result in a star of greater permanent brilliancy.

A marksman with his eyes shut, is less likely to make a bull's eye, than to hit the target, so stars are more likely to graze than to meet fair centre to centre. Stars when near attract. If the target were a powerful magnet, iron bullets would be attracted, and the attraction would make it more likely that they should hit the target, but it is kinetically impossible that such a deflection should make them hit the centre, hence grazing impacts are more likely than absolutely direct impacts. The probability of impacts is increased by attraction, and such impacts produced by attraction, must be tangential in direction.

PARTIAL IMPACT.

A pair of stars grazing one-tenth off each other at the assumed velocity of 200 miles a second, will pass each other in a scarred condition; the portions however, standing in each other's way, and actually colliding, will largely destroy each other's momenta, will be cut from the passing stars, will coalesce, and will be left behind rotating in space.

This is the first important deduction of this theory of cosmic evolution. In all grazes of stars where the collision does not cut off more than a third, a new star is produced, three bodies being formed from two. Were all the motion converted into heat, the temperature would have been 200,000,000° C. as given above. Had the stars completely collided, the velocity destroyed would have been the same, hence the temperature will be the same whether it be a partial or a complete impact. This is a fundamental kinetic fact of the Theory of Impact of supreme importance in cosmic physics, a fact that appears to have been overlooked in study of this subject. It has been shown above that the collision of ordinary stars does not make a nebula. It has also been stated, without giving the long proofs necessary, that such a collision could only diminish the density one-fourth. Although it is not essential to the theory, it can also be shown that the energy available in a complete collision is only half that necessary to make it a nebula, but although the gravitating energy of a complete collision, cannot produce an explosion, that of a partial impact can do so for the following reasons.

It has been shown that the temperature produced by stellar impacts is the same whether it be whole or partial. Obviously the amount of heat is not the same, the sum of the heat in any given material being of course the temperature multiplied by the calorific capacity. Hence if one-tenth be struck off, there will be one-tenth of the material at the same temperature.

Although the new star only contains one-tenth the quantity of the heat that it would have if it had been a complete collision, it is hot as though there had been a complete collision. The reason why the enormous energy of a complete collision is unable to make a nebula, is that the attraction of the vast mass is too great, but we have assumed that in the graze we are studying, one-tenth only, has been struck from each, hence the attraction of the new star is only one-tenth of what it would be in a complete collision. The temperature being the same, it is evident that if only a sufficiently small ratio be grazed off, a nebula must be produced. The new star is really a detonated explosive, the energy being great enough to expel every particle into distant space, and the fact observed at Lick and Yerkes that Nova Persei was an exploded star, is thus borne out by reasoning, for it has been shown that by far the greater number of stellar encounters must be of a grazing character, and we now see that a slight graze must be an explosion. When the grazed portion is over one-third, reasons have been urged in the "Romance of the Heavens"* why whirling coalescence ensues. The nebula in this case is not dissipated into space, but will form a permanent revolving nebula that will ultimately coalesce into a stellar system.

The effect of impact may be shown in another manner. Heat is molecular motion, and for each molecule, a certain tem-

* By A. W. Bickerton, (Swan, Sonnenschein & Co.: London, 1901.)

perature gives a definite velocity. Thus according to Sir Robert Ball, hydrogen at 200° absolute, moves one mile a second, at 200,000,000° the estimated temperature of a stellar impact it would be 1,000 miles a second as the temperature varies as the square of velocity.

The molecular velocity of gases is inversely as the square root of atomic weight. Hence oxygen will move a quarter the rate of hydrogen, that is 250 miles a second, and heavier elements slower still. There is a certain velocity associated with cosmic bodies known as the "Critical Velocity". It is the speed acquired by a body falling on it through infinity. It is 1½ miles a second for the moon, 7 miles a second for the earth, and, according to Tyndall, 390 miles a second for the sun. Authorities slightly differ with regard to the sun, some giving as low as 378 miles a second. Looked at another way, bodies shot from these cosmic spheres at these velocities, would be able to escape them entirely, that is to say, they would not fall back upon them.

In a complete collision the molecular velocity produced, would average below the Critical Velocity, because of the vast mass of the new body, but in the case of a small graze, the molecular velocity of the atoms would be the same as in a complete collision and might obviously be above the Critical Velocity. We assumed the gravitating velocity acquired by stars at impact to be 200 miles a second. Were all this converted into heat, the molecular velocity would also be 200 miles a second, but were only one-fourth so converted, and the remainder three-quarters, turned into rotation or in any other way lost, the velocity of the molecules would still be 100 miles a second, as the energy of 200 miles is four times that of 100 miles.* Hence the mean molecular velocity at a stellar collision would certainly average 100 miles a second, but were the new star only the mass of the earth, then 7 miles a second would suffice for complete dissipation. So that according to the dynamical theory of gases, a graze must produce dissipation, in other words, an explosion, in which a star is blown into independent molecules. Hence it is absolutely certain that did a grazing impact of dead suns or stars occur, a new star must appear in space, increasing enormously in brilliancy for

* With greater masses than here assumed, say 5,000 times in excess of that of the sun, the velocity of impact would be proportionately greater.—Ed.

some time, and ultimately dissipating into space. Such a new star would almost always have a double lined spectrum, due to the recession and advance of the two scarred bodies that grazed each other and for a time the middle body would produce a continuous spectrum. Thus it is seen that on dynamical grounds a grazing collision must certainly produce a temporary star having all the characteristics common to such bodies; and the spectra of such bodies coincides in every case with the spectra that was deduced 13 years before astronomical instruments were powerful enough to detect their character. Hence it is almost certainly the case that temporary stars are the result of grazing impact, and astronomical writers admit that the irresistible logic of facts has entirely dissipated every current theory of their origin, leaving the theory of "Partial Impact" alone competent to explain their genesis.

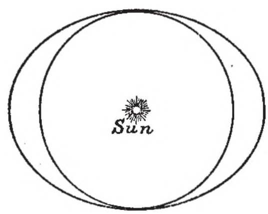
ELLIPTICAL SOLAR HALO,

OBSERVED IN SIMCOE CO., ONT., MAY 31, 1904.

BY

A. F. HUNTER, M.A., BARRIE, ONT.

AT 9.45 a.m. I first observed parts of both curves—the circle and the ellipse—at the right hand side. By 10 o'clock the ellipse had become intensely bright all around the sun, and the circle was less bright, the intensity being greatest at the top and the bottom where the two curves overlapped. By



11.20 a.m. the halo was clouded over, and only the circle showed very faintly through the cloud film. No trace of mock suns or other similar formations at the sides could be seen at any time. As the mock sun formations at the right and left are much the commonest form of solar halo, I think this unusual elliptical form is worthy of note. It may be added that the cyclone (of which this phenomenon was the forerunner) was of very general or widespreading form, as rain began to fall at 9 p.m. and continued with some interruptions for two days.

"MAN'S PLACE IN THE UNIVERSE."

BY

J. R. COLLINS.

WHEN man's knowledge of the universe was thought to be complete, the Ptolemaic theory of astronomy taught him to regard the visible universe as having no great dimensions. The second crystalline heaven itself was not so very far away. The earth, the central point of the system, quite properly, from this point of view, was the abode of man, Nature's chief and overseer—this was apparent to all. But when Copernicus had pushed the earth from its central position and forced the suggestion of other worlds and other suns; and when Galileo with his telescope, exhibited the moon in colossal proportions, revealed Jupiter with his whirling satellites and showed Saturn with his mystic rings; then philosophers awakened from their dreams of perfection, beheld with startled apprehension their beautiful and carefully-wrought crystal spheres shattered, and were appalled to find that the heaven of the fixed stars as well as the *primum mobile* had been flung back into infinity itself. Later when Henderson, Bessel and Struve had found a parallax for some of the so-called fixed stars, the distance indicated was so vast to the average mind as to seem utterly appalling,—α Centauri the nearest star, 138,000 times the diameter of the earth's orbit from us: 61 Cygni twice as far and others too far away to measure. These determinations at once enforced the idea of immensity and infinity. A German poet voiced the general sentiment when he said "end to the universe of God there is none". Men spoke of, in mathematical affairs, "infinite numbers": are not the stars a practical illustration of this? The proper motions of the stars as far as they could be traced were indicative, in general, of motion in every direction, resembling the molecular behavior of the particles of a gas, apparently without system or general plan. During all this time the human mind became reconciled to the new immensity by taking refuge in the thought that the moon, the planets, the stars—and even the sun—were the abode of living creatures of every possible variety, including the human, and in more congenial climes, beings probably more highly organized and developed than those found upon the earth. It is doubtful whether much attention would have been given to the subject of astronomy since the 15th century—by the popular mind at any rate—were it not for this idea or fancy. Popular expositions of astronomy and literary essays of the period were full of it. The great excitement created by what has become known in history as "the great astronomical hoax" is in the memory of many still living, namely, the announcement that Herschel's telescope just erected at the Cape, had revealed, through the clear atmosphere of South Africa, large buildings and figures like men walking and scurrying about on the surface of the moon. This was readily believed and it was with much reluctance that Herschel's denial of the truth of the statement, was received by the general public. Those gifted with "second sight" were sure that the moon, all the planets and the luminous sun, were the abode of happy beings like unto ourselves. Almost every philosopher discussing the subject urged that analogy bore this out: it must be so, else such tremendous waste was a libel on the Creator or on Nature, or on both. In fact it was generally regarded as the height of absurdity, born only of human egotism, to suppose that each and every body of planetary size in space could be anything else than a supporter of organic life of some sort. But gradually it became apparent that the rapidly advancing subject of astronomical physics gave this view less and less support. Nearly all the other planets in the solar system were found to have a density scarcely greater than water. The moon had practically no atmosphere and the sun appeared to be of a temperature unapproachable by any terrestrial furnace, and as to the stars it was uncertain what their physical condition was. Compte, founder of the Positive Philosophy, at this juncture, assured all men that it was an utter waste of time for serious persons to speculate about the nature of the sun or the physical composition of the stars, as these were questions completely beyond the power of man to solve, past, present or future. The echo of this statement, however, had scarcely died away before the spectroscope was made to flash out those lines which have made it possible—as Lord Kelvin once expressed it—for man to know more about the physical constituents of the sun and stars, than of the earth on which he lives.

Spectrum analysis and the other methods of determining the physical conditions of celestial bodies have shown it to be extremely doubtful, if not impossible, for life in any form to exist on the sun or any of the planets in the solar system but the earth and possibly Mars. All the luminous stars and nebulæ appear to have gone by the board or have vanished as "castles in Spain" when considered as probable supporters of organic life. But then it was urged that the stars though not the abode of life themselves are doubtless suns illuminating other planets swinging round them, and life may safely be predicated there. Algol with his dark companions, the host of variables with fluctuating light, and many other stars with swaying motions show that many dark bodies undoubtedly do exist. But now it appears that these dark bodies have a density—like most of the solar planets—scarcely as great as water, and are in such proximity to their primaries as to be subjected to tidal and heat action prohibitive of organic comfort. Then again double and multiple stars are rapidly being found to be the rule rather than the exception, and any satellites attending such would shortly pound each other to pieces, if they ever existed. Any stellar classification, Secchi's or those more recent, seem to indicate the solar type of stars typical only of some such physical condition as that existing within the solar system, and these are but 2% of the visible stars, and most of them appear to be binary or multiple, so that the rapid advances of our new astronomy would appear to suggest that by far the greater number of the heavenly bodies, are at present tenantless.

