OUT OF OLD BOOKS

By Helen Sawyer Hogg

For out of olde feldes, as men seith,
Cometh al this newe corn froe yeer to yere;
And out of olde bokes, in good feith,
Cometh al this newe science that men lere.

Chaucer, *The Parlement of Foules.*

AS the middle of the twentieth century draws near it would appear that many interesting observations and events of an astronomical nature have dropped out of the enormous background of astronomical knowledge of the present day. Only the oldest inhabitants remember them and most of us have only a hazy idea of them from condensed sentences in textbooks or current articles. With the destruction of European libraries during the war, much of this information will be lacking now in many of the great libraries. In view of these facts, it is our intention to publish from time to time extracts from old books and periodicals which may be profitable, and we hope, interesting reading for the Journal subscribers.

Krakatoa Eruption of 1883

The building up of the recent volcano Paricutin in Mexico, and the even more recent volcanic island rising off the coast of Japan the past month bring to mind the gigantic explosion of Krakatoa, with its effects on the atmospheric conditions the whole earth over.

The eruption was one of the most stupendous ever recorded. Krakatoa was an island between Java and Sumatra which had been built up in an old volcanic cone. The eruption occurred from August 26th to 28th, 1883. The large part of the volcanic island was blown sky-high. Where once an island rising 1,400 feet above sea level stood, a gaping hole was left in the sea floor, 1,000 feet deep in spots. The tonnage of material displaced in this violent manner is impossible to estimate. The column of stones, dust and ashes was hurled to a height of 17 miles or more in the atmosphere. The most serious effect of the eruption was the ocean waves, which reached a maximum of fifty feet in height, and caused the death of more than 36,000 human beings. The ocean wave was recorded as far as the English Channel, 11,000 miles away. The actual sounds of the volcanic explosions were heard as far away as 3,000 miles.

The heavier stones came down on the surrounding islands, to a depth sufficient to bury the forests! But the smaller ash particles were carried into the upper levels of the atmosphere and took many months to settle out. They were responsible for most extraordinary atmospheric conditions the world over. In the early fall of 1883 the reports of amazing sky conditions betray much bewilderment, but by the spring and summer of 1884 the scientific world in general held Krakatoa as responsible. A century earlier Benjamin Franklin had pointed out that the severe winter of 1783-84 might have been caused by the vast quantities of smoke hurled into the earth's atmosphere by volcanic action at Hecla, in Iceland.

The scientific periodicals of the year 1883 and 1884 contain literally dozens of eye-witness descriptions of the spectacular skies as seen from a variety of spots on the earth's surface. A few of the more interesting and significant will be quoted.

From *Knowledge,* January 4, 1884, p. 15.

Red Skies in America

I am glad to see the article upon "Extraordinary Sunsets" in the last number of *Knowledge* just received. It sems strange to me, however, that no accounts of the phenomenon have yet reached you from this side; but I suppose you will have plenty of them in due time. My attention was first called to the matter about the latter part of November—somewhere about the 20th, I think. I was in the office, when one of the clerks came in and said that there "was a tremendous fire somewhere up town." I at once went out, and saw the whole western heavens illuminated. The sight was such a strange one that we went on top of the building, and it was at once evident that it was no fire. As near as I can recollect the "red glare" extended for upwards of 40° from the horizon, and for 60° or so along it. All the large buildings, steeples, etc., of the city stood out clear and distinct upon the bright background of the sky, as if lit up by some immense conflagration. This was about six o'clock in the evening. A little before seven o'clock the phenomenon had pretty well disappeared. People in the streets were watching it, and wondering what was the matter in the sky.

One old "darkie" whom I first asked upon coming out of the office where the fire was, answered: "Dat's no fire, mister, dat's de elements; de world must be coming to an end." The general impression among the masses seemed to be that something was wrong in the sky.

I have not much of an eye for colour, but to me it appeared fiery red, without any green whatever. The next day I saw in the papers that one of the fire departments in a Northern city had been called out by mistake, having taken this "red glare" to be an immense fire.

This strange light has been hanging around the sun, more or less, ever since, although to me the phenomenon has never been as marked as upon that occasion.

W. H. Numsen

Baltimore, U.S.

A report of special interest to Canadian readers comes from Halifax, and is in *Knowledge,* February 8, 1884, p. 90.

After-Glow in America

- - I beg to inform you that on three successive evenings towards the close of November—I forget the exact dates—the after-glow at this place was very remarkable. I did not notice anything extraordinary at the time of sunset, which was a little after half-past four, but from about half-past five to quarter past six the whole western sky was lit up by a very vivid crimson light, rising very nearly to the zenith.

- - I have observed no discolouration of the sun either at sunrise or sunset, and I believe no such effect has been observed in these parts.

On the morning of Dec. 18 I happened to be up at quarter-past six. Looking out of an east window I saw all the effects of sunrise over the south-eastern horizon. The colour was amber shading into orange, and the clouds were tipped with an orange red. At a few minutes before seven I left my house, and saw the spires of the churches and the roofs of the higher houses gilded so strongly with sunlight that I fancied my watch must have deceived me and turned round to the eastward several times to see if the sun had actually risen.

From seven to half-past seven, or a little later, I was in a building. Coming out I looked to the east again, and saw the ordinary effects of a rather dull sunrise. The sun actually rose a few minutes later.

Last night again (26th) there was a remarkable after-glow, lasting from about quarter-past five to near six. It was at its brightest at about half-past five, and was of an orange-red colour, different to the crimson in November.

Chas. S. Akers, Col. R.E.

Halifax, Nova Scotia, Dec. 27

From *Nature,* November 22, 1883, p. 87.

Soft Sunlight

In a letter from Maranhoa, Brazil, the writer states that from August 31 up to September 6, the sun until 7 a.m. could be looked at without the least difficulty, its light being as soft and pale as the moon's.

From *Knowledge,* February 1, 1884, p. 77.

BLUE MOON

There is a very old Norfolk proverbial saying, once in a blue moon. Can it have had its origin in the actual and yet very infrequent observation of that phenomenon? Or is it a mere random shot at an illustration of rare events?

The moon here in November was of the intensest sapphire blue, the perfectly clear sky looking rather slaty. This morning at 6.30 there was a fine sky-glow, and so last week. It certainly appeared to come from aqueous vapour.

Jan. 22, 1884 CENTRE-NORFOLK

And a delightful bit of scientific humour from *Knowledge,* March 14, 1884, p. 166.

The red after-glow that has caused so much discussion among philosophers is now explained by a correspondent of the *Scientific American,* who asserts that the phenomenon is due to the red spot from the planet Jupiter. This great rosy cloud disappeared several months ago from the atmosphere of Jupiter, has had just time, according to this correspondent, to travel to our earth, and is now hovering over us, causing the ruby colouring of our skies night and morning. Nobody ever has or will be able to prove that this is not the fact; therefore, it must be true, says the correspondent. The question is settled; it is useless to talk further about cosmic dust, Java ashes, or aqueous vapour.

The various observations of colours are summed up briefly as follows by F.A.A.R. in *Nature,* June 12, 1884, p, 155.

The matter projected into the upper atmosphere appears to have passed around the globe westwards with great velocity, and to have diffused itself towards north and south much less rapidly. A stratum of fine dust thus formed itself at an elevation probably exceeding the altitude of the known upper currents. This stratum caused the sun to look green or blue on the Gold Coast, in the West Indies, at the Sandwich Islands, in India and the Indian Ocean, and last, as late as September 24, in the Soudan, nearly a month after the eruption of Krakotoa. The moon and stars were frequently greenish in Europe in December and January, up to four months and a half after the eruption, and the sun whiter than usual towards setting. The finely divided matter which thus deprived the sun and moon of part of the rays which go to form the compound white, was plainly of a different grain from the small particles commonly present in the sky.

Next month we shall consider some curious telescopic observations of this phenomenon.

NOTES AND QUERIES

Communications are invited, especially from amateurs. The Editor will try to secure answers to queries.

Optical Supplies for Canadian Observers

Amateur and professional astronomers in Canada will be intrested in a circular recently issued by the Clifford and Edwards Co., 213 Broadway Ave., Toronto. This firm has acquired a large stock of achromatic doublets, single lenses and various types of prisms. These are war surplus and reject stock manufactured by a leading Canadian optical firm. Almost a hundred items are listed. Prices are given for uncentred and centred lenses, for slightly chipped but completely serviceable prisms, and for unchipped perfect prisms.

Any of this material we have so far seen has been of fine quality. Amateurs wishing to make up highly corrected and achromatised eyepieces, monocular or binocular field glasses or small telescopes will be pleased to note the wide selection of high grade optical components available at reasonable prices. A complete list of the prisms and lenses may be obtained by writing to the above firm.

F. S. H.

A New Feature in the Journal

This month the Journal starts a new section, pages 161-164, entitled "Out of Old Books." During recent years Dr. Helen Sawyer Hogg has made very intensive bibliographical searches in connection with her studies of globular clusters, variable stars, and novae. In the course of these she has been impressed with the large number of interesting and valuable old articles which are becoming relatively inaccessible. In order to call some of these to the attention of newer astronomers, she now plans to publish selected excerpts from such articles from month to month in this Journal.

F. S. H.

Wise Words for Astronomers—and Others

All will agree that at present there is much confusion in our ideas on education. For half a century and more the validity of everything

OUT OF OLD BOOKS

By Helen Sawyer Hogg

LAST month, in the first number of this new section, we reprinted some vivid descriptions of the coloured sky phenomena visible around the world after the gigantic eruption of Krakatoa in 1883. In at least one city on this continent the sunset glow was so brilliant that the fire department was called out to extinguish what appeared to be a conflagration.

The subject of Krakatoa and its resulting phenomena has been summarized in a massive and scarce volume under the auspices of the Royal Society, entitled *The Eruption of Krakatoa and Subsequent Phenomena,* edited by G. J. Symons; printed by Harrison and Sons, London, 1888. This splendid volume summarizes hundreds of descriptions and discusses the various aspects of the event under five headings. (I.) On the volcanic phenomena of the eruption, and on the nature and distribution of the ejected materials. (II.) On the air waves and sounds caused by the eruption of Krakatoa in August, 1883. (III.) On the seismic sea waves caused by the eruption of Krakatoa, August 26th and 27th, 1883. (IV.) On the unusual optical phenomena of the atmosphere, 1883-6, including twilight effects, coronal appearances, sky haze, coloured suns, moons, etc. (V) Report on the magnetical and electrical phenomena accompanying the Krakatoa explosion.

This volume is strongly recommended to our readers who wish to study the subject in detail. It would appear to be very timely just now, in view of the projected atomic bombing in the Pacific, to consider the important atmospheric and astronomical effects which result from the hurling of large quantities of dust into the earth's atmosphere.

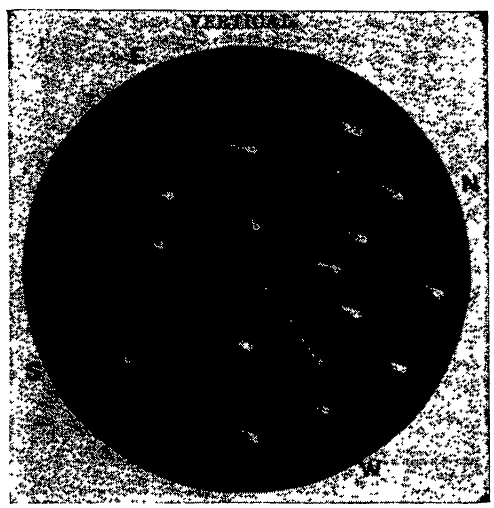
The Royal Society volume does not summarize the accounts of telescopic meteors and floating particles which appear below. Since they are recorded by such experienced and distinguished observers as Professors Brooks and E. E. Barnard, they are worthy of consideration. The account of a flight of telescopic meteors by Brooks is in the article "The Red Sunsets," by William W. Payne, Editor, in *The Sidereal Messenger,* vol 2, p. 294, Jan. 1884.

A SHOWER OF TELESCOPIC METEORS

In a letter from Professor William R. Brooks, Phelps, New York, under date of Dec. 8, 1883. He says: "While sweeping on the evening of November 28th, it was my pleasure to observe a wonderful shower or flight of telescopic meteors about ten degrees above the horizon, and near the sunset point. They were very small, none of them visible to the naked eye, most of them leaving a faint train, visible in the telescope for one or two seconds. The motion of most of them was to the northward, with an occasional group to the south of the Sun, moving southward.

This observation occurring at the time when the unusual red light phenomenon was at its height the theory is suggested of a possible connection between that phenomenon and the passage of the Earth through a mass of meteoric matter more or less attenuated.

Being deeply interested in the very remarkable observation, Professor Brooks was requested to give a drawing of what he saw which was kindly furnished for the *Messenger* and appears below.



Under date of Dec. 15, he further says: "As may readily be inferred, the wonderful sight is a difficult one to represent in a drawing; but I have endeavored to give some idea of the appearance at its maximum stage. The instrument used was a nine-inch reflector with comet eye-piece giving a field of one and one-half degrees. The field shown in the drawing was a few degrees north of the sunset point and about ten degrees above the horizon. The faithful comet seeker, frequently in a single night's work encounters numerous telescopic meteors, singly, very rarely two at once; but this flight is quite unprecedented in my experience."

Mr. E. E. Barnard, Nashville, Tenn., has given attention to these wonderful, and, as he says, unaccountable phenomena. His first notes were made Oct. 29, and since that date, glowing sunsets have been seen quite constantly when the sky was favorable. From observations which cannot now be given, he thinks the cause lies in the upper strata of the atmosphere, though not at all certain of it. He confirms the observation first reported by Professor Brooks.

As late as Dec. 15, he saw with the telescope small bright bodies close to the Sun. They were visible at the rate of five or six per minute, and were all moving to the north of east quite rapidly. Occasionally a larger body was seen to flash across the field, blurred by being out of focus. Generally they looked like little stars, many as bright as those of the first magnitude. Mr. Barnard could follow the slower moving ones with the telescope for five or six degrees from the Sun, where they became faint and were lost. He was unable to detect any crossing the Sun; they seemed to be some distance from it, and required generally an increase of focal distance to see favorably. He thinks they are small particles drifting with the air currents at considerable altitude. He gives no definite opinion concerning the cause of the red sunsets.

This discussion is continued in *The Sidereal Messenger,* vol. 3, p. 18, 1884.

January 9. Mr. Barnard, Nashville, Tenn., was observing with the telescope in the immediate vicinity of the Sun, and saw numbers of small bright particles passing swiftly across the field. He says: "At 3 o'clock p.m. I caught a pretty bright one and followed it for a distance of ten degrees, horizontally towards the north. It was not round, it was not a point, very white; it grew dimmer and dimmer until it faded from view, at the above distance from the Sun. Power, fifty-two diameters. The focus of the instrument was adopted sharply to an object less than a mile distant. So whatever the object was, it could not have been more than a few hundred feet above the Earth."

Mr. Barnard further says: "There is scarcely a day but that these objects are visible close to the Sun."

There is a very general agreement among physicists and astronomers in the thought that these phenomena are caused either by volcanic or meteoric dust in the atmosphere.

From the September 1884 issue of the same journal, we read of Barnard's conclusion, as follows:

The Red Skies

If the phenomenon of our red skies is due to the volcanic eruption in Java, and the proof seems to point to that as a cause, my frequent observations of bright, rapidly-moving particles near the Sun can readily be explained, on the supposition that they were minute particles of ashes drifting through the atmosphere, which had their origin in the awful catastrophe of Krakatoa. Though great numbers of these floating bodies were visible in the instrument, the number too small to be seen must have been far greater. Around the Sun to a distance of 15°, the sky has presented a greenish appearance. The outer border of this glare of light is always terminated by a brick-dust reddish ring, showing that the phenomenon of morning and evening attends the Sun for the entire day. During the first part of the year, the red glow preceded the rising of the full Moon, though of course not so conspicuous as in the case of the Sun, indicating that it does not exist simply in the direction of the Sun, for the full Moon appears almost constantly surrounded by a pearly glare, resembling that around the Sun.

E. E. B.

The atmospheric conditions, especially in the daytime, are a subject for discussion in an article in the October 1884 *Sidereal Messenger*:

RECENT PECULIAR ATMOSPHERIC CONDITIONS

By J. R. Hooper, (Baltimore)

Having noticed during the last six months, an unusual amount of bad "seeing" and especially so in daytime, the cause became a subject of inquiry, and as a result, I beg leave to submit for record, the following notes which may also serve to call out other and more valuable observations.

At night, up to August, the power of the telescope to define has been very unsatisfactory, and in daytime there has been a constant and marked trouble in seeing even second magnitude stars. The unprecedented cloudiness of the first three months of the year, has greatly lessened the number of clear days usually enjoyed. When the clear skies came, the difficulty of doing such daytime work as I had formerly done, was noticed, and still exists at this time of writing.

I therefore raise this query: Is there a change in the atmosphere, or is there in it a foreign material which is capable of modifying the visibility of other than the brightest objects?

Every observer is acquainted with the deceptive "apparent" blue skies, but as bearing on the above query, I append a few data from my own observations made at 8 A.M. and 4 P.M. from April to August of the present year, as follows:

The marked trouble in seeing stars as bright as the second magnitude; frequently not seeing Alpha Andromedae when near the zenith at 8 A.M.

A corresponding indistinctness in first magnitude stars.

In following Mercury in 1883, I readily found him sixteen days after inferior conjunction, though in the Sun's rays, and I could see the crescent very neatly defined a week later.

In 1884, though searching carefully, it was thirty-one days after inferior conjunction before I could see the planet, and then it was five days after greatest elongation. At no time has the disk been fairly defined, while last year the reverse was the rule.

In all observations during these months, the definition of details of every kind on the Sun's face has been superior to that of last year. Hence, the idea comes to mind that there may be some substance in the air which diffuses the Sun's light to an unusual degree, but does not affect the seeing at night, nor make bad definition for the Sun's surface. The stars may seem fainter because of the whiteness of the sky otherwise unnoticeable.

In January and February of this year, I sometimes noticed a peculiar pearly whiteness around the Sun before sunset, and outside of that a dingy reddish tint, the radius of the arc being from 20° to 30°. In March I began to see at midday, a complete halo of the same color, having a radius of about 20°. When the sky was very blue it was well defined, but on days when the whiteness was present it was not seen. In July I measured it on one occasion, and made the inner edge nine degrees from the Sun with a breadth of seven degrees.

In a letter to the *English Mechanic* for June 20, a Fellow of the Royal Society writes:

"As to the bad definition incident on the visibility of the after-glow, I should like to remark that for some time past, daylight definition of celestial objects has been worse than ever I remember it during my tolerably long observing experience.

Transit-taking in daylight, save with the large stars, has been quite impracticable, and over and over again I have looked in vain for Mercury. * * * Of course everyone who is in the habit of using a telescope in the daytime, is familiar with the fact that on many seemingly cloudless days there is an otherwise invisible kind of haze which impairs or destroys definition; and that the best and brightest vision is obtained in the blue sky visible below large floating Cumuli. But this curious obscuration has been just as apparent during the latter condition of the atmosphere as during the former."

These graphic accounts can certainly serve as a warning as to how much our atmospheric conditions may effect our astronomical observations.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Lunar Eclipses

THE lunar eclipse of October 4, 1884, is cited in several text-books in current use as being the last lunar eclipse during which the moon became totally invisible. Actually, from a perusal of eye-witness accounts at that time, it appears that the moon did not become invisible during this eclipse.

It is worthwhile then to consider accounts of the eclipses which have occurred since the invention of the telescope, during which the eclipsed moon disappeared completely, so that no trace of it could be seen in the sky, either with the naked-eye or with a telescope. There appear to be three such cases; the eclipses of April 25, 1642, of May 18, 1761, and of June 10, 1816. To these may be added the abnormal obscurity of the moon in the eclipses of December 1601, of June 1620 and October 4, 1884.

The eye-witness descriptions of these remarkable events, along with other comments on lunar eclipses follow. The descriptions of the 1884 eclipse will be summarized in our next issue.

From Chambers, G. F., *A Handbook of Descriptive and Practical Astronomy,* 4th ed. 1889, p. 327.

Even when most deeply immersed in the earth's shadow, our satellite does not, except on rare occasions, wholly disappear, but may be generally detected with a telescope (and frequently with the naked eye), exhibiting a dull red or coppery colour. This was exemplified in a very remarkable manner in the case of the eclipse of March 19, 1848, on which occasion the moon was seen so clearly that many persons doubted the reality of the eclipse.

Mr. Forster, who observed the eclipse at Bruges, writes as follows:—

"I wish to call your attention to the fact which I have clearly ascertained, that during the whole of the late eclipse of March 19, the shaded surface presented a luminosity quite unusual, probably about three times the intensity of the mean illumination of the eclipsed lunar disc. The light was of a deep red colour. During the totality of the eclipse, the light and dark places on the face of the moon could be almost as well made out as on an ordinary dull moonlight night, and the deep red colour where the sky was clearer was very remarkable from the contrasted whiteness of the stars. My observations were made with different telescopes; but all presented the same appearance, and the remarkable luminosity struck every one. The British Consul at Ghent, *who did not know there was an eclipse,* wrote to me for an explanation of the blood-red colour of the moon at 9 o'clock." (*M.N.*, vol. 8, p. 132, Mar. 1848).

As a complement to this observation, I may quote one by Wargentin of the total eclipse of May 18, 1761. He says that 11*m* after the commencement of the phase—

"The moon's body had disappeared so completely, that not the slightest trace of any portion of the lunar disc could be discerned either with the naked eye or with the telescope, although the sky was clear, and the stars in the vicinity of the moon were distinctly visible in the telescope."

Phil. Trans., vol. li, p. 210, 1761. The original runs thus: "Tota luna ita prorsus disparuerat, ut nullum ejus vestigium, vel nudis, vel armatis oculis, sensibile restaret, coelo licet sereno et stellis vicinis in tubo conspicuis." Other eclipses, where the same thing occurred, took place on June 15, 1620 (Kepler, *Epist. Ast.*, p. 825); April 25, 1642 (Hevelius, *Selenog.*, p. 117); and June 10, 1816 (Beer and Mädler).

von Humboldt, A., *A Physical Description of the Heavens.* Sabine's translation. 1867, p. 355.

In total lunar eclipses it happens in some exceedingly rare cases that the moon disappears wholly; it did so, according to Kepler's earliest observations (*Paralip. vel Astronomiae pars Optica,* 1604, p. 297), on the 9th of December, 1601; and in more recent times, on the 10th of June, 1816, in London, when it could not be discerned even with telescopes. A peculiar, not sufficiently explained, state of the several strata of the atmosphere in regard to transparency must be the cause of this equally rare and curious phenomenon. Hevelius remarks expressly, that, in a total eclipse on the 25th of April, 1642, the sky was covered with sparkling stars, the air being perfectly clear, and yet, with the very various magnifying powers which he employed, the moon's disc continued without a trace of visibility. In other also very rare cases, only some portions of the moon are faintly visible. In ordinary cases the disc appears, during a total eclipse, of a reddish hue, the colour being, indeed, of the most various degrees of intensity, passing even, when the moon is far removed from the earth, into a fiery glowing red. Whilst, more than half a century ago (29th of March, 1801), I was lying at anchor off the Island of Baru, not far from Cartagena de Indias, and observing a total lunar eclipse, I was exceedingly struck by seeing how much brighter the reddened disc of the moon appears in the sky of the tropics than in my northern native land.

From *Popular Astronomy,* by F. Arago (tr. by Smyth and Grant), 1858, Vol. II, p. 353.

Now, the annals of astronomy contain observations of many total eclipses of the moon, accompanied by an entire disappearance of that body. Hevelius relates that no trace of our satellite was visible during the eclipse of the 25th of April, 1642. Maraldi relates that he observed a similar phenomenon on several occasions. And according to MM. Beer and Mädler, the moon also disappeared entirely both in London and at Dresden, during the eclipse of the 10th of June, 1816.

As the cone into which the moon penetrates when she ceases to be visible, must have for its axis the line passing through the centre of the sun and the centre of the earth, it appears impossible that the eclipsed moon should ever be seen above the horizon at the same time with the sun.

Cleomedes remarks that a statement to this effect which had been recorded by authors earlier than himself, was a mere imaginary story invented to perplex astronomers.

Yet it is certain that during the eclipse of the 16th of June, 1666, seen in Tuscany, the moon rose eclipsed, the sun being still above the western horizon, a circumstance which seemed to imply that the two bodies were not diametrically opposite to each other relatively to the centre of the earth. We may also quote the eclipse that was observed at Montmartre, the 26th of May, 1668, by the members of the Academy of Sciences, on the occasion of which the same peculiarity occurred.

Yet this phenomenon is inconsistent, only in appearance, with the theory of eclipses. Atmospheric refraction causes both the sun and moon to appear in positions which they do not really occupy. Refraction accelerates the apparent rising of the moon, and retards the apparent setting of the sun.

Although in the two instances that we have cited, the two bodies were both below the horizon, the rays of light emanating from the various parts of their discs reached the eye of the observer by pursuing a curvilinear route through the atmosphere.

By taking from the tables - - the value of atmospheric refraction at the horizon, we shall explain the phenomenon of the simultaneous apparition of the two bodies, in 1666 and 1668, even to the minutest numerical details.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Lunar Eclipse of October 4, 1884

LAST month we reprinted descriptions of several lunar eclipses of the past three hundred years during which the moon became totally invisible, even in a telescope. The eclipse of October 4, 1884 is frequently cited in textbooks as belonging in this category, but the following extracts will show that this is not the case. Although the moon appeared as faint as the Milky Way, had the brightness of only a third magnitude star, and resembled a faint, white, hazy nebula, the reports agree that it did not completely disappear.

This eclipse was not visible in North America. The *Sidereal Messenger,* vol. 3, p. 288, Nov. 1884, dismisses the eclipse as follows:

The eclipses of the Moon and Sun on October 4 and 18, respectively, were of little interest to observers in America because invisible, and hence reports concerning them have been omitted.

Many reports of this lunar eclipse, however, appear in the journals from the other side of the water, and are most interesting reading. So many years have now elapsed since the moon appeared abnormally faint during an eclipse that it is difficult for us nowadays to visualize such an appearance. Extracts from some of these accounts to be found in Volume 45 of the *Monthly Notices,* 1884, appear below.

As observed at the Radcliffe Observatory, Oxford, by E. J. Stone.

I merely watched the eclipse with the naked eye. The eclipse was much the darkest that I have ever seen, and just before the instant of totality it appeared as if the Moon's surface would be invisible to the naked eye during totality; but such was not the case; for with the last appearance of the bright reflected sunlight there appeared a dim circle of light around the Moon's disk, and the whole surface became faintly visible, and continued so until the end of totality.

As observed at Dun Echt by J. G. Lohse, with the 6-inch Simms' Equatorial, power 91.

During the totality the Moon was very faint, and the copper tint, so conspicuous in other eclipses, was only seen occasionally, and then only feebly, and not uniformly spread over the Moon, but more intense in some parts than in others.

As observed at Stonyhurst, by the Rev. S. J. Perry.

This eclipse was observed at Stonyhurst under very favourable circumstances, although the heavy dew interfered occasionally with the definition of the telescopes.

With a good binocular the Moon's disk was easily seen throughout the whole of totality, but to the naked eye there was no outline, the Moon having the appearance of a bright patch of light. The usual copper tint of the eclipsed Moon was not perceived except towards the close of the eclipse, and then it was only very slight. The general appearance in the binocular was that of a dull white ball, rather more brilliant at one part of the surface than elsewhere. The telescopes used were two equatorials of 8 and 4 inches, a 9½-inch Cassegrain, and a 7-inch Newtonian.

As observed at Harrow by Lieut. Col. G. L. Tupman.

I used the 4½-inch Cooke Refractor which is fitted with position-circle and bar eyepiece, power 66, extremely convenient for occultations.

The total light reflected from the Moon was compared directly with the stars as follows:-

At 9^h 45^m	G.M.T. equal to	2 magnitude.
10 8	"	3 "
10 30	"	2 "
10 44	"	Capella

The very slight ruddy tint during totality was not perceptible until a direct comparison with white stars was made.

Observations at Bristol by W. F. Denning.

A perfectly cloudless sky enabled the total lunar eclipse of October 4 to be well observed from this city.

The most noteworthy feature in connection with the phenomenon was that the Moon, at the total phase, appeared far less luminous than usual. The remarkably opacity of the shadow became evident when it had well encroached upon the Moon's eastern limb, and the fact was fully confirmed by the aspect of her disc during the subsequent stages of the eclipse.

The firmament grew as dark as on an ordinary night when the Moon is entirely absent, and but for the indistinct outline of our satellite projected upon the dark background of the sky, there was nothing abnormal in the appearance of the heavens. Small stars could be distinguished with the customary readiness, and the Moon herself, high in the southern sky, looked like a large, ill-defined nebula with indeterminate outlines, or like a planet struggling feebly through very thick haze. Applying my 10-in. reflector, power 60, her sharply circular contour, however, still admitted of satisfactory observation, and many leading features of the surface were recognised amid the prevailing gloom in which her landscape was involved.

Observations at Clapham, by E. J. Spitta.

The sky was cloudless, but owing to the glare of the Moon no stars before No. 74 of the Pulkova list could be recognised with the 10-inch Calver Reflector, power 60. During totality the Moon was, generally speaking, exceedingly faint —indeed, at times barely visible to the naked eye—and presented none of the coppery colour usual on these occasions. It was bluish at the lower edge as seen in the inverting telescope about 10 o'clock, when the other portion seemed brighter than at any time. No markings were plain enough to be recognised.

A particularly graphic account is that of the Rev S. J. Johnson, Bridport.

On the evening of October 4 there was a conspicuous return of the sunset after-glow that was common last winter. A peculiar state of the strata of our atmosphere might, therefore, indicate that something unusual was to be expected in the eclipse following.

At 8^h, penumbra barely perceptible with opera-glass.

At $9^h\ 7^m$, for the first time, a very small portion of the eclipsed disk, extending only 4′ or 5′ inwards, could be discerned through the telscope, this appearing of a dark slate colour—the same tint as usually observed at the commencement of an eclipse.

At $9^h\ 10^m$, the whole of the lunar circle began to be seen through the telescope, but without a trace of the ordinary coppery redness.

$10^h\ 2^m$, middle of totality. To the naked eye nothing could be seen but a faint nebulous spot. That the obscurity of the Moon arose from lack of illumination, not from fog or cloud, was seen by the fact that stars of small magnitudes above and below the lunar disk shone as distinctly as on an ordinary dark night. The exact appearance of the Moon at this time would be described by quoting Kepler's words verbatim about the eclipse of June (not December) 1620. "Luna difficillimè apparuit, emicuit tamen instar tenuissimae nubeculae, longè debilior quam viâ lacteâ, sine omni rubedine."

$10^h\ 49^m\ 15^s$, sunlight breaking out.

The shadow left the Moon near the Mare Foecunditatis about 11.48¾, the penumbra being conspicuous to the naked eye at 11.51. Thus, while the Moon did not completely disappear during totality (except to one correspondent), the peculiar features of the eclipse were the complete invisibility of the eclipsed orb until nine-tenths were covered, also subsequent to the total phase after one-tenth was uncovered. In this respect it seems similar to the earliest instance of the kind given in modern times, that observed by Kepler in 1601, an eclipse of nine-tenths of the Moon's disk. His words are (Astronomiae pars optica), "Anno 1601 Decembri, tenuissimo cornu superstite, caliginosam tamen partem non vidi."

The usual explanation, that when the atmosphere is remarkably free from vapour the red rays would be absorbed, is hardly an adequate one.

(1) Because the atmosphere was in an equal state of dryness on the occasion of the eclipse of July 12, 1870, when the Moon assumed the usual dull red or copper colour.

(2) Because the last instance of the disappearance of the Moon was on June 10, 1816, when it could not be discerned even with telescopes, and this was one of the wettest summers in the century.

From Eruption of Krakatoa, Symonds. p. 226.

The only other subsequent lunar eclipse of importance, that of March 30th, 1885, was not visible in this country, but was observed in Tasmania by Mr. A. B. Biggs. He says that, at the time of maximum eclipse, "All within the shadow was utterly obliterated—lost in the dead slaty tint of the sky. I could not distinguish a single crater after once it was fairly within the shadow. Not the slightest trace of the coppery tint was visible throughout."

This peculiar absence of the coppery tint ordinarily visible and in circumstances which are described as having been very favourable for observation "the sky being free from clouds, and the moon in full view during the whole period of the eclipse," seems to favour the notion that the haze not only exerted a general absorption, but, as the appearance of the blue and green suns show, a selective absorption, more especially of the red end of the spectrum.

This is followed by an Editor's footnote:

Since this was in type an article by Professor Dufour has appeared in Flammarion's *'l'Astronomie,'* (January, 1888), in which he strongly supports the theory that the almost complete invisibility of the moon in 1884 was due to Krakatoa dust, and he refers to M. Flammarion having expressed the same view. On turning to M. Flammarion's original statement (*l'Astronomie,* 1884, p. 407) it will be found that he refers to the eclipses of April 25, 1642, May 18, 1761, and June 10 1816, as previous analogous cases. On comparing these and that of 1620 observed by Kepler, with Part IV., Sec. V., of this volume, it will be found that in each of these four instances there had been an eruption in the previous year—that of 1815 being the great one of Tomboro.

The eruptions referred to in the table are of Hecla, in 1619, of Taal, Luzon, in 1641, of Vesuvius, and Peteroa, Chile, in 1760. It could also be pointed out that the very violent eruption of Hecla in 1597 antedates by only four years Kepler's observations of the faint lunar eclipse of 1601.

From Flammarion, *l'Astronomie,* 1884, p. 407.

Nos lecteurs savent que l'astre des nuits a disparu entièrement pendant les éclipses de 1642, 1761 et 1816, à ce point qu'il était impossible de retrouver sa place dans le ciel.

By 1887, the atmosphere had taken on the transparency which it had temporarily lost by the Krakatoa explosion, as witness Dufour's account of the eclipse of that year, in *l'Astronomie,* 1888, p. 29.

Depuis 1884, il n'y a eu aucune éclipse totale de Lune; mais il y a eu deux éclipses partielles, une le 30 mars 1885 et l'autre le 24 septembre de la même année. Elles ont été toutes les deux à peu près invisibles pour nos latitudes.

L'eclipse du 3 août 1887 est, depuis 1884, la première pendant laquelle nous avons pu voir la région obscure; or, cette fois-là, la couleur orange sur cette région était très sensible soit avec une lunette, soit à l'oeil nu. C'est une preuve que notre atmosphère a repris, du moins en grand partie, la transparence qu'elle avait momentanément perdue en 1884.

We must conclude therefore that the last eclipse during which the moon disappeared completely, even in a telescope, was that of June 10, 1816.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Leonardo da Vinci

THE great Italian artist and scientist, Leonardo da Vinci, who lived from 1452 to 1519, is credited with first explaining that beautiful phenomenon known as the "old moon in the new moon's arms". As well as correctly interpreting the faint light on the dark side of the moon, he made in his voluminous note-books other astronomical remarks that are noteworthy. Some of them show a shrewdness of observation which is lacking in many people five hundred years later.

We quote material from the vast collections of his note-books, as compiled under different subjects by MacCurdy, in the two-volume work, *The Note-books of Leonardo da Vinci,* arranged by Edward MacCurdy. Jonathan Cape, London, 1938. Vol. 1, pp. 291-314, Astronomy.

It is not always possible to tell how much in Leonardo's note-books was his original idea, and how much was based on his extensive knowledge of classical thought. As MacCurdy says, "To estimate aright the value of his researches in the various domains of science would require an almost encyclopaedic width of knowledge." One cannot fail to admire this versatile genius who lived before the invention of the telescope and our present day ideas of the solar system as developed by Copernicus, Galileo, Kepler and Newton.

Make glasses in order to see the moon large.

. . . . . .

When the sun during an eclipse assumes the shape of a crescent, take a thin plate of iron and make a small hole in it, and turn the face of this plate towards the sun, holding a sheet of paper behind it at a distance of half a braccio, and you will see the image of the sun appear on this sheet in the shape of a crescent, similar in form and colour to its cause.

. . . . . .

The moon has every month a winter and a summer.

And it has greater colds and greater heats and its equinoxes are colder than ours.

The preceding quotations are from the manuscript known as the *Codice Atlantico,* in the Ambrosian Library, Milan.

The moon is not luminous in itself, but it is well fitted to take the characteristics of light after the manner of the mirror or of water or any other shining body.

. . . . . .

It may be readily understood that every planet and star is farther away from us when in the west than when it is overhead, by about three thousand five hundred [miles] according to the proof given at the side [of the page];

. . . . . .

The stars are visible by night and not by day owing to our being beneath the dense atmosphere which is full of an infinite number of particles of moisture. Each of these is lit up when it is struck by the rays of the sun and consequently the innumerable radiant particles veil these stars; and if it were not for this atmosphere the sky would always show the stars against the darkness.

. . . . . .

Some say that the sun is not hot because it is not the colour of fire but is much paler and clearer. To these we may reply that when liquefied bronze is at its maximum of heat it most resembles the sun in colour, and when it is less hot it has more of the colour of fire.

. . . . . .

Whether stars have light from the sun or in themselves:

It is said that they have light in themselves, since if Venus and Mercury had no light of their own, when they come between our eye and the sun they would darken as much of the sun as they cover from our eyes. This however is false, because it has been proved how a dark object placed against a luminous body is surrounded and entirely covered by the lateral rays of the remainder of this luminous body, and so it remains invisible. As is shown when the sun is seen through the ramification of leafless trees in the far distance, these branches do not conceal any part of the sun from our eyes. The same thing happens with the above mentioned planets, for though they are themselves without light they do not as has been said cover any part of the sun from our eyes.

It is said that the stars at night appear most brilliant in proportion as they are higher up, and that if they have no light of their own the shadow cast by the earth when it comes between them and the sun would come to darken them, since these stars neither see nor are seen by the solar body.

But those who say this have not considered that the pyramidal shadow of the earth does not reach many of the stars, and that in those which it does reach the pyramid is so diminished that it covers little of the body of the star, and all the rest is illuminated by the sun.

The foregoing group of quotations is from manuscripts in the Library of the Institut de France.

The moon has no light of itself but so much of it as the sun sees, it illuminates. Of this illuminated part we see as much as faces us. And its night receives as much brightness as our waters lend it as they reflect upon it the image of the sun, which is mirrored in all those waters that face the sun and the moon.

The above is from the Arundel manuscript in the British Museum.

And in the manuscript *Quaderni d'Anatomia,* in the Royal Library, Windsor, is written—

The sun does not move.

From the Leicester manuscript, owned by Pierpont Morgan, is another reference to the phenomenon of earthshine.

When the eye in the east sees the moon in the west near the setting sun it sees it with its shaded part surrounded by the luminous part; of which light the lateral and upper portions are derived from the sun and the lower portion from the western ocean, which still receives the solar rays and reflects them in the lower seas of the moon, and moreover it imparts as much radiance to the whole of the shaded part of the moon as the moon gives to the earth at midnight, and for this reason it does not become absolutely dark. And from this some have believed that the moon has in part a light of its own in addition to that which is given it by the sun, and that this light is due to the cause already mentioned, namely that our seas are illumined by the sun. (Diagram) moon, solar body, earth.

. . . . . .

Some have believed that the moon has some light of its own, but this opinion is false, for they have based it upon that glimmer which is visible in the middle between the horns of the new moon, which appears dark where it borders on the bright part, and where it borders on the darkness of the background seems so bright that many have assumed it to be a ring of new radiance which completes the circle where the radiance of the tips of the horns illuminated by the sun ceases.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

DEARTH OF SUN-SPOTS IN THE SEVENTEENTH CENTURY

AT the present time the sun presents a highly spotted appearance, quite in accordance with expectations of the approach toward maximum in the sun-spot cycle. Nowadays we take for granted the appearance of large numbers of spots on the sun's disc within several years of the maximum of the sun-spot cycle. It is worthwhile recalling that not always has the sun behaved in this manner, and that the seventeenth century is remarkable for a dearth of sun-spots.

An excellent summary of the observations of sun-spots in the seventeenth century is given in *Knowledge,* vol. 17, pp. 173-176, August 1, 1894, by Walter Maunder, who developed the famous "butterfly diagram" of sun-spot frequency. We quote the more significant excerpts from the article.

A Prolonged Sun-spot Minimum

The progress of the sun-spot cycle has been on the whole so regular since Schwabe first demonstrated its existence, that we might easily assume it to be a fundamental law of solar change. Maximum has succeeded maximum, and minimum minimum, at an interval in each case so closely approaching eleven years, that we might confidently expect the present great activity to begin to decline within the next year or so, and to reach a minimum about the end of the century, as a year or two ago we were looking for some such development of spots as we are beholding now.

The sequence of maximum and minimum has, in fact, been unfailing during the present century. Within the experience of living observers the appointed time has never come round without the appearance of spots, vast in area, violent in change, and many in number; and one might be forgiven for inferring that as it has been for so long, it must always be, and must always have been. And yet there is the strongest reason to believe that for something like half a century, if not indeed for the full term of "threescore years and ten" allotted as the span of human life, the ordinary solar cycle was once interrupted and one long period of almost unbroken quiescence prevailed.

If we turn back to the earliest volumes of *Philosophical Transactions* of the Royal Society, we find the first notice of a sun-spot occurs in No. 74, dated August 14th, 1671, where it is stated that "at Paris the Excellent Signor Cassini hath lately detected again Spots in the Sun, of which none have been seen these many years that we know of". So novel, indeed, was the observation felt to be, that Oldenburg, the Secretary of the Royal Society, and editor of the *Transactions,* evidently considered it due to those interested in science to give an account of the last observations of the kind that had been secured in England. These had been made "by our Noble Philosopher Mr. Boyl", in the months of April and May, 1660; that is to say, more than eleven years before.

Cassini's observations reached London in due course and appear in the next number of the *Transactions,* which bears the date September 18th, 1671. His first words, which have been "English't out of the French," run as follows, and also point to the great rarity of sun-spots at that period:—

"It is now about twenty years since that Astronomers have not seen any considerable Spots in the Sun, though before that time, since the Invention of Telescopes, they have from time to time observed them. The Sun appeared all that while with an entire brightness, and Signor Cassini saw him so the ninth of this month of August.

"But the Eleventh of the same, about six o'clock at night, being furnisht only with a three foot glass, he remarked in the Sun's disque Two Spots very dark, distant from his apparent Center about the third part of his Semi-diameter."

Then follows a description of the appearances of the spots and of their positions on the three days of observation, August 11th, 12th and 13th, but this, though so interesting to the astronomers of the time, need not claim our attention. The point of wonder to them was that any spots at all should be seen; the point of wonder to us is that they were not common and familiar objects.