With all this, and a great deal more, before him, Alfred Russell Wallace has essayed in his interesting book *Man's Place in the Universe* to provisionally substantiate the view that the earth is probably the only inhabited or habitable material body in the universe.

Of course it may be said that what has been stated here may possibly be true for the "visible" universe, or that part of it that has thus far been explored by man, but is not the universe infinite in extent and therefore infinite in possibilities?

During the period just sketched, the idea of infinity as regards the number of the stars and the extent of the universe had developed, but of late years, not a few eminent astronomers and physicists have found reasons for questioning this. It is recognized that the Milky Way encircles the star sphere and groups and clusters appear to abound in it. The main body of this luminous belt shows stars of diminishing density the deeper it is penetrated, whilst the stars thin out towards the poles of the Galaxy, where nebulæ tend most to abound. Groupings of certain characteristic stars suggest—somewhat vaguely it may be —that the entire stellar collection is a finite cosmos, full of systematized groups, streams, drifts and whirls, in some way physically connected, rather than an infinite chaos. Evidence of this has been accumulating for some time. Herschel, Gould, Ristenpart, Newcomb, Roberts, Gill, Sidney Waters and many others have found evidence favoring the view that our sun is a member of one such group, though the distances separating the individuals of the group is great, they are near each other in comparison to the distances of stars beyond the group. Evidence of systematic drift is accumulating and though the general motions at first appear—because of our perspective view—haphazard, rotations of groups and streams appear to be developing. Notwithstanding the fact that the sun's proper motion in space does not show any appreciable deviation at the rate of say five miles per second from a straight line, it has been shown * that a mass slightly greater than 16 million times that of the sun would possess gravitational influence sufficient to hold the runaway star Groombridge 1830, in an orbit having a radius twice as great as the distance from the sun to α Centauri ($4\frac{1}{3}$ light years).

The fact has been for some time noted that the average number of stars seen in any part of the sky, does not increase in proportion to the increase of aperture or light grasp of the telescope or photographic plate, which should be the case were the stars infinite in number. Dark nebulæ, meteoric dust, dark bodies in space or some absorptive characteristic of the ether, it has been urged, may account for this anomaly. The "coal sacks" and dark patches and rifts in the Milky Way, also, it has been

* Monthly Notices R. A. S.; 1902, Vol. LXIII, p. 56.

thought, may be due to this assumed obstructive or absorptive effect. But we must remember that though these regions are dark through scarcity of stars, still a number of stars are found there characteristic of regions generally assigned to the outermost limits of the Galaxy or beyond it, and of course it is needless to say that if dark matter or obstruction existed in sufficient quantity to screen the nearer stars from our view, the stars further away should not be seen at all.

Lord Kelvin in a recent calculation * has shown, that if we assume a finite spherical universe having a radius of about 3215 light years with material equivalent to 10,000 million suns, distributed with approximate uniformity through it, the gravitative effect of the material within such a sphere would be sufficient to produce, within the period of say 25 million years, an average stellar velocity of several hundred miles a second. If, however, 1,000 million suns only were within this sphere, then the mutual gravitative effect would produce velocities averaging 20 miles per second, and as 20 miles per second is apparently about the average stellar velocity, it would seem rational to conclude, provisionally, that 1,000 million is not very far from the number of stars that actually exist in the universe. The ether itself may be limited, though limitless space may exist beyond its boundaries. If we accept anything like Kelvin's theory of the nature of the ether and the constitution of matter—the latter being regarded as a specialized form or condition of the ether itself—there would be no danger that bodies having any finite velocity within the sphere, would pierce its boundaries and escape into absolute space beyond : for if the ether be anything like perfectly elastic, a boundary surface would necessarily act as a perfect reflector for runaway stars, or for ether waves or energy of any kind that it is capable of transmitting ; and thus the energy within the system would be conserved. There are many reasons beside these, that have led not a few astronomers and physicists to provisionally regard the universe as finite in extent, and this view may be said to be gaining a philosophical basis on a plane differing essentially from the mediæval or Ptolemaic concept. Dr. Wallace's book shows a profound acquaintance with astronomical problems in

* Phil. Mag., Jan. 1902, and Pub. Smithsonian Institution, 1904.

general and gives evidence of no hasty or superficial knowledge of the subjects discussed. His eminence as a biologist has enabled him to speak with unusual force and clearness on the delicate balance of complicated physical conditions necessary for the development and maintainance of organic life, and if his arguments are not convincing to every one, it is safe to say that the vast majority of his astronomical readers will acknowledge the book to be one worthy of careful perusal and that it deserves a place on the shelves of every astronomical library.

When Dr. Wallace's first—perhaps somewhat meagre—magazine article on the subject appeared, it was met with a storm of criticism, but recent reviews of his book show that astronomers generally now—with his elaborated views before them—accede a liberal commendation of his labors. The work has become so popular that the author has found it necessary to issue a new and popular edition containing an extra chapter dealing with an additional argument dependent on the theory of evolution.* As regards the method of discussing the always interesting though speculative problem of the pluralty of worlds, this book will doubtless come to be regarded as an epoch marking one.

Though Dr. Wallace appears to regard the meteoritic as being more acceptable to astronomers than the nebular hypothesis, he states in a communication to the writer: "To me it is simply a choice between *two* working hypotheses—which is the best—till one better than either is found, I adopt the meteoritic."

* McClure, Phillips, & Co., New York, and Chapman & Hall, London.

THE SHELBURNE METEORITE.

BY

L. H. BORGSTRÖM, PH.D.,

MINERALOGICAL LABORATORY, SCHOOL OF MINING, KINGSTON, ONT.

THE FALL OF THE METEORITE.

THE interesting phenomenon of a falling meteorite occurred at 8 o'clock on the evening of August 13th, 1904, near the village of Shelburne, Grey County, Ontario. The exact locality is Lat. 44° 7′ N., Long. 80° 11′ W. The meteorite was observed in the country between Lake Ontario and Lake Huron for a distance of more than 130 km. (80 miles) from Shelburne, as a meteor of great brilliancy and as a ball of fire followed by a streak of light, which shot with great velocity through the atmosphere nearly perpendicularly or slightly inclined towards the west. In the immediate vicinity of the place of impact, the fire-ball itself was not seen, but a strong flash of light illuminated the surrounding locality. A peculiar sound ("distant thunder", "boom of cannon", "drumbeats", "musical tone", "hissing sound", etc.) accompanied the fall and was heard over an area of 50 km. (35 miles) radius. One of the accounts of the fall tells us that the meteor left behind it a trail of smoke, which remained a little while after the meteor had passed. As a similar phenomenon has been observed accompanying the fall of other meteorites, this communication deserves attention although other eye-witnesses did not notice it. Further details of the fall are to be found in the following original reports from different places.

GRAVENHURST.—Mr. J. S. St. John says that the light of the meteor looked like an immense curtain of dark green across the southern sky, of intense brilliancy near its centre. No explosion was heard at this place.

Mr. T. M. Robinson of the Meteorological station, Gravenhurst, writes : "On the evening of August 13th I was away from home but on my return I was told that a meteor of great brilliancy had been seen at 8 o'clock or soon after. It was a ball of fire that made a flash like lightning and was going to the south-west, with an altitude of 30°–40°. One of the persons spoke of a hissing sound which the others did not appear to have heard. The sky was cloudless, with distant lightning in the south and north."

LAKE ROSSEAU.—T. M. Robinson. The meteor came into sight with a great flash of light and sparks flew from it as from a rocket. It descended nearly vertically and reached the horizon a little west of south from the place.

VAN VLACK.—"Elmvale Lance". A flying meteor was one of the attractions at the beach on Saturday evening August 13th.

COLLINGWOOD.—Geo. A. Carefoot. The meteor was seen from a steamer on Georgian Bay, 5 miles N. E. of Collingwood. When it was first seen it was east and somewhat south, and it passed west and slightly north. It was so low that the passengers expected to see it strike the water but it vanished over the hills to the westward. It appeared like a ball of fire, yellowish in color. It did not leave a streak of light, but a little flame seemed to be just behind it. A slightly muffled explosion was heard just as the meteor was first seen. This noise drew attention to it. The time was between 8 and 9 o'clock p.m.

NEWMARKET.—Geo. A. Carefoot. A person in Newmarket saw the meteor a little after 8 o'clock. It came from the east and passed west a little towards the north. It appeared to be a little north of here, and was very low, almost seeming to strike the ground just west of here. It appeared to him as a streak of light. He noticed no noise.

TWO MILES NORTH OF NEWMARKET.—Geo. A. Carefoot. A young man sitting in his house a little after 8 o'clock noticed a light and hurried outside to ascertain the cause. The meteorite was passing directly over his head and went west by north. It was very low. The meteor left a trail of smoke behind, which was visible for some time after the meteor itself had disappeared. The meteor appeared as a ball of fire with a short tail of flame after it. The light was a little more yellow than that of a coal-oil lamp.

OWEN SOUND.—G. A. Ferguson. The meteor presented the appearance of a fire-ball and was thought to have been a sky-rocket. The direction was south-east.

OWEN SOUND.—D. Whyte. The meteor was seen at Owen Sound in the twilight, illuminating the surroundings like a flash of lightning, passing north to south.

MARKDALE.—Mrs. Thos. Taylor saw a ball of fire darting towards the earth and heard at the same time a low rushing or rumbling noise.

CHESLEY.—"Chesley Enterprise". About 7 o'clock Saturday evening some men in this place saw a meteor shoot through the eastern sky and fall apparently near this village. It was like a ball of fire until it came near the earth, when it split into fragments making a display like fire-works.

DURHAM.—"Durham Review". Our correspondents mention the flight or explosion of a meteor last Saturday about 8 p.m. It was accompanied by a rumbling sound as of distant thunder or of a ricochetting rifle ball, only greatly intensified.

WALKERTON.—A. C. Day. The meteor had the appearance of a fire-ball having a streak of light behind it.

PRIMROSE.—R. Murphy. "I saw the flash of light, then followed three reports, which were not like thunder. I heard something strike the gronnd only a few rods away. I heard other pieces fall in the neighboring field. None of these were found. The explosion seemed to be in the northeast. The time from this explosion to when I heard the pieces strike the ground would be about two or three minutes. The place is about three miles east of where the pieces were found."

STRATFORD.—J. H. Lennox. The meteor appeared north by northeastward of Stratford and was going downward towards the northwest. It was somewhere about 9 o'clock. One person described it as a "red ball with a long tail"; another said that it seemed "to go straight down."

SHELBURNE.—"Shelburne Free Press." "A peculiar phenomenon visited this locality between 8 and 9 o'clock Saturday evening. The first intimation was like the boom of two cannon in succession, then a rally, like drum-beats, musical in tone but the vibrations lasted much longer than usual. The peculiarities of the musical sound were discussed on every side and many ideas were expressed on this question. At the same time the northern sky was bright red and the western horizon was a dense black cloud. From recent development the noise is supposed to have been caused by the rapid travelling of the meteor through the sky, that fell within two feet of the verandah of John Shields' residence on lot 8, con. 2, Melancthon Tp. and sank into the ground splattering mud all over the windows and side of the house. The fragment of stone no doubt fell straight down, as the hole where it embedded itself showed no indication of any curvature whatever."

GO HOME.—Dr. C. A. Chant of the University of Toronto witnessed the fall of the meteorite from Go Home about 65 miles north-northeast of Shelburne. To him the meteorite appeared to move nearly vertically, tending a little, perhaps 10°, to the west. The time was almost exactly 8 o'clock.

GLEN ALLAN.—According to a letter from Dr. C. A. Chant, the meteor was seen by Mr. J. L. McPherson, B.A., from Glen Allan. The meteor appeared north-northeast from him and he observed it as moving slightly to the west.

TORONTO.—Mr. J. R. Collins, Secretary of the Royal Astronomical Society, reports that the meteor was seen by several parties in Toronto and that their observations seem to indicate that the meteorite fell nearly vertically or a few degrees to the north-northeast of the perpendicular.

SHELBURNE.—Two men who saw the fall of the meteorite from the village of Shelburne say that they perceived no fire-ball but only a flash of light of a reddish color, which lighted up everything around. The flash was followed by several explosions, separated from one another by short intervals and more distinct than thunder.

HORNING'S MILLS.—Mr. T. Ostic was standing on a little hill about three miles north of the place where the meteorites were found, and observed, at about 8 o'clock, a sudden light over the northern sky. Immediately after the light he heard a whistling noise, and then four cracks like the striking of a drum, but loud like gun shots. As he heard the reports the light was brighter and seemed to throw out flashes and sparks. It was not a minute between the first light and the cracks.

SHELBURNE.—Mr. J. Shields and his family were in their house on the evening of August 13th and were alarmed by a loud thump which shook the house. Early the following morning the explanation to this phenomenon was found as they observed that a meteoric stone, weighing 13 lbs., had fallen on his premises not more than two feet from the house. The stone had struck with such force that it penetrated nearly two feet into the ground. Mr. Shields states that the meteorite cannot have been hot when it struck, since it carried with it into the earth a large burdock leaf, which, after he had dug it up, was still green and quite uncharred.

SHELBURNE.—" Shelburne Free Press," Sept. 1, 1904. "The excitement over the Shields meteorite was dying out, when another discovery was made that will attract attention for some days to come. On Tuesday of this week an excellent specimen of meteorite was found on the farm of Thos. Johnston, lot 10, con. 2, Melancthon. George Johnston who works his father's farm, was home on the evening of August 13th last and was a witness of the phenomenon that then occurred, and felt certain at the time that something had fallen in his oat-field. The field was a large one and he did not tramp down his oats in search of the meteorite but waited until the grain was ripe. On Tuesday he started to cut the grain and he kept a sharp lookout from his seat on the binder for any evidence of it. About 10 o'clock the machine passed over a freshly made hole and Mr. Johnston called his brother-in-law, Wm. Fleming, who was stooking the grain, to examine it. The spot was on the side of a knoll and the soil being light and easily removed, Mr. Fleming soon reached the meteorite which was buried two feet in the ground. The meteorite is of the same material as the Shields find and is much larger, weighing 28 lbs. The shape of the stone is very much like a plough-shear, tapering off to a flat point which was downward in the earth. There is not the slightest doubt that this is a part of the Shields meteorite and fell at the same time, Aug. 13, 1904. The place where this stone was found is northwesterly from the Shields homestead and distant about half a mile. This find backs up our theory, which is in opposition to some others, that the meteorite when the explosions occurred, was travelling in a southeasterly direction."

THE VELOCITY OF THE METEORITE AT THE TIME OF IMPACT.

The depth of the holes which meteorites make in their fall can give us a fair estimate of their velocity during the latter part of their flight. Military experts have found by numerous experiments that a definite relation exists between the depth of the impression of a bullet and its weight, diameter and velocity. They have succeeded in expressing this relation in a mathematical formula which can aid us in the calculation of the velocity of meteorites, For this purpose we write the formula thus :

$$v = \sqrt{\frac{a}{b}\left(e^{\frac{2b R^2 \pi x}{m}} - 1\right)}$$

where R is the radius, m the mass of the meteorite, e the base of the natural logarithms, x the depth to which it penetrates the earth, and a and b empirically determined constants. The formula is calculated for a spherical shot. Though meteorites are generally polyhedrons, we are obliged to use this formula since it is not possible to express in numbers the effect of variations from the spherical form. This calculation is not applicable to tubular bodies and others which are very far from a spherical form. In any case we must remember that our estimation is only approximate.

The constants a and b are not exactly known for the soil in Shelburne but in Crantz we find constants for *Dammerde* *, $a = 700{,}000$, $b = 42$, and these would apply approximately to the soil in question. A sphere with the specific gravity of the Shelburne meteorite, 3·5, and the weight 12·6 kg. has a radius of 0·096 m. and a mass of 1·28. For the smaller stone $R = 0{\cdot}074$ and $m = 0{\cdot}61$. The holes were respectively 0·55 and 0·40 m. deep. With these numbers the formula gives a velocity of 172 m. per second for the larger stone and for the smaller 165 m. per second. If we should use the constants a and b for "sand mixed with clay" the velocities would be somewhat lower.

* C. Crantz, "Compendium der Theoretischen äusseren Ballistik." Leipzig, 1896. *Dammerde* is a finely pulverulent soil.

In an earlier paper* the writer has shown that the celebrated Schiaparelli's calculations of the movement through the atmosphere of fire-balls and falling stars contain a method for the estimation of the velocities of meteorites. According to his theory, an iron meteorite must exceed 8000 kg. in weight if it is to retain a velocity of 1000 m. per second at the earth's surface independent of its initial velocity on entering the outer limit of the atmosphere. The stony meteorites are more easily retarded. The velocity of ordinary-sized meteorites (not exceeding a few hundred kilogrammes) is brought down to a certain minimum by the resistance of the air. This resistance can be expressed by the following formula :

$$W = 0{\cdot}014\, R^2 \pi v^2,$$

where R is the radius of a spherical body, v its velocity and W its resistance.

In approaching the earth, the meteorite is under the increasing influence of the earth's gravity. The force with which the earth attracts the stone is its weight P. Gravity accelerates the velocity of the falling body. If its force is greater than the resistance of the air, the velocity of the meteorite will increase, and if the resistance exceeds, the velocity will decrease. The velocity remains constant when P and W become equal. For a known body we can calculate the velocity at this stage. For if

$$P = W = 0{\cdot}014\, R^2 \pi v^2,$$

then

$$v = \sqrt{\frac{P}{0{\cdot}014\, R^2 \pi}}$$

The formula represents the velocity of a body with the weight P and the radius R moving under normal conditions (760 mm. barometric pressure and $g = 9{\cdot}81$). For the Shelburne stones P and R are known (p.74). The values give for the larger stone a velocity of 177 m. and for the smaller 157 m. per second.

There is an evident conformity between these numbers and those calculated from the depths of the holes. The writer has already pointed out a similar coincidence in the figures for the

* Die Meteoriten von Hvittis und Marjalahti. Helsingfors, 1903.

Hvittis meteorite namely 182 m. and 178 m. per second. The differences are indeed not greater than those attributable to errors in the calculations resulting from the adoption of a spherical form for the body and the supposition of normal conditions during the fall. The probable error can not exceed at most a few meters per second.

The fact that the results by both methods of calculation, agree closely, confirms the theory that meteorites do not reach the earth with a higher velocity than they would acquire under the influence of the earth's attraction alone.

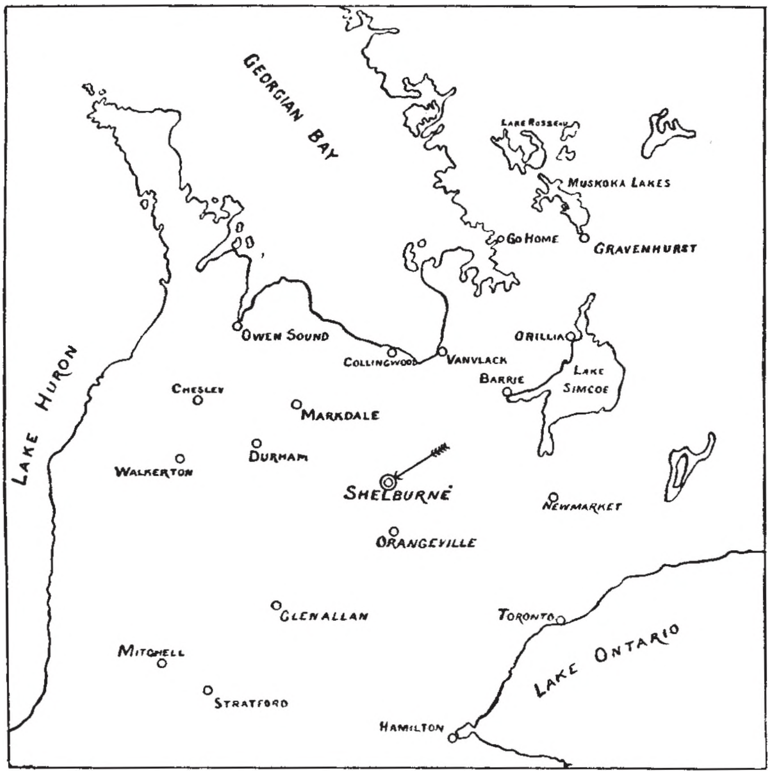
SIZE AND SHAPE.

The larger of the Shelburne stones weighs 12·6 kg. (27¾ lbs.) According to a report in "The Shelburne Free Press" the other weighed 6 kg. (13 lbs.)

The shape of both the stones is similar, but the smaller has a less varied surface structure than the larger. As the writer has had an opportunity of studying the larger one more closely, a detailed description of this one will be given. The stone is an irregular polyhedron. The edges are rounded and at many spots deep pittings are to be observed, but the general outline is that of an angular fragment. None of the enclosing faces is flat. They are all slightly concave or composed of two or more concave parts. If anything in the shape can distinguish this meteorite from most others, it is this predominence of the concave form. It gives the stone a peculiar aspect, so that, for example, a quick glance at fig. 1 on Plate II would give the impression that it is the photograph of an unfinished flint weapon of the stone age. A better idea of the shape of the stone will be obtained by comparing the different reproductions rather than from any verbal description.