This group was observed at its return in the next rotation, amongst others by Dr. Hooke, the celebrated Professor of Geometry at Gresham College. But after it passed off the disc the second time the sun appears to have remained spot-free until November in the following year, when Le Monnier records the passage of a group. Then again a blank interval follows until in July and August, 1676, a fresh outburst occurred which drew the attention of several astronomers, our own Flamsteed and Halley in England, as well as Cassini at Paris. Later in the same year Cassini records another group which was seen during four rotations, and of this he says: "This is the third spot which has appeared in the year 1676, in which they have been more frequent than they have been during the twenty years preceding." A group is recorded in April, 1677, two or three in 1678. In connection with one of these appearing towards the end of May, Cassini remarks that the spot "was quite close to the limb in the middle of a region brighter than the rest of the surface of the sun, and was followed by a great number of other little markings of a like character... These have been called Faculae by Scheiner, who has often observed them. They have already appeared once this year, after the dissipation of the spots which appeared from the 25th of February, to the 4th of March."

In 1680, spots were seen in May, June, and August, the group of May being a large one. In 1681, Siverus and Vagetius record spots in May and June. Then we come to 1684, when Flamsteed again has given us an observation of a spot on the sun's disc. "These appearances", he adds, "however frequent in the days of Scheiner and Galileo, have been so rare of late that this is the only one I have seen in his face since December, 1676."

This year, 1684, seems to have shown several spot-groups; 1686, two or three; 1687, none; for Cassini states in 1688 that "no matter what care he had taken to observe the sun when the sky was clear, he had not been able, since the year 1686, to detect any spot upon it until the 12th of May last." A few spots were seen in 1688 and 1689; then nearly six years passed without any being observed, for the next record is one of a great spot in May, 1695. Again a barren period set in, and it is not until the eighteenth century was about to dawn that we find any further observations. De la Hire and Cassini observed a great spot in November, 1700; the spot of May, 1695, was thus the only one recorded in a period of eleven years.

This long-continued dearth was now approaching the close. The Rev. William Derham, who made a special study of the sun at Upminster, has given a record of his observations in No. 330 of the *Philosophical Transactions,* and in most months of 1704 and 1705 one or more spots were seen. More remarkable still, in January, 1704, and again in October, 1705, the appearance of two separate groups on the sun at the same time was signalized. "Since the observations of Scheiner", is the comment of the *Histoire de l'Academie,* "made sixty years ago, one has scarcely ever seen two groups of spots on the sun at the same time". The wonder was, however, destined to become more common, for in 1707 it happened twice.

A new decline then set in. Derham's observation of a spot in January, 1710, and De la Hire's of another in October of the same year, appear to be the last for a considerable time, the sun being spot-free, according to Wurzelbauer, from the time of the disappearance of the last-mentioned group, October 29th, 1710, till May 19th, 1713. But a few years later quite a different state of things prevailed, and in 1716 the *Histoire* records: "This year has had still more spots than the preceding, and perhaps no other year has had so many. There have been twenty-one different appearances of them counting only as a single appearance that of several different spots at a time. Only the months of February, March, October, and December have been free from them. In the other months the appearances closely followed each other, and to compensate several lasted only a short time. The phenomenon of two different spots at the same time has entirely ceased to be rare. It has been seen on April 20th and 21st, May 11th, July 26th, but that which is yet more remarkable, from August 30th to September 3rd eight different and totally distinct groups of spots have been seen in different regions of the sun. No diminution in the brightness of the sun has, however, been perceived."

With this great increase in activity, which rose to a yet more decided maximum in 1718, the long period of depression came to an end. It is not so certain when it commenced. A maximum was due, according to the ordinary course of the eleven years period, in 1649, but there are very few observations of spots at the time, and not a few notices in the years from 1648 to 1660 that the sun was spot-free.

Perhaps the most striking evidence we have of the reality of this prolonged dearth lies in the evident excitement and interest which the occasional appearance of a spot called forth. Picard, who independently discovered Cassini's spot of 1671, when at sea near the Texel, declares that he "was so much the better pleased at discovering it, since it was ten whole years since he had seen one, no matter how great the care which he had taken from time to time to watch for them". Cassini, again and again, and Flamsteed also, announce the time when a spot may be expected to return, for the benefit of their brother astronomers, since it is a "phenomenon which it is not in our power to see whenever we wish." And the references to the more prolific times of Scheiner and Galileo are not infrequent.

On the whole, then, it appears probable that after the maximum of 1639-40 had fairly died out—that is, after about 1643—a prolonged period of almost entire absence of spots set in, a period broken only by the very rare apparition of a single spot now and then, and by the feeble revival of 1703-7, and which cannot be considered to have terminated until the setting in of the maximum of 1715-20. For all this period, or at any rate from 1655 to 1703, the ordinary progress of the solar cycle appears to have been in abeyance, and we can hardly speak of it but as an extraordinary and immensely prolonged minimum.

And yet, just as in a deeply inundated country the loftiest objects will still raise their heads above the flood, and a spire here, a hill, a tower, a tree there, enable one to trace out the configuration of the submerged champaign, so the few solitary spots observed seem to mark out the crests of a sunken spot-curve. 1660, 1671, 1684, 1695, 1705, the years most notable for such stray spots as were observed correspond as nearly as we can expect to the theoretical dates of maximum.

It may be objected, perhaps, that bearing in mind the feeble instruments of those days, and the paucity of observers, it may well have happened that many spots may have passed unnoticed during the seventy years in question. But no great instrumental power is needed to detect the presence of a sun-spot, and indeed a single diligent observer using his unassisted sight during the last twenty-five years would have compiled a far heavier record than all the astronomers of Europe have been able to hand down to us for the years between 1643 and 1715. Besides, telescopes were certainly considerably more powerful than those in use earlier in the century. Yet we have but to refer to the works of Galileo, or to the plates, drawn with such evident care and fidelity, which adorn Scheiner's huge "Rosa Ursina", to be assured that outbursts such as the sun presented in 1625-27 could never have passed unnoticed fifty years later.

Nor were the observers few. In England there were Flamsteed, Halley, Hooke, Gray, Derham; in Holland, Huyghens; in Germany, Vogelius, Siverus, Vagetius, Wurzelbauer; in France, Picard, De la Hire, Maraldi, and above all Cassini. Indeed Derham, writing in 1711, well anticipates this objection. "There are", he says, "doubtless great intervals sometimes when the sun is free, as between the years 1660 and 1671, 1676 and 1684. In which time Spots could hardly escape the sight of so many curious Observers of the Sun, as were then perpetually peeping upon him with their Telescopes in England, France, Germany, Italy, and all the World over; whatever might be before, from Scheiner's time." And the fact that a new spot was apt to be independently discovered by more than one observer shows that the watch was carefully kept. The sentinels were neither few nor inefficient, nor can many stranger spots have eluded their vigilance, except under the cover of continued cloud.

A few, perhaps, did pass unnoticed. The watchful Cassini thus missed the spot of January 22nd, 1710, which Derham detected. Flamsteed let slip several which Cassini observed, but after making the utmost allowance for imperfection in the record there can be no doubt that the seventy years in question were barren of spots to a degree to which there has been nothing to correspond since.

The unanimity with which, when the earlier days of revival had come, all observers speak of the great frequency of spots as quite a new experience, is irrefragable evidence that the change was a very real one... Derham, in 1711, refers to 1690-99 as a time "when Spots on the Sun were more rare than for these three or four years past they have been," and yet his record for these more prolific years, when compared with those of the present century, shows that 1704 and 1705, though so abnormally active as compared with the sixty years that had preceded them, were little (if any) richer than the years of an ordinary minimum. We expect now about sixty per cent. of the days of such a year to be free from spots; for the years immediately preceding or following minimum say about twenty-five per cent. Near maximum we expect to get five or six years running without a single instance of the sun being spot-free. And so far from two groups at once on the sun being a rarity, we look to have a dozen on the average, a number which may rise to seventeen or eighteen on occasion, as it did in December last; whilst a maximum year, like those of 1892 or 1893, will show three or four hundred distinct spot-groups, instead of the twenty-one which were considered so extraordinary a record in 1716. It would be quite impossible in these days for a Wurzelbauer, however small his instrument, to observe "daily" for five years without seeing a spot, or for a Cassini or Picard to do the same "from time to time" over a period of ten years, with a like barren result.

This long-continued solar rest suggests many questions to which we can find no reply. How we regret the absence of magnetic records, and how valuable would be a few of those ponderous volumes of rainfall and temperature that, when published by modern observatories and in these days, we are apt to look upon with scorn...

But there were a few total eclipses—those of 1654, 1659, and 1661—and they give us no hint of either red flames or of corona, whereas we get observations of both from the eclipse of 1715, and of the red flames from that of 1706. We cannot, therefore, say what shape the corona, which now appears to vary in such sympathy with the spot-curve, had during the period. Is it possible that, like the spots, it was in a state of partial abeyance?

In our next issue we will consider the auroral observations of the 17th century as an indication of how the earth's magnetic activity was affected by this lack of sun-spots.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Absence of Aurorae in the Seventeenth Century

LAST month we reprinted an article by Maunder in the August issue of Knowledge, 1894, describing the amazing dearth of sun-spots during the latter half of the seventeenth century. In the September 1894 issue of the same magazine, Agnes Clerke replies to this article by noting the absence of aurorae during the same period. We quote her letter to the Editor, as well as excerpts from a more detailed account of this phenomenon from the *Edinburgh Review.*

From *Knowledge,* volume 17, page 206.

A Prolonged Sun-Spot Minimum

To the Editor of Knowledge.

Dear Sir,- The partial suspension of solar activity established by Mr. Maunder in his article entitled as above (Knowledge, August, 1894), represents a curious phase of solar history. There is, besides, strong, although indirect evidence that the "prolonged sun-spot minimum was attended by a profound magnetic calm. This evidence is to be found in the auroral records of the time. For the connection between the occurrence of aurorae and the magnetic conditions of the earth is so close, that the absence of one kind of disturbance may safely be held to betoken the absence of the other.

Now in England, during the whole of the seventeenth century, not an auroral glimmer was chronicled. Stowe recounts that on the 14th and 15th of November, 1574, "the heavens from all parts did seem to burn marvellous ragingly"; and the next similar occurrence took place on March 17th, 1716. Upon his observations of this fine display, Halley founded his magnetic theory of aurorae. The event created an extraordinary sensation throughout the country, some slight and partial sky-illuminations in 1706 and 1709 having escaped general notice.

On the Continent, the auroral blank was much less complete than in this country. Gassendi's aurora of September 12th, 1621, was seen as far south as Aleppo; Cromerus registered the passage of "whole armies" across the sky in 1629; in 1640, south polar lights were visible in Chili every night of February and March, and some corresponding appearances were noted in northern latitudes. By the middle of the century, however, polar lights had virtually died out everywhere, except perhaps in northern Scotland, where the "merry dancers" were seen without surprise in 1691. But even in Iceland and Norway they became so rare as to be considered portentous, and their reappearance at Copenhagen in 1709 was greeted with consternation and amazement. De Mairan, in his "Traité de l'Aurore Boréale," makes the curious remark that a great extension of the zodiacal light attended the auroral outburst of 1716.

As regards the solar corona during the "prolonged minimum," it appears probable that (as Mr. Maunder suggests) its radiated structure was in abeyance, but there is positive proof that the inner corona maintained at least its average brightness in 1666. The partial solar eclipse of June 22nd in that year, being viewed through Boyle's sixty-foot telescope by Hooke, Pope (Professor of Astronomy in Gresham College), and others, "there was perceived a little of the limb of the moon without the disc of the sun, which seemed to some of the observers to come from some shining atmosphere about the body of either the sun or moon."

Yours faithfully
Agnes M. Clerke

From *The Edinburgh Review,* Oct. 1886, p. 416.

Under the designation 'Aurora Borealis' Gassendi included, in 1621, the luminous appearances vaguely and variously described by ancient and mediæval writers as 'chasmata', 'trabes', 'faces', 'capra saltans', 'draco volans', etc. The bestowal of the name was in itself no slight help towards a better understanding of the phenomenon. No longer a mere portent, it assumed thenceforward the character of a scientific problem. Curiosity on the subject largely superseded fear. Above all, some degree of distinctness was introduced into ideas about it, and accounts of its apparitions grew less bewildered; the older records often leaving it profoundly uncertain whether they refer to auroral arches and streamers, to comets, brilliant meteors, or even to conspicuous manifestations of the Zodiacal Light.

In general, however, we are safe in giving an auroral interpretation to the chronicled spectral battles, when

'Fierce fiery warriors fought upon the clouds,
In ranks, and squadrons, and right form of war',

such as were widely seen during the disastrous Cimbrian campaign of 113 B.C.; again at Rome,

'A little ere the mightiest Julius fell';

and such as Josephus describes among the presages of the destruction of Jerusalem.

Celestial conflagrations, too, may be unhesitatingly set down to the same cause. In 450 A.D., just before the irruption of Attila into Italy, the sky (Isidore of Seville narrates) 'turned red, as if with fire or blood;' and a 'blazing of the 'heavens' was quite a common sight in the following century, when St. Gregory of Tours wrote his 'Historia Francorum.' The illusion, indeed, has sometimes been complete. Thus, in the region of Tiberius, the cohorts hastened to the relief of Ostia, believed to be a prey to actual flames from the lurid glow hanging over it (Seneca, *Quaest. Nat.,* l'b. i, cap. xv); bodies of firemen were called out under similar circumstances in Paris, September 28, 1827; and in the rural parts of England, the crimson aurora of October 24, 1870, was explained as the reflection of a vast Prussian bonfire fed by the beleaguered French capital.

In Ireland, such fires turn to streams of blood. The slain at Balaklava were supposed, by the inhabitants of Berehaven, in the county of Cork, to have provided a show of northern lights visible in the autumn of 1854; and French blood flowed similarly, it was thought, in 1870. So that there is some reason for holding a red aurora to be commemorated in the shower of blood (lasting, according to one authority, three days and three nights) which celebrated the battle of 690 A.D. between the septs of Leinster and Ossory. The omen was emphasized by the sanguine hue assumed by butter, and by the human utterances of a wolf.

Not all antique 'showers of blood' can, it is true, be identified as aurorae. Abnormal rains were, in those days, too frequent and too fantastically various for profitable enquiry as to the grain of fact they conceal. Meteoric falls, luminous appearances, with aqueous showers tinged red by the presence of a microscopic alga (Palmella prodigiosa), have each a share which it would usually be mere waste of time to attempt to apportion. The bloody dews, however, by which Zeus celebrated the death of Sarpedon and the combat of Heracles with the robber Cycnus, may probably be referred to the last class of phenomena; nor is there to be found in any of the poems attributed to Homer and Hesiod a genuine vestige of an auroral reminiscence.

The first mention of an aurora borealis in England is by Matthew of Westminster, who states that in the year 555 A.D. 'an appearance as if of lances was seen in the air,' extending from the north as far as the west. Similar entries recur with tolerable frequency in the 'Anglo-Saxon Chronicles' and the 'Chronicum Scotorum.' A wider survey of such records shows them to be distributed with curious inequality over the centuries they embrace. No caprices of ignorance or unobservance will account for such wide intervals of scarity as Professor Hermann Fritz's great Catalogue of aurorae discloses. Fluctuations are obvious in the phenomenon itself, by which, during certain periods, it died down almost to extinction in Central Europe; then by its sudden revival astounded an unaccustomed generation. One of the longest and best authenticated of these pauses occurred in the seventeenth century. It is a remarkable fact that during its course not a single aurora was witnessed in any part of England, notwithstanding the keen watch kept by the Baconian philosophers of the Royal Society. The intermission (with one trifling exception in 1709) lasted, indeed, 142 years. On the night of October 7, 1574, 'burning spears' appeared above London; and on November 14 and 15 following, Stow relates that 'divers strange impressions of fire and smoke were seen in the air,' and that 'the heavens from all parts did seem to burn marvellous ragingly.' The ensuing prolonged silence as to such phenomena was at last broken by the clamour of mingled wonder, awe, and admiration which greeted the resumption of auroral activity on March 17, 1716. Halley, afterwards Astronomer-Royal, communicated to the Royal Society a detailed account of this fine display, an extract from which we here, with slight abbreviation, insert. . . .

'Many compared it to the concave of the great cupola of St. Paul's church, distinguished with streaks alternately light and obscure, and having in the midst a space less bright than the rest, resembling the lantern; and some there were that thought it liker to that tremulous light which is cast against a ceiling by the beams of the sun, reflected from the surface of water in a basin that's a little shaken; whose reciprocal vibrating motion it very much imitated. . . .'

In a time of civil disturbance, the sensation created by these uncommon appearances inevitably took a party tinge, and Whigs and Tories divided over the aurora as they wrangled over Dr. Sacheverell and the peace of Utrecht. The Jacobites muttered that such portents boded no good to the new dynasty. They talked of giants with flaming swords, fiery dragons embattled armies; the more imaginative, or the more rebellious, averring that they had heard the report of fire arms, and smelt the powder burnt by the spectral combatants. The cue of the Hanoverians, on the other hand, was to make light of the whole affair a mere natural phenomenon. The 'Flying Post,' remarking that 'the disaffected party have worked this up to a prodigy, and interpret it to favour their cause,' proceeded to write it down to the level of sulphurous exhalations, kindled vapours, and will-o'-the-wisp coruscations from the fens!

It proved, however, to be no transient appearance, this 'Great Amazing Light in the North' (as the almanacs called it). Its frequency is vouched for by a tract published in 1741 with the title: 'An O-Yes, from the Court of Heaven to the Northern Nations, by the Streaming Lights that have appeared of late years in the Air; or Mathematical Reasons, showing that the said Lights are no less than Supernatural,' In the Lowlands of Scotland the name they long went by, of 'Lord Derwentwater's Lights,' served at the same time to mark their previous unfamiliarity and to recall their occurrence on the eve of the execution of the rebel lords Derwentwater and Kenmure, February 23, 1716.

On the Continent, during the seventeenth century, displays of the kind were scarce, but not unknown. That witnessed by Gassendi, September 12, 1621, extended as far south as Aleppo; 'whole armies' according to Cromerus, traversed the sky, December 16, 1629; Cassini observed aurorae several times in Paris early in July 1687; and Horrebow, about the same period, saw one at Copenhagen. Yet it is significant of the rarity of the phenomenon that he saw it then for the first time; and Descartes, although he lived twenty years (1629-49) in Holland, and paid special attention to all meteoric appearances, never saw it at all.

Towards the close of the century polar lights almost disappeared from all parts of the world accessible to scientific enquiries. They still glimmered here and there in the far north, but their old intensity was gone. Even in Iceland they grew by scarcity to be prodigious. On their brilliant apparition at Copenhagen in 1709, the guard turned out under arms in readiness for some imminent catastrophe; their revival was greeted in St. Petersburg with stupefaction; at Bologna, they were supposed to have been unknown until 1723. Aurorae seen in China in 1718, 1719, and 1722, excited so much amazement, that engravings of them were struck off in thousands and were secretly (portents being contraband inside the Great Wall) diffused throughout the Empire.

The next prolonged period of scarcity began in 1794, and lasted a third of a century. Since 1827, aurorae have been frequent and splendid in these latitudes, with occasional extensions to tropical, and sympathetic responses from austral regions.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Halley's List of Nebulous Objects

IN the *Philosophical Transactions of the Royal Society* for 1715, the astronomer Edmond Halley published a descriptive list of six "luminous spots or patches." He himself was responsible for first calling attention to the unusual qualities of two of the objects on this list.

These six objects still remain, and always will remain, as among the most important objects in the sky. It is rather curious that among the six is to be found a representative of each type of large nebula or system of stars which we know to-day.

The first on Halley's list, the great nebula in Orion, is the outstanding example of luminous diffuse nebulosity. The second on Halley's list, the great nebula in Andromeda is the best known representative of the great spiral nebulae, those systems of tens of millions of suns beyond our own galaxy. With a distance of three quarters of a million light years, this is the most distant object which the unaided human eye can see. Although Halley credits Bullialdus with its discovery, the first telescopic examination was made earlier, by Simon Marius in 1612, when he wrote his famous description that the light was "like a candle seen through a horn."

The third item in Halley's list is Messier 22, one of the largest and richest of the great globular star clusters. The fourth, Omega Centauri, and the sixth, Messier 13 in Hercules, are also well known globular clusters.

The "small obscure spot at the right foot of Antinous," a constellation now extinct, is the well known rich galactic cluster of stars, Messier 11, in Scutum.

Halley's conclusion about these objects is as brilliant as his prediction of the return of his comet for he says "since they have no Annual Parallax, they cannot fail to occupy Spaces immensely great, and perhaps not less than our whole Solar System."

We reprint here the version of Halley's list as given in the abridged volume of the *Philosophical Transactions*. Halley gives the positions of these objects with respect to the signs of the zodiac, the symbols of which are identified on page four of the *Observer's Handbook;* current positions, brightnesses, distances and other data for all these objects except Messier 11, are given on pages 72-74 of the 1947 edition.

From *The Philosophical Transactions* (From the Year 1700 to the Year 1720), Abridg'd. Vol. IV, p. 224, 1721.

VII. Wonderful are certain luminous Spots or Patches, which discover themselves only by the Telescope, and appear to the naked Eye like small Fixt Stars; but in reality are nothing else but the Light coming from an extraordinary great Space in the Aether; through which a lucid *Medium* is diffused, that shines with its own proper Lustre. This seems fully to reconcile that Difficulty which some have rais'd against the Description *Moses* gives of the Creation, alledging that Light could not be created without the Sun. But in the following Instances the contrary is manifest; for some of these bright Spots discover no Sign of a Star in the Middle of them; and the irregular Form of those that have, shews them not to proceed from the Illumination of a Central Body. These are Six in Number, all which we will describe in the order of time, as they were discovered, giving also their Places in the Sphere of Fixt Stars.

The first and most considerable is that in the Middle of *Orion's* Sword, marked with θ by *Bayer* in his *Uranometria*, as a single Star of the third Magnitude; and is so accounted by *Ptolomy, Tycho Brahe* and *Hevelius*: but is in reality two very contiguous Stars environed with a very large transparent bright Spot, through which they appear with several others. These are curiously described by *Hugenius* in his *Systema Saturnium* pag. 8. who there calls this Brightness, *Portentum, cui certe simile aliud nusquam apud reliquas Fixas potuit animadvertere:* affirming that he found it accidentally in the Year 1656. The Middle of this is at present in ♊ 19°.00, with South Lat. 28°¾.

About the Year 1661 another of this Sort was discovered (if I mistake not) by *Bullialdus, in Cingulo Andromedae.* This is neither in *Tycho* nor *Bayer*, having been omitted, as are many others, because of its smallness: But it is inserted into the Catalogue of *Hevelius*, who has improperly call'd it *Nebulosa* instead of *Nebula;* it has no Sign of a Star in it, but appears like a pale Cloud, and seems to send forth a radiant Beam into the North East, as that in *Orion* does into the South East. It precedes in Right Ascension the Northern in the Girdle, or ν *Bayero*, about a Degree and three Quarters, and has Longitude at this time ♈. 24°.00′ with Lat. North 33°⅓.

The Third is near the Ecliptick between the *Head* and *Bow* of *Sagittary*, not far from the Point of the Winter Solstice. This was found in the Year 1665, by a *German* Gentleman *M. J. Abraham Ihle*, while he attended the Motion of

Saturn then near his *Aphelion*. This is small but very luminous, and emits a Ray like the former. Its Place at this time is ♑ 4°½ with about half a Degree South Lat.

A fourth was discover'd by M. *Edm. Halley* in the Year 1677, when he was making the Catalogue of the Southern Stars. It is in the *Centaur*, that which *Ptolemy* calls ὁ ἐπὶ τῆς τοῦ νώτου ἐκφύσεως which he names *in dorso Equino Nebula* and is Bayers ω; It is in appearance between the fourth and fifth Magnitude, and emits but a small Light for its Breadth, and is without a radiant Beam; this never rises in *England*, but at this time its Place is ♍ 5°¾ with 35°⅓ South Lat.

A Fifth was discovered by Mr. *G. Kirch* in 1681, preceding the Right Foot of Antinous: It is of its self but a small obscure Spot, but has a Star that shines through it, which makes it more bright. The Longitude of this is at present ♑. 9°. *circiter*, with 17°⅙. North Latitude.

The Sixth and last was accidentally hit upon by M. *Edm. Halley* in the Constellation of *Hercules*, in the Year 1714. It is nearly in a Right Line with ζ and η of *Bayer*, somewhat nearer to ζ than η: and by comparing its Situation among the Stars, its Place is sufficiently near in ♍ 26°½. with 57°.00. North Lat. This is but a little Patch, but it shews it self to the naked Eye, when the Sky is clear, and the Moon absent.

There are undoubtedly more of these, which have not yet come to our Knowledge, and some perhaps bigger, but though all these Spots are in Appearance but small, and most of them but of few Minutes in Diameter; yet since they are among the Fixt Stars, that is, since they have no Annual Parallax, they cannot fail to occupy Spaces immensely great, and perhaps not less than our whole Solar System. In all these so vast Spaces it should seem, that there is a perpetual uninterupted Day, which may furnish Matter of Speculation, as well to the curious Naturalist as to the Astronomer.

Readers who are interested in reviewing the life of Halley are recommended to read the excellent article by N. T. Bobrovnikoff in the Scientific Monthly, Nov. 1942, vol. LV, pp. 439-446, (Perkins Observatory Reprint no. 30). For a more exhaustive account, readers may consult the volume "Correspondence and Papers of Edmond Halley" arranged and edited by E. F. MacPike, Oxford, Clarendon Press, 1932.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Halley and the Brightness of Venus

THE third brightest object in our heavens is the planet Venus. In view of its importance, it is interesting to enquire at what point in its orbit will it reach its maximum brilliancy as seen from the earth? As Venus recedes from the earth, at what point is the increase in illuminated area offset by the decrease in apparent diameter of this planet, so that the brilliance fades? Most of us have learned that this point of greatest brilliancy, which last occurred on December 23, 1946, comes when Venus has the phase of a 5-day moon.

Apparently the first person to solve this geometrical puzzle was the famous Edmond Halley. His attention was drawn to the problem by the fact that Venus had been rather conspicuously visible in the day time in the region of London. He worked out the mathematics of the relations between the sun, earth, and Venus, to show at what point in the orbit of Venus she would attain maximum brilliance as viewed from the earth. Halley came to the conclusion that the point of maximum brilliance was about 40 degrees on either side of inferior conjunction. A computation with the elements of the orbit of Venus as accepted to-day gives 39°.8 as the value, astonishingly close. As well as admiring the cleverness of Halley in thinking through the explanation for the greatest brilliance of Venus, we marvel at his conclusion regarding the fixed stars, that their apparent diameters must be "inconceivably small."

His analytical development and solution of the problem are substantially those still used in current textbooks. His geometrical contruction is very simple, but as far as the writer is aware, has not appeared in any astronomical book for many years.

We reprint here his article from *The Philosophical Transactions* abridged, vol. 4, pp. 300-302, 1721.

An Account of the Cause of Venus being seen in the Daytime

by Dr. E. Halley

XXVIII. The late Appearance of *Venus* in the *Day time*, for many Days together, was generally taken Notice of about *London*, and elsewhere; and by some reckoned to be Prodigious. This put me upon the Enquiry, how it came to pass, that at that time the *Planet* should be so plainly seen by Day; whereas she rarely shews her self so, unless to those, who know exactly where to look for her. To resolve this, the following Problem arose, *viz.* To find the Situation of the *Planet* in respect of the *Earth*, when the *Area* of the illuminated Part of her Disk is a *Maximum*.

To investigate this *Maximum*, I found it requisite to assume the following Lemmata. I. That the visible *Areas* of the Disk of the same *Planet*, at differing Distances, are always Reciprocally as the Squares of those Distances; which is evident from the first Principles of Opticks. II. That the *Area* of the whole Disk of the *Planet* is to the *Area* of the enlightned Part thereof, as the Diameter of a Circle to the Versed-Sine of the exteriour Angle at the *Planet*, in the Triangle, at whose Angles are the *Sun*, *Earth*, and *Planet*. III. That in all plain Triangles, four times the Rectangle of the Sides containing any Angle, is to be the Excess of the Square of the Sum of the Sides above the Square of the Base, as the Diameter is to the Versed-Sine of the Complement of the Contained Angle to a Semicircle, which I call the exteriour Angle: This is a new Theorem of good use in *Trigonometry*, and is easily to be proved from the *12th* and *13th* of the II. *Elem. Euclid.*

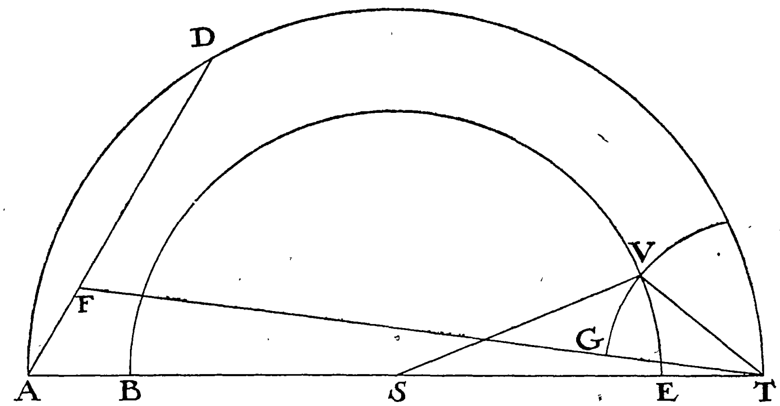
This premised, putting m for the Distance of the *Sun* and *Earth*, and n for that of the *Sun* and *Venus*, and x for the Distance of the *Earth* and *Venus*, or the third Side of the Triangle which we seek; by the third *Lemma*, $4\,n\,x$, will be to the Excess of the Square of $n + x$ above the Square of m, as the *Area* of the whole Disk of *Venus* to the *Area* of the Part enlightned; and by the *first Lemma*, the *Area's* of her whole Disk, are at all times as the Squares of x reciprocally; whence the Quantity

$$\frac{n\,n + 2\,n\,x + x\,x - mm}{4\,n\,x^3}$$

will in all Cases be proportional to the Area of the enlightned Part.

Now that this should be a *Maximum*, it is required that the Fluxion thereof be equal to o, or that the Negative Parts thereof be equal to the Affirmative; that is, that $\overline{2\,n\,\dot{x} + 2\,x\dot{x}} \times 4\,n\,x^3 = 12\,n\,x^2\,\dot{x} \times \overline{n\,n + 2\,n\,x + x\,x - m\,m}$; and dividing all by $4\,n\,x^2\,\dot{x}$, the Equation becomes $2\,n\,x + 2\,x\,x = 3\,n\,n + 6\,n\,x + 3\,x\,x - 3\,m\,m$. Consequently $3\,n\,n + 4\,n\,x + x\,x = 3\,m\,m$, and therefore $x = \sqrt{3\,m\,m + n\,n} - 2\,n$.

From hence a ready and not inelegant Geometrical Construction becomes obvious; for with the Center S, and Radius $ST = m$, describe the Semicircle $T\,D\,A$; and with the same Center and Radius $S\,E = n$, the Semicircle $E\,V\,B$; which two Semicircles shall represent the Orbs of the *Earth*, and *Venus*. Then make the Chord $A\ D$ = to the Radius $S\ T$, and from D towards A, lay off $D\ F$ = $S\ E$; draw $T\ F$, and thereon place $F\ G = B\ E = 2\,n$, and with the Centre T



and Radius $T\ G$ describe the Arch $G\ V$, cutting the Semicircle $B\ V\ E$ in V; and draw the Lines $S\ V$, $T\ V$; I say the Triangle $S\ T\ V$ is similar to that, at whose Angles are the *Sun*, *Earth*, and *Venus*; at the time when the *Area* of the inlightned Part of that Planet's Disk, as seen from the *Earth*, is greatest. How this Geometrical Effection follows from the Equation is too evident to need repetition.

In consequence then of this Solution, I find this *Maximum* always to happen, when the *Planet* is about 40 degrees distant from the *Sun*; and the times thereof about the middle between her greatest Elongations on both Sides from him, and her retrograde Conjunctions with him; when little more than a quarter of her visible Disk is Luminous, and resembling the Moon of about 5 days old; and tho' her Diameter is at that time but 50 Seconds, yet she shines with so strong a Beam, as to surpass the united light of all the fixt Stars that appear with her, and casts a very strong Shade on the Horizontal Plain, whereon they all shine: an irrefragable Argument to prove, that the Disks of the Fixt Stars are inconceivably small, and next to nothing; since shining with a *Native* Light, so many of them do not equal the *reflex* Light of one quarter of a Disk of less than a Minute Diameter.

In this Situation *Venus* was found in *July* last, on the Tenth Day; about which time, when the *Sun* grew low, she was very plainly seen in the Day time, for several Days together; as she might have been in the Mornings, about the latter End of *September*. But this, arising from the Causes we have now shewn, is nothing uncommon; for every Eighth Year it returns again, so that the *Planet* may be seen on the same Day of the Month and Hour, very nearly in the same Place.

Lastly it may not be amiss to note, that the Equation $x = \sqrt{3\,m\,m + n\,n} - 2\,n$ has a Limit; for if n be equal to $\frac{1}{4}\,m$, the Point V will fall on B; and the whole Disk of a Planet at that Distance from the *Sun* would be the *Maximum*, *viz.* when in its superiour Conjunction with the Sun. And the like if n were less than $\frac{1}{4}\,m$; the Arch $G\ V$ in such Case not intersecting the Semicircle $B\ E$.

In modern terminology, Halley's three Lemmas become:

Lemma I. Area of the visible disc varies as $1/x^2$

Lemma II. Area of whole disc: area illuminated: : 2: $(1 + \cos \angle SVT)$ where $\angle SVT$ is the elongation of the earth from the sun as seen from the planet.

Lemma III. $4nx : (n + x)^2 - m^2 :: 2 : (1 + \cos \angle SVT)$.

Then neglecting the eccentricity of the orbit of Venus, Brightness, $B \propto (n^2 + 2nx + x^2 - m^2) / 2nx^3$.

For condition maximum brightness Halley's "Fluxion" becomes

$$dB/dx = 0 = x^2 + 4nx + 3(n^2 - m^2)$$

whence $$x = \sqrt{3m^2 + n^2} - 2n$$

His "not inelegant Geometrical Construction," illustrated in the diagram, depends on making the angle DAT equal to sixty degrees.

By construction, $AD = m$; $DF = n$; $AF = m - n$; $AT = 2m$; $FG = 2n$

$$\begin{aligned} FT^2 &= AF^2 + AT^2 - 2AF.AT \cos \angle DAT \\ &= 3m^2 + n^2 \\ x &= TG = FT - FG \\ &= \sqrt{3m^2 + n^2} - 2n, \end{aligned}$$

which satisfies the condition for maximum brightness of the planet, as determined above from fluxions.

Readers who wish to hunt for Venus in the daytime are referred to the article by F. K. Dalton in this JOURNAL in February, on Interesting Attachments for the Telescope.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Chinese Observations of Solar Spots

Most of our readers are familiar with the fact that the Chinese records of sun-spots go back much earlier than the invention of the telescope in Europe; but probably few have any idea of the Chinese source of the records, or the dates on which naked-eye sun-spots were recorded. Accordingly we reprint below an article from *Monthly Notices,* Royal Astronomical Society, vol. xxxiii, p. 370, 1873, entitled *Chinese Observations of Solar Spots* by Mr. John Williams.

Having lately become possessed of a copy of the celebrated Encyclopaedia of Ma Twan Lin, I found, on examining the astronomical sections of that work, a considerable number of observations of solar spots, extending over a period of 904 years, and, considering that some account of these ancient observations might be of interest to the Society, I have requested permission to lay before you a translation of them, in the hope that they may be of some service to such of the Fellows as may be more particularly engaged in the investigation of these peculiar phenomena.

The Encyclopaedia of Ma Twan Lin is considered not only by the Chinese themselves, but also by such European Sinologists as have had occasion to refer to it, as a most remarkable and trustworthy work. Many eminent writers speak of it in the highest terms of approbation, and among these I take the opportunity of calling your attention to the opinion of Abel Remusat, the author of a well-known and excellent Chinese grammar, who, after giving in that work, an account of a number of the best Chinese works on different subjects, concludes thus:—"And above all, the excellent work of Ma Twan Lin, entitled *Wan Heen Tung Kao,* the finest monument of Chinese literature, a vast collection of memoirs on all sorts of subjects, a treasure of erudition and criticism, in which all the materials that Chinese antiquity has left us, relating to their religion, legislation, rural and political economy, commerce, agriculture, government, natural history, physical geography, and ethnology, will be found, brought together, classed and discussed, in admirable order, method, and lucidity,—in short, a production which, as I have had occasion before to remark elsewhere, is in itself a complete library, and which, did their literature offer nothing else of value, would deserve that the Chinese language should be acquired, were it for no other purpose than to read this work."

My copy of this highly esteemed work is in 100 Chinese volumes. The matters treated of are arranged under 24 principal heads. These are divided into 348 sections or chapters, which have also numerous subdivisions. The astronomical portion consists of 17 chapters, Nos. 278 to 295 inclusive. The subjects treated of are—The general distribution of the stars in the heavens, comprising the asterisms in the three great spaces and the twenty-eight stellar divisions, the whole of which are enumerated and fully described; thus forming a complete Chinese catalogue of stars.

The degrees and mansions of the twelve zodiacal divisions follow, and it may be here observed that the ancient Chinese zodiacal names alone are given. The course of the Milky Way comes next, followed by a general account of the Sun, Moon, and five planets. Next we have extraordinary appearances in the stars and in the heavens generally; these, however, consist principally of accounts of halos and vapours or extraordinary configurations of the clouds.

These are followed by a long account of eclipses of the Sun, commencing with that recorded in the Shoo King, said to have occurred B.C. 2158, of which I gave an account in the *Monthly Notices,* vol. xxiii, and ending with one in A.D. 1223, the number being more than 600; some account of these may, if agreeable to the Society, form the subject of a future communication. We have next extraordinary appearances connected with the Sun, among which are the observations of solar spots, which form the subject of the present paper. These are followed by eclipses of the Moon and singular appearances connected with that body. Next are observations of comets. These have been translated in the work I lately published on Chinese Comets. Prognostications or astrological deductions from the motions and positions of the heavenly bodies come next, followed by observations of shooting or other moving stars; they extend from the Chow dynasty, which commenced B.C. 1122, to about A.D. 1230. After which we have accounts of stars seen in the day-time. These are principally *Venus* and *Jupiter.* Next, an account of auspicious stars, then of temporary stars, and lastly, of extraordinary halos and rainbows.

The observations of solar spots which form the subject of this paper extend from A.D. 301 to 1205, a period of 904 years. They, however, amount to but 45, a number which, considering the time over which they are spread, must appear very small. But it must be recollected that naked-eye observations of the solar spots must of necessity be of very rare occurrence, and, as there is no mention of the employment of coloured or smoked glasses to take off the glare of the Sun, they could only be made under the favourable circumstances of a fog, and the Sun rarely presents us with spots sufficiently large to be seen even then, without optical assistance. Sir John Herschel, in his *Outlines of Astronomy,* remarks that, at least, on two occasions before the invention of telescopes, spots had been seen with the naked eye, and it appears evident to me that these were considered by him as occurrences of a very unusual kind. Here, however, in Ma Twan Lin's work, we have a record of 45 observations of these objects, some of which were seen for several days following. Many of these spots are recorded, as resembling plums of different kinds and sizes, and others are said to have been as large as a hen's egg, and one is mentioned as having resembled a duck. This may remind us of one of the observations of the late Capt. Shea, who describes a group of spots as being like a ship, and also of the drawing of a group of spots exhibited by Mr. Howlett some years ago, which bore a grotesque resemblance to a human skeleton. Most of the instances of their having been seen for several days in succession are within the bounds of possibility. One, however, which is said to have been seen for 19 days, could never have been observed for that length of time on account of the now well-known period of the rotation of the Sun, and it is singular that no conclusions were attempted to be drawn from the peculiar phenomena thus presented to their eyes, nothing of the kind being to be found in their writings.

These observations are continued in the supplement to Ma Twan Lin's Encyclopaedia, published since the accession of the present dynasty in 1644, in which the subjects treated of in the former work are brought down to the date just mentioned. I have had no opportunity, lately, of consulting this work, but I have found in the history of the Ming dynasty, that which preceded the present one, many observations of the solar spots, the latest being dated Nov. 29, 1638, only six years before the accession of the reigning dynasty, the Tsing. On the present occasion I shall confine myself solely to the observations contained in the original work of Ma Twan Lin. They are possibly of but little value in themselves beyond the mere fact that they were made by the Chinese astronomers at the dates specified, but I must here observe that, as it is stated in our astronomical works, that among Europeans, Galileo in 1610 was the first observer of the solar spots, it follows that the Chinese astronomers were far earlier in their observations of those remarkable objects. In fact, these very observations now placed before you were printed and published in the first edition of Ma Twan Lin's Encyclopaedia, which edition appeared in 1322. I must also express my conviction, founded, after a careful and critical examination, on the peculiar appearance of the characters, the date 1322, the paper and other indications, that in the volumes in my possession, I have really a copy of the first edition of Ma Twan Lin's work, in which opinion I am supported by the Rev. J. Summers, Professor of Chinese, King's College, London. Hence we find that these observations were published 288 years before Galileo's observations in 1610. I may also remark that, as these Chinese observations commence in A.D. 301, their astronomers had detected the spots in the Sun 1308 years before the assumed first discovery by Galileo. It must, however, be observed, that, previous to the commencement of the seventeenth century the intercourse between China and Europe was extremely limited, and little or nothing was known by Europeans either of the language, or of the general literature of that country until towards the close of that century. It was, therefore, next to impossible that Galileo could have known anything of what the Chinese had done so long before him, and he is therefore fully entitled to the credit given him by his contemporaries as regards the solar spots. These circumstances render these early observations of the Chinese historically curious; and if they have no other merit, they tend to establish the fact, with some appearance of certainty, that the physical constitution of the Sun has undergone little or no alteration since A.D. 301 to the present time, a period of 1572 years.

In extracting these observations I have confined myself merely to the year, month, and day of their occurrence. In the original we have the name of the Emperor and other particulars which are not unimportant; all that is required being the correct date, which, in every instance, has been carefully reduced from the Chinese mode of expressing it, to our reckoning, by means of the tables I published in my work on Chinese cometary observations. To this I have added, where necessary, the incidental remarks relating to the appearance or size of any particular spot, and the number of days, on which it was visible. I have, however, left out the astrological deductions, as in several instances the death of an Emperor, or some other important personage, is said to have shortly followed the appearance of a spot. In short, I have rendered them as concisely as possible, and have only to express my hope that they may not be found entirely useless in the investigation of these remarkable phenomena.