THE SURFACE STRUCTURE.

About half the stone is covered with comparatively even faces as shown in fig. 1, Plate I. The other faces are filled with pits and hollows. The face down to the left in the figure is composed of numerous facets of shallow concave form. These are of various sizes but commonly from 2 to 3 cm. in diameter and from 1·5 to 2·5 mm. deep. It is difficult to decide whether or not



MAP OF PORTION OF WESTERN ONTARIO.

SCALE $\frac{1}{3.300.000}$.

PLATE I.

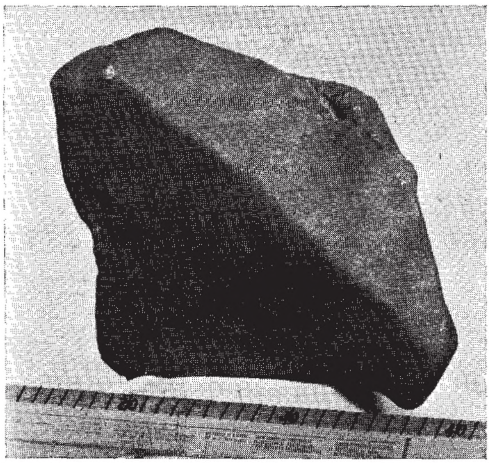


FIG. I.

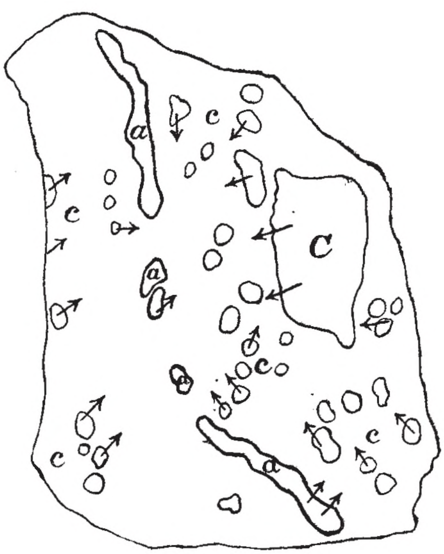
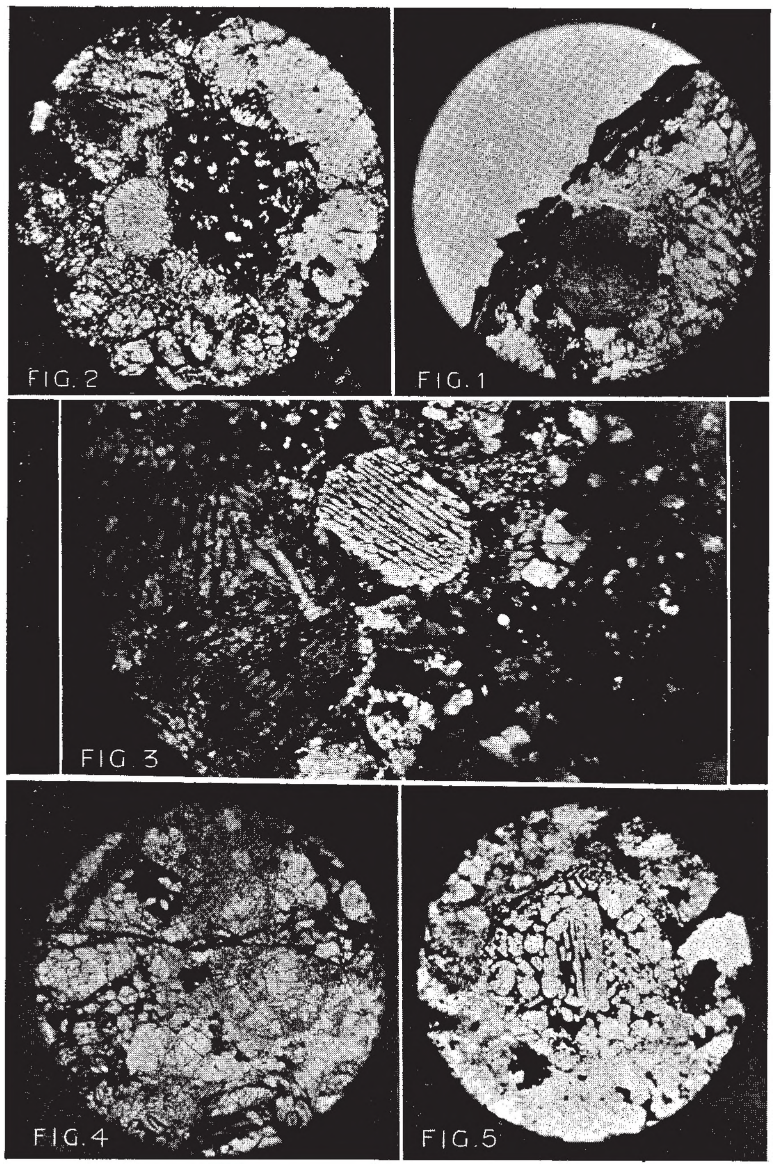


FIG. 2.
DIAGRAM OF THE METEORITE. THE SAME FACE AS ON FIG. I, PLATE II.

FIG. I.

PLATE II.

FIG. 2.



MICRO-PHOTOGRAPHS OF SECTIONS OF THE SHELBURNE METEORITE.

these are to be considered as the remaining marks of original pittings, which once covered all this face and which later have been eroded and worn away. To the writer it would seem that the facets on this face represent the fractured surface of detached fragments, which under some violent action have split away leaving flat cup-shaped depressions. Later the surface has been smoothed and the edges rounded or made to disappear. Apparently at the same time the surface has become coated with a crust.

The face pictured in fig. 1, Plate II, shows a large number of depressions of different sizes and shapes. To make an orientation easier, a diagrammatic sketch fig. 2, Plate I, has been made. The long furrows or series of furrows indicated by *a* in the sketch occur along a metallic vein which can be detected sometimes at the bottom of the depressions, sometimes on their sides. The vein in these is nearly at right angles to the general direction of the surface. The hollows following the veins are up to 8 mm. deep. The large depression marked *C* is also connected with a metallic vein. This vein makes a very acute angle with the surface of the stone. It seems as if a part of the stone, which formed a wedge between the old surface and the vein, had been broken out, since the comparatively even left half of the bottom of *C* consists largely of metal and this surface is a direct continuation of the vein, which can be traced in the bottom of the hole. The small and irregularly distributed pittings *c* do not seem to depend on the veins. They have a more or less extended oval form and are as a rule shallower than the holes following the veins.

The view of the meteorite, which fig. 2, Plate II, presents, is extremely rich in pittings. Some of the largest are situated along veins, as is the deep one at the lower end of the picture. The majority seem to be of the same nature as the pittings *c* in the diagram. They resemble the "thumbmarks" which are so typical of meteoric irons. They rarely exceed 2 mm. in depth, and 2 cm. in diameter. They are crowded together on the upper half of the face represented, but no difference in the composition of the stone can be found, which would account for such differences in the development of the surface.

The shape of the stone and the relief of the surface indicate that it is a fragment of a larger body, and that the surface is due to denudation and not to accretion. The cause of the pittings which are connected with the metallic veins, can be easily understood. As the surface became heated in the upper part of the earth's atmosphere, the iron conducted the heat inwardly more rapidly than the silicates. The coefficients of expansion for iron and for silicates are unlike. Therefore at the areas of contact of iron and silicates, difference in expansion on heating would arise, causing strain, which would produce cracks, and break loose pieces of the meteorite. The chief agent which has excavated the holes is not to be found in this process, for the furrows along the veins are so much broader than the veins themselves, and although there are many pits situated along the veins there still are many parts of the veins without pits. The similarity in general features of the furrows along the veins and the other pits, indicates that the agent which formed them all, was one and the same. It can safely be supposed that this was the friction of the air as the meteorite traversed the atmosphere. In the case of many iron meteorites it has been shown that hollows on their surface are places where there has been troilite and that this easily-oxidized and brittle mineral has been destroyed, leaving hollows in the shape of the former nodules of troilite. In other cases, as for example the Algoma meteorite,* schreibersite is the component which caused the holes. On some irons the hollows and pits are much more numerous than the troilite or schreibersite nodules in the interior of the mass, and some authors have gone so far as to presume that these meteorites had been richer in troilite on the surface than in the interior. For the stones, the structure of which is much more complicated, it has not been possible to find such a causal relation of the components to the pits. The Shelburne stone shows however a certain connection between the metallic veins and a number of hollows. According to the foregoing, it seems right to assume that the mineralogical differences both in the case of iron and of stone meteorites were only of secondary importance in producing the surface structure. The main cause was the chemical and mechanical (mostly mechanical) action of the air. Special components have only directed this force to certain spots, and a depression once formed has given a starting-point for a greater development at that spot.

* W. H, Hobbs, Bull. Geol. Soc. of Amer., Vol. 14, p. 97.

THE CRUST.

The whole of the surface of the meteorite is covered with a thin uninterrupted black crust, as is usual with the grey chondrites. Here and there interrupted ridges, which are outcrops of veins, can be followed around the stone. Even on the bottom of some of the furrows the veins appear as distinct ridges. These ridges consist of metal covered with a very thin dull crust. By shaving with a knife-blade the nickel-iron becomes visible. The ridges may be as much as 1 mm. broad and their height over the surrounding surface generally falls below 0·3 mm. The crust of the Shelburne meteorite, with the exception of the side pictured in fig. 1, Plate II, is so thin and adheres so closely to the underlying mass that often the underlying minerals and chondrules can be distinguished on the surface of the crust. Small protuberances which project a few tenths of a millimeter above the general level of the surface are spread over the stone. These are metallic grains covered with a thin crust like that on the veins. The larger part of the crust has a dull lustre not distinguishable from the crust on the metal. This dull crust must therefore correspond to the chief constituents of the stone, olivine and chondrules of olivine and glass. Some lustrous spots of rounded, (often perfectly circular) form are irregularly distributed over the surface and are sharply outlined from the surrounding parts. One of these areas was detached and investigated, and the underlying mineral was recognized as an enstatite chondrule. The crust adheres tenaciously to this chondrule as well as to the other components of the stone.

The face represented in fig. 1, Plate II, possesses a thicker crust than the other faces. Here it is impossible to recognize the underlying minerals. The evidence of fusion of the crust are more apparent on this side. A lustrous zone surrounds some of the pits and extends out over their borders. It seems as if a part of the molten crust has been blown out from the pit over its sides. The formation is more distinct along the border of the face and always occurs only on one side of the hole, indicating that the blowing gas-current intruded from the opposite side. On the diagram, the points where these evidences can be detected, are indicated by arrows, which also give the apparent direction of flow.