Chinese Observations of Solar Spots from A.D. 301 to 1205, extracted from the *Encyclopaedia of Ma Twan Lin,* vol. lxxxiii, chapter cclxxxiv:—

A.D.		
301	October 20	
321	March 8	
322	March 11	
342	September 3	
354	November 7	Large, like a hen's egg.
355	April 4	Like a peach.
359	September 8	Like a hen's egg.
369	November 3	
370	March 29	Like a large plum.
372	November 30	
373	November 26	Like a small plum.
374	April 6	Like a duck.
374	November 27	Like a hen's egg.
388	April 3	Like a small plum.
389	July 17	Like a small plum.
396	December 8	
400	December 24	
577	December 30	Like a cup.
826	March 25	
832	April 14	
837	December 15	Like a hen's egg.
841	December 31	
874	No month or day	
974	February 2	
1077	March 6	Like a large plum.
1078	March 10	Like a plum. The text says seen for 19 days?
1079	January 11	Like a plum. Seen for 12 days
1079	March 28	Like a plum. Seen for 10 days.
1104	November 12	Resembling a date.
1112	May 3	As large as a chestnut.
1118	November 17	Like a large plum.
1120	July 31	Like a date.
1123	January 5	Like a large plum.
1129	April 21	
1129	December 16	
1131	March 12	Like a large plum. Seen for 3 days.
1136	November 24	Like a large plum. Seen for 10 days.
1137	February 27	Like a large plum. Seen for 10 days.
1138	March 17	Resembling a large date.
1138	November 26	
1186	July 22	Like a date.
1193	December	
1200	August 23	Like a date. Seen for 6 days.
1201	January 4	Like a date. Seen for 12 days.
1205	February 7	Like a date. Seen for 13 days.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Derham's Catalogue of Nebulous Objects from Hevelius' Prodromus

SOMETIMES mentioned as the first catalogue of nebulae is the work by Dr. Derham, Canon of Windsor, in the Philosophical Transactions of the Royal Society for 1733, vol. 38, pp. 70-74. In this paper Derham printed a list of 16 nebulous objects he had gleaned from the star catalogue Prodromus Astronomiae by Hevelius, published posthumously in 1690. Derham's paper was translated into French and published by de Maupertuis in the Histoire de l'Academie royale des Sciences, Paris, 1734. Along with the list of 16 objects, Derham gives a quaint and interesting philosophical discussion of nebulae. The original work is rare, and the list of nebulae very inaccessible, since the Abstracts of the Philosophical Transactions do not reprint it, but merely summarize the ideas.

In order to appraise the scientific content of this paper, and identify the sixteen nebulous objects, I have borrowed the copy of the Philosophical Transactions from the University of Alberta. With the help of the positions obtained therein, and particularly with aid of work done by Bigourdan on early catalogues, published in the Paris Annals, Observations 1884, I can give the most probable identifications for these objects.

Obviously they have puzzled many astronomers since their publication, for there are few direct statements about them. Messier, especially, has commented on a number of them in the Connaissance des Temps for 1783, 1784, and 1787, on a page titled "Nébuleuses découvertes par différens Astronomes, que M. Messier a cherchées inutilement."

It is decidedly startling to discover that of the sixteen nebulous objects listed by Derham, only two are now contained in our modern catalogues of nebulae or star clusters. To a person who has been taught that Derham's was the first list of nebulae published as such, this comes as a real shock. Fourteen of the objects have no connection with nebulous objects at the present time. They consist of two or three stars close together, or bright patches of the Milky Way. This is more easily understood if one recalls that Hevelius' catalogue from which Derham derived most of his material was the last star catalogue to be made without the help of a telescope.

Derham follows his catalogue with a brief mention of six other nebulous objects he has taken from the article by Halley, which was reprinted in the February issue of this JOURNAL. These are all bona fide nebulous objects, representing the most important members of their various classes to be found in the heavens.

The first fourteen objects listed by Derham are drawn from Hevelius. Actually there would seem to be some discrepancy between early copies of Hevelius' work, as Derham, Bigourdan, and I do not always agree on what Hevelius has listed as nebulous. Derham did not include the Praesepe Cluster in Cancer, a true star cluster; and another group, now known as 4 and 5 in Vulpecula, not a true nebulous group. Both of these are listed by Hevelius.

The last two of the sixteen objects listed by Derham are taken from Halley's Catalogus Stellarum Australium. Derham does not indicate this and I am indebted to Bigourdan for this information. One of these is a genuine nebulous object, now known as the star cluster Messier 7. The other is a portion of the Milky Way. Table I gives the identifications of the sixteen objects, with the numbers from the catalogue of Hevelius.

Therefore, of the 14 nebulous objects which Derham extracted from Hevelius' catalogue, only the first one, the Great Nebula in Andromeda, is a true nebulous object. It is almost ironical then, that Derham ended his catalogue with the comment: "But if any one is desirous to have a good View of these, or any other of the Nebulosae, it is absolutely necessary that he should make use of very good glasses, else all his Labour will be in vain, as I have found by Experience, and before noted." And Derham was basing his catalogue on a list made without "glasses"!

It is possible that Derham had considerable trouble trying to find these objects in the sky. Of Halley's objects mentioned at the end, Derham states specifically that he has seen all five visible in England. But nowhere does he state seeing any mentioned by Hevelius,—rather he says "my Reflecter loseth its Excellence and

Power, by beginning to be tarnished." It would nowadays appear that Derham's reflector was too good to turn these chance naked-eye groupings into nebulous objects!

As a catalogue of nebulous objects then, Derham's list must be considered a somewhat pathetic failure. But Derham correctly grasped the importance of nebulae and was exceedingly shrewd in picking the Andromeda nebula as the most important when he says

TABLE I

IDENTIFICATION OF OBJECTS IN DERHAM'S LIST

Hevelius	Derham	Present Identification	
32	1	Great nebula in Andromeda, Messier 31.	
380	2	σ Capricorni	Messier doubts that these nebulous objects exist. The stars π, ρ, o Cap form a sort of cluster.
381	3	π Capricorni	
382	4	o Capricorni	
383	5	ρ Capricorni	
618	6	ω_1, ω_2, ω_3 Cygni.	Messier doubts that these nebulous objects exist.
619	7	Two stars of 6th mag., 1° apart; Milky Way.	
794	8	Region of 88 Herculis where neither nebulosity nor a cluster is visible to unaided eye.	
798	9	f Herculis.	
804	10	32, 33, 34 Ophiuchi, near 60 Herculis. Messier could find no nebulous object here.	
1113	11	34, 35, 37 Pegasi. Messier wondered if this object could be his No. 15 with position greatly in error.	
1259	12	Brilliant part of Milky Way near s Scuti.	
953	13	ξ_1 and ξ_2 Librae (13 and 15) with 17 and 18.	
1496	14	74 and 75 Ursae Majoris.	
Halley 20	15	Brilliant part of Milky Way NNE from ζ Scorpii.	
Halley 29	16	Bright galactic cluster Messier 7, NGC 6475.	

"some seem to be more large, and remarkable than others; but whether they are really so, or no, I confess I have not had an Opportunity to see, except that in *Andromeda's* Girdle, which is as considerable as any I have seen." The publication of Derham's paper served to open a door into one of the most important branches of astronomy which the development of the telescope has brought within our grasp.

III. *Observations of the Appearances among the Fix'd Stars, called Nebulous Stars. By* W. Derham, *D.D., Canon of Windsor, F.R.S.*

Having last Autumn made some good Observations, with my eight Foot Reflecting Telescope, of the Appearances in the Heavens, called *Nebulous Stars*, I think it proper to acquaint this Illustrious Society with them, to instigate others to make farther Observations of them, because I think there is much more in them worthy of the Inquiry of the Curious, than hath hitherto been imagined, and because I fear I shall not be able to pursue my Observations much farther, by reason my Reflecter loseth its Excellence and Power, by beginning to be tarnished.

But if any one would have a good View of these *Nebulosae*, it is of absolute Necessity that he makes use of very good Glasses, else all his labour will be Lost, as I found by Experience.

These Appearances in the Heavens, have born the Name of *Nebulous Stars*: But neither are they *Stars*, nor such Bodies as emit, or reflect Light, as the Sun, Moon, and Stars do; nor are they *Congeries*, or *Clusters* of Stars, as the *Milky-Way*: but whitish *Areae*, like a Collection of *Misty Vapours*: whence they have their Name.

There are many of them dispersed about, in diverse Parts of the Heavens. This Catalogue of them (which I transcribed from *Hevelius's Prodromus Astronomiae*) may be of good use to such as are minded to enquire into them.

Besides these Dr. *Halley*, in *Phil. Trans.* N° 347, hath mentioned one in *Orion*'s *Sword*; another in *Sagittary*; a third in the *Centaur* (never seen in *England*); a fourth preceding the right Foot of *Antinous*; a fifth in *Hercules*; and that in *Andromeda*'s *Girdle*.

Five of these six I have carefully viewed with my excellent eight Foot Reflecting Telescope, and find them to be Phaenomena much alike; all except that preceding the right Foot of *Antinous*, which is not a *Nebulose*, but a *Cluster* of *Stars*, somewhat like that which is in the *Milky-Way*.

Between the other four, I find no material Difference, only some are rounder, some of a more oval Form, without any Fix'd Stars in them to cause their Light; only that in *Orion*, hath some Stars in it, visible only with the Telescope, but by no Means sufficient to cause the Light of the *Nebulose* there. But by these Stars it was, that I first perceived the Distance of the *Nebulosae* to be greater than that of the *Fix'd Stars*, and put me upon enquiring into the rest of them. Every one of which I could very visibly, and plainly discern, to be at immense Distances beyond the Fix'd Stars near them, whether visible to the naked Eye, or Telescopick only; yea, they seemed to be as far beyond the Fix'd Stars, as any of those Stars are from the Earth.

And now from this Relation of what I have observed from very good, and frequent Views of the *Nebulosae*, I conclude them certainly not to be *Lucid Bodies*, that send their Light to us, as the Sun and Moon. Neither are they the *combined Light* of *Clusters* of Stars, like that of the *Milky-Way*: But I take them to be *vast Areae*, or *Regions of Light*, infallibly *beyond the Fix'd Stars, and devoid of them.* I say *Regions*, meaning Spaces of a vast Extent, large enough to appear of such a Size as they do to us, at so great a Distance as they are from us.

And since those Spaces are devoid of Stars, and even that in *Orion* itself, hath its Stars bearing a very small Proportion to its *Nebulose*, and they are visibly not the Cause of it, I leave it to the great Sagacity and Penetration of this Illustrious Society, to judge whether these *Nebulosae* are particular Spaces of Light; or rather, whether they may not, in all Probability, be Chasms, or Openings into an immense Region of Light, beyond the Fix'd Stars. Because I find in this

A CATALOGUE of the *Nebulosae*, extracted from *Hevelius*.

The Places of the *Nebulosae*	Their R. Ascent. *A*. 1660	Their Declinat. *A.D.* 1660
	gr. ′ ″	gr. ′ ″
In *Andromeda's* Girdle,	6 4 45	39 27 57 N.
In Forehead of *Capricorn*,	300 2 53	20 1 53 S.
Another preceding the Eye of *Capricorn*,	301 59 55	19 11 30 S.
Another following it,	302 35 9	19 36 0 S.
One above those, adjoining to the Eye of *Capricorn*,	302 25 31	18 48 58 S.
Preceding above the *Swan's* Tail, and last in its N. Foot,	304 54 8	47 54 20 N.
One following a Star above the *Swan's* Tail, out of the Constellation,	312 10 5	53 05 20 N.
On the outside of *Hercules's* left Foot,	264 52 46	48 9 10 N.
In the left Leg of *Hercules*,	265 38 37	38 5 50 N.
On the Top of *Hercules's* Head,	252 24 3	13 18 37 N.
At the Ear of *Pegasus*,	332 38 45	3 3 12 N.
In the Western Border of *Sobieski's* Shield,	272 32 34	14 23 35 S.
Under the Beam of the Scales of *Libra*,	219 26 15	9 16 27 S.
Above the Back of *Ursa major*,	183 32 41	60 20 33 N.
In the third Joint of *Scorpio*'s Tail,	12 43 00 ♐ Long.	19 1 0 S. Lat.
Between *Scorpio*'s Tail, and the Bow of *Sagittarius*,	24 32 00 ♐ Long.	11 25 0 S. Lat.

Opinion most of the Learned in all Ages (both Philosophers, and I may add Divines too) thus far concurred, that there was a *Region beyond the Stars*. Those that imagined there were *Crystalline*, or *Solid Orbs*, thought a *Coelum Empyraeum* was beyond them and the *Primum Mobile*: and they, that maintained there were no such Orbs, but that the Heavenly Bodies floated in the Aether, imagined that the Starry Region was not the Bounds of the Universe, but that there was a Region beyond that, which they called the *Third Region*, and *Third Heaven*.

To conclude these Remarks, it may be of use to take Notice, that in *Hevelius*'s *Nebulosae*, some seem to be more large, and remarkable than others; but whether they are really so, or no, I confess I have not had an Opportunity to see, except that in *Andromeda*'s Girdle, which is as considerable as any I have seen. In his Maps of the Constellations, the most remarkable are the three near the Eye of *Capricorn*; that in *Hercules*'s *Foot*; that in the third Joint of *Scorpio*'s *Tail*; and that between *Scorpio*'s *Tail* and the *Bow of Sagittary*. But if any one is desirous to have a good View of these, or any other of the *Nebulosae*, it is absolutely necessary that he should make use of very good Glasses, else all his Labour will be in vain, as I have found by Experience, and before noted.

REVIEW OF PUBLICATIONS

Making Your Own Telescope, by Allyn J. Thompson. Pp. 211 + xi, $5\frac{1}{2} \times 8\frac{1}{2}$ inches. Sky Publishing Corporation, Cambridge 38, Mass., 1947. Price $3.50.

This should prove a thoroughly useful book for anyone who wishes to commence the hobby of amateur telescope making. With this guide one should be able to make a first class reflecting telescope with a minimum outlay of money and time, and to commit a minimum of errors.

The book reflects the author's great experience in conducting classes of telescope makers in the Optical Division of the Amatuer Astronomer's Association, in the workroom at the Hayden Planetarium. The most direct and carefully tested steps are used throughout. The analysis of the grinding and polishing processes and results are discussed in detail, in a way that can be followed by hobbyists working alone or in groups.

The basic instrument discussed is a six-inch reflector, Newtonian arrangement, of four-foot focal length. This size gives a very effective telescope, without being too large and laborious for the beginner to construct without help. For such a telescope all the necessary materials are listed in detail. The mountings described are made of standard pipe fittings, which are also listed and fully diagrammed.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

CATALOGUES OF NEBULOUS OBJECTS IN THE EIGHTEENTH CENTURY*

By Helen Sawyer Hogg

FOR the past two years I have been working on an extensive bibliographical compilation of globular clusters, listing all references to these under the individual globular cluster concerned. This work is in press, and will appear this fall.

During the course of this investigation, which was carried back to the very beginnings of the work on nebulous objects in the late seventeenth century, some interesting points came to light. One of these was a long-overlooked letter written by Pierre Méchain, published in Bode's Astronomische Jahrbuch für 1786. I am indebted to the Harvard Observatory for use of their copy of this rare book. This letter clears up a point in Messier's Catalogue which has puzzled astronomers for years. The part of the letter which concerns nebulae is reprinted here on page 272. The letter was unknown even to such diligent researchers into Messier's work as Gore,[1] Flammarion,[2] Shapley and Davis,[3] and Bailey,[4] all of whom have published identifications and comments on the catalogue.

In order to portray the development of the knowledge of nebulous objects, a brief account of the eighteenth century nebular catalogues will be given here.

The first attempt to list several nebulae together was made in 1715 by Edmond Halley, in the Philosophical Transactions of the Royal Society. This article, describing six objects, has been reprinted in this Journal, vol. 41, pp. 69-71, 1947. Several of these objects had been noted by observers other than Halley. In 1733, Derham published in the Philosophical Transactions a catalogue of

*Communications from the David Dunlap Observatory vol. 1, No. 14.
[1]*Obs.*, vol. 25, 1902. [2]*Bull. Soc. Astr. France*, vols. 31-35, 1917-21.
[3]*Ast. Soc. Pac., Pub.*, vol. 29, 1917. [4]*Ibid.*, p. 179.

16 nebulae which he had gleaned from the star catalogue by Hevelius (made without a telescope), published posthumously in 1690. Derham also mentioned the nebulous objects published by Halley. Derham's paper was translated into French and published by de Maupertuis in the Histoire des Académie royale des Sciences, Paris, 1734. I have republished Derham's paper in the July-August issue of this JOURNAL this year, and shown that only two of the sixteen objects he catalogued are now considered nebulous objects.

In 1755 the Abbé de Lacaille published, also in the Histoire de l'Académie royale des Sciences, his catalogue of 42 nebulae in the southern sky, *Sur les étoiles nébuleuses du ciel austral.* This catalogue was reprinted later several times in the French almanac, Connaissance des Temps, for 1783, 1784, and 1787. By using three Roman numerals, Lacaille combined an attempt at classification with the numbering of the object. He began his list with the great globular cluster 47 Tucanae as Lacaille I 1.

In the next decade, around 1746, the young Swiss astronomer, de Chéseaux, wrote a letter to his grandfather, M. de Réaumur, for the French Academy, with a list of 20 nebulae he had observed, some of which were his own discoveries. This letter was unpublished and almost unknown until found by Bigourdan, and published in the Paris Annales, Observations 1884, in 1891. The name of de Chéseaux is associated with the famous six-tailed comet of 1746. He was not the discoverer of this comet, but was the first to announce its many tails.

Publication of drawings of nebulous objects was begun by Le Gentil in the Mémoires of the Académie des Sciences in 1759.

It was in the 1760's that Messier began his discoveries of nebulous objects. The catalogue of Messier is to-day the most frequently mentioned of the early catalogues, probably because in its final form it included about the hundred most conspicuous nebulous objects in the sky (excluding the southern polar cap).

Charles Messier, born in 1730, went to Paris in 1751 without very much education, to be a draughtsman and copyist for the astronomer Delisle. Delisle seems to have been a person of singular jealousy, and Messier must have laboured in the early years under considerable handicap. Indeed Delisle refused to permit him to announce the return of Halley's comet when Messier first observed it on January 21, 1759!

After the death of Delisle in 1768, however, Messier rose to a high place in French astronomy, being elected to the Academy of Sciences in 1770, and appointed "Astronome de la Marine" in 1771, living at the Hotel de Cluny. He is best known for his work on comets and nebulae. Louis XV named him "le furet des comètes." He observed 46 comets, of which 21 were his own discoveries. He published works on other subjects, however, such as the transit of Venus in 1761 and the rings of Saturn. The famous Lalande so admired the work of Messier that he proposed, on his celestial globe of 1775, a constellation named Messier. This constellation was between Cepheus, Cassiopeia, and Camelopardalus. Messier lived to an advanced age, and died in 1817.

His first list of nebulae was published in the Mémoires of the Paris Academy of Sciences in 1771, *Catalogue des nébuleuses et des amas d'étoiles que l'on découvre parmi les étoiles fixes sur l'horizon de Paris*. This list contains 45 nebulous objects that Messier himself had found during his extensive work on comets. He does not attempt to make a catalogue of all known nebulous objects. A number of these nebulae are indicated on the maps of comet paths; in particular, the globular cluster Messier 2 is found on the path of the famous first-predicted return of Halley's comet in 1759.

The first person to combine all known nebulae into one uniform catalogue was Johann Bode of Berlin. In 1777, Bode published in his Astronomische Jahrbuch für 1779 a catalogue of 75 nebulae, entitled *Ueber einige neuentdeckte Nebelsterne und einem vollständigen Verzeichnisse der bisher bekannten*. This was the most ambitious catalogue then published, and merits more recognition than it has had. Bode included all the earlier works on nebulae by Halley, Hevelius, Derham, Lacaille, and Messier, as well as observations of his own. Bode's catalogue appears little known, possibly because the positions of the nebulae were given by zodiacal sign in celestial latitude and longitude, an outmoded method of designation which makes identification laborious.

After this compilation by Bode, Messier published another list in 1780, in the Connaissance des Temps for 1783. This list contains the first 68 objects of what is now known as Messier's Catalogue. The first 45 nebulae announced in the Mémoires for 1771 are reprinted with additional comments, and 23 more nebulae are added.

All of these objects were observed by Messier, and he was the original discoverer of most of them.

In 1781, in the Connaissance des Temps for 1784, Messier published new nebulae, numbered from 69 to 103, and this list was republished again in 1784, in the Connaissance for 1787. This last is the complete list, and is the document usually referred to as Messier's Catalogue.

However, most of these later nebulae were discovered not by Messier, but by Méchain, a young worker at the Bureau des Longitudes in Paris. Pierre François André Méchain was born in 1744 at Laon. He was employed as a calculator in the office of the navy. He had the reputation of an exact observer and an indefatigable calculator. After computing all day, he spent his nights observing. He entered the Academy of Sciences in 1782, after having co-operated in the determination of the difference in longitude between Paris and Greenwich. From 1786 to 1794 he edited the Connaissance des Temps. His later longitude work was broken off by an attack of yellow fever, and he died in 1805. Between 1781 and 1799 he discovered eight comets.

Méchain seems to be receiving far too little astronomical credit to-day for his work. The numerous histories of astronomy have almost nothing to say about him. But he discovered Encke's comet,[5] Tuttle's comet,[6] and one-fourth of the nebulous objects in Messier's Catalogue!

After Méchain discovered the later nebulae in the catalogue, Messier then in the course of a few weeks helped to determine a precise position for publication. In the final catalogue, however, the last three nebulae, Nos. 101, 102, and 103 are listed as objects seen by M. Méchain only. For Nos. 102 and 103 no precise position is given, but a description of the position only. Bode published this catalogue, Numbers 46 to 103, in 1783 in his Astronomische Jahrbuch für 1786. He published it in German, with his own comments, and noted especially cases where nebulae had been first discovered by observers other than Messier or Méchain.

Apparently after the final Connaissance des Temps list had gone to press, Méchain gathered together his last notes on nebulae, in-

[5]Chambers, A handbook of descriptive and practical astronomy, vol. I, p. 416, 1889.

[6]*Ibid.*, p. 430.

cluding corrections and additions, and sent them for publication in Bode's Jahrbuch. Toward the back of the same volume of the Jahrbuch containing the final catalogue (Jahrbuch für 1786), on page 232, is the letter from Méchain which has been overlooked for nearly a century. It clears up at once what has been a troublesome problem, namely the identification of the object No. 102.

Of the three last objects, Messier 101, 102, and 103, listed as seen by Méchain only, No. 101 is definitely identified as NGC 5457, the fine spiral in Ursa Major, and No. 103 is a galactic star cluster, NGC 581 in Cassiopeia. But the identification of No. 102 has given astronomers much perplexity. The printed description of it reads: "Nébuleuse entre les étoiles *o* du Bouvier et *ι* du Dragon: elle est très-foible; près d'elle est une étoile de la sixième grandeur".

Gore in 1902 commented that he had found no identification for No. 102. In 1917, with the publication of Messier's list by Shapley and Davis, Prof. Bailey of Harvard came to the conclusion that it was NGC 5866. The footnote in this article says, "Subsequent to Messier there appears to be no further mention of No. 102. 'By a star chart, or the sky, you will see that, taken as it stands, no object could well be selected for M 102, since *o* Boötis is too far from *ι* Draconis. If, however, *o* is a misprint for θ, it becomes intelligible and M 102 is perhaps N.G.C. 5866, although in Norton's Atlas it is apparently identified as N.G.C. 5879'."

Flammarion independently came to the same conclusion as Bailey, although he listed three other nebulae in the field which fitted the description almost as well. He says,[7] "J'ai eu la plus grande difficulté à identifier cette nébuleuse, et je ne suis pas absolument certain d'y être parvenu. Il y a près de cette position 3 nébuleuses . . . . C'est une véritable pépinière de nébuleuses!"

The question, however, was settled by Méchain himself in this letter in 1783, when he announced flatly that Nebula No. 102 was an error, and the same object as No. 101. I translate Méchain's statement as follows:

> On page 267 of the Connaissance des temps for 1784 M. Messier lists under No. 102 a nebula which I have discovered between *o* Bootes and *ι* Draconis; this is, however, an error. This nebula is one and the same as the preceding No. 101. M. Messier confused the same as the result of an error in the sky chart, in the list of my nebulous stars communicated to him.

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[7]*Bull. Soc. Astr. France*, vol. 35, p. 287, 1921.

Therefore Messier 102 may now be stricken from the records as a non-existing object.

In this same letter, Méchain lists six additional nebulae which he has discovered, but which were not included in the Connaissance des Temps list. For two of these the remarks are too vague to permit certain identification. For four of them, however, identification is certain from the comments. In my opinion these should be added to the accepted list of Messier's catalogue as Nos. 104, 105, 106, and 107.

One of these nebulae on the southern boundary of Virgo has already been numbered by Flammarion. He found the object listed in Messier's own copy of his catalogue, in Messier's handwriting, as found by Méchain, and suggested that it should be listed as Messier 104. He says:[8]

"J'ai raconté que j'avais remarqué dans mon manuscrit du Catalogue de Messier une petite note, de la main de Messier, signalant que Méchain a découvert, le 11 mai 1781, une 'nébuleuse tres faible' dont la position était inscrite à

AR 187° 9′ 42″ et D − 10° 24′ 49″.

. . . C'est la nébuleuse qui porte le n° 4594 de *New General Catalogue* de Dreyer. . . . Je l'ai donc ajoutée au Catalogue de Messier, et l'ai inscrite sous le titre *Messier 104*."

Obviously Flammarion, who did years of work on Messier's catalogue, did not know of this letter by Méchain in Bode's Jahrbuch. This object which he has identified is one of the four objects published therein. Although this nebula is the second mentioned in Méchain's letter, we will keep Flammarion's numbering, and assign No. 105 to the first nebula mentioned, discovered on the 24th of March 1781. This I identify as NGC 3379, one of a group of three spirals in Leo. The nebula "neben dem Stern No. 3 der Jagdhunde" is NGC 4258, a large spiral in Ursa Major, and should be Messier 106. In April 1782 Méchain discovered a little nebula "am linken Schenkel des Ophiuchus" which is NGC 6171, a bright, loose globular cluster in Ophiuchus, and should be assigned No. 107.

Data on these objects are given in Table I. All of them were found independently later by Sir William Herschel, and his catalogue numbers are included in the table. Herschel, too, must have overlooked this letter, since in his catalogues of nebulae he tried to list only his own discoveries and to avoid those found by other observers.

[8]*Bull. Soc. Astr. France*, vol. 35, pp. 334, 355, 1921.

TABLE I

Messier	NGC	R.A. 1900	Dec.	Discovery dates Méchain	William Herschel
104	4594	$12^h 34^m.8$	$-11° 4'$	1781, May 11	1784, May 9, I 43
105	3379	$10^h 42^m.6$	$+13° 07'$	1781, March 24	1784, March 11, I 17
106	4258	$12^h 14^m.0$	$+47° 52'$	1781, July	1788, March 9, V 43
107	6171	$16^h 26^m.9$	$-12° 50'$	1782, April	1793, May 12, VI 40

DESCRIPTIONS

104 "7′ × 1′.5 in p.a. 92°; very bright. A remarkable, slightly curved, clear-cut dark lane runs along the entire length to the south of the nucleus; probably the finest known example of this phenomenon. There are very slight trace of spiral whorls." Curtis, *Lick Pub.*, vol. 13, 1918. Photo, Shapley, *Galaxies*, Fig. 66, 1944.

105 "This, with 3384 and 3389, forms a striking group, a right-angled triangle whose shorter sides are 7′ long. 3379 is nearly round, 2′ in diameter; very bright; no spiral structure discernible." Curtis, *Lick Pub.*, vol. 13, 1918.

106 "With the very faint extensions this spiral is nearly 20′ × 6′ in p.a. 165°; very bright, elongated nuclear portion, on the west of which is a short dark lane; there are numerous almost stellar condensations in the two principal spiral branches." Curtis, *Lick Pub.*, vol. 13. Photo, *Lick Pub.*, vol. 8, plate 33.

107 A bright globular cluster, with large angular diameter, but the stars are sparse and scattered. A chart of this cluster is published by Oosterhoff, *B. A. N.*, no. 310, 1938.

I am not aware of any discoveries of nebulae by either Messier or Méchain subsequent to this letter. When William Herschel entered the field of nebular sweeps, his discoveries so far outstripped all others that probably they made other astronomers feel that the few nebulae they might discover were hardly worth the effort. William Herschel's first *Catalogue of one thousand new nebulae and clusters of stars* was published in the Philosophical Transactions of the Royal Society, vol. 76, 1786, and was soon followed, in 1789, by the *Catalogue of a second thousand of new nebulae and clusters of stars: with a few introductory remarks on the construction of the heavens.* In 1802 he brought out his final *Catalogue of 500 new nebulae and clusters.* He did not number his nebulae consecutively, but attempted, as had Lacaille some years earlier, to classify them by appearance as he catalogued them. For a few objects this system is suitable, but for very many it soon becomes unwieldy, so that William Herschel's numbers are seldom used to-day.

After his death, his son John continued his work during the nineteenth century. John Herschel's first catalogue was published in the Philosophical Transactions, vol. 123, in 1833, *Observations on 2307 nebulae and clusters of stars made at Slough with a twenty-foot reflector between the years 1825 and 1833.* After this, John Herschel went down to the southern hemisphere in order to complete the work, and in 1847 published his famous catalogue, *Results of Observations at the Cape of Good Hope.* A copy of this rare book has recently been presented to the David Dunlap Observatory by courtesy of Sir Harold Spencer Jones, Astronomer Royal, on behalf of the Royal Astronomical Society. In 1864, John Herschel combined all existing data on nebulae and clusters in his well known *General Catalogue of nebulae and clusters of stars*, in the Philosophical Transactions, vol. 154, listing 5079 objects.

It is interesting to note that of the 99 clusters catalogued to-day as globular, William Herschel was the discoverer of exactly one-third, far outstripping his nearest rival. He found 33, James Dunlop (who catalogued the southern sky in the early nineteenth century) found 21; Messier 14; Méchain and John Herschel 5 each; Lacaille 4; and no other observer has found more than two.

All workers on nebulae and clusters must feel as Flammarion[9] did when he wrote, "La Terre tourne vraiment trop vite pour les contemplateurs de la nature, car ils ne sont jamais rassasiés."

Méchain's Letter

From Bode's "Astronomische Jahrbuch für das Jahr 1786," p. 231:—

Ueber die Bahn des zweiten Kometen vom Jahr 1781. Entdeckung einiger Nebelstern; die Elemente der Bahn des neuen Planeten und astronomische Beobachtungen; von Herrn Mechain in Paris, aus einem Schreiben desselben an Herrn Bernouilli, vom 6ten May 1783.

Was die Nebelsterne oder Nebelflecke anbetrift, so bin ich nicht im Stande Herrn *Bode* Verlangen völlig zu befriedigen, indem es mir bis jetzt an Zeit gefehlt, die genaue Stellung aller derjenigen die ich entdeckte habe, bestimmen zu können. Ich will daher nur die Oerter von einigen, die sich nicht in der Connoissance des tems f. 1784 befinden, angeben, und werde die übrigen so anzeigen, dass man sie wird auffinden können.*) Herr *Messier* erwehnt daselbst Seite 264 und 265 zweyer

*S. das Verzeichniss der von den Herren Messier und Mechain seit 1771 beobachteten Nebelsterne. Seite 164.

[9] *Bull. Soc. Astr. France*, vol. 31, p. 386, 1917.

Nebelsterne, welche ich im Löwen entdeckt habe. Ich finde bey den angegebenen Stellungen die ich durch Vergleichung ihres Standes gegen Regulus bestimmt habe, nichts auszusetzen. Es befindet sich aber noch ein dritter etwas nordlicher, der zugleich lebhafter wie die beyden vorhergehenden ist. Ich entdeckte denselben am 24sten März 1781, 4 oder 5 Tage nachdem ich die beyden andern gefunden. Den 10ten April verglich ich seinen Stand mit γ ♌ woraus sich seine gerade Aufsteigung 159° 3′ 45″ und seine nordliche Abweichung 13° 43′ 58″ ergab.

Seite 267 der Connoissance des tems f. 1784 zeigt Herr Messier unter No. 102 einen Nebelfleck an, den ich zwischen o Bootes und ι Drachen entdeckt habe; dies is aber ein Fehler. Dieset Nebelfleck ist mit dem vorhergehenden No. 101 ein und derselbe. Herr Messier hat durch einen Fehler in den Himmelscharten veranlasst, denselben nach dem ihm mitgetheilten Verzeichnisse meiner Nebelsterne verwechselt.

Den 11. May 1781 entdeckte ich einen Nebelfleck übern Raben, der mir keine einzelne Sterne zu enthalten schien. Er ist von einem schwachen Lichte und schwer zu finden wenn man die Fäden des Mikrometers erleuchtet. Ich habe ihn an diesem Tage und den folgenden mit der Spica in der Jungfrau verglichen und hieraus die gerade Aufsteigung 187° 9′ 42″ die südliche Abweichung 10° 24′49″ gefolgert. Er steht nicht in der Connoissance des tems.

Seite 262 und 263 erwehnt Herr Messier verschiedene Nebelflecke in der Jungfrau, die ich ihm angezeigt habe. Es befinden sich aber in der dortigen Gegend noch einige andere, die er nicht gesehen, und deren Lage ich zu bestimmen gedenke, wenn ich erst einen bequemen Beobachtungsort habe, welches sich nicht lange mehr verzögern wird.

Seite 265 No. 97. Ein Nebelfleck nahe bey β im grossen Bären. Herr Messier erwehnt, bey der Anzeige seiner Stellung, zween andere, welche ich gleichfalls entdeckt und wovon der eine nahe bey diesem, der andere nahe bey γ im grossen Bären sich befindet, ich habe aber ihre Oerter noch nicht bestimmen können.

Im Julii 1781 fand ich einen andern Nebelfleck nahe beym grossen Bären neben dem Stern No. 3 der Jagdhunde und 1° südlicher, ich schätzte seine gerade Aufsteigung 181° 40′ und seine nordliche Abweichung ohngefehr 49°. Ich werde nächstens die genauere Stellung desselben zu bestimmen suchen.

Im April 1782 entdeckte ich einen kleinen Nebelfleck am linken Schenkel des Ophiuchus zwischen den Sternen ζ und ϕ, dessen Ort ich noch nicht näher beobachtet habe.

Dies ist ausserdem, was in der erwehnten Connoissance des tems verkömmt, alles was ich wegen der Nebelsterne gethan habe. Ich werde darüber ein besonderes Memoire ausfertigen und ihre Oerter und Erscheinungen, näher bestimmen.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The 1769 Transit of Venus, as seen from Canada

Of special interest to Canadians are early and little-known astronomical observations made at places which are now in Canada. These are observations of the transit of Venus in 1769.

As most of our readers know, transits of Venus across the disc of the sun occur in pairs eight years apart, with an interval alternately of 105 and 122 years between the pairs. The last transit of the seventeenth century was the one of 1639, made famous by Horrocks' prediction and observation. Before his untimely death, Horrocks seems to have developed the idea that transits of Venus could be used for the all-important determination of the distance of the sun from the earth, by the principles of trigonometric parallax. Halley later in the century developed this idea mathematically, and suggested, in a paper to the Royal Society in 1716, an all-out effort on the part of astronomers to observe carefully the next transit in 1761. Halley's article has been reprinted by R. A. Proctor in "Transits of Venus," London, 1882, from which we quote:

> There remains, then, the transit of Venus over the sun's disc; whose parallax, being almost as great as the solar parallax, will cause very sensible differences between the times in which Venus will seem to be passing over the sun at different parts of the earth. And from these differences, if they be observed as they ought, the sun's parallax may be determined even to a small part of a second. Nor do we require any other instrument for this purpose than common telescopes and clocks, only good of their kind; and in the observers nothing more is needful than fidelity, diligence, and a moderate skill in astronomy. For there is no need that the latitude of the place should be scrupulously observed, nor that the hours themselves should be accurately determined with respect to the meridian; it is sufficient that the clocks be regulated according to the motion of the heavens, if the times be well reckoned from the total ingress of Venus into the sun's disc to the beginning of her egress from it; that is, when the dark globe of Venus first begins to touch the bright limb of the sun within; which moments I know, by my own experience, may be observed within a second of time.
>
> But, on account of the very strict laws by which the motions of the planets are regulated, Venus is seldom seen with the sun's disc; and during the course of 120 years it could not be seen once—namely, from the year 1639 (when

this most pleasing sight happened to that excellent youth Horrocks, our countryman, and to him only since the Creation) to the year 1761, in which year, according to the theories which we have hitherto found agreeable to the celestial motions, Venus will again pass over the sun on May 26,[1] in the morning; so that at London about five o'clock in the morning we may expect to see it near the middle of the sun's disc, and not above four minutes of a degree south of the sun's centre. But the duration of this transit will be almost eight hours —namely, from two o'clock in the morning till almost ten. Hence the ingress will not be visible in England; but as the sun will at that time be in the sixteenth degree of Gemini, having almost twenty-three degrees north declination, it will be seen without setting at all, in almost all parts of the north frigid zone; and therefore the inhabitants of the north coast of Norway, beyond the city of Nidrosia, which is called Drontheim, as far as the North Cape, will be able to observe Venus entering the sun's disc; and perhaps the ingress of Venus upon the sun when rising will be seen by the Scotch, in the northern parts of the kingdom, and by the inhabitants of the Shetland Isles, commonly called Thule. But at the time when Venus will be nearest the sun's centre the sun will be vertical to the northern shores of the Bay of Bengal, or rather over the kingdom of Pegu; and therefore in the adjacent regions, as the sun, when Venus enters his disc, will be almost four hours towards the east, and as many towards the west at the time of her egress, the apparent motion of Venus on the sun will be accelerated by almost double the horizontal parallax of Venus from the sun; because Venus at that time is carried with a retrograde motion from east to west, while an eye placed upon the earth's surface is whirled the contrary way, from east to west. And from this shortening of the time only we might safely enough draw a conclusion concerning the parallax which we are in search of, provided the diameter of the sun and the latitude of Venus were accurately known. But we cannot expect an exact computation in a matter of such subtility.

We must endeavour, therefore, to obtain if possible another observation, to be taken in those places where Venus will be in the middle of the sun's disc at midnight; that is, in places under the opposite meridian to the former, or about six hours or ninety degrees west of London, and where Venus enters upon the sun a little before its setting, and goes off a little after its rising. And this will happen under the above-mentioned meridian, and where the elevation of the north pole is about fifty-six degrees; that is, in a part of Hudson's Bay near a place called Port Nelson. For, in this and the adjacent places, the parallax of Venus will increase the duration of the transit by at least six minutes of time.

If therefore it should happen that this transit should be properly observed by skilful persons at both these places, it is clear that its duration will be seventeen minutes longer as seen from Port Nelson, than as seen from the East Indies. Nor is it of much consequence (if the English shall at that time

[1]June 6, according to new style.

give any attention to this affair) whether the observation be made at Fort George, commonly called Madras, or at Bencoolen, on the western shore of the island of Sumatra, near the equator. But if the French should be disposed to take any pains herein, an observer may station himself conveniently enough at Pondicherry, on the west shore of the Bay of Bengal, where the altitude of the pole is about twelve degrees. As to the Dutch, their celebrated mart at Batavia will afford them a place of observation fit enough for the purpose, provided they also have but a disposition to assist in advancing, in this particular, the knowledge of the heavens. And indeed I could wish that many observations of this famed phenomenon might be taken by different persons at separate places, both that we might arrive at a greater degree of certainty by their agreement, and also lest any single observer should be deprived by the intervention of clouds of a sight which I know not whether any man living in this or the next age will ever see again; and on which depends the certain and adequate solution of a problem the most noble, and at any other time not to be attained to. I recommend it therefore again and again to those curious astronomers who (when I am dead) will have an opportunity of observing these things, that they will remember this my admonition, and diligently apply themselves with all their might in making this observation, and I earnestly wish them all imaginable success: in the first place, that they may not by the unseasonable obscurity of a cloudy sky be deprived of this most desirable sight, and then, that having ascertained with more exactness the magnitudes of the planetary orbits, it may redound to their immortal fame and glory.

This appeal of Halley for observations did not fall upon deaf ears, for certainly a huge amount of energy and preparation went into the observation of the transits of 1761 and 1769. That of 1761 was observed by 176 persons at 117 stations in various sections of the world. Lengthy voyages of great hardship and inconvenience were undertaken, among which we might mention that of Chappe d'Anteroche to Tobolsk in Siberia and that of Le Gentil which probably constituted the most picturesque adventure of all. We quote the description of Sir David Gill in the introduction he wrote to his wife's book, "Six Months in Ascension."

The French sent Pingré to the Island of Rodriguez, and Le Gentil should have observed at Pondicherry.

Poor Le Gentil! He duly reached Mauritius on the 10th of July, 1760,—nearly a year before the Transit. War having meanwhile broken out between France and England, he was unable to reach Pondicherry; so he resolved to go to the Island of Rodriguez instead, to join Pingré, who was already there. When on the point of starting for Rodriguez, he learned that a French frigate was about to sail from Mauritius for the coast of Coromandel. Le Gentil decided to avail himself of the opportunity thus offered to reach the point chosen by the Academy; but one delay after another occurred, and it was not until the middle of March that he sailed again from Mauritius. There was not much time to be lost, for the Transit would occur on the 6th of June. Detained by frequent calms he did not reach the coast of Malabar till the 24th of May. Still there might have been enough time to prepare for the observation, had not the commander of the frigate learned that the English were masters of Mahe and Pondicherry. His only chance to escape capture was to make off as quickly as possible. This he did; steering a course for Mauritius again, to Le Gentil's utmost despair.

The 6th of June arrived. The sky was gloriously clear. From the deck of the vessel Le Gentil made the best observations he could; but, from so unsteady a platform, they could be of little value to science. . . .

But the failure of the Transit of 1761 only urged to new effort for that of 1769. Astronomers through the world felt that if the opportunity, which would then occur, was lost, another so favourable for determining the Sun's Distance would not occur again for 105 years. . . .

The French astronomer, Le Gentil, . . . had no sooner returned to Mauritius than he set out again for Pondicherry, determined to wait there, for eight years, till the next Transit of Venus.

The eventful 3rd of June, 1769, at last arrived.

The morning was fine, and everything promised a happy issue. But, just as the critical moment approached, an unfortunate cloud eclipsed the Sun, a torrent of rain descended, and the fruit of eight years' waiting was lost. Le Gentil had profitably employed his time in studying the astronomy of the Brahmins, so his eight years in Pondicherry had been well occupied; but the agony of disappointment he must have felt at the defeat of his noble endeavours cannot but enlist the sympathy of all who know his story.

The results of the transit of 1761 proved somewhat disappointing in that Venus at the moment of contact, instead of appearing circular, assumed a peculiar shape "like a ninepin". This is the familiar phenomenon of the "black drop".

Immediately after the transit, however, preparations were started for the observation of the next transit in 1769 on a wide scale. This transit was observed in many parts of the world, and values of the solar parallax were derived from it which are in good agreement with our present ideas of 8″.8. About four hundred memoirs on the subject were sent to the different learned bodies of Europe. It is small wonder then that the observations made from what is now Canada are so little known to-day, buried as they are in a multitude of observations.

The volume of the *Philosophical Transactions* of the Royal Society which prints them, vol. LIX, 1769, and which I have borrowed from McGill University, contains twenty-eight papers on this transit, signed by many famous names such as Hornsby, Rittenhouse, Wargentin, Winthrop, De la Lande, Jeremiah Dixon, Messier, Wollaston, Biddle, and Benjamin Franklin.

The most noteworthy of the Canadian observations is by Wales and Dymond from the northwest shore of Hudson's Bay. Since the actual observations have long since been utilized in the reductions of the transit, we print merely the remarks as written by these observers, including their comments that the appearance of Venus at contact might be caused by an atmosphere around it. The paper gives absolutely no reference to the experiences of these two observers as they journeyed from England to that desolate coast. Gill says (op. cit., p. xvi),

> Another English expedition was sent to Hudson's Bay. There Dymock [correctly, Dymond] and Wales, after encountering a good many hardships, successfully observed the Transit; and their observations acquired a special value.

Where the account of their hardships may be found, I do not know. Maybe some of our readers can supply the information. The telescope they used was two feet long, magnifying 120 times, and their north latitude was 58° 47′ 32″.

Astronomical Observations made by Order of the Royal Society, *at* Prince of Wales Fort, *on the North-West Coast of* Hudson's Bay. *By* William Wales *and* Joseph **Dymond.**

REMARKS:

1. All the measurements of Venus's diameter; and also all those of the Sun, which are not said to be horizontal, were taken with the micrometer, in the same direction that the last preceding distance of the limbs of Venus and the Sun was measured with.
2. We were obliged to alter the rack-work of the micrometer before we began to measure any distances of the limbs, etc., in order to make it take in the diameter of Venus, off the scale.
3. The heavens at the beginning, and for a considerable time both before and after, were frequently obscured by clouds: but in the intervals, the air was very clear, and the Sun's limb extreamely well defined.
4. Soon after Venus was half immerged, a bright crescent, or rim of light, encompassed all that part of her circumference which was off the Sun; thereby rendering her whole periphery visible. This continued very bright until within a few minutes of the internal contact, and then vanished away gradually.
5. We took for the instant of the first internal contact, the time when the least visible thread of light appeared behind the subsequent limb of Venus: but before that time, Venus's limb seemed within that of the Sun, and his limbs appeared

behind hers in two very obtuse points, seeming as if they would run together in a broad stream, like two drops of oil; but which nevertheless did not happen, but joined in a very fine thread, at some distance from the exterior limb of Venus. This appearance was much more considerable at the egress than at the ingress; owing, as we apprehend, to the bad state of the air at that time. We took for the instant of internal contact at the egress, the time when the thread of light disappeared before the preceeding limb of the planet, from which time W. W. took notice that he had told about 24″ when the limbs of the Sun and Venus were apparently in contact: a circumstance which he did not venture to attend to at the ingress.

6. We saw nothing like the appearance of an atmosphere around Venus (unless the above-mentioned phaenomena may be thought to proceed from thence) either at the beginning, end, or during the time of the transit: nor could we see anything of a satellite; though we looked for it several times.

7. It may not be improper to add, that the haziness, complained of at the egress, was not owing to any accidental bad quality of the air at that time; it is continually so here to 10° or 12° above the horizon, and often even to 16° or 18,° in what may be called the clearest state of the heavens.

The floor of the Observatory might be above 50 Feet above the level of the Sea at Low-water Mark.

Another observation in Canada was made near Quebec, by Thomas Wright, and Captain Holland, and Mr. St. Germain of the Seminary of Quebec (the precursor of Laval University).

An account of an observation of the Transit of Venus, made at Isle Coudre near Quebec. In a letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Mr. Thomas Wright, Deputy Surveyor of the Northern District of **America.**

Quebec, June 15, 1769.

Sir,

I was prevented landing at the bay of Gaspée, as I purposed (by blowing, thick weather); but, however, I had the good fortune to reach the Island of Coudre, where I landed, with all my apparatus, the 30th of May; and took up my abode at a house well situated, in every respect, for my purpose. The next morning I had a carpenter, who fixed my clock, very firm and perpendicular, against a beam of the house. I immediately set it a-going by my watch, which had not been set to true time for almost a fortnight; but, however, I doubt not but that the following observations of corresponding altitudes will shew exactly the time, as also the regular rate of going of the clock, which I did not venture to adjust, my time being short.

As it is likely I may stay here some time, and all next winter, I shall endeavour to make such observations as may be useful in further settling the longitude here.

Captain Holland observed the external contact, but not the internal, being prevented by clouds. He has sent them to you by this opportunity.

I am, SIR, Your most obedient, humble servant,

THO. WRIGHT.

Saturday, June 3, the morning cloudy, no altitudes taken. At 2^h $49' 22''$ by the clock, I happened to take my eye off from the very point where I afterwards found the external contact happened, imagining I saw it something more to westward; but finding my mistake, I returned to the former point, where I found Venus had made a very
2 50 25 small impression at $2^h 50' 25''$, as is set down in the margin.
3 07 48 time when Venus appeared compleatly round to the eye, and to appearance rather detached, and joined by a small dark thread or ligament, which prevented the rays of light from appearing.
3 08 19 time when the rays of light just appeared, at the internal contact. By means of two oblong smoaked glasses with different shades, made to slide in a groove fixed to my telescope, the phaenomenon appeared very distinct and pleasing to the eye, notwithstanding the weather was a little hazy, and very much so, near the horizon. The thermometer stood at 74 degrees at the time of observation, and the weather was remarkably close and sultry two days before, and quite calm till an hour before the transit happened, when it began to blow very fresh. June 4, the weather continued much the same, and about 9^h $30'$ in the evening, we had a shock of an earthquake, which lasted about four seconds, and alarmed all the inhabitants of the island.

The weather, at the time of the transit, was not clear enough to observe the least appearance of an atmosphere round the planet, supposing there really had been one.

Remarks by the ASTRONOMER ROYAL.

The instruments made use of by Mr. Wright, in the foregoing observations, were a 2 feet reflecting telescope; a pendulum clock beating half seconds; a brass Hadley's sextant, of about 15 inches radius; and a rectangular reservoir for holding quick silver, or any other fluid, which is sheltered from the wind by two glass sides inclined to one another, and ground truly plane: this last for taking the Sun's double altitude by reflection with the Hadley's sextant. . . . The place of observation is $35' 41''$ north of Quebec, and $31' 34''$ east of it.

Astronomical Observations made by Samuel Holland, Esquire, Surveyor- General of Lands for the Northern District of North-America and others of his Party. Communicated by the Astronomer Royal.

March 8, 1769, observed by Samuel Holland, Esquire, at his house, bearing south, 56° west from Quebec, distance from the castle of St. Lewis 2½ miles, with Bird's astronomical quadrant, the latitude, viz. . . .

Mean result of latitude observations, $46° 47' 15''$.

June 3, 1769, observed, by the same, at the same place, with the same instrument, the Transit of Venus, as follows: at 2 hours, 28 minutes, and 1½ seconds, perceived a luminous point on the lower part of the Sun's limb, by appearance; and, in the same place, 1½ seconds afterwards, the first external contact was formed, which rectified as the clock or time-piece of Graham was 15 seconds too fast at the time of observation (as proved by equal altitudes of the Sun taken with Bird's astronomical quadrant on the 1st, 2nd, 4th, and 5th instant) the equal or mean time of observing the first external contact will be at 2 hours, 27 minutes, and 48 seconds. Mr. St. Germain, of the seminary of Quebec, observed the same contact, at the same instant, with Short's 2 feet reflecting telescope. Clouds intervening, prevented the observation of the first internal contact; but at 6 o'clock the Planet might be seen with the naked eye on the Sun's disc, through the haziness of the atmosphere.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Von Humboldt's Account of the Magellanic Clouds

OBSERVERS in the northern hemisphere miss several of the most conspicuous sights in the southern heavens, the two Magellanic Clouds and the dark Coal Sack. The importance of the Magellanic Clouds, as the external galaxies nearest us, has become increasingly apparent in modern astronomy. An excellent historical account of these objects is given by Von Humboldt in his famous *Cosmos,* in which he attempted a description of all natural science. We quote from Sabine's translation, *A Physical Description of the Heavens,* London, 1867, p. 246 et seq.

We have next to speak in greater detail than could be done in the General View of Nature of an object unparalleled in the entire firmament, and which greatly enhances the picturesque beauty, so to speak, of the southern celestial hemisphere. The two Magellanic clouds (which were probably first called by Portuguese and then by Dutch and Danish navigators, *Cape-Clouds**), arrest the attention of the traveller, as I have myself experienced, in the most forcible manner, by their brightness, their remarkable isolated position, and their revolution at unequal distances round the southern pole. That the name which refers to Magellan's voyage of circumnavigation was not their earliest designation is proved by the express mention and description of these luminous clouds by the Florentine, Andrea Corsali, in his voyage to Cochin, and by Petrus Martyr de Anghiera, Secretary to Ferdinand of Arragon, in his work de Rebus Oceanicis et Orbe Novo (Dec. i. lib. ix, p. 96). Both these notices belong to the year 1515, whereas Pigafetta, who accompanied Magellan, does not mention the "nebbiette" in the journal of the voyage previous to January 1521, when the ship Victoria made her way from the Patagonian Strait into the South Pacific Ocean. The older name of "Cape-Clouds" is certainly not to be attributed to the proximity of the still more southern constellation of the Table-Mountain, which was itself only introduced by Lacaille. The name may more probably refer to the real Table-Mountain, and to the phenomenon, long dreaded by seamen as portending tempest, of a small cloud resting on its summit. We shall see presently that the nubeculae of the southern heavens, after having long been noticed but without receiving any name, as navigation extended and commercial routes became more frequented, gradually obtained names derived from those routes.

The frequent navigation of the Indian sea adjacent to the shores of Eastern Africa, especially from the time of the Ptolemies and in the voyages in which

*Lacaille, in the Mém. de l'Acad. année 1755, p. 195.

advantage was taken of the Monsoons, first made navigators acquainted with the constellations near the southern pole. As has been already remarked, it is among the Arabians that we find as early as the middle of the tenth century, a name for the larger of the Magellanic clouds which Ideler has identified with the (white) Ox, el-bakar, of the celebrated astronomer Dervish Abdurrahman Sufi of Rai, a town in the Persian Irak. In the "Introduction to the Knowledge of the Starry Heavens," written at the Court of the Sultans of the Dynasty of the Buyides, he says: "Below the feet of Suhel" (it is expressly the Suhel of Ptolemy, Canopus, which is here meant, although the Arabian astronomers also gave the name of 'Suhel', to several other large stars in the constellation of "el Sefina" or the Ship), "there is a white patch, which is not seen either in Irak," (in the region of Bagdad), "nor in Nedschd," (Nedjed, the northern and more mountainous part of Arabia), "but is seen in southern Tehama, between Mecca and the point of Yemen, along the shore of the Red Sea."* The position of the "White Ox" relatively to Canopus is here assigned with sufficient accuracy for the unassisted eye; for the Right Ascension of Canopus is $6^h\ 20^m$, and the eastern margin of the larger Magellanic cloud is in $6^h\ 0^m$ Right Ascension. The visibility of the nubecula major in northern latitudes cannot have been materially altered since the tenth century by the precession of the equinoxes, for in the course of the next ten centuries it reached its maximum distance from the north. Taking the most recent determination of the place of the larger cloud by Sir John Herschel, we find that in the time of Abdurrahman Sufi it was perfectly visible as far north as 17° N. Lat.; at present it is so nearly to 18°. The nubeculae might therefore have been seen throughout the whole of the south-west of Arabia, the incense-producing country of Hadhramaut, as well as in Yemen, the ancient seat of civilization of Saba and of the early immigration of the Joctanides. The extreme southern point of Arabia, at Aden on the Straits of Bab-el-Mandeb, is in 12° 45′, and Loheia is only in 15° 44′ North Lat. The rise of many Arab settlements on the intertropical east coast of Africa, both north and south of the equator, also naturally led to a more complete and detailed acquaintance with the southern constellations.

Of civilised navigators, the first who visited the West Coast of Africa beyond the Line were Europeans, and first, and more especially, Catalonians and Portuguese. Undoubted documents . . . shew that 178 years before the reputed first discovery of the Cabo Tormentoso (the Cape of Good Hope) by Bartholomew Dias, in the month of May 1487, the triangular configuration of the southern extremity of the African continent was already known. After Gama's expedition, the rapidly increasing importance of the commercial route round the Cape, forming the general object of all voyages along the western coast of Africa, led to the two clouds or nubeculae being called by navigators the "Cape Clouds," as being remarkable celestial phenomena seen in Cape voyages.

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*Ideler, Untersuchungen über den Ursprung und die Bedeutung der Sternnamen, 1809. The name Abdurrahman Sufi is abbreviated by Ulugh Beg from Abdurrahman Ebn-Omar Ebn-Mohammed Ebn-Sahl Abu'l-Hassan el-Sufi el-Razi.

On the east coast of America, the continued attempts to advance southward beyond the equator, and even to the southern extremity of the continent, from the expedition of Alonso de Hojeda, which Amerigo Vespucci accompanied in 1499, to the expedition of Magellan with Sebastian del Cano in 1521, and that of Garcia de Loyasa* with Francisco de Hoces in 1525, had the effect of continually directing the attention of navigators to the southern constellations. According to the journals which we possess, and to the historical testimonies of Anghiera, this was especially the case in the voyage of Amerigo Vespucci and Vicente Yañez Pinzon, in which Cape St. Augustin, in 8° 20′ S. Lat. was discovered. . . . Petrus Martyr de Anghiera, who was personally acquainted with all the discoverers of that remarkable epoch, and whose letters are written under the vivid impression received by him from their narrations, depicts in an unmistakable manner the mild but unequal light of the nubeculae. . . . The great celebrity and long duration of Magellan's voyage of circumnavigation (from August 1519 to September 1522), and the length of time during which the numerous party belonging to it remained under the southern heavens, obscured the remembrance of earlier observation, and the name of "Magellanic clouds" extended itself among the maritime nations bordering on the Mediterranean.

We have taken a single example of the manner in which the extension of the geographical horizon towards the South opened a new field to contemplative astronomy. Navigators advancing under these new heavens felt peculiar interest and curiosity in four objects: the search after a southern pole-star; the form of the Southern Cross, with its upright position when passing the meridian of the place of observation; the Coal-bags; and the revolving luminous clouds. From Pedro de Medina's "Arte de Navegar" which appeared first in 1545, and was translated into many languages, we learn that as early as the first half of the sixteenth century meridian altitudes of the "Cruzero" were employed in determinations of latitude; measurement, therefore, soon followed simple contemplation. The first examination into the position of stars near the Antarctic pole was made by means of distances from known stars whose places had been determined by Tycho Brahe in the Rudolphine Tables: the credit of it belongs, as has been already remarked, to Petrus Theodori of Emden, and Friedrich Houtman of Holland, who sailed over the Indian seas in 1594. The results of their measurements were soon adopted in the star-catalogues and celestial globes of Blaeuw in 1601, Bayer in 1603, and Paul Merula in 1605. These were the feeble commencements of investigations into the topography of the southern heavens previous to Halley (1677) and previous to the meritorious astronomical endeavours of the Jesuit Jean de Fontaney, of Richaud, and of Noël. The histories of astronomy and of geography, in intimate connection with each other, bring before us the same memorable epochs as conducive alike to the completion of the gen-

*The important, and not sufficiently noticed, discovery of the South point of the New Continent, in S. Lat. 55°, belongs to Francisco de Hoces, who commanded one of the ships of Loyasa's Expedition in 1525.

eral cosmical picture of the firmament, and of the outlines of the terrestrial continents.

The two Magellanic clouds, of which the larger covers forty-two and the smaller ten square degrees of the celestial vault, produce at first sight, as seen by the naked eye, the same impression as would be made by two detached bright portions of the Milky Way of corresponding dimensions. In strong moonlight the smaller cloud disappears entirely, while the larger one only loses a considerable portion of its light. The drawing given of them by Sir John Herschel is excellent, and accords perfectly with my most vivid Peruvian recollections. It is to the arduous exertions of the same astronomer at the Cape of Good Hope in 1837, that we owe the first accurate analysis of these wonderful aggregations of the most various elements. He found therein single scattered stars in great number; groups of stars and globular star-clusters; and both regular oval, and irregular amorphous nebulae, more closely crowded than in the nebular zone of Virgo and Coma Berenicis. From the complex character of the nubeculae, therefore, they ought not to be regarded either (as is too often done) as extraordinarily large nebulae, or as detached portions of the Milky Way. In the Milky Way, round star-clusters, and more especially oval nebulae, are extremely rare phaenomena, excepting in a small zone situated between the constellation of Ara and the tail of Scorpio.

The Magellanic clouds are neither connected with each other nor with the Milky Way by any perceptible nebulous appearance. The smaller nubecula is situated in what, excepting the vicinity of the star-cluster in Toucani, is a kind of starless desert; the larger Magellanic cloud is in a less scantily furnished part of the celestial vault. The structure and internal arrangement of the larger nubecula are so complicated, that masses are found in it (like No. 2878 of Herschel's Catalogue), in which the general form and character of the entire cloud are exactly repeated. The conjecture of the meritorious Horner, of the nubeculae having once been parts of the Milky Way, in which their former places can still be recognised, is nothing more than a myth; nor is the assertion of a progressive motion or change of position being perceptible in them from the time of Lacaille, better founded. The indefiniteness of their edges as seen in telescopes of small aperture had caused the positions formerly assigned to them to be inexact, and it has even been remarked by Sir John Herschel that nubecula minor is entered almost one hour in Right Ascension out of its true place in celestial globes and star maps generally. According to the same authority, nubecula minor is situated between the meridians of $0^h\ 28^m$ and $1^h\ 15^m$, and between 162° and 165° north polar distance; and nubecula major in $4^h\ 40^m$—$6^h\ 0^m$ R.A., and 156°—162° N.P.D. Of stars, nebulae, and clusters, he has given in Right Ascension and Declination no fewer than 919 in the larger, and 244 in the smaller nubecula. In order to distinguish the three classes of objects from each other I have counted up in the list:—

In nubecula major, 582 stars, 291 nebulae, 46 star-clusters:

In nubecula minor, 200 stars, 37 nebulae, 7 star-clusters.

The smaller number of nebulae in the nubecula minor is striking; their proportion to the nebulae in nubecula major being as 1:8; while the corresponding ratio of single stars in the two nubeculae is about as 1:3. These tabulated stars, almost eight hundred in number, are mostly of the 7th and 8th magnitudes,—some being between the 9th and 10th. In the midst of the nubecula major there is a nebula noticed as early as by Lacaille, (30 Doradus, Bode, No. 2941 of Sir John Herschel,) which is without a parallel in any part of the heavens. It hardly occupies 1/500th of the area of the entire nubecula, and yet within this space Sir John Herschel has determined the positions of 105 stars from the 14th to the 16th magnitude, which are projected against or detach themselves from the altogether unresolved, uniformly shining, and unchequered nebulous ground.

Opposite to the Magellanic luminous clouds, and at a greater distance from the Southern Celestial Pole, there revolve around it the black spots or patches which at an early period, at the end of the fifteenth and beginning of the sixteenth centuries, attracted the attention of Portuguese and Spanish Navigators. . . . The phenomenon of this class which has been longest known, and which is most striking to the unassisted eye,—viz. the dark patch in the Southern Cross, is situated on the eastern side of that constellation, and is pear-shaped, with a length of 8 and a breadth of 5 degrees. There is in this large space one star visible to the naked eye, (between the 6th and the 7th magnitude), and a large number of telescopic stars from the 11th to the 13th magnitudes. A small group of 40 stars occupies nearly the centre of the space. Paucity of stars and contrast with the surrounding brightness have been assigned as the causes of the sensible blackness of the space in question; and since the time of Lacaille this explanation has been generally received. . . . Whilst I remained under the southern tropic, and under the influence of the powerful impression made upon me by the aspect of the celestial canopy towards which my attention was continually drawn, the above explanation, from the effect of contrast, appeared to me, probably erroneously, to be an unsatisfactory one.

OUT OF OLD BOOKS

By HELEN SAWYER HOGG

For out of olde feldes, as men seith,
Cometh al this newe corn froe yeer to yere;
And out of olde bokes, in good feith,
Cometh al this newe science that Men lere.
CHAUCER, *The Parlement of Foules.*

SCHWABE'S DETERMINATION OF THE SUN-SPOT CYCLE

Astronomers, both professional and amateur, are familiar with the fact that the well-known cycle of sun-spots was first discovered just about a century ago by Heinrich Schwabe of Dessau in Germany. The careful records that he kept of sun-spots appeared from time to time in the Astronomische Nachrichten. One such paper, in 1844, has already been published in translation in *A Source Book in Astronomy,* by Shapley and Howarth.

The definite proof of the ten-year cycle came a few years later than this paper, when Schwabe was able to observe his third maximum, and the 1851 edition of Von Humboldt's Cosmos is usually cited as the source which first called the attention of the world to the sun-spot cycle.

An excellent summary of Schwabe's endeavours is given by M. J. Johnson, President of the Royal Astronomical Society, when the Society's Gold Medal was awarded to Schwabe in 1857, in *Monthly Notices,* vol. 17, p. 126, 1857. We quote from this account. Readers of the Journal may be surprised to learn that nearly a century ago some men considered astronomy an "exhausted science."

Gentlemen,—The order of our proceedings has now brought us to the presentation of the medal which, as you have heard, the Council has awarded to M. Schwabe, for his discovery of the periodicity of the solar spots.

M. Heinrich Schwabe is a gentleman resident in, and I believe a native of, Dessau, the capital of the Duchy of Anhalt Dessau, who, having no professional duties, has devoted much time and attention to scientific studies, and has attained considerable reputation in other departments of knowledge than that in which he is known to this Society. Among other accomplishments, I am informed that M. Schwabe is an excellent botanist, and has composed, though, I believe, not published, a flora of the neighbourhood of Dessau.

It was in 1826 that he entered upon those researches which are now to engage our attention, - - but I am not aware of any published results before those in No. 350 of the *Astronomische Nachrichten,* which appeared in April 1838. Here he gives a summary of twelve years' labour, and though at this time he must have begun to foresee the issue, for the indications of periodicity are plain, he makes no remark, nor does the memoir appear to have attracted the attention of astronomers.

From this time commence his annual contributions to the *Astronomische Nachrichten;* but it was not till the end of 1843, when he had passed through two periods of maximum and minimum, that he very modestly remarks that his observations heretofore had given indications of periodicity which that year's result tended to confirm. Still the subject attracted little attention. As far as I can discover, the only person who had taken it up was Julius Schmidt, the present indefatigable Director of the Observatory at Olmütz, then residing near Hamburg. But the philosopher of Dessau was not disconcerted; he went on accumulating fresh proofs of his great discovery, which, when announced in 1851, by Alexander Von Humboldt in the third volume of his immortal *Cosmos,* came upon the world with all the freshness of novelty, though in reality the secrets had been revealed eight years before. - - -

His instruments are two telescopes, by Frauenhofer, one a 3½, the other a 6-foot, with powers of 45, 64, and 96, the last only used in extreme cases. The apertures of his object-glasses were generally reduced to 1¾-inch and 2½-inches, an arrangement by means of which he obtained the double advantage of being able to protect his coloured glasses, and to use lighter tints than would have been otherwise practicable. He particularly recommends glasses of a certain blue tint which he obtained from Munich. - - -

Twelve years, as I have said, he spent to satisfy himself—six more years to satisfy, and still thirteen more to convince, mankind. For thirty years never has the sun exhibited his disk above the horizon of Dessau without being confronted by Schwabe's imperturbable telescope, and that appears to have happened on an average about 300 days a-year. So, supposing that he observed but once a-day, he has made 9000 observations, in the course of which he discovered about 4700 groups. This is, I believe, an instance of devoted persistence (if the word were not equivocal, I should say, pertinacity) unsurpassed in the annals of astronomy. The energy of one man has revealed a phenomenon that had eluded even the suspicion of astronomers for 200 years! [Christian Horrebow had suggested that the solar spots might show a periodicity.—Ed.]

Let us hope that the example will not be lost. Men are apt to speak of astronomy as an exhausted science, meaning that all that can be known is known. No doubt being the most perfect, it is in one sense the most exhausted science. But the astronomer of Dessau has taught us that there are still mines rich in ore, though they lie deep buried, and must be worked with more assiduity and with more care. I can conceive few more unpromising subjects, from which to extract a definite result, than were the solar spots when Schwabe first attacked them.

We reprint now the work cited by Johnson, from Von Humboldt's Cosmos, V. IV, p. 85, translation by Otte and Paul, New York, 1852. So much has been written now concerning sun-spots and weather that it is interesting to note from this article that Schwabe himself tried the first correlation of sun-spots against temperature or barometer readings, which he kept himself over long intervals.

From Cosmos, by Alexander Von Humboldt.

The attempts that have been made to prove, by means of meteorological observations prosecuted for many years at *individual* spots, that one side of the Sun (for instance, the side which was turned toward the Earth on the 1st of January, 1846) possesses a more intense heating power than the opposite one, have not led to more reliable results than the older Greenwich observations of Maskeleyne, which were supposed to prove that the Sun had decreased in diameter.

The observations made by Counselor Schwabe, of Dessau, for reducing the periodicity of the Sun's spots to definite numerical relations, appear to have a surer foundation. No astronomer of the present day, however admirable may have been his instruments, could have devoted his attention more continuously to this subject than Schwabe, who, during the long period of twenty-four years, frequently examined the Sun's disk upward of 300 days in the year. As his observations of the Sun's spots from 1844 to 1850 have not yet been published, I have presumed so far on our friendship as to request that he would communicate them to me, and at the same time answer a number of questions which I proposed to him. I will close this section of the *Physical Constitution of our Central Body* with the observations with which this observer has allowed me to enrich the astronomical portion of my work.

"The numbers contained in the following table leave no doubt that, at least from the year 1826 to 1850, the occurrence of spots has been so far characterized by periods of ten years, that its maxima have fallen in the years 1828, 1837, and 1848 and its minima in the years 1833 and 1843. I have had no opportunity," says Schwabe, "of acquainting myself with the older observations in a continued series, but I willingly concur in the opinion that this period may itself be further characterized by variability."

"I observed large spots visible to the naked eye in almost all the years not characterized by the minimum; the largest appeared in 1828, 1829, 1831, 1836, 1837, 1838, 1839, 1847, 1848. I regard all spots whose diameter exceeds 50″ as large, and it is only when of such a size that they begin to be visible to even the keenest unaided sight.

"The spots are undoubtedly closely connected with the formation of faculae, for I have often observed faculae or shallows formed at the same points from whence the spots had disappeared, while new solar spots were also developed within the faculae. Every spot is surrounded with a more or less bright luminous cloud. I do not think that the spots exert any influence on the annual temperature. I register the height of the barometer and thermometer three times in the course of each day, but the annual mean numbers deduced from these observations have not hitherto indicated any appreciable connection between the temperature and

Year	Groups	Days showing no spots	Days of observation	Year	Groups	Days showing no spots	Days of observation
1826	118	22	277	1839	162	0	205
1827	161	2	273	1840	152	3	263
1828	225	0	282	1841	102	15	283
1829	199	0	244	1842	68	64	307
1830	190	1	217	1843	34	149	312
1831	149	3	239	1844	52	111	321
1832	84	49	270	1845	114	29	332
1833	33	139	267	1846	157	1	314
1834	51	120	273	1847	257	0	276
1835	173	18	244	1848	330	0	278
1836	272	0	200	1849	238	0	285
1837	333	0	168	1850	186	2	308
1838	282	0	202				

the number of the spots. Nor, indeed, would any importance be due to the apparent indication of such a connection in individual cases, unless the results were found to correspond with others derived from many different parts of the Earth. If the solar spots exert any slight influence on our atmosphere, my tables would, perhaps, rather tend to show that the years which exhibit *a large number of spots* had a *smaller number of fine days* than those exhibiting few spots.

"William Herschel named the brighter streaks of light which are seen only toward the Sun's circumference, *faculae,* and the vein-like streaks visible only toward the center of the Sun's disk, shallows. I am of opinion that the *faculae* and *shallows* are both derived from the same conglobate luminous clouds, which appear more intensely bright at the circumference, but, being less luminous in the center of the Sun's disk than the surface, exhibit the appearance of shallows. I think it preferable to designate all the brighter portions of the Sun as *luminous clouds,* dividing them, according to their form, into globate and vein-like. These luminous clouds are irregularly distributed over the Sun, and when more strongly manifested occasionally impart a *mottled* or *marbled* appearance to the disk. This is often distinctly visible over the entire circumference of the Sun, and sometimes even to its poles, but yet always most decidedly manifested in the two proper zones of the spots, even when no spots are visible in those regions. At such times these bright zones of Sun-spots vividly remind one of Jupiter's belts.

"The fainter portions lying between the vein-like luminous clouds on the general surface of the Sun are deeper indentations, and always present a shagreen-like gray, sand-like appearance, reminding the observer of a mass of uniformly-sized grains of sand. On this shagreen-like surface we may occasionally notice exceedingly small faint gray (not black) *pores,* which are further intersected by very delicate dark veins. These *pores,* when present in large masses, form gray nebulous groups, constituting the penumbrae of the Sun-spots. Here the pores and black points may be seen spreading from the nucleus to the circumference of the penumbra, generally in a radiating form, which occasions the identity of configuration so frequently observed to exist between the penumbra and the nucleus."

The signification and connection of these varying phenomena can never be manifested in their entire importance to the inquiring physicist until an uninterrupted series of representations of the Sun's spots can be obtained by the aid of mechanical clock-work and photographic apparatus, as the result of prolonged observations during the many months of serene weather enjoyed in a tropical climate. The meteorological processes at work in the gaseous envelopes of the dark body of the Sun are the causes which produce the phenomena termed Sun-spots and conglobate luminous clouds. It is probable that there, as in the meteorology of our own planet, the disturbances of very multifarious and complicated character depend upon such general and local causes, that it can only be by means of prolonged observations, characterized by completeness, that we can hope to solve a portion of this still obscure problem.

OUT OF OLD BOOKS

By HELEN SAWYER HOGG

SABINE'S CORRELATION OF SUN-SPOTS WITH MAGNETIC DISTURBANCES

In the last number of this JOURNAL we reprinted Schwabe's discovery of the sun-spot cycle, and commented that Schwabe himself was the first person to attempt to correlate the spot cycle with temperature and barometer readings. Immediately upon the publication of his work in 1851, two men, Lamont in Germany and Sabine in England, who were working independently on magnetic observations, found that periodic terms in them were strongly correlated with the sun-spot cycle.

Of particular interest to the Canadian readers of this JOURNAL is the fact that a long series of observations made at Her Majesty's Magnetical Observatory at Toronto over some years, beginning in 1840, was used by Col. Sabine to show that there was a strong correlation between the disturbance of the magnetic declination, and the number of solar spots. Sabine used thousands of observations, taken hourly at Toronto and at Hobarton on Van Diemen Island, now Hobart, Tasmania, which involved an enormous labour both in the taking and the reducing. He published in the *Philosophical Transactions of the Royal Society* a series of three lengthy papers, entitled *On Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances*. The first was published in 1851 before Schwabe's work had become common knowledge. The second paper was published in 1852, shortly after the appearance of the Cosmos, and correlates the magnetic disturbances with Schwabe's assiduous spot records. The third paper was published in 1856 when the observations from Toronto were entirely finished and reduced, and in this Sabine pays special compliment to the diligence of the work at Toronto.

In his paper in 1851, Col. Sabine gave the summary of the magnetic disturbances of the years 1843, 1844, and 1845 as observed at Toronto and Hobarton. In the paper of 1852 he adds the observations from the years 1846, 1847, and 1848, and in a postscript, those of 1841 and 1842. And he is impressed with the correlation of the magnetic disturbances with the recently published sun-spot cycle of Schwabe. He says (*Phil. Trans.* vol. 142, p. 103, 1852):

I have had the satisfaction of finding that the observations of these years confirm every deduction which I had ventured to make from the analysis of the disturbances of the former period; whilst new and important features have presented themselves in the comparison of the frequency and amount of the disturbances in *different years*, apparently indicating the existence of a *periodical variation* which, either from a causal connection (meaning thereby their being possibly joint effects of a common cause), or by a singular coincidence, corresponds precisely both in period and epoch, with the variation in the frequency and magnitude of the solar spots, recently announced by M. SCHWABE as the result of his systematic and long-continued observations.

Sabine discusses the averages of the magnetic disturbances in three ways, 1, diurnal, through different hours of the day and night; 2, monthly, by different months of the year, and 3, in different years. He points out that Lamont has studied the mean monthly range of Declination and shown a periodic tendency in its fluctuations over some years, (op. cit. p. 118).

In a recent number of POGGENDORFF'S Annalen, 1851, No. 12, December 23 (which only reached the author of this paper when the greater part of it was already written), Dr. Lamont has published a Table of the mean monthly range of the diurnal variation of the Declination at Munich, from 1841 to 1850 inclusive, from which he also has been led to infer the probable existence of a periodical inequality, having its epoch of minimum in 1843.5, and of maximum in 1848.5, - - - Dr. Lamont confines himself entirely to the diurnal inequality of the Declination, leaving untouched the subject of the disturbances (or, as they are more usually termed in Germany, the magnetic storms).

Sabine collected the disturbed observations from the Toronto and Hobarton work. Since the observations were made hourly,

. . . . every hourly observation which was found to differ by a certain prescribed amount from the mean value of the Declination in the same month and at the same hour was separated from the rest, and a body of *disturbed observations* was thus collected, of which the recognised characteristic was simply that they were the disturbances of largest amounts occurring in the whole period. - - - The *mean* hourly, monthly, and yearly numbers and aggregate values in the whole period were then taken as the respective units, and the ratios to these units computed for each of the hours, months, and years. - -

Table XIII exhibits the ratios of the numbers and aggregate values of the disturbed observations at Toronto and Hobarton in the different years, to the average annual number and aggregate value respectively.

TABLE XIII

	Numbers		Values	
Years	Toronto	Hobarton	Toronto	Hobarton
1843	0.68	0.52	0.55	0.48
1844	0.76	0.81	0.73	0.82
1845	0.72	0.72	0.62	0.67
1846	1.31	1.09	1.26	1.03
1847	1.19	1.36	1.40	1.44
1848	1.37	1.50	1.43	1.60

- - - the facts which present themselves most obviously and unquestionably to our notice are, that in the years 1843, 1844 and 1845, the ratios were uniformly *considerably less than unity*, and that in the years 1846, 1847 and 1848, they were as uniformly *considerably greater than unity*. - - The ratios of disturbance in the years 1846, 1847 and 1848, were nearly *twice as great* as in the years 1843, 1844 and 1845. - - -

The variation in the amount of disturbance in the different years presented in this Table, has certainly far more the aspect of a *periodical inequality*, than of what may be called for distinction's sake, *accidental* variation. - - - But the existence of a periodical inequality of this nature, affecting at the same time, and in the same manner, parts of the globe most remote from each other, would be a circumstance of such extreme importance in theoretical respects, that we are bound to receive the facts which may appear to indicate it with the utmost caution, and to await the confirmation it may obtain from contemporaneous observations at other stations. - - -

In our present ignorance of the physical agency by which the periodical magnetic variations are produced, the possibility of the discovery of some cosmical connection which may throw light on a subject as yet so obscure, should not be altogether overlooked. As the sun must be recognized as at least the *primary* source of all magnetic variations which conform to a law of local hours, it seems not unreasonable that in the case of other variations also, whether of irregular occurrence or of longer period, we should look in the first instance to any periodical variation by which we may learn that the sun is affected, to to see whether any coincidence of period or epoch is traceable. Now the facts of the *solar spots*, as they have been recently made known to us by the assiduous and systematic labours of SCHWABE, present us with phenomena which appear to indicate the existence of some periodical affection of an outer envelope, (the photosphere,) of the sun; and it is certainly a most striking coincidence, that the period, and the epochs of minima and maxima, which M. SCHWABE has assigned to the variation of the solar spots, are absolutely identical with those which have been here assigned to the magnetic variations. In the third volume of Kosmos, - - Baron von HUMBOLDT has published a tabular abstract supplied by M. SCHWABE.- - - -

M. SCHWABE has not been able to derive from the indications of the thermometer or barometer any sensible connection between climatic conditions and the number of spots. The same remark would of course hold good in respect to the connection of climatic conditions with the magnetic inequalities, as their periodical variation in different years corresponds with that of the solar spots. But it is quite conceivable that affections of the gaseous envelope of the sun, or causes occasioning those affections, may give rise to sensible *magnetical* effects at the surface of our planet, without producing sensible *thermic* effects.

As the physical agency by which the phenomena are produced is in both cases unknown to us, our only resources for distinguishing between accidental coincidence and causal connection seems to be *perseverance in observation*. - - - For such continued investigation we must look to those observatories which are permanent in their institution; and in this particular problem, to those especially which combine astronomical and magnetical research. The hourly observations at the British Colonial Observatories, which, combined with M. SCHWABE'S observations of the sun in Germany, have led to the discovery of the existence of the coincidence during the years 1843-1848, ceased in 1848, having accomplished the special objects for which they were instituted. - -

Woolwich, March 16, 1852.

And in his third paper, (*Phil. Trans.* vol. 146, p. 357, 1856,) Col. Sabine sums up the problems, and pays a special tribute to the diligence with which the observations were made at Toronto, as follows:

The interest and the importance of the inquiry have doubtless been greatly enhanced by the remarkable coincidence, which it was the object of the paper communicated to the Royal Society in March 1852 to announce, between the above-described periodical inequality by which the magnetic variations referable to solar influence are affected, and the periodical inequality which has been discovered by M. SCHWABE to exist in the frequency and magnitude of the solar spots. The coincidence, as far as we are yet able to discover, is absolute; the duration of the period is the same, and the epochs of maximum and minimum fall in both cases in the same years. - - As a discovery which promises to raise terrestrial magnetism to the dignity of a cosmical science, we may feel confident, that, although the Colonial Observatories have been brought to a close, the investigations which they have thus successfully commenced will be pursued to their proper accomplishment, in those national establishments which have a permanency suitable for such undertakings. - - -

May it not be hoped, that the fruits which have recompensed the labour bestowed on the Toronto observations, may encourage some amongst the numerous physicists in Europe and America, who signified their desire to cooperate with the Royal Society in this inquiry, and to adopt the methods and processes of observation which have been followed out at Toronto, to apply themselves to the deduction of the laws of the occasional disturbances which, from the example of Toronto, they may expect to be able to disentangle from the great mass of observations on which their labour has been already bestowed; provided that those observations have been made with the care and perseverance which have distinguished those made by the Officers and Non-commissioned Officers of the Royal Artillery at the Toronto Observatory? Few may be willing to face a heavy labour of reduction before experience has shown that results will follow from the labour; but some may be expected to do so when an example is before them that this additional labour bestowed on their observations will not be without its recompense: a very few stations at which the investigation should be as full and as satisfactory as at Toronto, might, if widely distant from each other on the earth's surface, suffice to form a general theory of the phenomena of the magnetic disturbances. ...

Woolwich, February 1, 1856.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Wales's Journal of a Voyage in 1768

In this Journal, November 1947, we reprinted portions of the astronomical observations of the transit of Venus in 1769, made by Wales and Dymond at Prince of Wales Fort, now Churchill, on Hudson's Bay. I am indebted to Dr. Vilhjalmur Stefansson and Dr. J. Tuzo Wilson for information as to further records of this expedition; and especially indebted to Dr. Wilson for the loan of rare material from his personal Arctic library.

The journal of the experiences of William Wales was published in the *Philosophical Transactions of the Royal Society*, vol. LX, 1770, a year later than the report of the transit. Possibly the recent formation of the Arctic Institute has stimulated literature on early Arctic exploration. At any rate, the current issue of the *Queen's Quarterly* (Kingston, Ontario), vol. IV, no. 1, 1948, carries an article by Richard Glover on William Wales, entitled "An Early Visitor to Hudson Bay." We quote Glover's description of Wales:

> Dymond seems to have been nobody in particular, but Wales had a distinguished career. His visit to Churchill was his first big adventure. He followed it by sailing round the world as astronomer to Captain Cook on his second and third voyages and finally taught mathematics in Christ's Hospital. During his mastership three of the most brilliant of all Christ's Hospital boys passed through the school—Coleridge, Lamb, and Leigh Hunt. As 'Grecians' they were not in Wales's mathematical class, but his personality impressed at least two of them. Hunt remembered him as "a good man of plain, simple manners, with a large person and a benign countenance"; and Lamb, though awed by the rigour of his discipline, adds: "There was in William Wales a perpetual fund of humour, a constant glee about him, which, heightened by an inveterate provincialism of North-country dialect, absolutely took away the sting from his severities."

In "Hearne's Journey from Hudson's Bay to the Northern Ocean", (Champlain Society, 1911), J. B. Tyrrell says of Wales,

> His presence for more than a year among the little band of white men assembled at this remote fur-trading post on Hudson Bay must have had a helpful

influence in preparing Hearne for his great explorations overland to the Arctic Ocean.

In order to observe the transit in June from Churchill, Wales and Dymond were forced to spend thirteen months in the region, remaining through the winter of 1768 in order to be on hand for the transit which would occur before the shipping season opened in 1769. According to Dr. J. Tuzo Wilson, this voyage of Wales and Dymond was the only trip specifically for astronomical purposes in early days in Arctic Canada. Because of its importance from the standpoint of historical astronomy, and the great rarity of the original volume, we will reprint in this issue and that of July-August more than half of Wales's "Journal of a Voyage," in order that our readers may enjoy first-hand the quaint and vivid phraseology in which Wales describes the conditions that he found.

His "Journal" is written in a scientific manner, with personalities ignored, and almost no comments on the discomforts he must have endured, especially during the winter, when the ice on the planks of his bed was half as thick as the planks themselves! Actually his strongest remarks are applied to the "moschetto" and other flying insects. I have included all references of an astronomical or meteorological interest in this account. And I have departed from my usual custom of printing only astronomical subject matter to include such things as Wales's account of the Eskimaux woman "with a child in each boot top"; and the quaint remark that on the 22d and 23d of August "the people were allowed to write to their friends in England".