A glance at the diagram shows that these arrows (and accordingly the gas-currents) are directed from the circumference of the face towards its centre.

We have found that the crust on one side is different from that on the others, and that the air-current, which modelled this face, moved from its outer limits towards the centre. These results allow us to suppose that the stone had a definite position as it moved through the atmosphere and that the indicated side was the rear one. The Shelburne stone is therefore an "oriented meteorite" although this feature is not so prominently developed here as in many other meteorites.

A microscopical investigation of the crust gave the interesting result that the surface of the stone is built up of four different zones:—(1) The outer black crust, (2) a thin brownish layer, (3) a colorless layer, (4) a zone filled with opaque particles. The exterior layer is that which appears on the surface of the meteorite as the crust. This layer is always present, being sometimes the only one developed, and must therefore be considered as the proper crust. Its thickness is very variable, mostly in the neighborhood of 0·1 mm., but in some places it reaches twice this amount. This layer is opaque and no crystalline material, not even any metallic grains, can be distinguished in it. On the outside it shows rounded forms, and in the outer part rounded pores are common, giving the impression of an opaque, slaggy glass. This zone corresponds to the "*eigentliche Schmelzrinde*" of Tschermak. Often there is no sharp line of demarcation between the first and the second layer, so that in some places it seems as if the latter were only the inner part of the crust, which is cut to a thin wedge and in this way has become translucent. In other places the boundary is distinct and the second layer sharply defined as a special formation. This second layer consists of a seam of brownish glass only 0·02 to 0·03 mm. thick. The glass is however seldom isotropic, but has a very low and irregular double refraction. In a few cases it can be observed that the double refraction is due to numerous fine crystal needles or microlites, so that the whole of this layer seems to consist of numberless interwoven crystals. There is a marked contrast between this layer and the following or third one, which is built up of grains of the same silicate minerals as the interior of the stone. Where the line of a surface passes through a chondrule, this chondrule extends to the outer side of the third layer and is here sharply cut. This is also the case with larger crystals of olivine. Very rarely a metallic grain penetrates the third layer and occupies a position closely adjacent to the brownish layer or to the black crust. Where the opaque minerals are near the surface there is, as a rule, at least a thin layer of silicates between them and the crust. The third layer is from 0·02 to 0·10 mm. thick. Its constituents are seemingly unaltered grains of olivine or enstatite. In some places these silicates have apparently been pressed out to form a separation layer between the metallic minerals and the crust. However the silicates in these places are not so finely grained as one would expect after such a process. Where the structure of the meteorite is coarser, the grains in the third layer are also coarser; hence the method of formation of this layer is doubtful.

The so called fourth layer, which is from 0·1 to 0·4 mm. thick, consists largely of silicates having the same appearance as those in the interior of the stone, but its characteristic feature is the abundance of opaque material in and among the silicate crystals. A part of the black substance is metallic but other portions do not show metallic lustre. The quantity of nickel-iron and troilite is difficult to estimate, but it seems as if these minerals were more abundant in the fourth layer than in the other part of the meteorite. The non-metallic opaque constituent occurs as veins and irregular grains, often as a fine dust among the other minerals. The lack of any characteristic structure and the mode of occurrence seem to indicate that the black substance is amorphous and therefore probably a glass. The distribution of this black mineral is irregular, and in many places the fourth layer is altogether wanting. There is never any distinct boundary between the fourth and third layers or between the fourth layer and the interior mass of the stone. The existence of this zone depends solely on the local concentration of the opaque minerals. Its occurrence along the surface of the stone is evidence enough to prove its causal dependence on the agents which at one time operated on the surface of the meteorite. The fourth layer of the Shelburne stone is identical with the formation which Tschermak has named the "Infiltrations zone." He thinks that molten substance from the surface has penetrated into this layer and here solidified into a black glass. The examination of the meteorite now before us does not confirm this supposition. The third layer seems not to have been in a molten state, because the olivine and the enstatite, as well as the chondrules so often appear in this layer, in that form which they usually have in the interior of the stone. In a few cases enstatite chondrules were to be seen, extending from the third layer through the fourth into the stone proper. At the place where the fourth layer crossed these chondrules, a significant impregnation of opaque particles could be followed across the chondrules. It seems therefore that the dark substance did not come from the surface but was formed nearer its present situation, and is due to an alteration of the glass or subvitreous mass which fills the interstices of the chondrules. The sharp boundary between the first two layers and the underlying layers, and the absence of a definite boundary between these latter ones and the interior of the mass supports the view that only the first and the second layer have been in a completely plastic or molten condition.

THE CHEMICAL ANALYSIS.

THE DETERMINATION OF THE NATIVE METALS was made as follows :—A fresh piece of the meteorite, weighing 2·2074 g., containing no metallic veins, was pulverized and digested with a solution of copper-ammonium-chloride in an atmosphere of hydrogen and yielded,

Fe 7·66 per cent.
Ni 0·71 " "
Co 0·04 " "

0·09 per cent. of Ni was found in the insoluble residue.

ONE DETERMINATION OF SULPHUR, made by treating 1·9426 g. of the meteorite with aqua regia, gave 1·51 per cent. sulphur. After fusion with $Na_2Co_3 + KNO_3$ another portion, 0·856 g., gave 1·61 per cent. S. The higher value is to be considered the better. The amount of sulphur found indicates the presence of 4·43 per cent. troilite, FeS, in the meteorite.

DETERMINATION OF PHOSPHORUS. 1·1024 g. of the meteorite contained 0·06 per cent. P, which, calculated as Fe_2NiP

corresponds to 0·22 per cent. Fe, 0·12 per cent. N, and 0·40 per cent. schreibersite.

ANALYSIS OF THE SOLUBLE SILICATES. 2·5979 g. of the meteorite were digested for several hours on the waterbath with hydrochloric acid, sp. g. 1·1. The solution was decanted and the residue washed several times. After that the residue was treated with a new portion of the acid to which this time was added a little HNO_3. As the latter solution contained only 0·0147 g. Fe_2O_3, 0·0040 g. CaO and 0·0155 g. MgO, it is evident that all the soluble components were removed. The silica of the soluble part was recovered partly from the acid solutions, partly from the residue by extraction with a solution of Na_2CO_3, to which was added a few drops NaOH. From the iron actually weighed the amount occurring as metallic Fe was deducted, and also the amounts of this metal which belong to troilite and schreibersite. The amount of iron thus obtained was supposed to be present as FeO. Alkalies were found only in traces. These, as well as the small quantities of Al_2O_3 and CaO which the analysis show, belong probably to the aluminum silicates in the insoluble part and indicate that these were slightly attacked by the acid. The oxygen ratio of the silica to the divalent base in the soluble portion is 1 : 1.059, which agrees with the formula of olivine.

THE SOLUBLE PART.		CALCULATED O.
SiO_2	16·36 per cent.	8·925 per cent.
FeO	10·43 " "	2·318 " "
Al_2O_3	0·14 " "	0·065 " "
Cr_2O_3	0·03 " "	0·009 " "
CaO	0·15 " "	0·043 " "
MgO	17.82 " "	7·128 " "
	44·93 " "	

THE INSOLUBLE SILICATES were decomposed by fusion with Na_2CO_3. The combined precipitates of Fe_2O_3, Al_2O_3 and Cr_2O_3, which still contained a little Mn were fused with NaOH, adding a small quantity of sodium peroxide. The alkalies were determined in a separate portion of the meteorite which had not been treated with the Na_2CO_3 solution.

ANALYSIS OF THE INSOLUBLE PART.

SiO_2	22.83 per cent.
FeO	4·63 " "
MnO	0·12 " "
Al_2O_3	2·01 " "
Cr_2O_3	0·21 " "
CaO	1·60 " "
MgO	8·36 " "
K_2O	0·22 " "
Na_2O	0·73 " "
Chromite	0·51 " "
	41·22 " "

THE 0·51 PER CENT CHROMITE was fused with Na_2CO_3 + KNO_3 and yielded 0·38 per cent Cr_2O_3, 0·10 of FeO and 0·06 of MgO.

A combination of the different determinations gives the general composition of the meteorite.

ANALYSIS OF THE SHELBURNE METEORITE.

SiO_2	39·19 per cent.
Fe	10·70 " "
FeO	15·16 " "
Ni	0·78 " "
Co	0·04 " "
MnO	0·12 " "
Al_2O_3	2·15 " "
Cr_2O_3	0·62 " "
CaO	1·75 " "
MgO	26.24 " "
K_2O	0·22 " "
Na_2O	0·73 " "
S	1·61 " "
P	0·06 " "
	99·37 " "

From the foregoing analysis it is possible to make an approximate calculation of the quantities of the different minerals present. The amount of nickel-iron is directly determined; troilite, schreibersite and chromite are calculated from the weighed amount of S, P and Cr_2O_3 respectively. The soluble silicates are regarded as olivine, and the insoluble part is divided up between enstatite (bronzite) and an aluminium-silicate, the amount of Al_2O_3 in the analysis furnishing the basis for this calculation.

MINERALOGICAL COMPOSITION OF THE SHELBURNE METEORITE.

Nickel-iron	8·50	per	cent.
Troilite	4·50	"	"
Chromite	0·80	"	"
Schreibersite	0·40	"	"
Olivine	45·00	"	"
Enstatite	27·80	"	"
Aluminium-silicate	13·00	"	"
	100·00	"	"

The specific gravity of the meteorite, 3·499, was determined with the balance by suspending in water and in air a piece from the interior of the stone weighing 24 g.

MICROSCOPICAL INVESTIGATION.

The thin sections of the Shelburne meteorite revealed a kaleidoscopic mixture of silicate-crystals and chondrules of different types with grains of nickel-iron and troilite. Among the silicates, olivine predominates. The high interference colors distinguish it sharply from the other constituents. The mineral, which is colorless and filled with cracks, forms chondrules of great variety. Polysomatic chondrules are common, in which the crystals of olivine, outlined by pinacoids and domes, lie pell-mell in a more or less turbid ground-mass of glass. Other chondrules consist entirely of olivine in irregularly oriented grains close to one another and without distinct form. Again, other chondrules consist of olivine lamellæ. Whole groups of lamellæ have the same interference color and extinction and belong therefore to one individual. These chondrules usually show two or three or at least a low number of separate individuals. Monosomatic chondrules are rare and even these seldom represent one compact individual, but have a more or less highly developed skeletal structure. All intermediate stages are represented from the perfect crystal up to the finest growth of irregular feather-like crystallites. The mass between the lamellæ or crystallites of olivine in the chondrules is sometimes a clear colorless glass, but usually the glass includes small black particles, which render it turbid or completely opaque. This black substance is seldom in such coarse grains that its true nature can be determined, and in this case it represents an opaque brownish black mineral with non-metallic lustre, probably chromite. This supposition is confirmed by the high percentage of chromium shown by the chemical analysis, which can not otherwise be accounted for. In a few chondrules the ground-mass consists wholly or partly of the mineral, which later is described as maskelynite. The lamellæ of olivine in the thin sections mostly give parallel extinction and have positive optical character, which shows that the plates and rods of olivine have their smallest dimension parallel to the axis of highest ether-elasticity. The olivine belongs to the iron-rich variety, approaching fayalite, the ratio of Mg to Fe being nearly 3:1 as the analysis of the soluble part of the meteorite shows.