By modern standards the departure of the ship from England was leisurely indeed, as nearly a month elapsed after the ship sailed from Gravesend on the Thames before it finally left Hoy-head in the Orkneys. Wales makes little comment on the rigours of the voyage, but one cannot help wondering what the feelings of the sailors were when the ship entered the ice pack, and to avoid hitting the ice floes "they turned the ship several times in a minute; the wind blowing a strong gale all the time."

Our present planetary symbols are used to indicate the day of the week throughout this account.

Received January 25, 1770

XIII. Journal of a Voyage, made by Order of the Royal Society, to Churchill River, on the North-west Coast of Hudson's Bay; of Thirteen Months Residence in that Country; and of the Voyage back to England; in the Years 1768 and 1769: By William Wales.

Read March 8 and 15, 1770.

It must be observed, that the Astronomical, and not the Nautical Day, is every where to be understood in the following Journal.

1768.

☉ May 29th. HAVING settled all my affairs in London; about 22 hours I set off for Greenwich, where I received my instructions from the Rev. Mr. Maskelyne, his Majesty's Royal Astronomer.

☽ the 30th. About 2 hours went on board a Gravesend boat; got to that place about 7, and went directly on board the ship. A. M. delivered my instructions to Mr. Dymond, for him to copy, according to Mr. Maskelyne's directions.

♂ the 31st. About 2 hours weighed, and the wind being contrary, we tided it all the way from that place to Yarmouth road; where we arrived and came to an anchor, about 20 h. on ♄, June the 4th.

We lay in Yarmouth Road until the 7th, when we unmoored, and came to an anchor in Cairstown harbour on ☉, the 12th, about 14 hours; having had strong gales, and thick weather, with drizzling rain almost all the time.

We lay in this place, and in the road, till ♃, the 23d, taking in ballast and live stock; having for the most part nasty thick, and cold fogs: About 16 hours the commodore made the signal to un-moor, and about 18 h. we got under way, and stood through Hoy-sound. At 20 h. Hoy-head bore S.E. by compass, dist. about 4 miles. At noon I observed the sun's meridian altitude to be 54° 10′¼, whence the true lat. of the ship was 59° 3′ ¾; the course by compass since 20 h. was W.N.W. at the rate of $4\frac{6}{7}$ miles per hour. Hence the lat. of Hoy-head is 59° 2′ N. and if we account its long. west of Greenwich 3° 20′. the long. of the ship at this time was 4° 5′ W.

. . .

☿ June 29th. . . . At 13^h 5′ the eclipse of the moon was considerably begun; I estimated it about 3 digits. At 14^h 11′ I judged the beginning of total darkness happened; but clouds rendered it a little uncertain. . . .

♂ July 5th. Being by account in long. 45° ¼ W. and by observation in Lat. 57° 43′ N. I made the following observations for determining the long. of the ship. . . .

I did not make use of the telescope when I made these observations, as its field is too small to use when the ship has much motion, which was the case at this time.

We were certain that we were now well a-breast of cape Farewell; having the two preceding days passed several pieces of driftwood. . . .

♄ the 16th. The former part of these 24 hours we ran through several very strong replings of the tide, which made us suspect that we might be nearer the entrance of the Straights than our accounts shewed us to be; and therefore about 11 h. the whole fleet brought to, as the fog was exceeding thick. . . .

About 16 h. we saw the first isle of ice; but it was at too great a distance for me to give any farther account of it.

☽ July the 18th. This day, and yesterday, we have run through several very strong riplings of tide; and have passed by many islands of ice; but their distance, and the thickness of the fog, rendered it impossible for me to give any account of them.

♂ the 19th. Passed within a cable's length of a very large island of ice, or rather frozen snow, for it appeared to me to be nothing else. It was about as high out of the water as our main-top, and was adorned both on its top and sides with spires; and indented in the most romantic manner that can be imagined.

. . .

♄ July the 23rd. About ½ past 2, we made the island of Resolution, which forms the north shore, at the entrance of Hudson's Straits, bearing from us N.W. b. W. It lies, by my account, in lat. 60° 29′ ½ N. and long. 65° 9′ W.

☉ the 24th. . . . About 10 there came along side of us a boat, with several Eskimaux women, and two or three boys; but no men. They traded with the people some of their cloaths, and a few toys of their own making; such as models of their bows, harpoons, &c. but I saw nothing else that they had to trade; nor had they any weapons, either of offence or defence, along with them. The boat is so well described and delineated in Crantz's history of Greenland, that it is entirely needless to attempt it here. . . .

☽ July the 25th. This afternoon I told 32 islands of ice as I stood on the quarter-deck. This number is about double of what I have ever seen before, at one time.

1st. . . . This day, as I was observing the sun's meridional altitude, there came along side of us three Eskimaux in their canoes, or, as they term them, Kiacks, but who had very little to trade, except toys. None of these had along with them any weapon that I saw, except a kind of dart, evidently constructed for sea purposes, as it had a buoy fixed to it, made of a large bladder blown up.

The men have on their legs a pair of boots, made of seal skin, and soled with that of a sea horse; these come barely up to their knees; and above these they have breeches made of seal, or deer skin, much in the form of our seamens short trousers. The remaining part of their cloathing is all in one piece, much in the form of an English shift; only it comes but just below the waist-band of their breeches, and has a hood to it, like that of a woman's cloak, which serves instead of a cap. Over these they have a kind of foul-weather jacket, made of the same leather with the legs of their boots, which they fasten very tightly about their necks and wrists; and when they are in their Kiacks (which also are extremely well described by Mr. Crantz) are likewise fastened in such a manner round the circular hole which admits the man's body, that not the least drop of water can get into it, either from rain or the spray of the sea.

The dress of the women differs not from that of the men, excepting that they have long tails to their waistcoats behind, which reach quite down to their heels; and their boots come up quite to their hips, which are there very wide, and made to stand off from their hips with a strong bow of whalebone, for the convenience of putting their children in. I saw one woman with a child in each boot top.

. . .

☿ July 27. This evening I told 58 islands of ice, all going directly across the Straits from the mouth of the above-mentioned inlet, [the North-Bay] at the rate of several miles per hour. . . .

♀ July 29th. At 15 h. we hauled the wind to the southward, the ice being quite thick a-head of us. At 19 h. hauled the wind to the N.W. and stood through the ledge of ice, as, for aught that appeared to the contrary, it might reach quite to Cape Walsingham, which now bore S.W. It consisted of large pieces close jambed together: in the place where we attempted to pass through, it was not quite so close. It is really very curious to see a ship working amongst ice. Every man on board has his place assigned him; and the captain takes his in the most convenient one for seeing when the ship approaches very near the piece of ice which is directly a-head of her, which he has no sooner announced, but the ship is moving in a quite contrary direction to what it was before, whereby it avoids striking the piece of ice, or at least, striking of it with that force which it would otherwise have done. In this manner they turned the ship several times in a minute; the wind blowing a strong gale all the time. . . .

♄ July 30th. This evening I staid upon deck till after midnight, in hopes to have observed the ☽'s distance from a star; but, after trying for near an hour, I was obliged to give it up, on account of the twilights, which are amazingly bright in these high latitudes. There is another great inconvenience which attends observations of this kind here, viz. a red haziness round the horizon, to a considerable height, rendering the stars very dim; but at the same time large, something like the nucleus of a comet. I have been disappointed by one or other of these, two or three times before; but this is the more vexatious, as we are now amongst many islands, headlands, &c. whose longitudes are entirely unknown, and on which account an observation would have been singularly useful. . . .

☉ August 7th. About 5 saw the low land of Cape Churchill, bearing from the S. to S.W. b. S. but the haziness of the horizon made the land put on a different appearance every 4′ or 5′. I cannot help taking notice of one circumstance, as it appears to me a very remarkable one. Though we saw the land extreamly plain from off the quarter deck, and, as it were, lifted up in the haze, in the same manner as the ice had always done; yet the man at the mast head declared he could see nothing of it. This appeared so extraordinary to me, that I went to the main-top-mast-head myself to be satisfied of the truth thereof; and though I could see it very plain both before I went up, and after I came down, yet could I see nothing like the appearance of land when I was there. I had often admired the singular appearance of the ice in these parts, which I have seen lifted up 2° or 3° at a distance of 8 or 10 miles, although when we have come to it, we have found it scarcely higher than the surface of the water.

At 21 h. we fired a gun, and thought we heard one in answer to it; which, if true, must have been from the factory.

☽ August 8th. We saw the flag-staff of the factory, with the colours on it, bearing S.W. by W. but lost it again in the haze a few minutes afterwards. At 3 we saw the factory-land, and the flag-staff very plain, S.W. b. W. At 4 made the appointed signal, which was properly answered; after which, we bore away directly for the mouth of the river, and at 5 anchored, there being little wind, and the ebb tide was running out very strong. At this time Cape Merry bore S.W. and Eskimaux Point N.W. b. W. from whence, and the run of the ship since noon, I infer that the latit. of the factory is 58° 59′ N. and by Mr. Dymond's observation in 95° 33′ W. or, according to mine, 95° 2′ W.

A little before noon we weighed, and worked up the river to the usual place where the ship lies, where, about two the 9th, she was safe moored.

☿ the tenth, we went on shore, for the first time. We were met on the beach by Captain Richards, who went with us up to the factory, and introduced us, in form, to the governor, Mr. Moses Norton, who, as well as Mr. Fowler, the person who succeeded him, behaved to us with great civility, and kindness. After breakfast, the surgeon of the factory was so kind as to walk with us several miles, to shew us the country. . . .

I found here three very troublesome insects. The first is the moschetto, too common in all parts of America, and too well known, to need describing here. The second is a very small flie, called (I suppose on account of its smallness) the sand-flie. These in a hot calm day are intolerably troublesome: there are continually millions of them about one's face and eyes, so that it is impossible either to speak, breathe, or look, without having one's mouth, nose, or eyes full of them. One comfortable circumstance is, that the least breath of wind disperses them in an instant. The third insect is much like the large flesh-flie in England; but, at least three times as large: these, from what part ever they fix their teeth, are sure to carry a piece away with them, an instance of which I have frequently seen and experienced.

August 11th, 12th, 13th, 15th, 16th, 17th, and 18th, we got on shore the observatory and instruments; but the people were all so busy unloading the ship, and repairing the quay, craft, &c. that we could not begin to put any part of the observatory up.

♂ the 16th, I went with Mr. Fowler about ten miles up the country, which, as far as we went, was nothing but banks of loose gravel, bare rocks, or marshes, which are over-flowed by the spring tides, and do not get dry before they return, and overflow them again. Our errand was, to see if we could not find some land likely to produce corn; and in all that extent we did not find one acre, which, in my opinion, was likely to do it. In some of the marshes the grass is very long, and with much labour they cut and dry as much hay as keeps three horses, two cows, a bull, and two or three goats, the whole winter. . . .

August the 19th and 20th. We laid the foundation of the observatory in its proper place and position, which was on the S.E. bastion, the higher and lower observatories nearly N.N.E. and S.S.W. of each other respectively. This place and position, though inconvenient in some respects, were, in my opinion, the most eligible for our purpose. We also got up the sides thereof, and fixed up a stiff plank of dry English oak to screw the clock to; this plank was about 5½ feet out of the ground, 4 feet in it, 16 inches broad, and 4½ thick, and supported with spurs to make it steady. There was likewise placed at the foot of it, in the most solid manner possible, a stone of about a quarter of a ton weight, with a flat surface, to set the bottom of the clock-case on; so that the clock stood entirely independent of the observatory.

The 22d and 23d, the people were allowed to write to their friends in England, so I employed myself to the same purpose.

The 24th, 25th, 26th, and 27th. The carpenters were employed in making us bed-places, &c. having hitherto had no where to lie but on the floor.

The 29th, 30th, and 31st, were employed on the observatory; we got on the circular parts and roof of each. On the 31st the ship sailed for England.

September 1st, 2d, 3d, 5th, 6th, and 7th. We were employed in finishing the observatory. On the 8th we set up the two clocks. This morning the snow was about two inches deep on the plains. The 9th, put up the stove in the observatory, the two thermometers, and repaired such parts as had been broken in the carriage. ♄ the 10th, we filled the barometer, and put it up; we also took out the quadrant, which we found much tarnished, especially the arc, and adjusted it ready for observation.

☽ Sept. 12th. I found that the roof of the observatory would not permit us to take zenith distances of any stars on the arch of excess of the quadrant, without moving it farther to the southward; and as I could not hit on any method of determining the error of the line of collimation, which to me appeared satisfactory, except by observations of stars near the zenith, I resolved to take up the floor of the observatory, and remove the piles on which the quadrant stood farther south; and which, with the assistance of the house carpenter, I effected on the 16th, so as answer our purpose completely.

From this time to August the 28th, 1769, I kept no journal, except of the weather; the original of which has been given in to the Royal Society; and which is, in reality, the only thing we have to keep a journal of here in the winter season; and therefore, what I have farther to offer is in short memorandums, which I made when the circumstances mentioned occurred to me; but as they will scarcely appear intelligible, in that form, to any but myself, I shall endeavour to throw it into a sort of historical account of the seasons, and manner of living, in that part of the world.

(*To be continued*)

OUT OF OLD BOOKS

By Helen Sawyer Hogg

(*Continued from May-June Journal*)

IN this section of Wales's Journal he recounts the various aspects of the life at Churchill which seemed to him to merit note-keeping. We read with some amusement of his difficulties with an alarm clock, when the severe cold necessitated additional weight to make it go. His observation of the lack of spectacular aurorae at Churchill is rather surprising, since Churchill lies in a zone of great auroral frequency. Possibly this was due to the fact that the sun-spot cycle was then about two years before its maximum, and the greatest frequency of aurorae comes a year or two after spot maximum. The comet which Wales saw on his return voyage to England was one of very long period, found by the famous comet observer Messier, on August 8, 1769.

Although Wales's voyage was certainly successful, one cannot help considering the ending somewhat of an anticlimax. His precious arctic clothing, which had been given him by the Hudson's Bay Company, and which he apparently treasured, was forcibly taken from him by the British customs officers when his ship sailed up the Thames, and he was never able to recover it. And the winds were from the wrong direction when the ship reached the English coast, so that after stalling around for several days, William Wales and Joseph Dymond made the final lap of the return voyage to London by stage coach!

Nearly two centuries after Wales wrote his journal, its reading is still "conducive of pleasure."

We arrived at Churchill just in the height of what is called the small bird season, which consists of young geese, ducks, curlews, plover, etc. This begins about the latter end of July, and lasts till the beginning of September, when the greater part of these birds leave that part of the country. The geese then begin to go fast to the southward, and continue to do so until the beginning of October. This is called the autumnal goose-season, in which every person, both native and European, that can be spared, is employed; but they seldom kill more geese at this time than they can consume fresh.

By the middle of October the ground is generally covered with snow. The partridges then begin to be very plentiful; and as soon as that happens, the hunters repair to such places as they think most probable to meet with plenty of game in. The English generally go out in parties of three or four, taking with them their guns, a kettle, a few blankets, a buffalo, or beaver skin coverlid, and a covering for their tent; which is made of deers skins, dressed by the natives, and sewed together, so as to make it of a proper form and size. In pitching their tents, they have an eye likewise to their own convenience with respect to shelter from the winds, and getting of fire-wood; which, it will easily be imagined, makes a considerable article here in the necessaries of life: I mean at this season of the year.

Much about this time, likewise, we who stayed at the factory began to put on our winter rigging; the principle part of which was our toggy, made of beaver skins: in making of which, the person's shape, who is to wear it, is no farther consulted, than that it may be wide enough, and so long that it may reach nearly to his feet. A pair of mittens and a cap, of the same, are all the extraordinary dress that are worn by those who stay at the factory, unless we add a pair of spatter-dashes, made of broad cloth, which we wear over our common stockings, and two or three pair of woollen socks, which we have on our feet. Those who go out add to the fur part of their dress a beaver skin cap, which comes down, so as to cover their neck and shoulders, and also a neckcloth, or cravat made of a white fox's skin, or, which is much more complete, the tails of two of these animals sewed together at the stump-ends, which are full as long and thick as those of the Lincolnshire weathers before they are shorn. Beside these, they have shoes of soft-tanned moose skin, and a pair of snow-shoes about 4 feet, or 4½ feet long. Most of these articles of dress I was furnished with by the honourable Hudson's Bay company; but my chest was broken open, after the ship came up the river, and every article, except the snow shoes, taken away by the officers of the customs. And though there was not one thing which was not an article of dress; and though a petition was preferred to the Commissioners, in favour of Mr. Dymond and myself, yet, for some reason or other, they could not be restored.

But, to return to Hudson's Bay. November the 6th, the river, which is very rapid, and about a mile over at its mouth, was frozen fast over from side to side, so that the people walked across it to their tents; also the same morning, a half-pint glass of British brandy was frozen solid in the observatory. Not a bird of any kind was now to be seen at the factory, except now and then a solitary crow, or a very small bird about the size of a wren; but our hunters brought us home every week plenty of partridges and rabbets, and some hares; all of which are white in the winter season; and the legs and claws of the partridges are covered with feathers, in the same manner as the other parts of their bodies. We now killed two or three hogs which captain Richards had been so kind to leave with the governor, which before they well opened, and cut into joints, were froze like a piece of ice, so that we had nothing to do but hang them up in a place where they would remain in that state, and use them when we thought proper. We used some of these, I believe, in the month of May, which were as sweet as they were the moment they were killed, and much more tender and delicate. . . .

In the month of January, 1769, the cold began to be extremely intense: even in our little cabbin, which was scarcely three yards square, and in which we constantly kept a very large fire; it had such an effect, that the little alarm clock would not go without an additional weight, and often not with that. The head of my bed-place, for want of knowing better, went against one of the outside walls of the house; and notwithstanding they were of stone, near three feet thick, and lined with inch boards, supported at least three inches from the walls, my bedding was frozen to the boards every morning; and before the end of February, these boards were covered with ice almost half as thick as themselves. Towards the latter end of January, when the cold was so very intense, I carried a half-pint of brandy, perfectly fluid, into the open air, and in less than two minutes it was as thick as treacle; in about five, it had a very strong ice on the top; and I verily believe that in an hour's time it would have been nearly solid. About the beginning of December we began to use spirits of wine for the plumb-line of the quadrant, which would have been evaporated to about half the quantity in a fornight's time, the spirituous part shooting up the plumb-line, and sides of the glass like coral; but perfectly white. What remained would then freeze, but not before. . . .

It was now almost impossible to sleep an hour together, more especially on very cold nights, without being awakened by the cracking of the beams in the house, which were rent by the prodigious expansive power of the frost. It was very easy to mistake them for the guns on the top of the house, which are three pounders. But those are nothing to what we frequently hear from the rocks up the country, and along the coast; these often bursting with a report equal to that of many heavy artillery fired together, and the splinters are thrown to an amazing distance.

On Sunday, March 19th, it thawed in the sun, for the first time, and on the 26th it thawed in reality. The yard of the factory was that day almost covered with water. After this, it continued to thaw every day about noon when the sun was out; and by the 23d of April, the ground was in many places bare. On the 26th it rained very fast, almost the whole night, which was the first rain we had after October the 3d, 1768. It was really surprizing next morning to see what an alteration it had made in the appearance of the country. We had now alternately snow and rain, frosts and thaws, as in England; the grass began to spring up very fast in the bare places, and the gooseberry bushes to put out buds; in short, we began to have some appearance of spring.

The latter end of April, the hunters began to come home from the partridge tents, in order to prepare for the spring goose season, which is always expected to begin about that time; and is, in truth, the harvest to this part of the world. They not only kill, so as to keep the whole factory in fresh geese for near a month, but to salt as many as afterwards make no inconsiderable part of the year's provision. . . .

Toward the latter end of May, the country began to be really agreeable; the weather being neither too hot, nor so cold, but that one might walk any where without being troubled with any disagreeable sensation; and the dandelion, having grown pretty luxuriant, made most excellent salad to our roast geese.

On June 16th, the ice of the river broke up, and went to sea; we now set our nets, and caught great plenty of fine salmon; I have known upwards of 90 catched in one tide. . . .

About the beginning of July we likewise got plenty of very fine radishes; and the tops of our turnips began to grow large enough to boil for greens to our beef and salt geese. Moreover, towards the middle, we had very fine lettuce, so that if the muschettos had not paid us a visit about the beginning of the month likewise, the two or three last months would have been extremely agreeable; but, taking altogether, I cannot help thinking that the winter is the more agreeable part of the year. . . .

The air in this country is very seldom, if ever, clear for twenty-four hours together; but we were not so much troubled with fogs as I expected we should be, from the accounts which I had read of the country, and from what we experienced in our voyage out; but in this point, as well as every other which respects the weather, the journal which we kept will, I presume, be most satisfactory.

I have before mentioned the haze which is continually found near the horizon here. This, I apprehend, is the cause why the sun's rising is always preceded by two long streams of red light, one on each side of him, and about 20° distant therefrom. These rise as the sun rises; and as they grow longer, begin to be inflected towards each other, till they meet directly over the sun, just as he rises, forming there a kind of parhelion, or mock-sun. These two streams of light seem to have their source in two other parhelia, which rise with the true sun; and in the winter season, when the sun never rises out of the above-mentioned haze, all three accompany him the whole day, and set with him, in the same manner that they rose. I have, once or twice, seen a fourth parhelion directly under the true sun; but this is not common.

The aurora-borealis, which has been represented as very extraordinary in those parts, bears, in my opinion, no comparison to what I have seen in the north parts of England. It is always of the same form here, and consists of a narrow, steady stream of a pale straw-coloured light, which rises out of the horizon, about E.S.E. and extends itself through the zenith, and vanishes near the horizon, about the W.N.W. It has very seldom any motion at all; and when it has, it is only a small tremulous one on the two borders.

I shall now resume my journal. Monday, August 28, we took down the instruments, packed them up, and put them on board the ship, expecting to have sailed the next day; but unforeseen accidents detained the captain until ♄ September the 2nd, when we took leave of the governor and officers of the factory, and came on board the ship. We were after this detained by contrary winds until the 7th; on which day, about 15^h we saw the comet, which was observed this year in England, in a right line between ζ Orionis and Procyon; and also in a right line with Aldebaran, and α Orionis; but below both. About 18^h we sailed out of the river with a fine breeze from the west; and at noon I observed, with great care, the sun's meridional altitude to be 36° 2′ ¼; from whence the latit. of the ship is 59° 12′ ¼ and that of the factory 58° 55′ ½.

The prodigious difference between the latitude of Churchill factory, as laid down from observations made by Hadley's quadrant, and that deduced from the observations made with our astronomical quadrant on shore, has often employed my most serious attention; but I cannot think on any probable cause for such difference, unless it lie in the very great refractive power of the air in these parts. I have mentioned how the ice and land appear to be lifted up, when we stand on the ship's deck: and if the visible horizon be lifted up in like manner, it must make its apparent distance from the sun, or, which is the same thing, the sun's apparent altitude less than it otherwise would be; and consequently, the latitude greater than the truth; and also greater than it will be shewn by a land quadrant, which depends not on the horizon, agreeable to what we find it in the case before us. . . .

♀ September the 8th. About $14^h \frac{1}{2}$ I saw the comet again, in a right line with Saturn, and β Canis minoris; and also in a right line with Capella, and γ Geminorum. Its tail passed directly over ϵ Orionis and might be traced about as far beyond it, as that star was from the head of the comet.

☉ September the 10th. At 16^h the comet was in a right line with Castor and Procyon; and about 5° or 6° below the latter. This was the last time I saw it. . . .

We were, by my account, abreast of Cape Farewell on the 26th; about which time we were taken with very rough, and contrary winds, with which we were troubled almost all the way till we got abreast of Ireland. . . .

☽ October the 9th. About 20^h we struck soundings in 87 fathom water: the bottom fine white sand. The 10th at 21^h made the Scilly light-house from the mast-head; and at noon it bore directly north, by true card; the ship being in long. 5° 40′ W. by my account, in 5° 42′ ½ by my last observation, and in 6° 21′ by the former.

☿ October the 11th, latit. 49° 45′ N. . . . At noon the Lizard light-houses bore N.E. by N. dist. by estimation about eight miles. . . .

I here closed my account of the ship's way; but in the evening of the 12th proving very fine, and having an opportunity of observing the ☽'s distance from Aldebaran to the east, and from α Aquilae to the west of her; I took the following ones, as they may be of use, if compared together to shew what degree of accuracy these observations will admit of. . . .

On the 13th the captain, finding that he gained nothing by beating in the Channel, took a pilot on board for Plymouth; and on the 14th, about one o'clock, we came to an anchor in Hamoze. On the 16th, finding the easterly winds still likely to continue, Mr. Dymond and myself took places in the stage, and arrived in London on the 19th about 9 in the evening.

I flatter myself that no gentleman will think that I have laid the preceding remarks before this learned Society, under a presumption that they can, in any respect, merit their notice. There are many of them on subjects which I am but little acquainted with: these were made only for my own amusement, and are now submitted to the Royal Society, at their command, and under a thorough conviction that they will be read with those candid allowances, which, I am well convinced, they stand much in need of. At the same time, I sincerely declare, that it would give me the highest satisfaction if they should be found to contain one useful hint, or be conducive of pleasure to any person whatsoever.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Titius-Bode Law and the Discovery of Ceres

IT would appear that many astronomers of the present generation are somewhat confused in their impressions of the discovery of Bode's law and its influence on the discovery of the first asteroid, Ceres. During the last three years this so-called law has come again into prominence with the important theory evolved by von Weizsäcker on the origin of the solar system. This theory when applied with reasonable mathematical constants, emerges with Bode's formula for planetary distances, $r = a + b \cdot 2^n$, where a and b are constants with value 4 and 3 respectively. Weizsäcker's theory, published in the *Zeitschrift für Astrophysik,* 1945, has been reviewed by Chandrasekhar in *Reviews of Modern Physics,* 1946, and by Gamow and Hynek in the *Astrophysical Journal,* March 1945.

It is a return to the idea that the planets were formed from a single star. It postulates our sun as surrounded by a gaseous ring, which condensed into planetesimals, and, after an interval of about 10^8 years, built up into planets. The chief new assumption is that one-tenth of the solar mass was distributed in this ring, 99 per cent. of which was composed of the lighter elements hydrogen and helium, which have long since dissipated into space, thereby leaving the present planets with 98 per cent. of the angular momentum of the solar system, but only one-tenth of one per cent. of the mass. The relationship of Bode's Law is worked out from numerical considerations of the possible orbits of the "vortices" of particles round the sun.

The present confusion about the development of the law, and whether it was directly responsible for the discovery of Ceres is probably due mostly to the tendency of some text-books of the twentieth century to condense details in a manner which is somewhat ambiguous, and liable to misinterpretation, even by careful readers.

The so-called Bode's law was proposed originally by Titius of Wittenberg, although Kepler in the previous century had called attention to the curious gap in the progression of planetary distances between Mars and Jupiter. A clear and reliable account of the development of this law is given by W. T. Lynn, in a letter to *Observatory,* vol. xvi, p. 178, 1893, and we quote this letter in its entirety, with its references to the original papers. We note that while Titius noticed the arithmetical progression and the gap, he appears to have thought that the gap might be filled by the discovery of satellites of Mars or Jupiter. Bode's name became attached to the law, however, when he correctly prophesied that a new planet would be found (with period 4½ years) occupying the gap. Not one large planet, but a host of tiny ones, have now been found in this space. The translation of Bode's treatise of 1802, mentioned by Lynn, is to be found in the *Source Book of Astronomy,* by Shapley and Howarth.

The so-called Law of Titius or Bode.

Gentlemen,—

The failure of this so-called law of planetary distances after Uranus has caused that much less interest has been felt in it since the discovery of Neptune than before. Still some facts seem to have been forgotten respecting its first enunciation to which it may be well to call attention. That there was a species of progression in the planetary distances, in which a gap occurred between Mars and Jupiter, had been noticed by Kepler. But the numerical expression of this in the form 4, $4 + 3$, $4 + 3 \times 2$, $4 + 3 \times 2^2$, &c. was first put forth by J. D. Titius, of Wittenberg, and as Bode mentions this in his treatise, "Von dem neuen zwischen Mars und Jupiter entdeckten achten Hauptplaneten des Sonnensystems," it is somewhat strange that the progression should have acquired the name of Bode's law. (Prof. Newcomb, oddly enough, calls it Bode's law in one place in the index to his "Popular Astronomy", but the law of Titius in the text to which reference is there made.) Probably, however, this is in great measure due to the fact that Bode, as Sir John Herschel writes, "would appear to have first drawn attention to this interpretation of its interruption," *i.e.,* to its indication of the probability of the existence of an unknown planet, smaller or less capable of reflecting light than the others, revolving between the orbits of Mars and Jupiter.

I have thought, therefore, that it would be of some interest to your readers to refer to the different idea of Titius. It is in a note to a German translation of the "Contemplation de la Nature", of Charles Bonnet, that he gives this numerical progression, pointing out the gap. He then adds (I quote from the second edition, published in 1772):—"Und der Bauherr sollte diesen Raum ledig gelassen haben? Nimmermehr! Lässt uns zuversichtlich setzen, dass dieser Raum sonder Zweifel den bisher noch unentdeckten Trabanten des Mars zugehöre; lässt uns hinzuthun, dass vielleicht auch Jupiter noch etliche um sich habe, die bis isst noch mit keinem Glase gesehen werden." It would be attributing very large orbits to these hypothetical satellites of Mars and Jupiter to suppose that they filled in any considerable part of the gap between the orbits of those planets; nor can these words be twisted into a prophecy of the discoveries of Professors Asaph Hall and Barnard. Still Titius seems to have suggested new satellites rather than planets. Bode, on the other hand, calling attention to the progression (in the second edition of his 'Anleitung zur Kenntniss des gestirnten Himmels,' published in 1772), confidently expresses the view that it points to the existence of another primary planet, which would, he remarks, occupy, according to Kepler's third law, about 4½ years in its revolution round the Sun. Nor did he fail, on the discovery of Ceres in 1801, to claim (in the treatise referred to above) that his conjecture was thus confirmed. He probably little thought that she was to be followed by hundreds of sisters, most of them more diminutive than herself. The discovery, however, of another planet (Pallas), early in the following year, led Herschel to think it probable that the number of those bodies (for which he suggested the name asteroids, since felt to be unsuitable) was great.

Yours faithfully,

W. T. LYNN.

Blackheath, 1893 Mar. 13.

It is interesting to note that over a century ago an attempt was made to apply the same type of law to the distances of the satellites of the solar system from their primaries. In the same volume of *Observatory,* page 359, Lynn calls attention to the attempt of Prof. Challis in this direction. Prof. Challis of Cambridge is known for his pre-discovery work on the planet Neptune, with Adams's position, and his actual viewing of the planet before its discovery, though he failed to recognize it for what it was.

From the same volume, p. 359.

The so-called Law of Bode as applied by Challis to the Satellites.

Gentlemen,—

In vol. iii of the 'Transactions of the Cambridge Philosophical Society,' the late Prof. Challis contributed a paper (read on December 8, 1828) "On the Extension of Bode's Empirical Law of the Distances of the Planets from the Sun to the Distances of the Satellites from their Respective Primaries," in which, by certain modifications of the so-called law, expressed in the more general form—

$$a, a + b, a + rb, a + r^2b, \text{\&c.},$$

he considered that he was able to show that it is applicable to the satellites of the solar system. And he draws the "curious inference, which is equally certain with the reality of the law," that "there can be no planet nearer the Sun than Mercury, and no satellite nearer the several primaries than the nearest of those in each system, which have been discovered." The last part of this inference reads oddly now, in view of Prof. Barnard's recent discovery in the system of Jupiter. But the addition, in 1848, of Hyperion (seventh in order of distance) to the system of Saturn was equally inconsistent with the conclusions formed in this paper. Prof. Challis was obliged to introduce an extra term in considering the satellites of this planet, and express their distances in the form—

$$a,\ a + b,\ a + rb,\ a + r^2b,\ a + r^3b,\ a + r^3r'b,\ a + r^3\ r'^2b.$$

The r' therefore had to be introduced for Titan, no. 6 in order of distance, and Hyperion cannot be fitted into the series, which appears, when worked out numerically, in the shape—

$$4,\ 4+1,\ 4+1\times2,\ 4+1\times2^2,\ 4+1\times2^3,\ 4+1\times2^3\times3,\ 4+1\times2^3\times3^2;$$

or 4, 5, 6, 8, 12, 28, 76.

For Uranus, Challis obtained conformity with a series of the same form as that for Jupiter* ($a,\ a + b,\ a + rb,\ a + r^2b$), adding two more terms of the form $a + r^5b,\ a + r^7b$. But this is by accepting the whole of the six satellites announced by Herschel, four of which have long since ceased to be regarded as real. Challis remarks that their existence had been doubted, but thinks that the conformity of their distances to this law confirms their reality, though they were probably smaller than the two which were undoubted. The so-called law, however, cannot apparently be fitted in any shape to the distances of the four satellites which are now known and probably form the whole system. These distances are approximately in the proportion 4, 5½, 9, 12, or 3, 4, 7, 9.

The discovery of Neptune, in 1846, it need hardly be said, showed that the so-called law of Titius or Bode failed in the planetary system itself after Uranus.

Yours faithfully,

W. T. Lynn.

Blackheath, 1893 Sept. 4.

Although Grant, in his *History of Physical Astronomy,* 1852, overlooks the contribution of Titius to Bode's law, he gives an excellent account of the relationship between this law and the discovery of the first asteroid. This account is given in a shortened form by C. A. Young in the well-known text, *General Astronomy.* We see from Grant's account, page 237 (which we quote) that the astronomers hunting to fill the gap shown by Bode's law were not the actual discoverers of Ceres.

In 1772, Bode published a treatise on astronomy in which he first announced the singular relation between the mean distances of the planets from the sun, which has since been distinguished by his name. This relation . . . exhibited in a very striking light the exaggerated leap from Mars to Jupiter, and suggested the strong probability of a planet revolving in the intermediate region. This

*Numerically for Jupiter 7, 7 + 4, 7 + 4 × 2½, 7 + 4 × (2½)², . . . or 7, 11, 17, 32, . . .

conjecture was rendered still more plausible by the discovery of the planet Uranus, in 1781, the distance of which from the sun was found to conform exactly to the law of Bode. In Germany, especially, a strong impression had been produced that a planet really existed between Mars and Jupiter; and, through the active exertions of De Zach, an association of twenty-four astronomers was formed, having for its object to effect the discovery of the unknown body. For this purpose the zodiac was divided into twenty-four zones, one of which was to be explored by each astronomer; and the conduct of the whole operation was placed under the superintendence of Schroeter. Soon after the formation of this society the planet was discovered, but not by any of those astronomers who were engaged expressly in searching for it. Piazzi, the celebrated Italian astronomer, while engaged in constructing his great catalogue of stars, was induced carefully to examine, several nights in succession, a part of the constellation Taurus, in which Wollaston, by mistake, had assigned the position of a star which did not really exist. On the 1st January, 1801, Piazzi observed a small star, which, on the following evening, appeared to have changed its place. On the 3rd he repeated his observations, and he now felt assured that the star had a retrograde motion in the zodiac. The daily change of position in right ascension was 4′, and the change in declination towards the north pole was 3′ 30″. On the 24th of January, he transmitted an account of his discovery to Oriani and Bode, communicating the positions of the star on the 3rd and 23rd of that month, and adding that its motion, from being retrograde on the 11th, had become direct on the 13th of the same month. Piazzi continued to observe the star until the 11th of February, when he was seized with a dangerous illness which completely interrupted his labours. His letters to Oriani and Bode did not reach these astronomers until the latter end of March, but the planet had then approached too near the sun to admit of their obtaining a verification of his discovery by actual observation, and it was necessary for this purpose to wait until the month of September, when the planet would have effectually extricated itself from the solar rays. Its re-discovery, after the lapse of so considerable a period subsequent to the most recent observation, could not be expected to be accomplished without a pretty accurate knowledge of the orbit in which it was moving; but the data communicated by Piazzi were insufficient for this purpose. They merely served to indicate that the body revolved in a circular orbit between Mars and Jupiter, at a distance agreeing very nearly with that assigned by Bode's law, and so far offered a satisfactory confirmation of the views of the German astronomers. Meanwhile Piazzi, fearing lest he should be deprived in any degree of the glory attached to the discovery of the planet, communicated to astronomers all the observations of it made by him down to the 11th February. Gauss found that they might be all satisfied within a few seconds, by an elliptic orbit, of which he calculated the elements; and, with the view of aiding astronomers in searching for the planet, that illustrious geometer also computed an ephemeris of its motion for several months. After a careful examination of its geocentric path, the planet was finally discovered by De Zach on the 31st of December, and by Olbers on the following evening. A year had therefore elapsed between the original discovery of the planet by Piazzi and its subsequent re-discovery by the German astronomers. Piazzi conferred on it the name of Ceres, in allusion to the titular goddess of Sicily, the island in which it was discovered; and the sickle has been appropriately chosen for its symbol of designation.

Readers may find the translation of the letter Piazzi wrote Bode to announce the discovery of the new object (referred to as a comet), in the *Source Book of Astronomy* already mentioned.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Early Discovery of Four Globular Clusters

MUCH has been written about the importance and the thrill of the discovery of a new planet. Even the non-astronomical public tries to imagine the feelings of "some watcher of the skies when a new planet swims into his ken." Too little, however, is written about the early discoveries of those vast aggregations of suns, the globular clusters. A new planet may seem to us here on earth to be a momentous discovery, but in the cosmic scheme of things it is less important than a gigantic stellar system composed of thousands upon thousands of stars—where the possible number of planets is quite unknown.

The actual accounts of the discoveries of globular clusters are to be found in scarce volumes. Up until the time of William Herschel most of the discoveries had been made in continental Europe, so that most of the discovery announcements are not in English.

Until two centuries ago, to the best of my knowledge, seven globular clusters had been discovered. In order of discovery, by our accepted nomenclature, these are: Messier 22 in Sagittarius, Omega Centauri, Messier 5 in Serpens, Messier 13 in Hercules, Messier 4 in Scorpio, Messier 15 in Pegasus, and Messier 2 in Aquarius. Halley's account of the discovery of Messier 13, the great globular cluster in Hercules, together with his description of the two clusters found earlier, has already been reprinted in this Journal, February 1947. In this issue we reprint the discovery announcements of the other four globular clusters known before 1750.

Messier 5 was discovered on May 5, 1702 by the German astronomer Gottfried Kirch, 1639-1710. He is also noted as the discoverer of the comet of 1680, and the famous variable Chi Cygni. In 1705 he was appointed Director of the Royal Observatory in

Berlin. Not the least interesting fact about Kirch is that his wife Margarethe assisted him in observing, recording and computing. She herself discovered a comet in 1702.

Actually the discovery of the globular cluster Messier 5 made by Gottfried Kirch is given to us in the words of his wife Margarethe. The account is published by J. L. E. Dreyer in A Supplement to Sir John Herschel's "General Catalogue of Nebulae and Clusters of Stars", *Transactions of the Royal Irish Academy*, vol. 26, p. 397, 1878, as a note to the catalogue.

M. 5. Discovered by Gotfried Kirch on the 5th May, 1702. The following is an extract from Marie Margarethe Kirch's diary, now in the possession of Lord Lindsay:—"Durch solches Suchen [for the comet then visible] fand mein Mann durch eben diesen 3 Sch. Tub. hoch über μ [Serpentis, mentioned in the foregoing] ein neblicht, aber doch deutliches Sternchen, es hatte viel feine andere Sternchen um sich, doch eins stand sonderlich per Tubum über diesen ungefähr also [then follows a rough sketch of a star and the "nebulous star" below it] . . . May 6. Das neblichte Sternchen haben wir deutlich auf seiner vorigen Stelle gefunden." At 10.30, P.M., on the date mentioned, 5 M would be about 8° above μ Serpentis, and the sketch made by M. M. Kirch represents exactly the relative position of 5 M and the star 5 Serpentis, as seen in an inverting telescope (per tubum). Communicated by Dr. R. Copeland.

We learn from this account that Gottfried Kirch, while looking for a comet in 1702, found this nebulous star on May 5, and that the next night, May 6, he and his wife together confirmed the discovery of the object. It was Margarethe Kirch who drew the chart of its position and made up the record. Almost fifty years later the object became known as Messier 5 when Messier published his first list of nebulae and star clusters in 1771.

A decade after the discovery of Messier 5, Halley found the globular cluster in Hercules. Thirty years elapsed before the next globular cluster was found. Probably the next one to be sighted was Messier 4, which was found by de Chéseaux in about 1745. The exact year is unknown. M. de Chéseaux is best known for the comet of 1744 which bears his name. This comet was actually discovered by Klinkenberg at Haarlem on December 9, 1743, but it was de Chéseaux who called attention to its six tails. At about this time he compiled a list of 21 nebulous objects which he sent to his grandfather, M. de Réaumur, then president of the French

Academy. There is reference to this work by Maraldi and Legentil in the Mémoires of the Academy in 1746, but the actual letter and list remained filed away and unpublished until 1891, when Bigourdan reprinted it in the *Paris Annales*, Observations, 1884. In his letter, de Chéseaux states that most of the nebulae in Derham's list do not truly merit the name, but that he himself has drawn up a list of 21 true nebulae and star clusters. The criticism de Chéseaux made of Derham's list is absolutely correct (cf. this Journal, July-August, 1947, p. 235). His own list is the best and most complete catalogue of nebulous objects and stellar groups to be compiled up until that time, and has received too little recognition. Number 19 on this list is the globular cluster Messier 4, which de Chéseaux describes as follows:

19. Une qui est près d'Antarès, dont j'ai trouvé, pour cette année, l'A.D. de 242° 1′ 45″ et sa déclinaison 25° 23′ 30″. Elle est blanche, ronde, et plus petite que les precedentes [Omega Centauri and Messier 22]; je ne sache pas qu'on l'ait jamais remarquée.

Apparently de Chéseaux suspected that he had discovered a hitherto unknown object here, but the literature then was so scattered that he could not be sure.

The discovery of the next two clusters, Messier 15 and 2, was made by Giovanni Maraldi in 1746 probably just a bit later than de Chéseaux's discovery of Messier 4. Maraldi, a relative of the famous Cassini family, assisted at the Paris Observatory and edited the Connaissance des Temps. He found his two famous globular clusters while observing a comet in September 1746. This comet, which passed perihelion early in 1747, was discovered by de Chéseaux on August 13, 1746, and last observed by Maraldi on December 5 of that year. In the *Mémoires* of the *Académie des Sciences*, 1746, page 58, Maraldi describes his discovery in his paper entitled, Observations de la comète qui a paru au mois d'août 1746.

Le 7 Septembre j'aperçus entre l'étoile ϵ du Pegase et β du Petit-cheval, une étoile nébuleuse assez claire, qui est composée de plusieurs étoiles, dont j'ai déterminé l'ascension droite de 319^d 27′ 6″, et la déclinaison septentrionale de 11^d 2′ 22″.

Le 11 Septembre j'en observai une autre dont l'ascension droite est de 320^d 7′ 19″, et la déclinaison de 1^d 55′ 38″ vers le midi, à peu près dans le même parallèle ou devoit être la Comète. Celle-ci est ronde, bien terminée, et plus claire au milieu; elle occupe environ 4 ou 5 minutes de degré, et n'est environnée d'aucune étoile, même à une assez grande distance: on n'envoyait aucune dans toute l'ouverture de la lunette, ce qui me parut fort singulier; car la plûpart des étoiles qu'on appelle *nébuleuses* sont environnées d'un grand nombre d'étoiles, ce qui a fait juger que la blancheur que l'on y découvre, est l'effect de la lumière d'un amas d'étoiles trop petites pour être aperçues par les plus grandes lunettes. Je pris d'abord cette nébuleuse pour la Comète, je crus qu'enfin le temps étoit devenu favorable et le ciel assez serein pour nous permettre de voir distinctement l'atmosphère de la Comète avec son noyau.

La difficulté de voir la Comète à la vûe simple dans le temps même le plus serein, qui m'a donné occasion de voir ces nébuleuses, me fit sentir aussi la nécessité d'ébaucher la théorie de la Comète dès le commencement que nous l'aperçumes, afin de la retrouver en cas que nous fussions quelque temps sans la voir, comme il pouvoit arriver par le mauvais temps, ou par la clarté de la Lune.

Maraldi's description of Messier 15 in Pegasus is very brief, but he was apparently greatly impressed, as have been many sky watchers since his time, with the striking appearance of the spectacular cluster Messier 2 set in a field almost devoid of other stars, when viewed in a telescope. This appearance of globular clusters, a rich cluster against an almost starless background, was noted by William Herschel half a century later, and led to his frequent use of the term "insulation" as applied to globular clusters, which seemed to him, quite rightly, set apart from the few scattered stars surrounding them.

Although Maraldi was interested in his new object, in his account we detect a certain amount of disappointment, that what he at first took for a particularly fine view of the comet, in which he was seeing its atmosphere distinctly, proved merely to be a nebula.

It is well known that Messier made his catalogue of nebulous objects as a result of his cometary studies, but we see from the references here quoted that decades before his time, the pursuit of comets was responsible for the discovery of important "nebulous stars."

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Jean Sylvain Bailly, The Guillotined Astronomer.

To be remembered by the historians, but forgotten by the astronomers, is the twentieth century fate of the French astronomer, Jean Sylvain Bailly, 1736-1793. Probably no astronomer who ever lived has played a more important role in the shaping of his country's political destiny. Small wonder it is, then, that the political aspect of the last years of his life has, in less than two centuries, overshadowed the fact that his earlier years were devoted successfully to science, in particular to astronomy. Jean Bailly is not to be confused with the English astronomer Francis Baily (Baily's beads), who lived a half century later and appears better known to-day, at least on this continent.

A vivid general picture of Bailly's dramatic career is given by St. A. Berville in the *Collection des Mémoires relatifs à la révolution française* 1821. We give a translation of the opening paragraph of this account, Mémoires de Bailly.

> A noble and touching picture to present to posterity, is that of a man who, already celebrated in the sciences, recommended by all the private virtues, finds himself, almost unwittingly, carried by public esteem to eminent functions; maintains his modesty in the midst of highest dignities, his moderation in the midst of the most violent political dissensions; goes through an outrageous revolution without letting himself be swept along with it; associates with all its glories, remains free from all its excesses; defends liberty against power and power against licence, and crowns the life of a sage with the death of a hero. Such was Sylvain Bailly, first deputy of Paris to the States-General, first president of the Constitutional Assembly, first Mayor of Paris.

Bailly was born September 15, 1736, at the Louvre, where his father was keeper of the king's paintings, an hereditary position which was intended to be passed on to young Jean. His early training was in this direction, but his own inclination was toward more literary pursuits. Indeed, one curious circumstance of his life is that at the age of sixteen he composed a tragedy, Clothaire, which related the tortures inflicted on a mayor of Paris by a deluded multitude—almost a prophecy of the ending of Bailly's own life.

Through his friendship with the celebrated Lacaille, Bailly early became interested in science, and devoted his labours to it. His first astronomical work was a calculation of the comet of 1759, that is, of Halley's comet on its first predicted return. Possibly as a result of the enthusiasm engendered by the return of this famous comet, Bailly established an observatory in his place of residence, the Louvre, in 1760. The observatory apparently consisted of a window in the upper story of the south gallery, with a few instruments. At this time France did not possess a first-rate observatory. The earliest observations made by Bailly are dated in the beginning of 1760, when he was not yet twenty-four years of age, and are of an opposition of Mars. In the same year he determined oppositions of Jupiter and Saturn.

In 1761 he was associated with Lacaille in observations of the transit of Venus (cf. this JOURNAL, vol. 41, p. 321, 1947). The decade from 1759 to 1769 must have given a great impetus to astronomy, bringing as it did the return of Halley's comet, and the occurrence of a pair of transits of Venus, an event so rare that we in the twentieth century will never witness one. Bailly calculated the parabolic orbit of the comet of 1762; he discussed 42 observations of the moon by La Hire, a starting point for lunar theory. In 1763 he published a reduction of observations made by Lacaille on zodiacal stars and in 1764 he competed for a prize offered by the Academy of Sciences for a dissertation on the theory of Jupiter's satellites. Lagrange was the successful competitor on this occasion, but Bailly's paper established his reputation as an astronomer.

During these years he became known for his elegant literary style in the composition of eulogies of a number of important men. As a result of his ability in several fields he had the almost unique distinction of being elected to all three French academies, a distinction which had previously befallen only Fontenelle. He was elected to the Academy of Sciences in 1763, to the French Academy in 1784, and to the Academy of Inscriptions and Belles Lettres in 1785.

Bailly devoted a large share of his astronomical life to the writing of astronomical histories and researches in the history of the science. His most extensive work was a history of astronomy, the first volume of which, *Histoire de l'Astronomie Ancienne*, was published in 1775. Three volumes, *Histoire de l'Astronomie*

Moderne, followed in the years 1776-1783. In 1787 his final historical volume was published on Indian and Oriental astronomy, which was a mixture of erudition and fantastic speculation.

It is recorded by Arago, *Biographies of Distinguished Scientific Men*, tr. by Smyth, Powell, and Grant, p. 77, 1857, that

In 1775, Bailly sent the first volume of his history to Voltaire. In thanking him for his present, the illustrious old man addressed to the author one of those letters that he alone could write, in which flattering and enlivening sentences were combined without effort with high reasoning powers. "I have many thanks to return to you (said the Patriarch of Ferney), for having on the same day received a large book on medicine and yours, while I was still ill; I have not opened the first, I have already read the second almost entirely, and feel better."

Voltaire, indeed, had read Bailly's work pen in hand, and he proposed to the illustrious astronomer some queries, which proved both his infinite perspicacity, and wonderful variety of knowledge.

The force and charm of Bailly's writings are typified by the opening paragraph from the *Histoire de l'Astronomie Ancienne*.

DISCOURS PRELIMINAIRE

De l'objet de l'Astronomie, de la nature de ses progrès et de son utilité.

L'histoire de l'Astronomie est une partie essentielle de l'histoire de l'esprit humain. Cette science née dans les champs & parmi les Bergers, a passé des hommes les plus simples aux esprits les plus sublimes. Imposante par la grandeur de son objet, curieuse par ses moyens de recherche, étonnante par le nombre & l'espece de ses découvertes, elle est peut-être la mesure de l'intelligence de l'homme, & la preuve de ce qu'il peut faire avec du tems & du génie. Ce n'est point qu'il ait trouvé ici la perfection qui lui est par-tout refusée; mais dans aucun genre l'esprit humain n'a déployé plus de ressources, ni montré plus de sagacité. Il est intéressant de se transporter aux tems où cette science a commencé, de voir comment les découvertes se sont enchaînées, comment les erreurs se sont mêlées aux vérités, en ont retardé la connoissance & les progrès; & après avoir suivi tous les tems, parcouru tous les climats, de contempler enfin l'édifice fondé sur les travaux de tous les siécles & de tous les peuples.

And again, from the first page of Livre Premier, we quote in translation:

Most of the sciences have been born from the needs of man, Astronomy is due only to his curiosity. The apportionment of land has produced geometry; wealth and commerce have made arithmetic necessary; the transport of loads and architecture have demanded mechanics; injuries and illnesses have required recognition of medicinal plants, of the structure of the human body, and thus have been born botany, anatomy, and medicine. Everywhere man has called his industry to the aid of his weakness; everywhere need has drawn him from his natural slothfulness. Here alone as the spectacle of the heavens has drawn his interest he has not been pressed by the spur of necessity. Struck with admiration.

he has fallen into a profound reverie, he has followed tranquilly, and without effort, the course of the ideas which have presented themselves to his intellect. While around him on the earth everything moves with noise, movement accompanied by silence has instilled in him reverence; the uniformity of movement which unceasingly reappears again the same, has given him the idea of an order immutable and eternal; the particular movements of the heavenly bodies which are accomplished simultaneously without injury to one another, and which are not destroyed although opposite in general movement, announce to him a profound wisdom, which has regulated all by laws always fulfilled; he has felt the presence of the Supreme Being, and he has wished to understand the more to admire. Also while the other sciences have taken birth in the middle of the tumult of cities, this one has been born in the heart of the country. It is the science of repose, of solitude and of self-contentment. Troubled men, agitated by passions, would not have fathomed it, or would have disdained it as useless. It is due to simple men, whose free soul, without desire, without design for the future, having no need to concentrate on itself, can be lavished abroad; and these simple men, in watching over their flocks, have founded that one of all the sciences which the human spirit should one day enlarge the more.

Such is the description of astronomy as Bailly fashioned it nearly two centuries ago, a beautiful description, and one which we twentieth century astronomers are still trying to impress upon the non-astronomical public.

By contrast, Bailly expresses his opinion of astrology in no uncertain terms. We translate from the preliminary discourse of the same work, p. XII, calling special attention to two statements of great irony in this paragraph. The first is when Bailly says, "If the future should open before him, his existence would become only a burden." In view of his tragic death on the guillotine, this statement of Bailly's opinion of the future is dramatic. The second statement is his comment that the evils of astrology rule still in certain countries where the light of science has not penetrated. When we consider the volume of astrological literature that is flooding this continent two hundred years after Bailly, we can see that astronomy has not made as great progress in the public mind as could be wished.

Judicial astrology is a malady of the human spirit not less deplorable. It is born without doubt from abuse of astronomy. Everyone, impatient to look into the future, wishes at least to know what awaits him; the sage alone knows that this knowledge would be deadly. Unfortunate in the past, discontented with the present, man lives only by hope. The uncertainty of his destiny sustains him in a course that he exerts himself to accelerate. If the future should open before him, distressed by bad prospects become things of the present, not very sensitive to good things used before their possession, his existence would become only a burden. Divine wisdom has spared us these evils that Astrology has wished to spread over the earth. They rule still in certain countries where the light of the sciences has not penetrated. In Europe even, it was not long ago that the people had their soothsayers and the princes their astrologers. Catherine de Medici, given over to this error, had constructed the column of the Hotel de Soissons, to consult the stars there; for the evildoers especially desire to know the future, and the reproaches of their conscience are a certain astrology against which they have need of being reassured. The death of Henry IV was predicted on all sides, both before and after this unfortunate event. Let us tell that the celebrated Jean-Dominique Cassini was given to astronomy by this very taste for astrology. He was soon undeceived, and his works, in spreading the light, have undeceived his century. The profound knowledge of the movement of the heavenly bodies has opened all eyes. The recognized distance of the stars has shown that they are too far away to shed their influences on our globe. Further, these bodies which, by the diurnal motion of the earth, seem to turn every day about us, ought to act every day in the same manner. They are thus insufficient to explain or to announce the diversity of characters, of passions, and of destinies. We have seen that their aspects, their encounters, determined for all eternity by invariable motions, announce nothing to mankind, that their spheres separated from ours by immense intervals, forbid all communication, all emanation; if it is not that of light, which is without doubt the same for all the stars, and which besides falls equally on all mankind.

In 1777 Bailly made the acquaintance of the famous Benjamin Franklin, and, for two brilliant minds, their first interview seems curious in the extreme. We translate the account given by St. A. Berville, *op. cit.*, which is similar to that given by Arago.

Among the commissioners was Franklin, celebrated in the history of the sciences and in the history of liberty. Franklin was acquainted with Bailly; their acquaintanceship had begun in a very singular manner. Bailly had a country home at Chaillot; it was there that he often retired to work in freedom. Chance also brought to Chaillot the American philosopher. Bailly, learning of his arrival, hastened to call upon him. He entered the home of Franklin, who knew him by reputation, and who received him with the most cordial air.

"Bonjour, Monsieur Franklin; comment vous portez-vous?"

"Fort bien, Monsieur."

After these first words, Bailly seated himself beside Franklin, and, afraid of being indiscreet in addressing a second question to him, waited until his host began to speak. Franklin, silent by nature, more silent still in his rank of ambassador, did not open his mouth. After a silence sufficiently long, Bailly, to begin the conversation, offered Franklin a pinch of snuff; Franklin made a wave of the hand that he did not take any. This silent interview lasted about two hours: at last, Bailly arose, Franklin accompanied him to the door, shook his hand as he repeated the single words: *fort bien*. Such was the origin of the acquaintanceship of these two celebrated men. Bailly loved to recall this anecdote and said often that *fort bien* were the only words that he had ever obtained from Franklin, when he found himself in a private interview with him.

To be continued.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Jean Sylvain Bailly, The Guillotined Astronomer

(Continued from May-June Journal)

SOME years after their first meeting, Bailly and Benjamin Franklin were both appointed to a commission to investigate animal magnetism, or mesmerism, which at that time was causing a furore in France. Mesmer had left France a second time at the end of 1781, leaving behind him some ardent disciples whose conduct at last instigated the government to appoint an investigating commission. Four doctors of the Faculty of Paris were joined in this committee by five members of the Academy of Sciences, among them Lavoisier, Franklin, and Bailly, with the latter as reporter. Bailly's report occupied several years of time. As Arago says, "Nothing equals the credulity of men in whatever touches their health. This aphorism is an eternal truth." Many interesting and rather sensational exhibitions of magnetism were investigated. Bailly remarked, "Animal magnetism may exist without being useful, but it cannot be useful if it does not exist." According to Arago, the commission established, finally and experimentally, that "the action of the imagination can both occasion the crises to cease, and can engender their occurrence."

From this work, Bailly appears soon to have turned his attention to a report on the Paris hospitals. The description of conditions at that time in the famous Hôtel Dieu taxes present-day imagination. There was little segregation of patients with different diseases, and great overcrowding. New arrivals at the hospital were laid in the same sheets from which deceased patients had just been removed. Two madmen were confined in the same bed. In the men's small-pox ward, six men or eight children were laid in one bed a metre-and-a-half wide. Normally the beds contained only four patients, placed alternately head to foot! Each had for his share of space nine inches, and had to lie on his side. The operations of that time were of course performed without anaesthetic—but in the presence of the next victims! Certainly there were many reforms for a group of scientists to institute in the hospital. Apparently the investigation with its report did help to improve conditions, and as one result, each patient was given a bed to himself.

Next, Bailly worked, again as the reporter, on a commission with Coulomb, Lavoisier, and LaPlace, among others, to investigate the slaughter-house conditions. The descriptions of the hospitals are mild compared with those of the conditions around the slaughter houses, some of which were located near the centre of the crowded capital. The cattle, driven in droves down crowded streets, escaped with resulting damage and confusion. The by-products of the slaughter were carried away by open gutters. The immediate neighbourhood of a slaughter house must have been unbearable. The reform suggested by the commission appears obvious and reasonable. Bailly wrote, "We ask that the shambles be removed to a distance from the interior of Paris." Actually it took fifteen years before this logical suggestion was carried out. Referring to this delay, Arago says, "Unfortunately there was nothing exceptional in this; he who sows a thought in a field rank with prejudices, with private interests, and with routine, must never expect an early harvest." Though it was the conditions of the eighteenth century which prompted Arago to make this comment, it applies equally well to the reforms of any century!

In November 1787 Bailly married a widow who was an intimate friend of his mother, though his own age. Madame Bailly adored her husband, and lavished every attention on him. She felt that her duty was to remain at home, while her husband's was to appear in public. Consequently only once as wife of the Mayor of Paris did she appear at a public ceremony.

A minor and amusing incident in Bailly's life at this time seems to give his biography a timeless flavour—it could happen here and now! He wrote anonymously a biography of Gresset, the French poet. One copy had been sent to the Marchioness of Créqui, a lady in high society. Some days after, Bailly called upon her, possibly hoping to hear her speak in praise of the new work. To the contrary, the lady condemned the eulogium in no uncertain terms. Though Bailly tried to change the subject, the Marchioness insisted that he see for himself how poorly the pamphlet was written, and begged him to read some of it aloud, interrupting him with derogatory remarks. The harrowing situation continued until the arrival of another guest.

The denouement of the situation, however, came two years later when Bailly was Mayor of Paris. At this time some booksellers collected all his smaller works and published them. Much to Bailly's amusement, the Marchioness, who had by this time forgotten the incident, overwhelmed Bailly with compliments on this very work which she had previously condemned. A partial explanation of this seemingly two-faced behaviour is that the early pages of the pamphlet were somewhat obscure, but that later on the writing becomes more lucid.

In the last four years of Bailly's life, from April 21, 1789, when he was elected a deputy to the Third Estate, until November 11, 1793, when he met his death stoically on the guillotine, are crowded the events which have overshadowed his earlier reputation as scientist and man of letters. The record of part of these events is left to us in the words of Bailly himself, a set of three volumes in the series, *Mémoires sur la Révolution française,* entitled *Mémoires d'un Témoin de la Révolution, ou Journal des faits qui se sont passés sous ses yeux, et qui ont préparé et fixé la constitution française.* Some historical opinion (F. M. Fling, Univ. of Nebraska Studies, vol. 3, pp. 331-353, 1903), indicates that Bailly himself wrote the first two volumes of the famous *Mémoires d'un Témoin,* but that the third volume was written by someone else from Bailly's notes. These *Mémoires* are a long and detailed, day by day account, of the happenings of that important epoch in French history. There is evidence that Bailly worked at their composition as rapidly as possible, but that he never had time to finish them. At any rate, the *Mémoires* are given only to October 1789.

From the time of his first election as deputy, throughout his term in public life, the conduct of Bailly set a very high standard. As Fling says, (*op. cit.,* p. 340),

> It is clear that in 1789 Jean-Sylvain Bailly was one of the most distinguished of the conservative burgesses of Paris. It may well be believed that when the King learned of his election as the first deputy of the Third Estate from Paris he remarked, "J'en suis bien aise, c'est un honnête homme." (*Mém.,* Vol. I, p. 71).

Bailly's election as deputy was apparently a surprise to him, and in telling of it he stresses that he certainly furnishes an example that a man can arrive at the top with the highest honours without intrigue. This should be said, says Bailly, for the consolation of honest people, and to encourage youth to follow the right path. (*Mém.*, vol. 1, p. 9):

> Enfin les districts furent formés et ouverts sur la convocation du roi, le 21 avril 1789. J'habitais alors ma maison de Chaillot, où je terminais quelques réparations et dispositions intérieures; je me rendis aux Feuillans, en me promenant, avant huit heures du matin. Assis pour me reposer sur la terrasse des Feuillans, un jeune homme, que je ne connaissais pas, passa, et me dit: "Vous allez au district des Feuillans, vous y serez nommé électeur." Je le remerciai de cette opinion et n'y comptai pas plus. Je raconte ces bagatelles parce qu'elles servent à prouver que les circonstances m'ont porté ou j'ai été élevé, et que je n'y ai contribué en rien. Nul homme à Paris ne peut dire que je lui aie demandé ou fait demander son suffrage, pas même que j'aie témoigné aucun désir des places où je suis parvenu. Je suis un exemple bien sûr qu'on peut parvenir à tout et aux premiers honneurs sans intrigue. Ceci doit être dit pour la consolation des honnêtes gens, et pour l'encouragement de la jeunesse à suivre le droit chemin.

A few days after his election as deputy, Bailly was named Secretary of the Assembly. Thus in the beginning it was he who drew up the famous proces-verbal of the meetings of the electors of Paris, the records so often used by historians.

On Tuesday May 12 the general assembly of electors voted for the nomination of first deputy of Paris, and Bailly was chosen, though not by a wide margin, since he received 173 votes out of 317, only 14 more than needed for a majority. In his *Mémoires* he comments, "I observed in the Assembly of the Electors a great dislike for literary men, and for the academicians." How true in any century!

Bailly had spoken only once (on the question of voting by being seated or standing) when, on June 3, he was named Senior of the Deputies of Communes. It was at this time that the Dauphin was critically ill; he died on the fourth of June. Bailly had been requested by the Communes to seek an audience with the King, and much misrepresentation has been made of him as being heartless to intrude on the King at such a time. The truth is that Bailly pressed for the interview only as far as his position demanded, and Louis understood this perfectly, vindicating Bailly in a note. After the death of the Dauphin, Bailly and other members formed a delegation to call on the King to express their sympathy.

The delay in the reception of the Communes some days earlier had apparently been occasioned by difficulties of the ceremonial. The Third Estate did not wish to speak kneeling, though such had long been considered the custom. Bailly recounts this dramatically (*Mém.*, vol. 1, p. 104); the translation is from Arago.

"This custom," said M. de Barentin, "has existed from time immemorial, and if the King wished - - - "

"And if twenty-five millions of men do not wish it," exclaimed Bailly, interrupting the minister, "where are the means to force them?"

"The two privileged orders," replied the Guard of the Seals, somewhat stunned by the apostrophe, "no longer require the Third Estate to bend the knee; but, after having formerly possessed immense privileges in the ceremonial, they limit themselves now to asking some difference. This difference I cannot find."

"Do not take the trouble to seek for it," replied the President hastily; "however slight the difference might be, the Communes will not suffer it."

Thus did Bailly interpret the rising spirit of democracy in France.

During the memorable days from the 17th to the 23rd of June, Bailly presided over the Third Estate. On the 17th, the Deputies of the Communes, wearied from the changing principles of the other orders, determined in case of need to act without them, and adopted the title of National Assembly. On the 20th of June great excitement was fomented because the hall of the National Assembly was closed to the members without notice, ostensibly in preparation for a visit from the King two days later. At the suggestion of Guillotine, the Assembly moved to a tennis court. With five or six benches, and a table to write on, they stood all day, and took the famous oath "never to separate, but to assemble wherever circumstances might render it requisite, until the Constitution of the Kingdom should be established and confirmed on solid foundations."

When the King later addressed the Assembly, he ended his speech with the following imprudent remark. "I order you, Gentlemen, to separate immediately." The whole of the nobility and a portion of the clergy retired, while the Deputies of the Communes remained quietly in their places. The Grand Master of the Ceremonies having remarked it, approached Bailly saying, "You heard the King's order, Sir?" The illustrious President answered, "I cannot adjourn the Assembly until it has deliberated on it." "Is that indeed your answer, and am I to communicate it to the King?" "Yes, Sir," replied Bailly, and immediately addressing the Deputies who surrounded him, he said, "It appears to me that the assembled nation cannot receive an order."

On July 2 Bailly quitted the chair of President of the National Assembly, and returned to his native Chaillot, where the residents feted him with fireworks and, in recognition of his achievements, made him honorary churchwarden. But he was destined not long to enjoy a pastoral existence. The Bastille was taken on the 14th of July. On the 15th the National Assembly received permission from the King to send a deputation to Paris to try to restore peace and order. Though Madame Bailly tried to persuade her husband to remain at home, he replied, "After a presidency that has been applauded, I am not sorry to show myself to my fellow-citizens."

The Assembly, having accomplished its purpose, was about to break up when, in a gesture of spontaneous enthusiasm, it proclaimed La Fayette Commander-in-Chief of the National Guard, (which had just been created) and Bailly Mayor of Paris. (*Mém.*, vol. 2, pp. 24-26.)

> M. le marquis de La Fayette, accepting this honour with all signs of respect and recognition, drew his sword; he swore to sacrifice his life in the conservation of this liberty so precious, whose defence they had deigned to give him.
>
> At the same instant everyone proclaimed M. Bailly provost of the guilds. One voice made itself heard saying: "Not provost of the guilds, but mayor of Paris." And by an acclamation, all those present repeated, "Yes, Mayor of Paris."

At this sudden, and apparently unexpected, tribute, Bailly was overcome with emotion, and his description of his reply may shed some comfort on those of us who, in moments of stress, find ourselves at a loss for words.

> I know not whether I wept, I know not what I said; but I remember well that I was never so surprised, so confused, and so beneath myself. Surprise adding to my usual timidity before a large assembly, I rose, I stammered out a few words that were not heard, and that I did not hear myself, but which my agitation, much more than my mouth, rendered expressive. Another effect of my sudden stupidity was, that I accepted without knowing what a burden I was taking on myself.

This quotation, with its concluding sentence (so true still of people who accept heavy obligations) was written after Bailly had lived through many months of the heavy burden of the mayoralty. For when Bailly became Mayor, famine threatened the city. The supply of grain and flour in Paris would be exhausted in three days. The severe hail storm a year before, on July 13, 1788, had destroyed a large part of the harvest that year, and before the next harvest was ready the spectre of famine was stalking the streets of Paris. The day after Bailly became Mayor, all the overseers of the food administration in the city quietly disappeared, so that a hungry and infuriated populace could not punish them. On his second day as Mayor, Bailly's thoughts and energies had to turn immediately to the problem of getting flour and grain for the hungry. For some weeks it was a daily struggle to bring enough food into the city to satisfy the multitudes. Blunders of inefficient or stupid underlings frequently hampered the government's efforts to bring in wagon or barge loads of food for this hand-to-mouth existence. Bailly remarked later, "when I used to pass the bakers' shops during the scarcity, and saw them besieged by a crowd, my heart sunk within me; and even now that abundance has been restored to us, the sight of one of those shops strikes me with a deep emotion." The intense strain of this epoch appears to have undermined his health permanently.

Bailly was Mayor for two years and four months, during which time Paris was for the most part a peaceful city, by no means dripping with the blood that was later to be shed there. Bailly used his power in many good ways, such as an attempt to suppress the gambling houses. He refused to accept as part of his remuneration any funds which came from this source; actually he used a great deal of his own money in the duties of his office, and left it poorer than when he became Mayor. He also suppressed public exhibitions of animal fighting, and attempted reforms of prisons, especially urging that arrested persons be brought to trial more speedily. In such ways his outlook on life seems to have been well in advance of his century.

During the spring of 1791 as the populace became more restless, Bailly tried to pursue a sane and tolerant course. He attempted to moderate the demands of the mob, and opposed such men as

Marat and Hébert. By April 1791, however, his influence was beginning to wane, and two events of the early summer definitely destroyed his favour with the people. The first occurred on June 20-21 when the King and his family fled from the capital. Actually Bailly had taken measures to prevent such a flight, but the populace blamed both him and La Fayette for the King's escape, and for a few days both of them were in great danger.

It was, however, the disastrous events of the 17th of July which were probably eventually responsible for Bailly's execution. Early in July the National Assembly had discussed the idea of substituting a republic for a monarchy. Naturally this occasioned great fomentation on all sides. A petition was drawn up by the club of Jacobins, in favour of the republic, which was to be signed on the 17th of July on the Champ de Mars, on the altar of the country. The National Assembly considered this an anarchical movement, and on the 16th of July it ordered the city of Paris to use force if necessary to put down any rebellious movements. When the crowd was collected on the Champ de Mars for the signing of the petition on July 17th, it was reported that two good citizens had been killed, and a turmoil resulted. The Municipal Council ordered martial law proclaimed, and a red flag flown from the Hôtel de Ville. At six o'clock the National Guard, headed by La Fayette and Bailly, with the red flag and some cannon, began to march toward the altar. At this early period, the National Guard was a troublesome and insubordinate unit. At seven thirty this body arrived on the Champ de Mars, where they were met with heckling, stone throwing, and by the firing of a pistol. In the confusion the soldiers fired on the mob, as a result of which forty persons were killed and more than a hundred wounded. Order was then restored, but at a great price. Bailly was afterward held responsible for the order to fire, and this act cost him his popularity with the people.

Several points may be noted in Bailly's defense. He did not know that the gathering at the Champ de Mars was a relatively peaceful one; the reports of events which reached him were coloured and incorrect. He was not aware that the National Guard had loaded their muskets, and probably he gave no actual order to the Guard to fire. In the confusion, and later as the revolution itself gathered momentum, it was very easy to make Bailly the scapegoat of the incident.

Bailly continued in the mayoralty for some months after this unfortunate day, but left it November 12, 1791, after installing his successor Pétion, and retired to Chaillot. His health was poor, however, and his physician urged him to undertake a journey for a change. The middle of June 1792 Bailly and his wife went to Nantes, where they took a small lodging with some congenial people. A pleasant existence was not long allowed him however. The Council of the Communes decreed that there was 6000 livres tax owing on the house and offices that he had occupied as Mayor, and that this tax must be paid immediately. To raise the funds Bailly had to sell all his excellent and treasured library at auction. The central government then began to treat him in the nature of a parolled criminal, and demanded that he report every eight days. He also received a letter from the Minister of the Interior informing him that the apartments in the Louvre which his family had occupied for over half a century had been taken from him. As Bailly had shortly before been obliged to sell his house at Chaillot, he was left with no home near Paris, the city where he had recently been first citizen. Small wonder it is that Bailly did not attempt any creative writing during this period, but contented himself with reading the latest novels as fast as the library received them. As Arago says, "Tranquillity of mind is not less requisite than vigour of intellect, to those who undertake great works."

(*To be concluded*)

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Jean Sylvain Bailly, The Guillotined Astronomer
(concluded from September-October Journal)

After the execution of Louis XVI on January 21, 1793, a wealthy friend tried to persuade Bailly and his wife to sail for England, possibly with America as their ultimate destination. Bailly's sense of patriotism was so strong that he refused to flee from his country. However, on June 30, 1793, Nantes was besieged by eighty thousand Vendeans. Certainly Bailly's life would have been worth nothing had they captured the city, and after they were repulsed Bailly decided it was time to move to one of the quieter, non-insurgent provinces. Until then Mélun had been perfectly tranquil. M. LaPlace was living there in retirement, and had suggested that Bailly and his wife join them. So on the 6th of July M. and Madame Bailly started for Mélun, but unfortunately just as a detachment of the revolutionary army left for that city too. Madame LaPlace immediately wrote a guarded note of warning to Bailly, saying that the house they were intending to occupy was too damp and would be the death of Madame. Imagine then, the consternation of M. and Madame LaPlace when they looked out, about the end of July, and saw Bailly coming up their garden path! The LaPlaces feared that Bailly had missed the meaning of their discouraging letter, but he assured them that he had not; he had come to Mélun in order to have a house of his own in which to be arrested!

He had no chance to enjoy the honour of being a domiciled citizen there, for two days after his arrival he was recognized by a soldier of the revolutionary army and arrested. When the soldier ordered Bailly to accompany him to the town hall, Bailly replied, "I am going there. You may follow me there." The magistrate of Mélun, who was honest and courageous, tried to persuade the people that there was no reason to arrest Bailly, but the most lenient course that he was allowed to follow was to advise Paris of Bailly's whereabouts, meanwhile keeping him under surveillance in his house. At this point Bailly could have escaped, but he refused to do so, believing it would discolour his own honour and greatly compromise the magistrate M. Tarbé.

The Committee of Public Safety ordered the magistrate of Mélun to transfer Bailly to one of the prisons in Paris. On the day of the transfer Madame LaPlace urged him to escape, but again he refused to do so. Upon his arrival in Paris he was first imprisoned at Madelonnettes, and a few days later at La Force. His wife and nephews were allowed to visit him there. It was from there that he was summoned as a witness in the trial of Marie Antoinette. At this famous trial he defended the Queen, protesting with horror at the slanderous accusations made against her. Because he took this stand, the tribunal viewed him harshly, almost as an accused person rather than as a witness. At this trial Bailly showed his courage and honesty to a marked degree. He might even have bought his own life at this point by siding with the false accusers of the Queen.

After his appearance at this trial Bailly wrote and had circulated a most touching pamphlet (sixteen printed pages) entitled *"J. S. Bailly à ses concitoyens."* In this he sets forth that, contrary to the accusations, he and La Fayette attempted to prevent any escape of the King from the Tuilleries, and that any part which he had in the events on the Champ de Mars was due to his responsibility of office to uphold the laws of the municipality. At the conclusion of his appeal to his fellow-citizens he writes, (*Mém.,* vol. 1, p. 411)

> I have gained by the Revolution only that which my fellow citizens have gained: liberty and equality. I have lost by it some useful situations, and my fortune is nearly destroyed. I could be happy with what remains of it to me and a clear conscience; but to be happy in the repose of my retreat, I require, my dear fellow-citizens, your esteem: I know well that, sooner or later, you will do me justice; but I require it while I live, and while I am yet amongst you. I have deserved it during fifty years of sustained probity; and almost three years of a devotion entirely to your interests, without other price than this esteem, could only increase and consolidate it.

Bailly's own appearance as accused before the Revolutionary Tribunal was set for November 10, 1793. The chief accusations against him were regarding the escape of the royal family, and the catastrophe of the Champ de Mars. We have already discussed these events, and Bailly's probable share in them. Many versions and stories have been given of both of them, so that it is hard to know where the exact truth lies. Certainly Bailly's acts did not merit a death sentence, but he was unanimously condemned to be executed the following day. When the President of the Tribunal asked him if he had anything to say regarding the sentence, Bailly replied, "I have always carried out the law, I shall know how to submit myself to it, since you are its organ."

Bailly spent the evening of the 11th of November being visited by various comrades, among them his nephew with whom he played a game of piquet. The account given by Arago of the day of his execution is very graphic, and appears reliable in its facts. Various conflicting reports regarding the execution are in existence. We quote from Arago, *op. cit.* p. 157.

Bailly had risen early, after having slept as usual, the sleep of the just. He took some chocolate, and conversed a long time with his nephew. The young man was a prey to despair, but the illustrious prisoner preserved all his serenity. The previous evening in returning from the Tribunal, he remarked, with admirable coolness, though springing from a certain disquietude, "that the spectators of his trial had been strongly excited against him. I fear," he added, "that the mere execution of the sentence will no longer satisfy them, which might be dangerous in its consequences. Perhaps the police will provide against it." These reflections having recurred to Bailly's mind on the 12th, he asked for, and drank hastily, two cups of coffee without milk. These precautions were a sinister omen. To his friends who surrounded him at this awful moment, and were sobbing aloud, he said, "Be calm; I have rather a difficult journey to perform, and I distrust my constitution. Coffee excites and reanimates; I hope, however, to reach the end properly."

Noon had just struck. Bailly addressed a last and tender adieu to his companions in captivity, wished them a better fate, followed the executioner without weakness as well as without bravado, mounted the fatal cart, his hands tied behind his back. Our colleague was accustomed to say: "We must entertain a bad opinion of those who, in their dying moments, have not a look to cast behind them." Bailly's last look was towards his wife. A gendarme of the escort feelingly listened to his last words, and faithfully repeated them to his widow. The procession reached the entrance to the Champ de Mars, on the side towards the river, at a quarter past one o'clock. This was the place where, according to the words of the sentence, the scaffold had been raised. The blinded crowd collected there furiously exclaimed that the sacred ground of the Champ de la Fédération should not be soiled by the presence and by the blood of him whom they called a great criminal. Upon their demand (I had almost said their orders), the scaffold was taken down again, and carried piece-meal into one of the fosses, where it was put up afresh. Bailly remained the stern witness of these frightful preparations, and of these infernal clamours. Not one complaint escaped from his lips. Rain had been falling all the morning; it was cold; it drenched the body, and especially the bare head, of the venerable man. A wretch saw that he was shivering, and cried out to him, *"Thou tremblest, Bailly." "I am cold, my friend,"* mildly answered the victim. These were his last words.

Bailly descended into the moat, where the executioner burnt before him the red flag of the 17th July; he then with a firm step mounted the scaffold. Let us have the courage to say it,—when the head of our venerable colleague fell, the paid witnesses whom this horrid execution had assembled on the Champ de Mars burst into infamous acclamations.

Thus ended the life of a man who was years ahead of his time in thought and outlook.

After her husband's death, Madame Bailly was a crushed soul, living for some time in poverty. To add to her pitiful situation was the fact that she had placed 30,000 francs from the sale of the house at Chaillot in the wadding of a dress, and forgot the whereabouts of the money, though she was in great need, until it was no longer of value. Brighter days came to her later, after the appointment of General Bonaparte as Consul and M. de LaPlace Minister of the Interior, when a pension of 2000 francs was granted her, with the Consul demanding that the first half year should be paid in advance, and at once. Mme. de LaPlace had then the overwhelming pleasure of calling upon the pathetic widow to bring her a purse full of gold.

In preparation of this account of the life of Jean Sylvain Bailly, I have been fortunate in having access to several thousand pages of Bailly's writings in their original editions, in the library of the University of Toronto. These comprise the volumes of the *Histoire de l'Astronomie,* as well as the three memorable volumes *Mémoires d'un Témoin.* The long biography written by Arago has also been very helpful, as well as the comprehensive sketch of Bailly's life in the ninth edition of the Encyclopaedia Brittanica, 1875. In most of the summaries or histories of astronomy which have been published in English during the last century, references to Bailly are sadly lacking. In summarizing his life I have tried always to select the most reliable version of an incident. As I have followed through the course of his amazing existence, with his steady climb upward in public recognition and in achievement, a climb attended with burdens and problems almost unbearably heavy, the words of the ancient poet have kept flitting through my mind, "Let us now praise famous men."

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Astronomical Observatories a Century Ago

During this present great year, when the most stupendous telescope yet conceived has gone into action for astronomical research on the top of Palomar Mountain, it is interesting to turn back the pages of time a hundred years and consider the observatories of the world at the middle of the nineteenth century. The account of the locations of the telescopes which existed then may come as a slight surprise to some of us who have become used to the great industrial and intellectual power of this continent to-day. A concise account of the various observatories existing at that time is given by François Arago in his *Popular Astronomy*, vol. II, p. 804, 1858, translated by Smyth and Grant.

The editor of this volume made several corrective footnotes to the account, one of which Canadians in general and Torontonians in particular will find exceedingly amusing; but Arago's information is for the most part accurate. For a complete account of the Toronto Observatory listed here, the reader is referred to Thiessen's articles in this Journal, vol. 34, 1940, et seq. The Bailly mentioned in the second sentence of the account is the same astronomer whose life has just been summarized in recent issues of this Journal, May-December, 1949.

ON OBSERVATORIES, ON ASTRONOMICAL OBSERVATIONS, AND ON THE CIRCUMSTANCES WHICH FAVOUR THEM

It cannot be expected that astronomical discoveries of great importance will be achieved, except in establishments specially constructed with a solidity which will stand any test, and with the aid of delicate instruments. As Bailly says, we must for the observation of the stars, have a place which combines both the serenity of the heavens and the silence of retirement.

In 1561, William IV., Landgrave of Hesse, caused an observatory to be built at the Castle of Cassel, wherein he placed instruments which enabled him to construct the catalogue of 900 stars, for which science is indebted to him.

It was in the island of Huen, that the observatory of Uranibourg was erected, in which Tycho-Brahé, between the years 1580-1597, executed the countless and beautiful observations which have been employed in establishing modern astronomy.

The solicitations of Longomontanus induced Denmark, at a subsequent period, to establish the observatory of Copenhagen, which was commenced in 1632 and finished in 1656.

In 1641 Hevelius built at Dantzic, on his own house, an observatory which was the theatre of all his labours.

The observatory of Paris was constructed under the reign of Louis XIV. from a design of Claude Perrault, the celebrated architect of the colonnade of the Louvre. On the 20th and 21st of June, 1667, the original members of the French Academy of Sciences determined the direction of the principal walls by astronomical observations, but operations were not seriously commenced till the next year.

The edifice was completed in the year 1671; it cost two millions of francs. According to an original project, the observatory would have contained not only all the means of following the movements of the heavenly bodies, but models of the various machines which might appear susceptible of useful application, and even laboratories for the pursuit of chemistry. The Academy of Sciences was to have held its sittings there. This project having been modified during the course of the operations, the edifice was exclusively set apart for the cultivation of astronomy. Unfortunately, the architect, although imperfectly acquainted with the requirements of observers, consulted them rarely, or refused to follow their advice. In his opinion an observatory must necessarily be very lofty. He accordingly constructed an immense edifice, in which it was impossible to perceive all the regions of the heavens, except by occupying a position in the open air, upon the flat roof. Everywhere else the mass of the building offered a most annoying obstacle to observation.

Perrault supposed that modern astronomers could not dispense with a gnomon. He accordingly placed in the centre of his edifice, an immense apartment of great height, ranging from south to north. If its commencement was signalised by this primitive instrument, the Observatory of Paris has, at any rate, been furnished in succession with the most perfect appliances which astronomers and physicists have from time to time invented, and which the best artists have constructed.

The Observatory of Greenwich was built during the reign of Charles II. It was finished in the month of August, 1676, at which time Flamsteed established himself in it, to commence the numerous series of observations which form the basis of the "Historia Coelestis Britannica."

Several other observatories were soon afterwards erected. The Observatory of Leyden was built in the year 1690; the Observatory of Nuremberg in 1692; that of Bologna in 1709; the Observatory of Berlin in 1710; that of Altorf in 1713; the Observatory of Lisbon in 1722; the Observatory of St. Petersburg in 1725, and that of Utrecht in 1727.

In the nineteenth century all the Governments of Europe appear to have concurred in either improving the existing observatories or founding new ones. In Great Britain, besides the Observatory of Greenwich, rendered illustrious by Flamsteed, Halley, Bradley, Maskelyne, Pond, and Airy, there have been constructed the observatories of Edinburgh, Glasgow, Cambridge, Durham, Oxford, Dublin, Armagh, Markree, Liverpool, Aberdeen, Ashurst, Bedford, Birr Castle

Blenheim, Bushey Heath, Kensington, Makerstoun, Ormskirk, Portsmouth, Regent's Park, Richmond, South Kilworth, Starfield, and, finally, that of Slough, celebrated for the labours of Sir William Herschel.[1]

Denmark, besides the Observatory of Copenhagen, possesses at Altona a model observatory. Sweden has established excellent observatories at Stockholm, at Upsala, and Christiania.

Russia—not content with having founded very useful observatories at Dorpat, at Abo, at Helsingfors, at Kieu, at Mitau, at Kasan, at Moscow, at Wilna, at Warsaw, at Nicolaieoff, on the Black Sea—was desirous that St. Petersburg should possess a veritable astronomical monument, and has accordingly erected such an observatory on the eminence of Pulkowa.