OLIVINE FROM THE SHELBURNE METEORITE
CALCULATED TO 100 PER CENT.

SiO_2	36·41	per cent.
FeO	23·22	" "
Al_2O	0·31	" "
Cr_2O_3	0·07	" "
CaO	0·33	" "
MgO	39·66	" "
	100·00	" "

The enstatite is colorless and forms polysomatic chondrules, which generally have an ex-centrically radiated structure and circular outline. In these the enstatite is developed as prismatic and not as tabular crystals, and as a consequence some sections of the chondrules seem to be built up of small rounded grains and others of oblong crystals in radiated position. The longer direction of the crystals is parallel to the crystallographic *c*-axis. Another kind of enstatite chondrules shows short prismatic individuals irregularly interwoven. The enstatite chondrules consist almost entirely of this mineral, the quantity of glassy ground-mass being exceedingly small. In some places the enstatite contains a large number of metallic inclusions or is stained dark by a fine black dust, probably the same substance, which occurs in the glass of the olivine chondrules. A few mixed chondrules of olivine and enstatite were observed in which both minerals are present as irregularly oriented rounded grains. Some chondrules in which the enstatite seems to have a cyclic or spiral-like grouping can be interpreted as ex-centric sections of the common radiated enstatite chondrules.

Amongst the chondrules and in the interstices of the chondrules, scanty grains of a mineral with a refractive index identical with that of Canada balsam occur. This mineral shows a very low double refraction so that sections often appear isotropic. The interference colors are never up to clear white of the first order in thin sections where olivine has reached blue of the second order, so that the double refraction of the mineral is about 0·005. The mineral shows no cleavage, the cracks, which are common, being irregular. Compared with the other minerals this one is poor in inclusions. This circumstance and the low refractive index make the grains of the mineral appear as clearer transparent spots in the thin sections. The shape of the grains is always determined by the surrounding minerals which gives the impression that this mineral is the youngest of the components of the meteorite, except the metallic veins. Grains of this mineral, which in ordinary light under the microscope seem to form one entire mass, show between the crossed nicols that they consist of numerous individuals, of which some apparently have single refraction. The low double refraction of the mineral makes it impossible to state the true optical nature of the apparent isotropic grains in convergent light. So far as the minute grains of the mineral permit of an investigation, its peculiarities (viz., the refractive index, the double refraction and the absence of cleavage) agree with those of maskelynite. The mineral maskelynite, although seemingly common in meteorites has never been fully investigated and defined so that it is difficult to decide whether the mineral present, belongs to this species or not. The analysis of the meteorite points to the presence of an aluminium-silicate, which also favors the supposition that our mineral is maskelynite. The occurrence of the maskelynite between fresh chondrules and crystals of olivine and enstatite and close to troilite and nickel-iron forbids an interpretation of it as a re-fused feldspar. The maskelynite in the meteorite from Shelburne is a true mineral species and no alteration product.

The nickeliferous iron forms 8·41 per cent of the meteorite, but as its specific gravity is twice that of the stone, it plays only a subordinate part in the microscopical sections, where it occurs as irregularly outlined grains between the predominating silicates. The boundary seems to depend on the form of the neighboring chondrules and grains of olivine and enstatite so that the nickel-iron must be considered as a younger product than these. The chondrules of nickel-iron, of which only a couple were found, have however, a smooth rounded form like that of the other chondrules. The nickel-iron forms the metallic veins which already have been mentioned in the description of the surface of the meteorite. Although several attempts were made to produce etching-figures on the iron they were not successful. The metals dissolved in copper-ammonium-chloride solution, give us the composition of the nickel-iron.

ANALYSIS OF THE NICKEL-IRON.

Fe	91·08	per cent.
Ni	8·44	" "
Co	0·48	" "
	100·00	" "

In the thin section the troilite is easily detected in reflected light on account of its bronze-colored reflection. The troilite usually does not form large grains like the nickel-iron but occurs in aggregates. A large number of grains of various sizes are grouped together locally in the stone. Among and around these grains are silicates and often even chromite. The nickel-iron is not associated with the troilite in this form. Where troilite occurs in proximity to a metallic grain, it seems to take a form similar to that of the nickel-iron and then forms larger lumps. Troilite takes an important part in the building up of the metallic veins.

The analysis shows the presence of nearly one per cent of chromite. The thin sections contain, in addition to the opaque minerals, nickel-iron and troilite, which are recognized by their metallic lustre, a considerable quantity of another opaque substance, occurring as fine grains, often so fine that they appear as dust even when a high-power microscope is used. Less frequently they have a somewhat larger size so that their form can be recognized. Then they seem to be rounded grains, and while irregular in form they do not show prolongation in any definite direction. A few showed an octahedral form with truncated or rounded edges. The chromite occurs as inclusions in olivine and enstatite. How large a part of the fine black dust in these minerals and in the glass, belongs to this species it is naturally impossible to decide, but the high percentage of chromium in the analysis seems to prove that more chromite exists in the meteorite than the few crystals which could be determined as such under the microscope.

In the chondrules among the crystals of olivine and enstatite and in the spaces among the crystal-skeletons of these minerals, an isotropic substance, having a low refractive index, is common. This substance does not have any cleavages or other signs of a crystalline structure, and therefore is to be considered a glass. It is always colorless, but seldom clear as is the case with the glass which fills the spaces in the monosomatic olivine of fig. 3, Plate III. (The picture is taken with crossed nicols, hence the glass appears black). As a rule the glass is clouded or turbid to opaque. The cause of this phenomenon is a fine black dust, which has been identified as chromite, though part of it might be another mineral.

As the description of the minerals shows, the Shelburne meteorite consists chiefly of perfectly or nearly round chondrules mixed with chondrules of angular or fragmentary form. Among the larger chondrules, the form of which can be distinguished, numerous smaller chondrules or mineral particles occur, which so far as their outlines can be traced, seem to have a more irregular form than the larger ones. It is often impossible to follow the boundaries between the grains in this finer mass. The distinct chondrules lie beside one another in immediate contact or are separated by the finer mineral. In a few cases one chondrule apparently has made an impression in another neighboring one.

The structure of the meteorite shows so few analogies with that of any terrestrial body of known origin, that it is impossible to form a definite idea regarding the mode of its formation. The skeleton olivine chondrules and the porphyritic chondrules have apparently solidified in a way similar to an eruptive magma. Even the other constituents permit of a similar interpretation. Each individual chondrule represents a structure of cooling and crystallization from the molten state, and as their structure shows an intimate relation to the boundary of the chondrule, it must be supposed that each chondrule at the time of its solidification was a separate unit. Because chondrules of the same chemical composition have a different structure, they must have been formed under different physical conditions. Since such a variety of conditions can not have existed in the narrow space in which the different structures now are met with, the chondrules must have accumulated after solidification. The compactness of this accumulated mass seems to be due to a pressure which also accounts for the undulate extinction noticeable in some places. The metallic veins have been formed after the other part of the meteorite already had its present structure and are the results of some "geological" factors similar to those which produce the veins or dykes in the crust of the earth, These processes in the history of the meteorite are such that they indicate that the stone was an integral part of a larger body, and lead to the theory that meteorites are fragments of dispersed planets.

The investigation of the Shelburne meteorite indicates that in the Rose-Tschermak system of meteorite classification it belongs to the group of "veined grey chondrites (cg a)."

MAGNETIC DISTURBANCES, 1882–1903, AS RECORDED AT THE ROYAL OBSERVATORY, GREENWICH, AND THEIR ASSOCIATION WITH SUN-SPOTS.*

BY

E. WALTER MAUNDER, F.R.A.S.,
ROYAL OBSERVATORY, GREENWICH.

THE following conclusions appear to result from the facts now before us :

FIRST. The origin of our magnetic disturbances lies in the Sun ; not in any body or bodies affecting both. This is clear from the manner in which these disturbances mark out the solar rotation period ; not the actual sidereal period, but the synodic period ; the period as it appears to us.

SECOND. The areas of the Sun giving rise to our magnetic disturbances are definite and restricted areas, as the definiteness with which certain longitudes are indicated proves. Our storms are not due to a general action or influence diffused over the whole solar surface.

THIRD. The region of the Sun wherein these "magnetically active", if for the sake of distinction we may so term them, areas are situated, rotates with the chief of the spot-bearing zones, viz. latitudes 0° to 30°.

FOURTH. As shown in my former paper the greatest magnetic storms are clearly connected with great sun-spots ; the instances of synchronism between individual storms and individual spots being too numerous and precise to be accidental.

FIFTH. These active areas on the Sun can, it would seem, be magnetically active before the visible formation of a spot-group ; they evidently can continue to be magnetically active after the spot-group has disappeared. It would appear, then, that spot-formation is an important phase of the activity of these

* We have received from Mr. Maunder a revised copy of his paper bearing this title, and regret that at its arrival our space was so taken that we are not able to publish it in full. We give, however, a complete statement of his conclusions. For the original paper see Monthly Notices, Nov. 1904. —Editor.

areas, but that other phases of that activity can both precede and survive such spot-formation, just as faculæ both precede and survive spots.

SIXTH. The influence proceeding from the Sun, whatever its character, does not act equally in all directions. It does not radiate like light or heat, but its action is confined to a definite and very restricted direction. This appears from a consideration of the characteristic "sharp" movement with which so many magnetic disturbances, and all the more violent of them begin.* It would be possible to account for this sudden and instantaneous effect, instantaneous over all the earth, as the impact of a wave of energy radiating in all directions from the Sun as a centre, if such storms bore no relation to each other. It is not possible so to account for such an effect when it is followed by others exactly at the interval of one or more synodic rotation-periods of the Sun. Such a relation can be only explained by supposing that the earth has encountered, time after time, a definite stream, a stream which, continually supplied from one and the same area of the Sun's surface, appears to us at our distance, to be rotating with the same speed as the area from which it rises.

SEVENTH. The average diameter of such streams may be roughly estimated from noting the time which an average storm lasts. Those in Table I. give an average duration of thirty hours, in which time the longitude of the centre of the Sun's disc appears to us to change by 16°·5. This would imply an average diameter of these stream-lines of 20° supposing them to be circular in section. An average stream-line will therefore occupy not more than $\frac{1}{130}$th part of the entire sphere, instead of the whole of it, as the magnetic wave from the Sun would do if it spread out equally in all directions.