Prussia, besides erecting a new observatory at Berlin, possesses similar establishments at Bonn; at Bilk[2], near Düsseldorf; at Breslau; and at Königsberg.

Bavaria may boast of the astronomical establishment founded at Munich. Hanover possesses one at Göttingen. The Grand Duchy of Baden has one at Manheim. The towns of Hamburgh and Bremen have erected observatories. Switzerland has similar establishments at Geneva, Berne, and Zurich. In Austria there are the observatories of Kremsmünster, Prague, Senftenburg[3], and Buda. In Belgium there is the Observatory of Brussels; in Spain, that of Cadiz.

In France, besides the Observatory of Paris, there are astronomical establishments at Marseilles and Toulouse.

In Italy there are the Observatories of Avulli, Bologna, Verona, Palermo, Capo di Monte, Florence, Milan, Padua, Turin, Parma, and Rome.

The New World, in its turn, has taken a deep interest in astronomical researches. The United States of America possess magnificant observatories at Cincinnati, Washington, Toronto[4], and Cambridge. At the Antilles there is an observatory in the Isle of St. Croix. In the State of Chili, in South America, there is the Observatory of Santiago.

In the English colonies, we may mention the excellent observatories of Malta, the Cape of Good Hope, St. Helena, Sidney, Madras, and Benares.[5] Finally, we must also mention the remarkable observatory constructed by the Rajah Trevandrum, near Cape Comorin; and in China, the Imperial Observatory of Pekin.

Taking all into account, there exist at least ninety observatories in the middle of the nineteenth century.

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[1]Of these only the observatories of Greenwich, Oxford, Cambridge, Edinburgh, Durham, Liverpool, Regent's Park, Birr Castle, and Markree, are at present in active operation.—(*Editor.*)

[2]The Observatory of Bilk is a private establishment.—(*Editor.*)

[3]This is a private observatory.—(*Editor.*)

[4]Toronto is not in the United States. It is the capital of Canada West. Moreover, the observatory is a meteorological, not an astronomical establishment. —(*Editor.*)

[5]The only observatories in this list at present in a state of activity are those of the Cape of Good Hope and Madras. An observatory has quite recently been erected at Sidney, at which operations are about to commence, or already have commenced.—(*Editor.*)

The public have always exhibited a strong desire to visit observatories. The operations executed at those establishments are not, however, of a nature to satisfy curiosity. It is only during the most profound tranquillity, removed from every kind of distraction, that astronomers are able to give to their observations all the precision which the progress of science now demands.

The most able of practical astronomers have frequent cause to be astonished that during a sky which, from its purity, would appear to be very favourable to the study of the physical constitution of the heavenly bodies, large telescopes do not work well. The circumstances which render telescopic images diffuse, ill-defined, and undulating, are not yet either completely known or defined with sufficient precision.

I shall here collect together various remarks which amateurs of astronomy will perhaps read with interest. They are chiefly to be found scattered throughout the numerous papers of Sir William Herschel.

No delicate operation—that is to say, no observation requiring a high magnifying power—will succeed, if we endeavour to execute it by looking through the window of an apartment, or through the aperture in the roof of an observatory.

It is right to avoid sheltered places, even when the telescope is placed in the open air.

If the wind blows, the telescopic images are not in general very distinct. The wind must occasion this injurious effect by mingling together the atmospheric strata of different temperatures.[1]

The aurora borealis sometimes vitiates astronomical observations. It appears to render objects undulating. Usually, however, it produces no effect.

If it be true, as Herschel supposes, with several meteorologists, that the aurora borealis is an indication (cause or effect) of great changes of temperature in the different regions of the atmosphere, its influence ought to be assimilated to that of the wind.

A celestial body never appears well defined when the rays by which we see it, pass a little above the roof of a building. In effect, above a roof there is always a movement of the air, occasioned by the mingling together of strata unequally heated.

When the atmosphere is very dry, telescopes do not act well.

When the atmosphere, on the other hand, is much loaded with moisture, the images of objects are remarkably well defined.

This clearness of definition also exists during a hazy state of the atmosphere, and especially during a fog. A fog leaves to images all the sharpness of their

[1]Herschel, perhaps, when he drew up this aphorism of Practical Astronomy under the influence of a special fact, was led to generalise too hastily. In effect, I find the following remarks in the "Philosophical Transactions" for 1815, page 322:—"The wind does not injure the definition of telescopic images. During violent winds the observer may employ considerable magnifying powers, provided the instrument is steady." I do not know if the great observer ever remarked to what extent he has contradicted himself in regard to the influence of the wind on astronomical observations.—(*Author.*)

contours until the instant when the obscuration is so great as to cause them totally to disappear.

Sometimes it happens that, during apparently very favourable weather, the stars appear ill defined. This circumstance may arise from the presence of a dry air which an east wind has introduced into the upper regions of the atmosphere, or from the mingling together of strata of different temperatures—the result of a conflict of winds in the upper regions blowing in different directions.

When a sudden frost succeeds mild weather, when a thaw all at once follows a long-continued frost, the stars appear ill-defined in telescopes. Nor can good results be expected when a telescope is hastily removed from a warm apartment into the open air.

To generalize, we may remark, that if the mirror of the instrument is not at the temperature of the air which surrounds it, the vision will be indistinct. In that case, it will not be attended with any advantage to employ high magnifying powers.

Few astronomers, either professional or amateur, have not at some time waited a bit impatiently for an instrument to be completed. Some of us, smarting under the shortages of the last great war, may have been misled into forgetting that earlier astronomers lived through wars and shortages too. For these reasons I found the following passage both amusing and comforting when I came across it in the Harvard Observatory copy of the delightful old periodical *Correspondance Astronomique* by Baron de Zach. In this periodical, vol. 3, September, 1819, in the division of the journal entitled Nouvelles et Annonces, p. 304, in an article by de Zach on the Observatoire Royal de Marlia, we translate:

This portrayal explains still another reason why it is so difficult to obtain some instruments that one needs from these artists. M. *Cassini*, for example. had ordered in 1787 a six-foot transit instrument, and a mural quadrant of eight feet; M. *Ramsden* promised the meridian telescope during the course of the year 1788; it arrived at the Paris Observatory in 1804; that is to say, SEVENTEEN years after having been ordered, SIXTEEN years after having been promised, FIFTEEN years after having been paid for, and FOUR years after the death of M. Ramsden.[1] This instrument had been finished by his chief workman M. Berge; from 1789 on it had been for the most part paid for. As for the mural quadrant M. Cassini has always believed that this great artist occupied himself with it solely in words and meditation.

Here is an admonition to all our kind readers, be they astronomers, be they amateurs, be they artists, let each one do his part; I count also in doing my share; for I hope by this to awaken some consciences, to calm some anxieties, and to avoid some inconveniences.

[1]M. Ramsden died at Lourdes the 5th of November 1800.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Charles Babbage and the Calculating Machine*

Probably no device has saved as much time for astronomers as has the computing machine. During the present decade, when many newspapers and magazines contain references to the amazing development o electronic computing machines, it is interesting to think back to the early development of this instrument.

The first beginnings of the calculating machine are represented by the abacus, developed many centuries ago, and still in efficient use principally in the countries of or near the Orient. In the early 17th century John Napier developed a set of numbering rods, popularly referred to as Napier's bones, for multiplying numbers. The invention of the first real calculating machine, in the accepted use of the term, an adding machine operated by turning wheels, and in which the result appeared in sight holes as in the modern machine, is attributed to Blaise Pascal in 1642. The first such machine to be produced on a commercial scale was developed by Charles Thomas of Alsace in 1820.

The importance of Charles Babbage is that in the early nineteenth century he conceived the notion of a calculating machine of a very different type from the existing ones, and one which is the basis for the elaborate computing machines of the present day. An English mathematician and mechanician of the highest skill, Babbage was born in Teignmouth in 1792, and died in 1871, the same year as his life-long friend Sir John Herschel. The volume of the *Monthly Notices* (vol. 32, page 101) which contains the obituary notices of both of these men states:

> Sir John Herschel and Mr. Babbage were the last survivors of the small band of scientific men who met together at the Freemasons' Tavern on January 12th, 1820, "to take into consideration the propriety and expediency of establishing a Society for the encouragement and promotion of astronomy," a meeting which resulted in the foundation of the Royal Astronomical Society.

*I am indebted to Dr. Harlow Shapley for suggesting that Babbage's book, *Passages from the Life of a Philosopher*, would be interesting material for this column.

Not merely was Babbage one of the founders of this society, but he was the first to receive its gold medal, in 1824.

Babbage conceived the idea of constructing a machine which would calculate and print mathematical tables such as logarithms. For this purpose he employed the principle of differences, and hence referred to his first machine as a difference engine. The story of his years of struggles and tribulations is told in his remarkable book, *Passages from the Life of a Philosopher*, 1864. As well as telling of Babbage's development of the difference engine, the book recounts many amusing incidents in his life with fine humour, but also reflects the bitterness which he felt when his difference engine failed to receive its due support and recognition. I am indebted to Dr. C. A. Chant for the use of his personal copy of this scarce book, which was presented to him by Mr. Richard Babbage of Montreal, a great grandson of the author.

We quote from Chapter V of this volume, which tells how the idea of the difference engine came to Babbage, and how he developed it.

CHAPTER V.

DIFFERENCE ENGINE No. 1.

CALCULATING MACHINES comprise various pieces of mechanism for assisting the human mind in executing the operations of arithmetic. Some few of these perform the whole operation without any mental attention when once the given numbers have been put into the machine.

Others require a moderate portion of mental attention: these latter are generally of much simpler construction than the former, and it may also be added, are less useful.

The simplest way of deciding to which of these two classes any calculating machine belongs is to ask its maker—Whether, when the numbers on which it is to operate are placed in the instrument, it is capable of arriving at its result by the mere motion of a spring, a descending weight, or any other constant force? If the answer be in the affirmative, the machine is really automatic; if otherwise, it is not self-acting.

Of the various machines I have had occasion to examine, many of those for Addition and Subtraction have been found to be automatic. Of machines for Multiplication and Division, which have fully come under my examination, I cannot at present recall one to my memory as absolutely fulfilling this condition.

The earliest idea that I can trace in my own mind of calculating arithmetical Tables by machinery arose in this manner:—

One evening I was sitting in the rooms of the Analytical Society, at Cambridge, my head leaning forward on the Table in a kind of dreamy mood, with a Table of logarithms lying open before me. Another member, coming into the room, and seeing me half asleep, called out, "Well, Babbage, what are you dreaming about?" to which I replied, "I am thinking that all these Tables (pointing to the logarithms) might be calculated by machinery."

I am indebted to my friend, the Rev. Dr. Robinson, the Master of the Temple, for this anecdote. The event must have happened either in 1812 or 1813.

About 1819 I was occupied with devising means for accurately dividing astronomical instruments, and had arrived at a plan which I thought was likely to succeed perfectly. I had also at that time been speculating about making machinery to compute arithmetical Tables.

One morning I called upon the late Dr. Wollaston, to consult him about my plan for dividing instruments. On talking over the matter, it turned out that my system was exactly that which had been described by the Duke de Chaulnes, in the Memoirs of the French Academy of Sciences, about fifty or sixty years before. I then mentioned my other idea of computing Tables by machinery, which Dr. Wollaston thought a more promising subject.

I considered that a machine to execute the mere isolated operations of arithmetic, would be comparatively of little value, unless it were very easily set to do its work, and unless it executed not only accurately, but with great rapidity, whatever it was required to do.

On the other hand, the method of differences supplied a general principle by which *all* Tables might be computed through limited intervals, by one uniform process. Again, the method of differences required the use of mechanism for Addition only. In order, however, to insure accuracy in the printed Tables, it was necessary that the machine which computed Tables should also set them up in type, or else supply a mould in which stereotype plates of those Tables could be cast.

I now began to sketch out arrangements for accomplishing the several partial processes which were required. The arithmetical part must consist of two distinct processes—the power of adding one digit to another, and also of carrying the tens to the next digit, if it should be necessary.

The first idea was, naturally, to add each digit successively. This, however, would occupy much time if the numbers added together consisted of many places of figures.

The next step was to add all the digits of the two numbers each to each at the same instant, but reserving a certain mechanical memorandum, wherever a carriage became due. These carriages were then to be executed successively.

Having made various drawings, I now began to make models of some portions of the machine, to see how they would act. Each number was to be expressed upon wheels placed upon an axis; there being one wheel for each figure in the number operated upon.

Having arrived at a certain point in my progress, it became necessary to have teeth of a peculiar form cut upon these wheels. As my own lathe was not fit for this job, I took the wheels to a wheel-cutter at Lambeth, to whom I carefully conveyed my instructions, leaving with him a drawing as his guide.

These wheels arrived late one night, and the next morning I began putting them in action with my other mechanism, when, to my utter astonishment, I found they were quite unfit for their task. I examined the shape of their teeth, compared them with those in the drawings, and found they agreed perfectly; yet they could not perform their intended work. I had been so certain of the truth of my previous reasoning, that I now began to be somewhat uneasy. I reflected that, if the reasoning about which I had been so certain should prove to have been really fallacious, I could then no longer trust the power of my own reason. I therefore went over with my wheels to the artist who had formed the teeth, in order that I might arrive at some explanation of this extraordinary contradiction.

On conferring with him, it turned out that, when he had understood fully the peculiar form of the teeth of wheels, he discovered that his wheel-cutting engine had not got amongst its divisions that precise number which I had required. He therefore had asked me whether another number, which his machine possessed, would not equally answer my object. I had inadvertently replied in the affirmative. He then made arrangements for the precise number of teeth I required; and the new wheels performed their expected duty perfectly. . . . . . .

The first Difference Engine with which I am acquainted comprised a few figures, and was made by myself, between 1820 and June 1822. It consisted of from six to eight figures. A much larger and more perfect engine was subsequently commenced in 1823 for the Government.

It was proposed that this latter Difference Engine should have six orders of differences, each consisting of about twenty places of figures, and also that it should print the Tables it computed.

The small portion of it which was placed in the International Exhibition of 1862 was put together nearly thirty years ago. It was accompanied by various parts intended to enable it to print the results it calculated, either as a single copy on paper—or by putting together moveable types—or by stereotype plates taken from moulds punched by the machine—or from copper plates impressed by it. The parts necessary for the execution of each of these processes were made, but these were not at that time attached to the calculating part of the machine.

A considerable number of the parts by which the printing was to be accomplished, as also several specimens of portions of tables punched on copper, and of stereotype moulds, were exhibited in a glass case adjacent to the Engine.

Before the difference engine had been finally completed, Babbage developed an idea for a much more complicated engine, an analytical engine, which was based on the principle of the Jacquard loom, which "is capable of weaving any design which the imagination of man may conceive." At this stage, the government had already contributed about 17,000 pounds towards the difference engine, and Babbage had spent about 20,000 pounds of his own personal fortune. In this connection we mention an amusing incident of a meeting of Babbage with an emissary from China. He was told that the Chinese were most anxious to know whether the machine would go into the pocket. Babbage told the emissary that he might safely assure his friends in the Celestial Empire that it was in every sense of the word an *out-of-pocket* machine.

One can hardly wonder that governmental authorities were loathe, before the difference engine was completed, to undertake to finance a much more complicated machine, one so complex that even Babbage could interpret his own drawings only with difficulty. Each operation of the analytical engine required two sets of perforated cards, one with the formula to be developed, and the other with conditions pertaining to it. Of the analytical engine, Babbage says (page 145):

> Finally, to whatever degree of simplicity I may at last have reduced the Analytical Engine, the course through which I arrived at it was the most entangled and perplexed which probably ever occupied the human mind.

Though Babbage's request for additional funds was strongly sponsored by leading scientists, the Chancellor of the Exchequer finally refused to grant more funds. There was in the government no real provision for proper consideration of such a scientific question. An example of the lack of understanding of Babbage's project by members of the House is given in the following incident (page 67):

> On two occasions I have been asked,—"Pray, Mr. Babbage, if you put into the machine wrong figures, will the right answers come out?" In one case a member of the Upper, and in the other a member of the Lower, House put this question. I am not able rightly to apprehend the kind of confusion of ideas that could provoke such a question.

Babbage's disappointment at the lack of governmental support is reflected when he says of the difference engine:

> It can not only calculate the millions the ex-Chancellor of the Exchequer squandered, but it can deal with the smallest quantities.

The following incident (page 193), is an excellent illustration of Babbage's keen sense of humour, recording a conversation he had with the poet Samuel Rogers:

Once at a large dinner party, Mr. Rogers was speaking of an inconvenience arising from the custom, then commencing, of having windows formed of one large sheet of plate-glass. He said that a short time ago he sat at dinner with his back to one of these single panes of plate-glass: it appeared to him that the window was wide open, and such was the force of imagination, that he actually caught cold.

It so happened that I was sitting just opposite to the poet. Hearing this remark, I immediately said, "Dear me, how odd it is, Mr. Rogers, that you and I should make such a very different use of the faculty of imagination. When I go to the house of a friend in the country, and unexpectedly remain for the night, having no night-cap, I should naturally catch cold. But by tying a bit of pack-thread tightly round my head, I go to sleep imagining that I have a night-cap on; consequently I catch no cold at all." This sally produced much amusement in all around, who supposed I had improvised it; but, odd as it may appear, it is a practice I have often resorted to. Mr. Rogers, who knew full well the respect and regard I had for him, saw at once that I was relating a simple fact, and joined cordially in the merriment it excited.

One of the greatest plagues of Babbage's life was the incessant playing of street music near his house. Although there was a law prohibiting such, it was not enforced and street music was a real problem to people who were annoyed by it. Babbage estimated that a quarter of his time was wasted by its interminable distraction. Actually he spent several dozen pounds a year in having the offended musicians arrested and brought into court, and used much of his energy trying to stamp out the nuisance. Possibly this resulted in an even greater concentration of street music around his residence.

Such are some of the aspects in the life of a really great man. His obituary notice (*op. cit.*) states:

It is impossible in a small compass to give an adequate estimate of such an intellect. Its salient features were universality, grasp, method, patience. What do not these four qualities, inspired by intense love of truth and never-flagging enthusiasm, and sustained by enormous power of work, imply?- -All careers were open to such a man. In any he must have succeeded.- -It is as a scientific mechanician that Babbage will hereafter, no doubt, be regarded. In this field he had no equal. The combination of the highest mathematical genius with a lofty and vivid imagination, and the power of descending to the most minute details of technical construction, has never been observed in the same degree, and well-balanced harmony of proportion, as in Babbage.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

A Manuscript of Wales's Observations in 1769

Through the great kindness of Dr. J. B. Tyrrell, the David Dunlap Observatory has just been presented with an original manuscript copy of an article on the first purely astronomical observations ever made in the Canadian arctic. This article first appeared in the *Philosophical Transactions of the Royal Society,* vol. LIX, 1769, entitled "Astronomical Observations by order of the Royal Society, at Prince of Wales' Fort on the North-west coast of Hudson Bay. By William Wales and Joseph Dymond."

Dr. J. B. Tyrrell is himself a far northern explorer of great note. Now in his ninety-second year, living amongst acres of his beautiful apple orchards near Toronto, he has an unequalled fund of knowledge of northern Canada. His journeys of exploration in the Canadian subarctic are now part of Canadian history. During the 1880's and 90's he made numerous trips of exploration in the Canadian far north and the Barren Lands, including a 600-mile trek on snowshoes from Churchill (formerly the Prince of Wales' Fort of the above manuscript), to the northern end of Lake Winnipeg. He is a fellow of the Royal Society of Canada, and the Tyrrell medal of that society was named in his honour.

Dr. Tyrrell bought this manuscript, inscribed in the hand-writing of William Wales, from a bookshop in London, England, in 1928, believing that an item of such note in Canadian history should repose in Canada. He has now presented it to the David Dunlap Observatory. The manuscript is in an excellent state of preservation. The records were beautifully written, and carefully and systematically entered.

Curiously enough, in November 1947, the writer reprinted in the Old Books column of this Journal, vol. 41, p. 323, certain portions of this manuscript. These were copied from Volume LIX of the *Philosophical Transactions* borrowed from McGill University. At that time the writer had no notion that the original copy of this interesting manuscript was located right in Toronto. Further excerpts of this interesting expedition to the Canadian arctic from Wales's Journal of a Voyage were published in this same column in Volume 42 of this Journal, May and July 1948.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Kirkwood's Gaps in Asteroid Orbits

From decade to decade, or century to century, the interest of astronomers swings around the heavens, concentrating for a while on certain objects, and then returning to them with the passage of years. So it has been with astronomical interest in the asteroids. During the first quarter of the twentieth century, when large numbers of uninteresting asteroids were found, astronomers were beginning to regard them largely as a nuisance—"vermin of the skies". The past decade or two however, has seen a considerable revival of interest in these objects for two reasons. First, the discovery of a number of remarkable asteroids such as Hermes and Icarus, which come well within the earth's orbit at perihelion, has shown us that there are sizable bodies in space which come closer to us than we had realized. And second, recent theories of the origin of the solar system from the original "solar nebula", and the formation of the asteroids by the collision of planet-sized bodies originally in the asteroid ring has turned speculative attention to these objects.

During the meeting of the American Astronomical Society this June, at the Kirkwood Observatory of the University of Indiana, a symposium was held on asteroids, honouring the fiftieth anniversary of the observatory and the work of Daniel Kirkwood (1814-1895), professor of mathematics at Indiana University some ninety years ago. His name is well known to most students of astronomy as the discoverer of "Kirkwood's gaps" in the asteroid ring.

Dr. Dirk Brouwer, Director of the Yale Observatory, who discussed Hirayama's families of asteroids at this symposium, commented that the discovery of the gaps in the asteroid ring by Kirkwood was quite parallel to the discovery of the solar apex by Sir William Herschel nearly a century earlier. In both cases, an astronomer with a brilliant hunch derived a most important result from a handful of observations. Herschel had so few stars, and Kirkwood so few asteroids, with which to work, that a statistician would not have put much faith in the result. And yet when, in the following decades, the available data was multiplied many fold, the results that each of these men had obtained were most beautifully confirmed. Kirkwood based his first findings on only 87 asteroids—those whose orbits were then known. Nowadays the results from around 1500 asteroids confirm his discovery that in certain portions of the asteroid ring, at certain distances from the sun, there is a marked lack of asteroid material. These gaps are at distances where the periods of the asteroids are commensurable with the period of the great planet Jupiter, which would pull them out of their course when he came near them.

The first statement Kirkwood made of the asteroid gaps, apparently, and also of the gaps in the rings of Saturn, was before the Buffalo meeting of the American Association for the Advancement of Science, in August 1866. This particular meeting of the Association was "originally appointed to have been held at Nashville, Tenn." in 1861. - - - "In the mean time, the great rebellion breaking out, the meeting was not of course, called together, as that place was not either a fit or a safe one for loyal members to visit." (*Proceedings of the American Association for the Advancement of Science,* vol. 15, 1867). Kirkwood's comments on the asteroid gaps, with 87 known orbits, are published in this volume very briefly. These were expanded some months later in the statement of his hypothesis in *Monthly Notices,* vol. 29, page 96, 1868. Shapley and Howarth in their "Source Book of Astronomy" have already published a later paper by Kirkwood on this subject from the Annual Report of the Smithsonian Institution for 1876.

We reprint excerpts from the earlier paper in *Monthly Notices,* not including his lengthy table listing 97 individual asteroids with their distances, but publishing his conclusions from this tabulation. For the asteroids he mentions by name, we have inserted parenthetically their distances as taken from his table. The asteroid distances are all discussed in terms of the astronomical unit, distance sun to earth equals 1.

On the Nebular Hypothesis, and the Approximate Commensurability of the Planetary Periods. By Daniel Kirkwood, LL.D., Professor of Mathematics, Indiana University.

The views of Laplace in regard to the development of our planetary system have been confirmed in a remarkable manner by the recent progress of astronomical discovery. We have now (1868) 100 minor planets between *Mars* and *Jupiter,* of which 96 have been detected since 1845. The mean distance of *Flora,* the innermost member of the group, is 2.20; that of *Sylvia,* the most remote, 3.49. The breadth of the zone is, therefore, greater than the distance of the

Earth from the Sun; greater even than the entire interval between the orbits of *Mercury* and *Mars*. Moreover, the *perihelion* distance of *Sylvia* exceeds the *aphelion* distance of *Harmonia* by a quantity equal to the interval between the orbits of *Mars* and the Earth. - - - -

Why did not the Asteroid-zone form a single planet, and the rings of Saturn, one or more satellites?—In regard to the ring between *Mars* and *Jupiter* perhaps no satisfactory answer can yet be given. We may remark, however, (1) that being situated just within the orbit of *Jupiter*, the perturbations were greater than in any other part of the system; and (2), that, as the total mass of the asteroids is very small, the matter of the primitive annulus was extremely rare, so that the intersection of orbits, resulting from the perturbations, would be less likely to produce large planetary nuclei. - - -

As these parts of the solar system, viz. the rings of *Saturn* and the minor planets between *Mars* and *Jupiter*, appear to furnish strong arguments in favour of the nebular hypothesis, their phenomena may perhaps be further suggestive as to the *mode* of planetary formation.

The mean distance of a planet having—

A period equal to	1/2	that of Jupiter is	3.2776
"	4/9	"	3.0299
"	3/7	"	2.9574
"	2/5	"	2.8245
"	1/3	"	2.5012
"	2/7	"	2.2569

These distances all fall between the greatest and least mean distances of the asteroids. Now, do we find wider intervals in these portions of the ring than elsewhere? and if so, what is their physical cause? For the purpose of comparison we have, in the following table, arranged the minor planets in the order of their mean distances from the Sun. - - - - -

The mean between the distances of *Flora* [2.2014] and *Sylvia* [3.4927] is 2.8470; and as small bodies in the remoter part of the ring are more difficult of detection, the zone will be considered under two divisions. The inner section contains 72 of the 97 asteroids whose elements are known; the mean interval between them being 0.0081. The greatest gap in the order of distances occurs between *Ariadne* [2.2034] and *Feronia* [2.2654]. It is 0.0620, or nearly eight times the mean. This includes the distance at which seven periods of an asteroid would be equal to two of *Jupiter*. The chasm next in order of breadth is between *Thetis* [2.4737] and *Hestia* [2.5178]. This is 0.0441, or more than five times the mean. In the outer section the mean interval is 0.0286. The greatest hiatus is between *Undina* [3.1917] and *Freia* [3.3877]; the breadth being 0.1960, or more than eight times the mean. This includes the distance at which two periods of a planet would be equal to one of *Jupiter*. The second is between *Bellona* [2.7784] and *Terpsichore* [2.8563] (0.0779), and the third between *Leucothea* [3.0060] and *Aegle* [3.0549] (0.0489). The widest chasms in the zone of minor planets are thus found to include those distances at which the periodic times would be commensurable with that of *Jupiter*. These coincidences are not accidental. The *primitive* ring undoubtedly contained nebulous matter at these as well as the intervening distances; and the present existence of such intervals may be accounted for as follows:—

A planetary particle at the distance 2.5—in the interval between *Thetis* and *Hestia*—would make precisely three revolutions while *Jupiter* completes one; coming always into conjunction with that planet in the same parts of its path. Consequently its orbit would become more and more eccentric until the particle would unite with others, either interior or exterior, thus forming the nucleus of an asteroid. Even should the disturbed body not come in contact with other matter, the action of *Jupiter* would ultimately change its mean distance, and thus destroy the commensurability of the periodic times. In either case the primitive orbit of the particle would be left destitute of matter. The same reasoning is, of course, applicable to other intervals.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Constellations

All astronomical readers know that some of the constellations date back to very early—even prehistoric—times. However there are in the sky, particularly in the southern hemisphere, much more recent constellations. Who introduced these, and when? Which are the early constellations and which the later?

A very concise answer to this problem may be found in G. F. Chambers' "A Handbook of Descriptive Astronomy", where all the constellations are listed under their sources. A study of this list will probably clear up many points of confusion. We see at once that the best known constellations date back to Ptolemy's catalogue of 137 A.D.; presumably these were taken from the earlier star catalogue of Hipparchus. Not until the end of the sixteenth century were any other constellations added to the sky. Tycho Brahe introduced the well known Coma Berenices about the same time that Bayer, who must have been a naturalist, scattered birds and reptiles over the southern sky. Of nine constellations named for birds, Bayer is responsible for five. All the constellations Bayer added to the list are accepted to-day.

Only two constellations added by Royer in 1679, Columba and Crux, still remain. We recognize the importance of his Nubes Major and Minor, but do not regard them as separate constellations. Charles's Oak and Charles's Heart added about the same time by Halley and Flamsteed have now disappeared.

Hevelius a few years later had better success in selecting constellations, and nine of our present ones are due to him. A half century later La Caille introduced in the southern sky a list of thirteen new constellations, all of which are still accepted, though in some cases with slightly altered name. La Caille seems to have been as much interested in machinery and geometrical equipment as does Bayer in birds. We find that he was responsible for placing such curious things in the heavens as an airpump and a chemical furnace—Fornax, in which a decade ago was discovered the remarkable cluster. He also introduced the most appropriate constellation of Telescopium.

The name of Messier was once placed among the constellations. Though it does not survive there, it is permanently attached to a hundred of the most important nebulae and clusters in the sky. Even though Bode was a great astronomer, by the end of the eighteenth century the sky had been so thoroughly divided into constellations that none of the groups to which he assigned names has survived—not even his tribute to the great William Herschel in Telescopium Herschelii.

Since the time of La Caille the only new constellations to be accepted have resulted from the division of the great Argo Navis into its three constituent parts by Sir John Herschel. At present, by international agreement, 88 constellations are recognized. Our readers may find a concise alphabetical list of these, with abbreviations, on page five of the Observer's Handbook. We reprint here the lists of constellations given by Chambers, op cit., 3rd edition, 1874, pp. 554-562. To the original list of Chambers I have added numbers in order to show which constellations are still accepted. In some cases I have inserted the present name in brackets, if it is different from the old. Under Ptolemy's old constellation of Argo Navis which sprawled for seventy-five degrees across the southern sky I have added the three modern sub-divisions, Carina, Puppis, and Vela.

The International Astronomical Union has now settled the boundaries of constellations, presumably for all time, so that our readers may feel some encouragement to know that any constellations they may learn now will never be changed.

THE CONSTELLATIONS

Ptolemy enumerates 48 constellations: 21 northern, 12 zodiacal, and 15 southern, as follows:—

Northern

1. Ursa Minor.	The Little Bear.
2. Ursa Major.	The Great Bear.
3. Draco.	The Dragon.
4. Cepheus.	
5. Boötes, or *Arctophylax*.	The Bear Keeper.
6. Corona Borealis.	The Northern Crown.
7. Hercules, *Engonasin*.	Hercules kneeling.
8. Lyra.	The Harp.
9. Cygnus, *Gallina*.	The Swan.

10. Cassiopeia.	The Lady in her Chair.
11. Perseus.	
12. Auriga.	The Charioteer.
13. Serpentarius, [Ophiuchus].	The Serpent Bearer.
14. Serpens.	The Serpent.
15. Sagitta.	The Arrow.
16. Aquila, *Vultur volans.*	The Eagle.
17. Delphinus.	The Dolphin.
18. Equuleus.	The Little Horse.
19. Pegasus, *Equus.*	The Winged Horse.
20. Andromeda.	The Chained Lady.
21. Triangulum.	The Triangle.

Zodiacal

22. Aries.	The Ram.
23. Taurus.	The Bull.
24. Gemini.	The Twins.
25. Cancer.	The Crab.
26. Leo.	The Lion.
27. Virgo.	The Virgin.
28. Libra, *Cheloe.*	The Balance. *The claws* [of Scorpio].
29. Scorpio.	The Scorpion.
30. Sagittarius.	The Archer.
31. Capricornus.	The Goat.
32. Aquarius.	The Water Bearer.
33. Pisces.	The Fishes.

Southern

34. Cetus.	The Whale.
35. Orion.	
36. Eridanus, *Fluvius.*	Eridanus, The River.
37. Lepus.	The Hare.
38. Canis Major.	The Great Dog.
39. Canis Minor.	The Little Dog.
Argo Navis.	The Ship "Argo".
40. [Carina].	[The Keel].
41. [Puppis].	[The Poop].
42. [Vela].	[The Sails].
43. Hydra.	The Snake.
44. Crater.	The Cup.
45. Corvus.	The Crow.
46. Centaurus.	The Centaur.
47. Lupus.	The Wolf.
48. Ara.	The Altar.
49. Corona Australis.	The Southern Crown.
50. Piscis Australis.	The Southern Fish.

Tycho Brahe (d. 1601) added—

51. Coma Berenices.	The Hair of Berenice.
Antinoüs.	

(Both Northern Constellations.)

Bayer (d. 1603) added—

52. Pavo.	The Peacock.
53. Toucan.	The American Goose.
54. Grus.	The Crane.
55. Phoenix.	The Phoenix.
56. Dorado, *Xiphias*.	The Sword Fish.
57. Piscis Volans, [Volans].	The Flying Fish.
58. Hydrus.	The Water Snake.
59. Chamaeleon.	The Chameleon.
60. Apis, [Musca].	The Bee, [The Fly].
61. Avis Indica, [Apus].	The Bird of Paradise.
62. Triangulum Australe.	The Southern Triangle.
63. Indus.	The Indian.

(All Southern.)

Royer, in 1679, added—

64. Columba Noachi.	The Dove of Noah.
65. Crux Australis.	The Southern Cross.
Nubes Major.	The Great Cloud.
Nubes Minor.	The Little Cloud.
Fleur-de-lys.	The Lily.

(All Southern Constellations.)

Halley, about the same period, added—

Robur Caroli.	Charles's Oak.

(A Southern Constellation.)

Flamsteed's maps also contain—

Mons Maenalus.	The Mountain Maenalus.
Cor Caroli.	Charles's Heart.

(Both Northern Constellations.)

Hevelius, in 1690, added—

66. Camelopardus.	The Cameleopard.
67. Canes Venatici, *Asterion et Chara*.	The Hunting Dogs.
68. Vulpecula et Anser.	The Fox and the Goose.
69. Lacerta.	The Lizard.
70. Leo Minor.	The Little Lion.
71. Lynx.	The Lynx.
72. Clypeus, or Scutum, Sobieskii.	The Shield of Sobieski.
Triangulum Minor.	The Little Triangle.
Cerberus.	

(All Northern: and)

73. Monoceros.	The Unicorn.
74. Sextans Uraniae.	The Sextant of Urania.

(Southern Constellations.)

La Caille, in 1752, added—

75. Apparatus Sculptoris, [Sculptor].	The Apparatus of the Sculptor.
76. Fornax Chemica.	The Chemical Furnace.
77. Horologium.	The Clock.
78. Recticulus Rhomboidalis.	The Rhomboidal Net.
79. Caela Sculptoris, [Caelum].	The Sculptor's Tools.
80. Equuleus Pictoris, [Pictor].	The Painter's Easel.
81. Pixis Nautica, [Pyxis].	The Mariner's Compass.
82. Antlia Pneumatica.	The Air Pump.
83. Octans.	The Octant.
84. Circinus.	The Compasses.
85. Norma, or Quadra Euclidis.	Euclid's Square.
86. Telescopium.	The Telescope.
87. Microscopium.	The Microscope.
88. Mons Mensae, [Mensa].	The Table Mountain.

(All Southern Constellations.)

Le Monnier, in 1776, added—

Tarandus.	The Rein Deer.
Solitarius.	The Solitaire.

(The former in the Northern, the latter in the Southern hemisphere.)

In the same year Lalande placed Messier's name in the heavens, by forming a constellation in his honour, near Tarandus.

Poczobut, in 1777, added—

Taurus Poniatowskii.	The Bull of Poniatowski.

(Between Aquila and Serpentarius.)

Hell formed in Eridanus—

Psalterium Georgianum.	George's Lute.

And, finally, in Bode's maps we meet with—

Honores Frederici.	The Honours of Frederick.
Sceptrum Brandenburgicum.	The Sceptre of Brandenburg.
Telescopium Herschelii.	Herschel's Telescope.
Globus Aërostaticus.	The Balloon.
Quadrans Muralis.	The Mural Quadrant.
Lochium Funis.	The Log Line.
Machina Electrica.	The Electrical Machine.
Officina Typographica.	The Printing Press.
Felis.	The Cat.

Making in all 109 constellations. This number by no means exhausts the list of those which have been proposed by different persons. A writer in the *English Cyclopaedia* very pertinently remarks: "In fact, half-a-century ago, no astronomer seemed comfortable in his position till he had ornamented some little cluster of stars of his own picking with a name of his own making."

Sir J. Herschel said: "The constellations seem to have been almost purposely named and delineated to cause as much confusion and inconvenience as possible. Innumerable snakes twine through long and contorted areas of the heavens, where no memory can follow them; bears, lions, and fishes, small and large, northern and southern, confuse all nomenclature," &c.

Many of the above smaller constellations are very properly rejected by modern uranographers. — — —

According to Argelander, the number of stars visible to the naked eye at Berlin is 3256. The number, of course, increases as we approach the equator, owing to the wider expanse of heavens opened up by the diurnal movement.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Blue Sun

Inhabitants of southern Ontario were startled on Sunday, September 25, 1950 at a brief glimpse of the sun as a pale, bluish-mauve disc. At the same time the western sky became a dark, terrifying mass of cloud and haze, as though a gigantic storm were approaching, while to the north and east the sky toward the horizon was a clear steel-blue. The darkness was so marked at 3:30 in the afternoon that the writer observed a group of six wild ducks going to sleep quietly in the middle of a pond, with their heads nodding or tucked under their wings.

On the following day, Monday, September 26, the celestial phenomena were remarkable in a heavy haze or dry fog which permeated the whole sky. In the afternoon for several hours, at least in the region around Toronto, the blue-mauve sun could be seen in the heavens, casting no shadow and shining without rays.

We learned on Sunday even as the eerie darkness descended, that all this was attributable to smoke from heavy muskeg fires in distant Alberta, two thousand miles away. In subsequent days the smoke pall moved eastward, causing similar phenomena to be seen in eastern Canada and the United States, and in the British Isles several days later.

It should be noted that this remarkable apparition of a blue sun was missed by many people. Since the light was so dim, a casual observer would think that the sun was completely obscured. Only if one's glance was directed to the sky was one rewarded with a sight which few of us had ever previously seen. Many people for the first time became acquainted with the fact that the old expression "once in a blue moon" had a basis in scientific fact, designating an event which is likely to occur only once in a lifetime.

Unfortunately the haze and clouds were so heavy that the eclipse of the moon was clouded out in the Toronto region except for occasional glimpses, and no blue moon was observed here.

Now that we have had the good fortune to experience a blue sun, it is interesting to look back at other records of coloured appearances of the sun or moon. Reference to a blue moon has already been made in this column, (this JOURNAL, vol. XL, p. 164, 1946) in connection with the great explosion of Krakatoa in 1883, which gave the world the greatest spectacle of coloured suns and moons that has ever been recorded. These reports were gathered together and summarized in the splendid volume "The Eruption of Krakatoa, and Subsequent Phenomena," the Report of the Krakatoa Committee of the Royal Society, London, 1888. A whole section of this volume, by Mr. E. Douglas Archibald, is devoted to "The Blue, Green, and Otherwise Coloured Appearances of the Sun and Moon in 1883-84."

The violent eruption of Krakatoa occurred on August 26-27, 1883. The weird and beautiful sky phenomena resulting from the cloud of dust and ash hurled into the atmosphere lasted for many months over most of the earth. The coloured suns and moons, however, were confined to the tropical regions, and were remarkable in that the appearances of the blue sun travelled around the earth from east to west in thirteen days, making two complete circuits of the world before the dust had dissipated in the atmosphere. The sun was not constant in its colouring. Though it was usually described as green or blue in the tropical regions, metallic descriptions were also applied to it, as coppery, silvery, or leaden. In the above-mentioned section we read that

> It appears that the "blue" sun was chiefly seen at great distances from Java. - - - The "green" sun was visible at first only in the Indian Ocean, but afterwards more generally than either of the other colours, and finally the "silvery sun", when at a high altitude, appears to have been almost entirely confined to a narrow zone near the Equator, and more especially on its southern side. If to this we add the cases in which the sun appeared coppery, dim, and sensibly obscured, we find that they were all close to the Equator in each hemisphere.

At any one place the colour of the sun was not necessarily constant during the day, but varied markedly with altitude. A fine description of this phenomenon is given by a Government officer who was travelling in Ceylon from Mannâr to Trincomalee, September 12th, 1883.

> The sun for the last three days rises in a splendid green when visible, *i.e.,* about 10° above the horizon. As he advances he assumes a beautiful blue, and as he comes further on looks a brilliant blue, resembling burning sulphur. When about 45° it is not possible to look at him with the naked eye; but even when at the zenith the light is blue, varying from a pale blue to a light blue later on, somewhat similar to moonlight, even at midday. Then, as he declines,

the sun assumes the same changes, but *vice versa.* The moon, now visible in the afternoons, looks also tinged with blue after sunset, and as she declines assumes a very fiery colour 30° from the zenith.

In the same volume the Hon. Rollo Russell traces the geographical course of the coloured suns, as well as of the peculiar sky glows, and shows how the coloured sun phenomena actually circled the earth twice, going from east to west with velocity of more than seventy miles an hour. The first records of a green sun came from Batavia and Ceylon on the 27th of August and must have been caused by a heavy current of thick dust, moving northward from the eruption.

The mass of the ejecta was carried with great velocity westwards and south-westwards at a very high level. - - - The green sun of the 27th at Ceylon and Labuan lasted only a short time, and was not generally observed in any large area. In fact, the absence of green or blue suns in the Indian Ocean before September 8th deserves particular notice. - - - On September 1st the blue sun band extended across the Atlantic from east to west, reaching even to Guayaquil at 80° W. in the latitude of 2° S.; and between 12°.7 S. 27°.3 W. and 10° 40′ N. 26° 30′ N. in the mid-Atlantic. - - -

On September 2nd the whole northern part of South America, between the Antilles and Peru, and between Panama and Paramaribo, seems to have had a blue sun; - - - South of the Equator there is little mention of a blue sun, but much of a persistent grey haze in the upper air. - - - On September 5th the zone of blue or green sun in the Pacific reached as far north as 21° 30′ N. for a short time, and southward at least as far as 13° 17′ S., and in breadth was wider than hitherto. - - - From September 9th to 12th a green or blue sun was visible over a great part of India.