EIGHTH. It follows therefore, that if sun-spots be the real seat of the activity exciting our earth-magnetic disturbances, the majority of them must fail to affect us. A similar conclusion results from comparing the numbers of magnetic disturbances and of spot-groups; for whilst my complete catalogue contains only 276 entries, the Greenwich sun-spot record for the same period

* This "sharp" commencement is both actually and relatively more frequent at the times of solar maximum, when "great" and "very active" storms are most abundant.

gives more than 4,500 spot-groups, of which more than 600 might be classed as considerable; the least important having been visible for at least eight consecutive days, and having a mean area of 200 millionths of the Sun's visible hemisphere.

NINTH. It follows from the fifth and eighth conclusions that, though sun-spots and magnetic disturbances are intimately connected, large sun-spots will be observed when no disturbances are experienced, whilst sometimes disturbances will be experienced when no spots with which they can be associated are visible. The familiar and oft-repeated phenomenon of "intermittent spot-activity" suggests that often, if not always, the spot should be regarded in these cases as dormant rather than as having ceased to exist, the spot-forming forces being possibly still at work below the photosphere.

In a valuable paper by the Rev. Walter Sidgreaves, recently published, "On the Connection between Solar Spots and Earth Magnetic Storms,"* an immense amount of material has been discussed, and the results, as the author expressly remarks, afford proof of a real connection between spots and magnetic storms. He was, however, held back from the natural conclusion that the cause of these storms resided in the Sun by two considerations, the one observational, the other theoretical. The observational difficulty was the fact to which I called special attention more than twelve years ago†—that great spots have been seen when there have been no storms, and storms experienced when there have been few or no spots. That difficulty is now removed, since it is seen that spots ought not always to be accompanied by storms on the one hand, whilst the storms themselves show their solar origin apart from any question of individual spots on the other.

On the theoretical difficulty Father Sidgreaves wrote: "The question, 'Is the source of energy affecting our magnets on the Sun?' is a question admittedly settled in the negative theoretically," and he quoted the well known presidential address of Lord Kelvin to the Royal Society in 1892. But Father Sidgreaves strangely passed over without notice a most significant qualification in Lord Kelvin's conclusion. Lord Kelvin wrote: "Thus

* Memoirs R. A. S., vol. liv. pp. 95 and 96.

† Knowledge, vol. xv, May 1892, p. 93.

in the eight hours of a not very severe storm as much work must have been done by the Sun *in sending magnetic waves out in all directions through space* as he actually does in four months of his regular heat and light." I have italicised certain words because these form the basis of Lord Kelvin's computation, and it is to these that his conclusion applies. It is only as we assume this condition, so explicitly stated by Lord Kelvin, that we can reach his conclusion. And that condition does not hold good. As I have shown in this paper the magnetic storms themselves supply absolutely conclusive evidence that they are not due to magnetic waves spreading out from the Sun equally in all directions through space. They are due to action along definite restricted lines.

There is no necessity for me to expound at length the magnitude of the change thus made in our way of regarding the solar action. The difference between the universal action of a "polarized magnetic sphere" and the action of restricted stream-lines is fundamental.

Stream-lines proceeding from the Sun have been actually photographed. In 1898, after the eclipse of that year, my wife and I wrote of the photographs taken by her : "The chief features shown by these long-exposure photographs are four long rays. . . . . The lengths given for the rays are, of course, their apparent lengths ; their real lengths are probably considerably greater, for we do not know in what plane they lie, nor how far their apparent lengths have been diminished by fore-shortening ; the values given above" (13·9 lunar radii in the extreme case) "therefore are a minimum. The rays in appearance are straight, narrow, and rod-like up to the limits given."*

This was the first occasion upon which these rod-like rays were clearly photographed. The present paper, by an entirely different class of evidence, has shown that stream-lines analogous in form are being driven off from the Sun. The same photographs showed also for the first time the real significance of the synclinal structures of Mr. Ranyard. We wrote : "But their bases" (*i.e.* of the long rays) "are of an altogether different form. Each one rises from one of those 'synclinal structures' to which Mr. Ranyard called attention in his great eclipse volume (*Memoirs R. A. S.* vol. xli.) Only four of these structures were seen in

* The Indian Eclipse, p. 117.

this eclipse, and in each case we now see from these photographs that they terminate in one of these rod-like rays. The bending towards each other of these synclinal curves is, therefore, not apparent only, as being due to some effect of perspective, nor accidental, but is of the very nature of their structure." The building up of these synclinal structures was shown in the same eclipse on photographs taken by Mr. C. Thwaites with the fine photo-visual telescope lent to him by Mr. G. J. Newbegin. Concerning these we wrote : "These show us that over the principal prominences and at some little distance an arch of coronal matter is formed. This is succeeded by a larger arch outside, and so on for a succession, the outer arches being less definite and complete than the inner ones. Outside all we find the curves defining the boundaries of the synclinal group. . . . From the apex of the synclinal structure we find the coronal matter driven outwards in a straight line, which probably indicates an immense velocity. It must be noted that this eruptive action is not always radial. One of the long rays in 1898 was tangential and another was oblique."*

As to the physical cause of these streams and the condition of the matter composing them it does not lie within my province to offer any suggestion, nor would it be consistent with the scope of the present paper.

That, therefore, which Lord Kelvin spoke of twelve years ago as "the fifty years' outstanding difficulty" is now rendered clear. Our magnetic disturbances have their origin in the Sun. The solar action which gives rise to them does not act equally in all directions, but along narrow, well defined streams, not necessarily truly radial. These streams rise from active areas of limited extent. These active areas are not only the source of our magnetic disturbances but are also the seats of the formation of sun-spots, and their activity is ordinarily most easily and continuously manifested to us by the presence of sun-spots, and by the changes which such spots undergo. But these areas can be "magnetically active" both before a spot has formed and after it has disappeared. Though, therefore, sun-spots and magnetic disturbances have an ultimate connection the latter can occur when no spots are visible. On the other hand, since the solar

* Ibid. p. 121.

action is restricted in its direction, many great spots may be visible to us without any effect being produced on the Earth's magnetism. But that the disturbances have an intimate connection with the spots is clear from the fact that they occur at intervals corresponding to the rotation period of the Sun as determined by sun-spots, and to the special rotation periods of those zones of the Sun where sun-spots most congregate, whilst they exhibit in the times of their returns some of the chief sun-spot characteristics, and in not a few instances individual storms have been clearly associated with individual groups of sun-spots.

THE TOTAL SOLAR ECLIPSE OF 1905.

THE accompanying map shows the path across Canadian territory of the shadow during the total phase of the eclipse of the sun, Aug. 30, 1905.

The Canadian Government expedition in which the R.A.S.C. are to take an important part, propose to observe at the Hudson Bay post called Northwest River, situated at the head of Lake Melville, about 150 miles from the sea. The location is indicated on the map by a star.

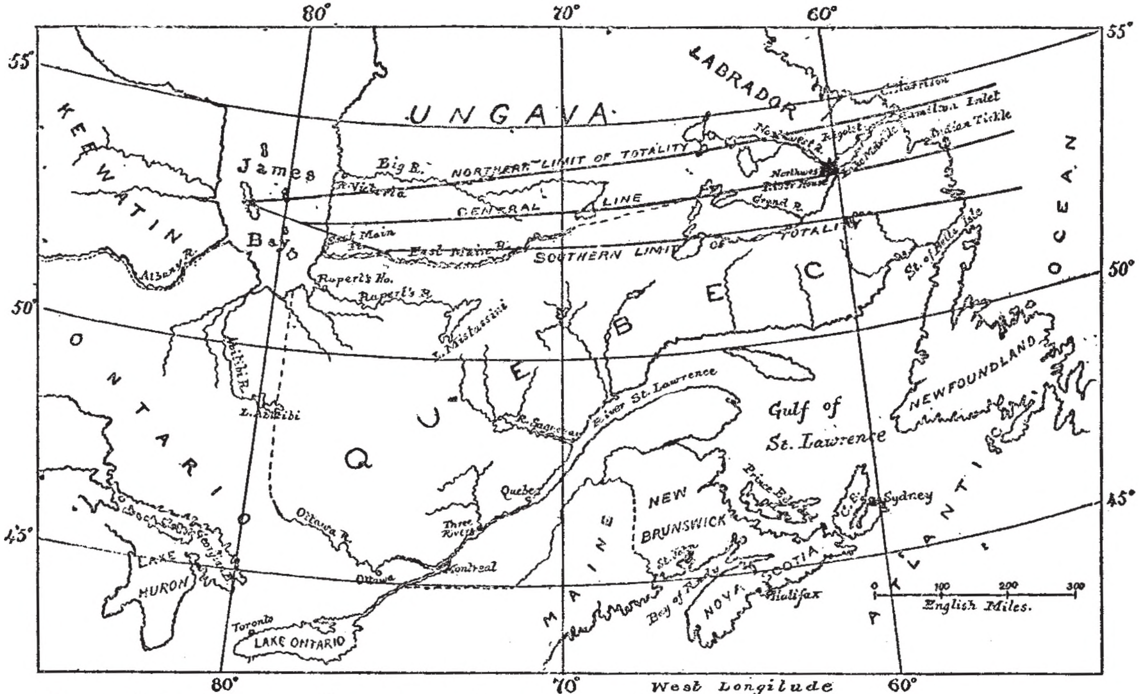
Some U. S. parties, (Lick Observatory, University of Illinois and perhaps others), will observe at Indian Tickle on the coast. Both places are very close to the central line of the path.

The total phase, on the central line of the path, begins at Long. 79° 8′·8 W., Lat. 53° 32′·2 N., at $23^h\ 44^m$ G. M. T. At Domino Harbor, Long. 55° 46′ W., Lat. 53° 29′ N., where the central line reaches the Atlantic Ocean, totality begins at $23^h\ 54^m\ 10^s$ G. M. T., and lasts for $2^m\ 38^s{\cdot}1$, the altitude of the sun being 27°. The partial phase will extend from $23^h\ 8^m\ 18^s$ to $1^h\ 21^m\ 6^s$ G. M. T.

At Northwest River the times will be about 4 m. earlier and the total phase will be about 10 sec. shorter.

Mr. F. L. Blake, one of our Life Fellows, gives the following information in the Canadian Almanac, 1905, p. 14 :—

The eclipse will be partial at Toronto, beginning at 5.39 a.m., sunrise, Aug. 30 (Standard Time). The maximum will be at



ROYAL ASTRONOMICAL SOCIETY OF CANADA. TRANSACTIONS FOR 1904.

PATH OF TOTALITY ACROSS CANADA.

6.34 a.m. and the end at 7.34 a.m. At maximum 0.774 of the sun's diameter will be eclipsed.

At Ottawa the beginning is at 5.41 a.m., maximum at 6.36 a.m., end at 7.38 a.m. and the magnitude 0·846 of the diameter.

At Montreal the beginning is at 5.40 a.m., maximum at 6.38 a.m., end at 7.40 a.m.; magnitude, 0·811.

IN MEMORIAM : ARTHUR HARVEY.

[SEE FRONTISPIECE.]

JUST as the last pages of this volume were on the press came the news of the death, at his late residence, 80 Crescent Road, of our former president Mr. Arthur Harvey. We append a short sketch of his active and useful life.

Mr. Arthur Harvey was born in England, April 23 rd, 1834, and educated chiefly in France and the Netherlands, with the latter of which countries his family had long been connected. Returning from the Continent he entered Trinity College, Dublin, in 1852, and in 1855 added a special course in actuarial science, in London, where Prof. De Morgan was the great lodestone for students. Coming to Canada in 1856, Mr. Harvey first took service as assistant editor, or "scissors", to a newspaper in Brantford, but soon removed to Hamilton, where he became associated with the "Spectator." Being one of the two swiftest shorthand writers in Canada, and as well able to follow a French as an English orator, he lived in Toronto during the sessions of Parliament, and, on the removal of the seat of government to Quebec, took up residence there as confidential correspondent of the Spectator, and engaged in literary work generally, as a writer of magazine articles. For a time Mr. Harvey was editor of the Quebec "Chronicle", and developed a liking for statistics. A small pamphlet on the grain trade of the basin of the Lakes, in which graphic statistics were used for the first time in Canada, brought him the friendship of Mr. (afterwards Sir) Alexander Galt, whom he assisted in preparing the Budget of 1862, which in turn led to his being appointed to a position in the Department of Finance, nominally as statistical clerk but really as confidential aid to the minister of Finance. In this capacity he served under several ministers, being entrusted with important inquiries for each. Thus, for Sir Alexander Galt he investigated the working of the Reciprocity Treaty, and was the secretary of the commission sent to Washington by the Five Provinces to negotiate for its renewal. For Mr. Holton he investigated the expenditures for printing and supplies to the Departments and organized a new and regular tariff of charges and a system of checks which resulted in large public savings. For Mr. (now Sir) William P. Howland he examined Interprovincial Trade and its probable development on the removal of tariffs and the completion of an Intercolonial Railway. For Mr. Galt, again minister, he collected the statistics of the several Provinces in view of their approaching confederation, spending several months at the capitals of the Maritime Provinces, for this purpose. With the leave of the Government a great part of this work was published as the Year Book of British North America, 1867, and of Canada, 1868 and 1869, and Mr. Harvey always regarded it as his *magnum opus*. It entitles him to be looked on as the father of Canadian statistics. The collection, completion and summing up of materials independently and often imperfectly gathered is no slight work. The general summary, communicated to his chief, Mr. Harvey understood to have been used in London in laying down the basis for Confederation; and the Year Book, which was in more complete and scientific shape than any national statistical work except that officially published for Italy, was the standard for reference during all the Provincial debates on that union which followed. Under Sir John Rose the chief work done by Mr. Harvey was the suggestion and preparation of the first Canadian insurance law, which called for the making of regular annual returns and for the deposit of a sum of money as a guarantee of permanency. All these ministers had been Mr. Harvey's personal friends, but when Sir Francis Hincks was appointed to the office, Mr. Harvey resigned his most agreeable and (for a civil servant) well paid position, and came to Toronto in 1870 to take charge of the Provincial Insurance Company. After several years' labor in building up the finances of the company, on the eve of success, a conflagration year came along, and with the fire at St. John, N.B., (1877) as a climax, he thought it most honorable to wind up its affairs. From that time he did not engage in important public enterprises.

Mr. Harvey has always been actively concerned in the work of scientific, literary and other societies. He was secretary of the Horticultural Society at Hamilton, and the real founder of the Hamilton (Scientific) Association. He was a hard-working secretary of the St. George's Society at Quebec and a member of the Literary and Historical Society there. At Ottawa he formed and was Sec'y-Treasurer of the Civil Service Building and Savings Society, and was largely instrumental in the erection of St. Alban's church—both urgently needed. On coming to Toronto, several building societies here and in other places wished him to value their terminable mortgages, and being unwilling to divert his attention from the affairs of the Provincial Insurance Company, he published the Tables he had prepared for his own use, which were the first tables anywhere printed for the valuation of mortgages repayable by monthly payments. In due time he joined the Canadian Institute and was its President in 1891 and 1892. In 1890 he was a delegate to a function at Montpelier, France, where he addressed the meeting in French, which the other delegates were surprised to find was not a *patois* ; and he expressed the hope that some day France would take a less narrow view of the Newfoundland French Shore question. He became a member of the Astronomical Society and was its President in 1898 and 1899. The Transactions of these Societies contain several papers from his pen. His specialty was the investigation of the connection between solar and terrestrial phenomena (see p.xiv *ante*) for which the records of the Magnetic and Meteorological Observatory here give many of the necessary data. In recognition of his work on solar phenomena he was elected Honorary President and Director, La Institutio Solar Internacional, Monte Video, Uruguay ; and just shortly before his death was elected a Fellow of this Society. In 1894 he was elected a Fellow of the Royal Society of Canada, and the bibliography which each Fellow has to prepare, for election, can be referred to in the proceedings for that year (Vol. XII) as an evidence of the fertility of his pen. Later he published a work on "Decimals and Decimalisation", being a historical *resume* of the movements preceding the adoption in France and other countries of the metric system, of which system Mr. Harvey was a warm advocate. The Transactions of this Society for 1902–3 were edited under his supervision. Though Mr. Harvey preferred his literary to his scientific papers his most recent contribution to the Canadian Institute, on "The Principles of

Insurance, with Special Reference to Sick Benefits'', (the ''proofs'' of which he was correcting an hour or two before his death), seems to indicate a desire to aid in the establishment of a system of relief in sickness and old age, not based on German precedent but adapted to Canadian conditions.

Mr. Harvey was a most versatile man. He had a remarkable mastery of languages living and dead, and was highly accomplished both in music and art. In debate he was a strenuous fighter, but when the fight was over no one was gentler or kinder than he.

APPENDIX I.

AFFILIATED SOCIETIES.

REPORT OF OWEN SOUND ASTRONOMICAL AND PHYSICAL SOCIETY.

THE Annual report of the Owen Sound Astronomical and Physical Society will be brief indeed, as we are in our infancy yet and have barely commenced walking. We organized about a year ago with a membership of twelve. Our meetings are held on the second Monday evening of each month in the office of Mr. Sampson, one of our active members, who has kindly placed his room at our disposal. The officers of the Society are:

J. H. Packham, B.A., President;
Miss A. Dobie, Vice-President;
A. L. Danard, M.D., Corresponding Sec. and Treas.;
Miss C. Pearson, Recording Secretary.

The work of the year consisted in studying a textbook and the reading of papers by the members. The subject being new to many of us, we commenced at the bottom and are endeavoring to ascend the astronomical ladder. The papers during the year were as follows:

"Astronomical Observations" by Past-Pres. Judge Morrison,
"The Celestial Garden" by Ewing Buchan,
"Meteors" by A. L. Danard,
"The Future of the Solar System" by J. H. Packham,
"Time Reckoning" by Thos. Murray, B.A.

On account of lack of funds we have not been able to acquire much apparatus but hope to add a little each year until we have an equipment suitable for amateurs. Under the energetic and active leadership of Mr. Packham we look forward to a year of progress and mutual benefit.

A. L. DANARD, SECRETARY.

Owen Sound,
Dec. 19, 1904.

APPENDIX II.

LIST OF LANTERN SLIDES IN THE POSSESSION OF THE SOCIETY.

A. SOLAR SUBJECTS.

General.

A 1. Drawing of Sun by Langley.
2. Drawing of Sun's surroundings, Langley.
3. Sun showing spots and prominences—ideal view.
4. Solar prominences.
5. Sun-spots and faculæ—ideal view.
6. Sun-spot Drawing by Langley.
7. Typical sun-spot—Langley.
8. Portion of Sun's disk showing group of spots; 15–5–1894–Lick.
9. Group of spots, Sept. 21–22, 1870. Drawing by Langley.
10. Portion of Sun's disk showing groups of spots—from negative made by Capt. Ash, Quebec, 1869 or 1870.

A 11. Portion of Sun's disk showing another part of the same negative.
12. Portion of Sun's disk showing group of spots, 19–6–1894. Lick (enlarged).
13. Portion of Sun's disk showing group of spots, 4–9–1893. Lick.
14. Solar disk with sun-spot group
15. Portion of Solar disk with sun-spot group (18–6–1894, Lick Photo.)
16. Detail of Sun-spot.
17. Sun's surface showing sun-spots and rice grain structure of photosphere.
18. Zones of sun-spots.

Spectro-Heliograph.

A 19. Solar disk showing spots and faculæ, photo. Prof. Hale, Kenwood Obs.
20. Chromo-sphere showing prominences without an eclipse. Photo. by Prof. Hale.
21. Sun showing calcium flocculi (H_2 level), Aug. 12, 1903.
22. H and K lines in electric arc and section of solar surface.
23. Hydrogen and calcium flocculi. July 7, 1903.
24. Rapid development of spot group and calcium flocculi, July 1903.
25. Faculæ and sections of calcium flocculi at different levels, Apr. 1903.
26. Comparison of calcium and hydrogen flocculi. June–July 1903.

A 27. Faculæ and sections of calcium flocculi at different levels. Ang. 1903.
28. Calcium flocculi, H_2 level, Oct. 1903.
29. Calcium flocculi, low N level, Oct. 9, 1903.
30. Calcium flocculi, H_2 level, Oct. 9, 1903.
31. Hydrogen flocculi, Oct. 1903.
32. Calcium flocculi, low H level, Oct. 10, 1903.
33. Calcium flocculi, high H level, Oct. 10, 1903.
34. Great sun-spot of Oct. 1903, for stereoscope comparison.
35. The great sun-spot of Oct. 1903. Calcium flocculi (H_2 level) Yerkes Photo.

Eclipses.

A 36. Total solar eclipse of Dec. 1889. J. M. Schaeberle—Lick.

37. Total solar eclipse of Apr. 16, 1893. Lick Photo.

38. Section of total solar eclipse, Apr. 16, 1893. Lick Photo.

39. Total solar eclipse of Apr. 16, 1893. Lick Photo. (original).

40. Total solar eclipse of 1898 showing the planet Venus. Photo. by Mrs. Maunder, made in India.

41. Drawing of total solar eclipse of 1898 on small scale showing region around sun.

42. Drawing of total solar eclipse of 1898 on large scale showing region around sun.

43. Total solar eclipse, short exposure. No date.

44. Total solar eclipse, medium exposure. No date.

45. Total solar eclipse, long exposure. No date.

46–55. Total solar eclipse, May 28, 1900. Chabot Obs., Dolbeer Eclipse Expedition, Siloam, Ga. Series of 10 slides.

56. Total solar eclipse, May 28, 1900. Drawn by Lumsden.

57. Partial eclipse, May 28, 1900, as seen at Toronto. Photo. by D. J. H.

A 58. Projected images of the sun as seen through foliage on pavement.

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