There can be no doubt, from a comparison of the data given in the general list, that the blue sun and yellow haze passed round the world from east to west in a gradually widening zone, and that the matter concerned in producing them was unequally distributed in clouds or streams of different density. The wonderful red twilights were seen only where the haze was much thinner, either before the dense main cloud of matter covered the sky, or after it had passed, or at its edges on the northern and southern borders. - - -

The denser part of the main cloud of matter in the first circuit, causing a blue, green, or silvery sun, and a yellow or white haze, covered a much narrower area than the after-glows. In the Indian Ocean it seems to have extended from Diego Garcia to the Seychelles on August 28th—that is, between 20° S. and 5° S., and probably some degrees further north; in the Atlantic, between 10° 40′ N. and 14° S., and in the Pacific between 10° 19′ N. and 13° 17′ S. - - -

On September 9th when the main cloud of matter had reached India after a circuit of the earth, its breadth appears to have been greater in the northern than in the southern hemisphere. A green sun was visible over Southern and Central India on September 10th. - - - On September 22nd the green sun had returned to Southern India after a second circuit of the globe, and lasted two or three days as before. After this no well-attested observation of a green or blue sun, except at Duem, 14° N. 32° 30′ E. on September 24th, when it rose green, and at 9° S. 35° W. on September 28th, when it set pale blue, occurs in any contemporary record. - - -

On the whole, the tendency of the matter causing the twilight phenomena was to spread northwards and southwards as well as westwards during the rapid circuit of the blue sun matter from east to west within the tropics. - - - We find the northern limit near the end of the first circuit to have been about 22° N. at Honolulu, or 28° north of Krakatoa, and the southern limit about 33° S. at Santiago, or 27° south of Krakatoa. - - - At the end of the second circuit, about September 22nd, the glows may be roughly stated to have extended from between 20° and 30° N., to between 30° and 40° S., but their distribution was not regular within these limits.

This volume also tells of other instances of coloured suns more local in nature. Mr. Archibald remarks that he witnessed a blue sun produced experimentally by Professor Kiessling of Hamburg from a cloud of chloride of ammonium, and from aqueous vapour mixed with ordinary dusty air. He lists several appearances of a blue sun due to dust storms in the Sahara and in central Asia, and at a plant in Eastbourne, England where fine dust rising from stone-crushing operations gives the sun a blue tinge. We might also note that persons living on this continent in the midst of prairie dust storms have seen the sun as a clear pale blue.

Records of blue suns seen over widespread parts of the globe, however, are quite rare. The table near the end of the Krakatoa volume lists all known volcanic explosions since 1500, and unusual atmospheric phenomena. In general there is a close correlation between the two. Records of red and coppery suns and brilliantly coloured twilights are relatively common, but widespread reports of the blue, green, or rayless sun are few in number. Twice before the explosion of Krakatoa in 1883 are there numerous records of a rayless sun, both times after gigantic volcanic explosions. The first was in 1783, the year of two great eruptions, one of Asama, Japan and the other of Skaptar Jökull, Iceland. Again in 1831 was the sun seen as rayless, or blue or violet. There were three major eruptions in this year, as well as several smaller ones, those of Graham's Island, Babujan Islands, and Pichincha being exceedingly noteworthy. In this year, on August 4, at Canajoharie, New York

The sun at 5 p.m. was dim and violet. At Albany, from August 12 to 31, the western sky was deep red after sunset. One afternoon the sun was pale, like the moon, and slightly green. - - -

The extraordinary dry fog of 1831 was observed in the four quarters of the world. It was remarked on the coast of Africa on August 3, at Odessa on August 9, in the south of France and at Paris on August 10, in the United States on August 15, etc. The light of the sun was so much diminished that it was possible to observe its disc all day with the unprotected eye. On the coast of Africa the sun became visible only after passing an altitude of 15° or 20°. M. Rozet, in Algeria, and others in Annapolis, U.S., and in the south of France, saw the solar disc of an azure, greenish, or emerald colour. The sky was never dark at night, and at midnight, even in August, small print could be read in Siberia, at Berlin, Genoa, etc. On August 3, at Berlin, the sun must have been 19° below the horizon when small print was legible at midnight.

Because of the recent blue sun, the unexplained appearance of a blue sun in 1821 takes on added interest. The authors of the Krakatoa volume could find no volcanic explosion to account for it, and apparently did not look for any other cause.

On August 18 a blue sun, seen in London, Sussex, Worcester, etc. There was a haze and the sun looked like quicksilver. At 9.20 on August 18 the blue sun was observed by a great number of persons in the streets. It lasted about half an hour. The atmosphere on the following days was hazy. At Paris, on August 18, the sun at 5 p.m. was enfeebled by dense vapours, and absolutely white.

I suggest that this unexplained blue sun may have been caused by a heavy forest fire on this continent, just as the blue sun of 1950 was so caused. The middle of August is in the forest fire season. I have hunted some early Canadian records for the year 1821 with the hope of finding a reference to such a fire. None has yet been found, but it is possible that one of our readers might come across one. In those early days, however, the burning of some one's shed was thought more worthy of record than the burning of a vast forest.

However, on the microfilm copy of the *Kingston Chronicle* for August 17, 1821, in the University of Toronto library, we read that

the weather during the last three weeks has been uncommonly dry as well as oppressively hot. The ground is parched, and the vegetables in the garden droop and wither from the want of moisture.

Over a certain section of Canada, then, conditions were certainly ripe for a fire which might have caused the blue sun of 1821.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Le Gentil and the Transits of Venus, 1761 and 1769

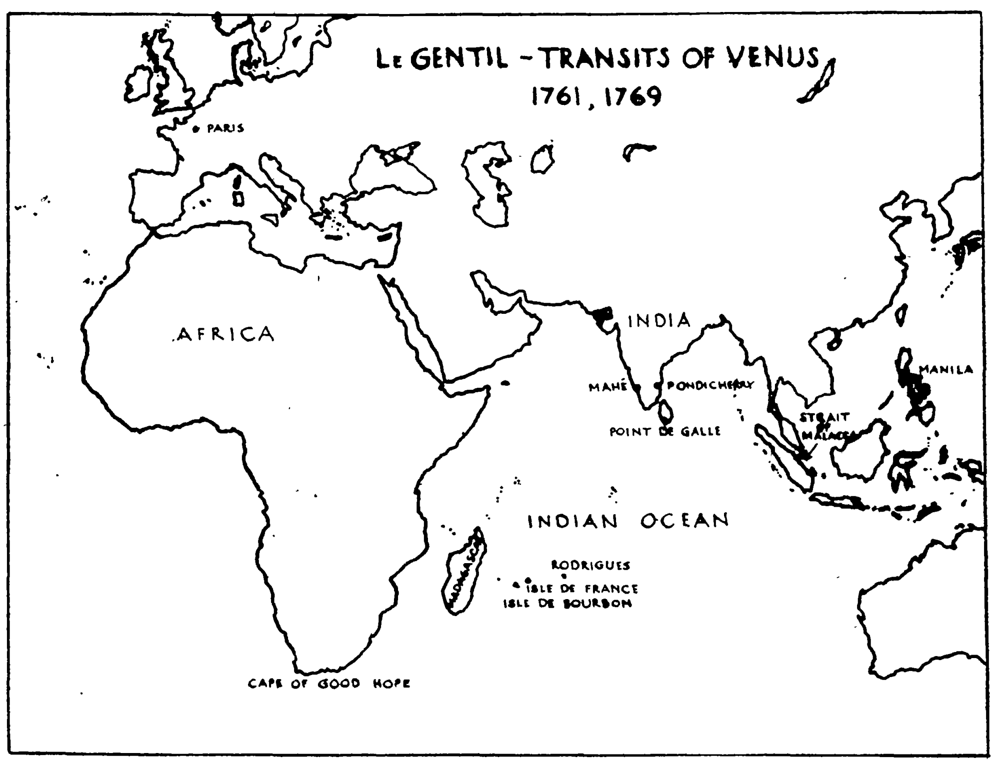
The eleven-year voyage of the French astronomer Le Gentil to the Indian Ocean to observe the transits of Venus in 1761 and 1769 is probably the longest lasting astronomical expedition in history. In fact, it is quite possible that, except for interplanetary travel, there will never be astronomical expeditions to equal in duration and severity those made for that particular pair of transits.

At that time the most important astronomical problem was considered to be the determination of the distance from the earth to the sun by observations of the transit of Venus. The astronomer Halley pointed out this fact in 1716, and urged that astronomers should be sent to distant parts of the world for these transits, which would occur after his death. Observations were needed over a wide range of latitude and longitude. We know now that there are more reliable means of determining the distance of the earth from the sun, so that transits of Venus are unlikely to assume again the importance they held at that time.

The expeditions for this pair of transits, however, constitute one of the most picturesque aspects of historical astronomy. We have already published in this column the experiences of William Wales who made, in 1768, the first astronomical expedition to the Canadian Arctic (this Journal, Nov. 1947, May–August 1948). In the first of these articles we referred to the heart-breaking experience of Le Gentil who, for all his lengthy voyages, failed to achieve any useful observations of either transit. There is little information in the English language about his remarkable journeys. The brief remarks made in the volumes "Transits of Venus", by R. A. Proctor, 1882, and Sir David Gill's Introduction to his wife's volume "Six Months in Ascension", 1878, contain serious misstatements about Le Gentil's trip.

Le Gentil himself published the account of his amazing voyage in two volumes totalling over seven hundred pages, in 1782, entitled "Voyage dans les mers de l'Inde fait par ordre du roi, à l'occasion du passage de Vénus sur le disque du soleil le 6 juin 1761, & le 3 du même mois 1769". By means of a loan of these rare volumes from Harvard University I have been able to follow Le Gentil's wanderings and studies in the region of the Indian Ocean. Le Gentil's death occurred during the upheaval of the French revolution, and his eulogy was published some years later by J. D. Cassini IV in 1810. I am indebted to the Library of Congress for the loan of this volume entitled "Mémoires pour servir à l'histoire des sciences et à celle de l'observatoire royal de Paris, suivis de la vie de J. D. Cassini, écrite par lui-même, et des Éloges de plusieurs académiciens morts pendant la révolution". A study of these two works, besides giving the background of Le Gentil's expedition, also clears up false impressions created by the above-mentioned English references. He did not, as they state, stay eight years in Pondicherry studying the astronomy of the Brahmins. He was in Pondicherry only two years, and Indian astronomy was only one of his many pursuits there.

By means of excerpts which Miss Sally Hogg and I have translated from these two works, we can follow briefly the life of Le Gentil, and more fully some of the picturesque aspects of his eleven years of wanderings around the Indian Ocean. On the accompanying map, the places which Le Gentil visited are indicated. We translate the account of his early years as given by Cassini.



EULOGY OF M. LE GENTIL

Guillaume-Joseph-Hyacinthe-Jean-Baptiste Le Gentil was born at Coutances, Sept. 12, 1725. His father, a not very well-to-do gentleman of Normandy, nevertheless knew how to make sacrifices to procure a good education for him. . . .

After having made his first studies at Coutances, the young Le Gentil left his province and came to Paris. Not knowing at first to what career he wanted to commit himself, he began by studying theology and took the ecclesiastical garb. . . . M. Le Gentil kept the apparel of abbé only until the title of "savant" procured him a less equivocal esteem and existence.

The abbé Le Gentil, in pursuing his course of theology, had the curiosity to come sometimes to the Royal College to hear the celebrated Professor Delisle. The lessons of astronomy soon detracted from those of theology. The young man found it much more agreeable to spend the evenings in observing the heavens than to spend a part of the day on the benches of the school disputing vain arguments. He continued his new course, and distinguished himself to his illustrious professor, whose goodwill he knew how to merit. One of his friends having proposed to take him to the Observatory and to present him to MM. Cassini, he eagerly seized the opportunity to form a liaison so profitable to his budding taste for the science of the stars.

Jacques Cassini, then 71 years old and dean of the astronomers of the Academy, received him with that grace, that patriarchal good will which touched and won so easily the heart of a young man. The old man regarded as his children all those who wished to devote themselves to astronomy. Informed of the inclination of the young Le Gentil, he proposed to him to come to work at the Observatory under the direction of Cassini de Thury, his son, and of Maraldi his nephew, already members of the Academy of Sciences. . . .

In a few years the new astronomer became familiar with the use of the instruments, the most delicate observations, and the most difficult calculations. His zeal and his acquired knowledge opened to him the doors of the Academy of Sciences; he was received into it in 1753, and soon justified his nomination by a great number of Memoirs on different points of astronomy that he treated with a great deal of sagacity. Some years after, in 1760, there occurred a brilliant occasion to show a great zeal and a beautiful devotion for the sciences; M. Le Gentil did not let it escape him.

The epoch was approaching for this first transit of Venus on the sun, awaited for such a long time, which should finally decide a great question on the parallax and on the distance of the planets. This determination of one of the most important points of the system of the world had occupied for centuries astronomers who were not at all agreed amongst themselves. . . . Everybody appeared therefore to be interested in the preparation for the voyages which were to be carried out by the savants of all nations, to go to different points of the globe to observe the passage of Venus on the disk of the sun, which was to take place June 6, 1761. Everyone was most thankful to the courageous men who were consigning themselves to these distant journeys; and everyone formed for their success wishes as ardent as those which formerly accompanied the departure of the expedition of the Argonauts. . . . M. Le Gentil solicited and obtained the honor of being in the number of voyageurs proposed by the Academy and named by the Government. The abbé Chappe was destined for Siberia; the abbé Pingré, for the Isle of Rodrigues; Mason, for the Cape of Good Hope, and Le Gentil, for Pondicherry.

In order to get to Pondicherry, a French possession on the south-east coast of India near the tip, Le Gentil had to take the long route around the Cape of Good Hope to the Isle de France, now called Mauritius, where he would hope to contact a vessel bound for India. He had a choice of two vessels of the East India company ready to sail for the Isle de France, and chose the Berryer, of fifty cannon. The Duke de la Vrilliere had given the East India company very definite orders for Le Gentil's passage to India. He embarked on the vessel the 26th of March, 1760, and arrived at the Isle de France the 10th of July. He makes little comment on his voyage to this island, but we are led to infer that his first sea trip was not completely pleasant when he remarks, apropos of a trip some years later, "Sea voyages no longer cost me anything, I had become so familiar with this element."

At the Isle de France he learned that war had flared up in India between the French and the British, and that he would have considerable difficulty getting there. Too, the season was approaching when the north-east monsoon forced the vessels to sail by a long and tedious route. There was no vessel bound for India from the Isle de France. As Le Gentil waited there during the summer and fall he became afflicted with a type of dysentery which led him to fear that even if a ship sailed, he might be unable to go. For some time he toyed with the idea of going to the island of Rodrigues a hundred leagues to the east, where M. Pingré was to observe the transit.

Just as he was making preparation to go to this island, a frigate arrived from France on February 19, 1761, bearing news of the utmost importance for India. This determined the governor and the commander-in-chief of the naval forces to despatch a frigate to India immediately. Everyone assured Le Gentil that even in the contrary season a frigate like the Sylphide would make the passage from the Isle de France to the coast of Coromandel in two months. Le Gentil left the Isle de France on this frigate the 11th of March, and left the Isle de Bourbon, now Reunion, the 23rd of the same month. As long as they were in the control of winds from the south-east they made thirty to forty-five leagues a day, but when they entered the region of the monsoons, at latitude 7 degrees south, the favourable wind abandoned them and the north-east monsoon blew them directly opposite to the route they wished.

In this way we wandered around for five weeks in the seas of Africa, along the coast of Ajan, in the Arabian seas. We crossed the archipelago of Socotra, at the entrance to the gulf of Arabia. We appeared before Mahé, on the coast of Malabar, the 24th of May; we learned from the ships of this country that this place was in the possession of the English, and that Pondicherry no longer existed for us. Without stopping further, we set sail. I would not yet have despaired if we had followed our first object to go to the coast of Coromandel; but they made, to my great regret, the resolution to return to the Isle de France. Nevertheless we did hesitate about twenty-four hours at Point de Galle, island of Ceylon; the Dutch there confirmed the news that we had learned at Mahé.

The winter at Galle was in full force; we went through a hard time. We left this coast the 30th of May; the great breeze with which we were accompanied took us to the Isle de France the 23rd of June.

I shall not enter here into greater discussion about this expedition, of the route that I had thought that we would take, and that which we did take. . . . This memoir which is only an extract from my journal which I kept quite regularly day by day shows that I busied myself as I ought with my observations, that my aim was always to go to the coast of Coromandel and that I should not be blamed if I did not appear there; it is a justice which I beg astronomers to do me and which I will have reason to expect from them when they see the details of my memoir. On June 6 I was at 5 degrees 45 minutes of south latitude, and almost 87 degrees 15 minutes of longitude east of Paris. I observed as best I could the transit of Venus, its beginning and end. This observation which I neither published nor calculated has remained as it was made with remarks in a sealed memoir of which I spoke above.

One can imagine the feeling of complete frustration that Le Gentil must have had, to have sailed thousands of miles and spent over a year only to be turned back from land when he was but a few hundred miles from his goal. Although he saw this first transit, his observations, made from a moving ship with uncertainty in the position and the time, could be of no value.

Though he failed to get useful observations of the transit, he was determined to bring back scientific knowledge of the places he was visiting. He was particularly interested in accurate mapping and determination of latitude and longitude, and states

It is known that geography owes its actual perfection to the progress which astronomy has made during the last fifty years, and for that reason astronomers must be regarded as the true geographers. . . .

Before going back to Europe I should have liked to go to visit the archipelago which is north of the Isle de France and to determine its position; I wanted to do the same thing along the east coast of Madagascar, an island which we frequent a lot and of which we know very little. This work which required several years' stay in these seas compensated me to some extent and made me wait for the transit of Venus in 1769, the sole and last transit that the present generation could hope to see. I resolved then not to leave the Indian Ocean until this time, to make all the observations that I could on geography, natural history, physics, astronomy, navigation, winds, and tides. I was unable to visit the archipelago to the north of the Isle de France; but I made several voyages to Madagascar. I began with Fort Dauphin where from the time of M. de Flacourt we had had an establishment whose remains I still saw in 1761. I was singularly struck, to say so in passing, with the beauty of this island and the fertility which it appeared to have in comparison with the Isle de France.

The provisions are excellent at Fort Dauphin: fish are very abundant and very good, poultry the same; the beef offends only because it is too rich; it causes many people a very dangerous sickness of which M. de Flacourt speaks in his history of Madagascar: he tells at the same time of the means to avoid it. It was with this sickness that I was attacked three or four days after my return to the Isle de France because I had not used the methods set forth by M. de Flacourt. This sickness was a sort of violent stroke, of which several very copious blood-lettings made immediately on my arm and my foot, and emetic administered twelve hours afterwards, rid me quite quickly. But there remained for seven or eight days in my optic nerve a singular impression from this sickness; it was to see two objects in the place of one, beside each other; this illusion disappeared little by little as I regained my strength. . . .

For the next few years Le Gentil stayed at the Isle de France, from which he made numerous excursions to Madagascar and the Isle de Bourbon. He made a map of the east coast of Madagascar which he states to be much better for navigation than any existing map; he studied the customs and garb of the people there, as well as much natural history. He carried on many observations of refraction, of the winds and the monsoons.

These different occupations had led me up to the year 1765. It was time then to think of the second transit of Venus. . . .

After having calculated this transit for India, for Manila, for the Mariana Isles, Mexico, and finally for Europe, I saw that evidently the Mariana Isles and Manila were the sole spots east of Paris the most advantageously placed; not that one could observe at Manila or at the Mariana Islands a greater difference in parallax than on the coast of Coromandel, but because the elevation of the sun above the horizon at the moment of the egress of Venus was to be very great, and would give by this means more hope of succeeding than one could hope for on the coast of Coromandel, where the sun was to be quite low at the moment of the egress of Venus. I knew that the coast of Malabar was then plunged into the most frightful winter; I thought that for this reason it was useless to think of Malabar. I therefore did not waver in making up my mind and in searching for means of going to Manila. The execution of this plan was not difficult by taking the route to China; because the ships of the India company which passed by the Isle de France to go to China would have taken me as far as there; and from Canton to Manila one finds opportunities every year. All that would still have made a little trouble however, from which I was rescued by the most fortunate event; it was the warship the Bon Conseil from his Catholic Majesty, with sixty-four cannon, which was going from Cadiz to Manila and which various circumstances forced to put into port at the Isle de France.

I soon became acquainted with the captain Don Juan de Caseins, through Don Juan de Langara, one of the first officers of the ship. I had seen M. de Langara in Paris. When he knew my plan, he very obligingly offered me passage on his ship. This opportunity appeared so good to me that I did not hesitate to accept it. . . .

I finally left the Isle de France May 1, 1766, quite resolved to say good-bye forever to that island; and indeed I had conceived the plan of going back to Europe by way of Acapulco, and thereby finish my trip around the world; but I had not foreseen what was to happen to me at Manila, and that a last adventure had destined me for the Isle de France. We arrived at Manila August 10th. Our voyage was rather long; it had its difficulties and its wearinesses.

Don Juan de Caseins introduced me to the governor of Manila in the letter in which he informed him of our arrival. August 13th, four days afterwards, I went down with M. de Caseins and I went to see the governor to whom I gave a letter of recommendation which had been given to me by M. Desforges, governor of the Isle de France.

We were anchored at Cavité, the port of Manila, about three leagues from that city. We found in this port a little vessel with three masts ready to sail for the Mariana Islands. Although the voyage which I had just made had quite tired me and I needed a little rest, I forgot when I saw this ship, all the fatigues which I had just undergone, and I wanted only to go aboard in order to pursue my course to the Marianas. . . . M. de Caseins, to whom I had promised before his departure from Manila to determine the longitude of the city, was the reason that I took no serious steps to embark. He had assured me that I should not lack opportunities to go to these islands by embarking on one of these galleys, and that he would recommend me for that to the governor. Therefore I abandoned for the moment the project of the Marianas. It was a great piece of good fortune for me; for the ship and everything in it perished when it was going out of the Strait of the Philippines to enter the south seas. It is true that only three or four people were drowned, those who were the most eager to save themselves, which is what almost always happens in shipwrecks. I cannot answer that I would not have increased the number of persons eager to save themselves; but I would have lost all my journals and my astronomical instruments, an irreparable loss for me.

After we had acquitted ourselves of a vow which Don Juan de Caseins had made when approaching the Philippines, in bad weather, with which we were troubled for seven days, my first care was to determine the longitude and latitude of Manila. . . .

I became acquainted with Don Estevan Roxas y Melo, and with Don Andres Roxo. The elder Melo was a native of Lima, and canon of the cathedral church of Manila; he was a very inquisitive man, learned, having studied, keen on books and mathematical instruments. The Peruvians have excellent qualities of heart and are very good friends. Don Estevan Melo did me the greatest services during my stay in Manila. Don Andres Roxo was Mexican; he had come to Manila with his uncle Don Manuel Antonio Roxo, Archbishop of Manila; he was his secretary. . . . I divided my time between the house of Don Andres Roxo and the house of Don Estevan Roxas y Melo. Our intimacy has lasted even until now. I have received regularly every year since my departure from Manila news of these two friends; the last told me that death had carried off one of the two, Don Estevan Roxas y Melo.

Manila is without contradiction one of the most beautiful countries in the seas of Asia; the climate is excellent there, the soil is of the greatest fertility. The Philippines have fifteen or sixteen fine ports, and they are covered with the finest woods for building. At Cavité I saw some of these woods which were of a huge size, and a table in the sacristy of the Jesuits in Manila which was eleven feet in diameter and proportionately thick; it was made of a single piece drawn from the trunk of one of these trees. This table is at present in Spain; M. de Caseins took it to the king. . . .

The wood called *tindalo* is one of these fine kinds of wood; it is of a dark red, ferruginous, solid, and very heavy; it is capable of being very finely polished which makes it shine like a mirror; this wood does not warp nor spoil. Don Estevan y Melo had made from this wood expres ly for me a table with a rim and folding stool with which he presented me two weeks before my departure from Manila. This table was twenty-two inches broad and twenty-eight long and made of a single board, and it gave me so much pleasure through the beauty of its wood that I regretted using it on ship. I wrapped it up as best I could, and I brought it with me to Paris where it arrived in a very good state, and where I keep it as a curious piece of furniture and as a monument which always recalls to me with pleasure the memory of so true a friend as Don Estevan Roxas y Melo.

(To be continued)

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OUT OF OLD BOOKS

By Helen Sawyer Hogg

Le Gentil and the Transits of Venus, 1761 and 1769

(cont'd from January-February issue)

Le Gentil had arrived at Manila August 10, 1766, all ready to begin his preparations for the transit of 1769. After some months there, however, doubt began to assail him that this was the best place for his observation. The circumstance which finally decided him to leave the Philippines and go to Pondicherry to observe the transit was the attitude of the Spanish governor. The governor was not kindly disposed to the French, and actually was such a tyrannical character that two years after Le Gentil's departure he was arrested, and died in prison.

Before leaving the Isle de France May 1, 1766, Le Gentil had written to France for letters of recommendation from the Court of Spain to the governor of the Philippines, since in these islands he would be venturing upon foreign soil and possible hostility. When on July 10, 1767, Le Gentil received replies from France to his letter and showed them to the governor, the latter said that a year and two months was too short a time for a reply to be received from France, and that therefore the letters must be forgeries!

July 10, 1767, I received an answer to the letters which I had written to France the preceding year, 1766, before my departure from the Isle de France; these new letters came through Mexico and Acapulco in the galleon the Saint Charles. I was told of letters of recommendation from the Court of Spain. M. le duc de Chaulnes, with the consent of M. le duc de la Vrilliere, took all necessary steps so that the matter would not languish. My letter found the illustrious M. Clairaut dead; M. de la Lande did not neglect taking an interest in this new enterprise. It was not possible to expect more speed by sea; there was not a single moment lost; hence the governor of Manila, to whom I showed them, a restless man, of evil intentions to the French in general, regarded me in Manila with a jealous eye, collected the times of all my letters and of my departure from the Isle de France, those of the departure of M. de Caseins from Manila. He made a combination of all these times and he concluded that I could not yet have received any answer to my letters, and that those which I had shown him were necessarily forgeries. . . .

This odious and injurious suspicion caused me a good deal of sorrow, and gave me also some worry for the rest of the time which I had still to remain in Manila.

. . . M. de la Lande pointed out to me at the end of his letter that M. Pingré had read at the Academy a memoir in which he complained that I was going too far: he would have wanted me to return to Pondicherry; to the others it was all the same, according to M. de la Lande, whether I remained at Manila or whether I returned to Pondicherry and he told me that I could decide as I wished. I had a great deal of time before me to make up my mind again and to go to the coast of Coromandel.

When I had reflected well and calculated the inconveniences of the two coasts I decided to leave for Pondicherry. The climate of Manila was the only thing which swayed me a little. . . . I considered, I said, that I was running very great risks by staying at Manila; that I was exposing myself not only to the risks of cloudy weather on the day of my observation, but also to the caprice of him who governed. I saw that the governor acted despotically and tyrannically in everything; I saw that, in this distant country, reasons would not be lacking to arrest a man in the course of the most serious and important affairs; I had very striking examples of this before my eyes which would be too long to relate here. . . .

I decided to go in search of a free country since I could choose to do so. Sea voyages no longer cost me anything, I had become so familiar with this element. I wrote to M. le duc de Chaulnes and to M. de la Lande that I should be at Pondicherry in time for the transit of Venus; . . .

When these preparations were finished I used the opportunity of a Portuguese ship from Macao: this vessel had come from Madras and it was returning there. We sailed February 2, 1768, at six o'clock in the evening by a little breeze from the south-east; but our vessel, badly loaded, could not go two leagues without furling the sails; it even went down quite considerably although having only its four lower sails; and although the wind was quite good we were obliged to put about and to go back and anchor at Manila.

Three days later this vessel again got under way, and Le Gentil had a good journey on it, reaching Malacca February 18th and Pondicherry itself after only thirty-two days' sailing. In a long letter to his friend Don Estevan y Melo, Le Gentil writes more fully about the actual happenings aboard ship than he does in other parts of the volume, and we reprint two of these incidents which give a vivid idea of sailing ships in those days.

We therefore sailed as you know February the 5th on the Saint Antoine, and I was on it by the grace of God so to speak, on a ship rather badly ballasted, and which had neither surgeon nor chaplain. We were at first led by a little breeze from the east, by which we passed the isle of Corregidor during the night. . . . Here I shall praise to you the vessel on which I was. For a ship built at Surate by Marates or Malabar Indians, that is to say, by people who do not understand the construction of the Europeans (they do not even bother themselves with it at all), it had very good qualities. It was a fine flute of about 500 tons, very well and very solidly constructed, but without having the grace which our new builders give to ours.

We arrived at la Viole at seven-thirty in the evening; it was night, the weather was not very clear, and the moon in its course did not lend us its light: we were between two equally redoubtable dangers: one is la Viole, the other a sand bank, on which there are several trees and which is not farther than a league and a half from la Viole. It was necessary to pass between the two, and it was night. I shall confess here to you that I was a little worried; not precisely because of the spot in which we were, because the passage which some sailors consider perilous is not so to every sailor; but in connection with a little dispute which arose between the captain and the first pilot.

To recount the affair to you exactly, you will suppose in the first place that in the ships of Macao the captains understand navigation not at all or very little; the first pilot alone is in charge of guiding the vessel, the captain must not meddle in it in any manner, he is only a figure-head in this regard; everything else depends on him. Unhappily for us our captain and our first pilot did not agree very well; it was this which was the cause of the little story which you are going to hear.

We were scarcely through la Viole when the captain cried from above the poop where he was, to the first pilot to make a manoeuver (I do not remember what) of which the latter did not approve; the pilot answered rather brusquely that he would not do it, alleging that he was in charge of the conduct of the ship, that he knew what he had to do, and how it was necessary to navigate. The captain insisted, wanting, he said, to be obeyed. The pilot kept answering him in the same tone: finally the dispute became heated; the latter went into a passion and went and locked himself in his cabin, abandoning his ship to the pleasure of the wind.

I was on the poop taking the air with the captain when this scene took place. As our two men had spoken very quickly as is the custom in all disputes, and as I was not familiar enough with Portuguese, since moreover I was far from the pilot who was on the castle, I had lost a good deal of what was said; and I did not know the decision which our pilot had made. The captain did not stir from his place: so that I did not know the danger which we could run. I went down on the castle a moment after the scene; I looked for the pilot, I did not find him; I went to his cabin, he was in it but he was sulking, and nothing that I could say to him was capable of making him take up the helm again.

I went to find the Armenians and especially the supercargo, as they had a personal interest in the ship because they had freight and because it was returning loaded with piastres for their reckonings, I did not doubt that they would bring my man back to reason.

However, the ship kept on advancing. I took over here for the first time the office of pilot: while the Armenians flew to the cabin where he was and while they were entreating him, I went to the tiller to see if the steersman were not going away from his course.

The Armenians had frightful difficulty in getting the pilot out of his cabin; but when the Seigneur Melchisedek, with that great phlegmatic air which you know in him, had spoken to him about conscience, he finally succeeded in conquering his obstinacy. "*Hombre*", he said to him, (from what he told me a moment afterwards), "*tiene usted conscientia?*" "Are you a man who has any conscience?" He submitted at this word conscience, coming from the mouth of an Armenian, and he resumed the conduct of the vessel. This pilot was a brusque and gross man, he became moody easily; otherwise it appeared to me that he understood this sort of voyage very well.

Near the coast of Malay and Pol Pinang the following incident occurred:

Our two first pilots and a passenger had gone to land in the ship's boat, incited by curiosity to see the island. They tried to get me to go to land with them; but my policy is never to quit my ship unless it is in a port or in a sure roads; although it was very fine weather when they descended, they could not persuade me to decide to go with them. How thankful I was for my resistance when I saw the bad weather and when I perceived all the horror of the condition to which the travellers were reduced! They had astonishing trouble in getting back on board ship: twice they arrived to take the route back from the island; it was darkest night, and there was a very heavy rain; they were led only by the waves of the sea which appeared all on fire. Finally the desire to sleep on board ship rather than pass the night on land without any shelter made them make a vigorous effort a third time. They finally gained shipboard at eight o'clock in the evening; we heard them a long time before seeing them; they shouted to us to throw them a mooring rope. Their repeated cries in the middle of the night, the bad weather, the noise of the sea, the efforts which they were making to come alongside, all that represented for me the picture of shipwrecked people. . . .

You see by this tale what Portuguese vessels are. I have never heard of a European vessel anchored in an open roadstead, in which there was so little discipline that the two first pilots could thus abandon their ship for a pleasure jaunt. Only the captain remained, and he was as little in condition to conduct his vessel as I am to lead an army, and for pilots, two old automatons to whom I would not have entrusted the conduct of a launch.

In spite of these somewhat harrowing experiences, Le Gentil considered his trip an excellent one, and concludes his account of the trip by saying:

It is not possible to have a more fortunate voyage than that. You will see by recapitulating all the dates reported in this letter that we took only thirty-two days sailing to go from Manila to the coast of Nagpatnam although we left Manila late: this passage is one of the finest which has been seen for a long time. As to the manner in which I was treated aboard the ship, the Saint Antoine, I cannot complain of it since the captain treated me as himself.

(*To be continued*)

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Le Gentil and the Transits of Venus, 1761 and 1769

(With Plate V)

(*Continued from March-April* Journal)

The brief accounts of Le Gentil's trip in nineteenth century literature agree in a misstatement, namely that Le Gentil stayed eight years at Pondicherry. This error has been perpetuated in the works of Camille Flammarion, Richard Proctor and Sir David Gill, and we can only hope that by reprinting here the direct translation of Le Gentil's own statements we may correct this false notion permanently. It was not until March 27, 1768, that Le Gentil arrived at Pondicherry for the first time, after seven years spent at the Isle de France and on trips to the surrounding islands and to the Philippines, from which he sailed to Pondicherry.

On the 27th, at five-thirty in the morning, we sighted Pondicherry: we crowded sail toward it and at six-thirty we anchored a half league from land: . . .

When we were anchored, a little boat was sent to us from land, in which I embarked with all my possessions and astronomical instruments of which you know. It was therefore on March 27th [1768] at nine o'clock in the morning that I saw myself on the land which fate had marked for me. My first step was to present myself to the governor. . . . M. Law therefore gave at once the order to disembark my possessions and to be careful of them; he had me get into his open carriage with him and took me to his country house where I found a large and pleasant company, good music, and an excellent dinner. I spent the day in enjoying myself and at eleven o'clock in the evening I returned to Pondicherry with the governor. The next day he told me to go to look for a site to build an observatory for myself: he himself went with the chief engineer to reconnoitre the spot which I had pointed out, and ordered that masons be sent there at once.

Such is, dear friends, the fate which awaited me at Pondicherry and which I owe to M. Law, governor general for the King of all the French establishments in India. Under his auspices I enjoyed at Pondicherry that sweet peace which is the support of the muses; I occupied myself in the midst of this peace in devoting happy moments to Uranus; with my soul content and satisfied I await with tranquillity until the approaching ecliptic conjunction of Venus with the sun comes to terminate my academic courses.

In Volume II Le Gentil gives a description of his observatory. Plate V is a reproduction of the original drawing of the observatory (HI).

As we read his description we wonder if he is the only astronomer who ever pursued his labours tranquilly above sixty thousand weight of powder!

On the ruins of the citadel were seen the remains of the magnificent palace built by the late M. Dupleix; there were two great pavilions partly upset or destroyed, which had been built on strong walls twelve to fifteen feet high and on a vault six to seven feet thick. All this excellent masonry of brick, of lime-stone, and of sand, had resisted the effects of the gunpowder and was quite whole and quite solid; but the pavilions were partly fallen. I went to visit the remains of these pavilions; I looked at the most easterly, that which appeared to me in best condition, the most suitable to my plan, and which at the same time demanded the least in the way of expense to make a comfortable observatory of it; besides, under the vault which sustained the remains of the other pavilions was one of the powder magazines; it is true however, that the basement of my observatory served also, in the end, for over six weeks as a magazine for more than sixty thousand weight of powder. In spite of that, since M. Law had given me the liberty of inhabiting my observatory, this circumstance did not interrupt the course of my observation.

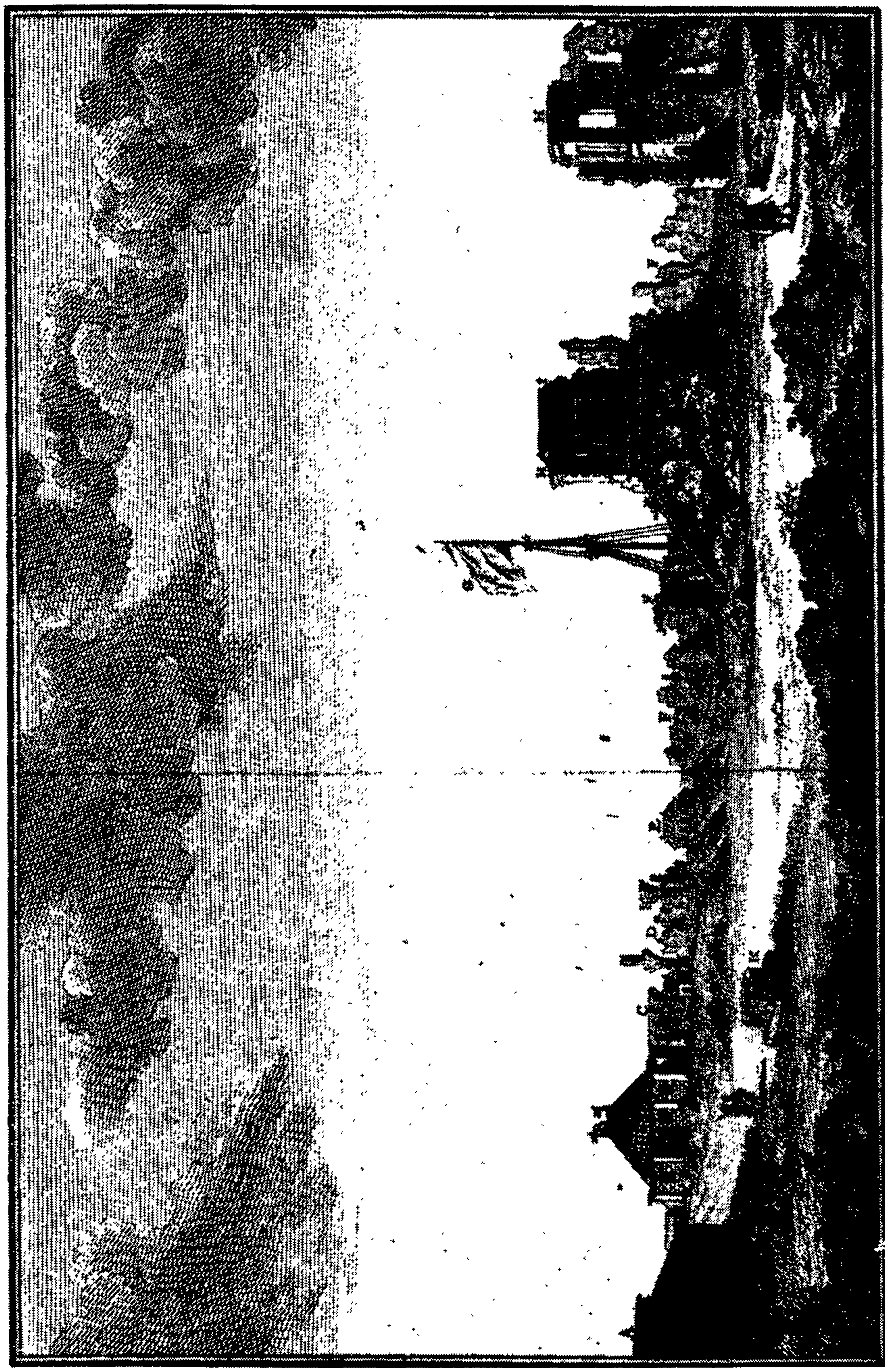
I gave an account of my examination to M. Law: he took the trouble of going to that place some days afterward accompanied by the chief engineer; he gave the necessary orders, and they began on April 18th to build the observatory on the plan I had asked for. But the great rains held up the work for several days: it was not finished until May 24th. June 11th the doors and windows were finished and put in place, and I went to take possession of the observatory; I had my instruments and my belongings transported there; it was my dwelling and my retreat during my stay at Pondicherry; I was more in touch with my work there.

As soon as I was in possession of a proper place for observing I worked at cleaning my quadrant and my clocks; and July 14th I was in a position to take the corresponding heights of the sun. My first care was to fix in a more precise manner than had ever been done previously the latitude and longitude of Pondicherry.

Another type of work appeared very interesting to me: I had begun it at the Isle de France, I finished it at Pondicherry in 1768; it is an account of the monsoons, and an examination of the different routes which are to be followed when going from the Isle de France to India.

During his two years at Pondicherry Le Gentil made a study of various aspects of Indian astronomy, and recounts in detail the native myths concerning eclipses and the behaviour of the people during them. The Brahmin method of calculating eclipses particularly intrigued him.

PLATE V



Le Gentil's Observatory is the building (HI) at the right of the flagpole.

I amused myself also during my stay at Pondicherry in making some acquaintance with the astronomy, the religion, the habits, and the customs of the Indian Tamoults whom very improperly we call Malabars.

What I had heard of their astronomy had piqued my curiosity; but what finally sharpened it was the ease with which I saw calculated before me, by one of these Indians, an eclipse of the moon which I proposed to him, the first which occurred to me. This eclipse, with all the preliminary elements, took him only three quarters of an hour of work. I asked him to put me in a position to do likewise, and to give me every day an hour of his time. He consented to it; and when I asked him in how much time I could hope to be in a position to calculate an eclipse of the moon according to his method, he answered me, with an air rather indicative of conceit, that with ability I would be able to do as much as he at the end of six weeks.

This answer did not rebuff me, it only made me still more curious. I bound myself to take for about an hour every day my lesson in Indian astronomy. Whether it was the fault of my master or whether it was mine, or whether it was that of the interpreters (I changed them three times), I needed more than a month of work at an hour per day to be able to calculate an eclipse of the moon, although the method has appeared to me since then very simple and very easy. The eclipses of the sun gave me much more trouble, because the calculation is much more complicated.

As to the exactness of this method, the agreement with observation has appeared to me quite singular in lunar eclipses; the error in several which I have calculated amounts to no more than 25 minutes of time.

. . . The Brahmins know nothing about comets: the Indians believe that they are a sort of sign of the wrath of the sky. They were quite astonished to see me spend part of the nights in observing the comet which appeared in 1769; they asked me many questions about the cause of this phenomenon. What finally surprised them was to see that comet again in the evening at the end of the months of October and in the first days of November, just as I had predicted to them as well as to all Pondicherry.

Although the Brahmins do not observe, they are able to trace the meridian line by the gnomon method; they use it every time that they build a pagoda, because their religion teaches that temples are to be oriented according to the four cardinal points; so that the four faces of the pyramids which serve as entrance and as portal to their pagodas, are exactly north and south, east and west.

Le Gentil finally received, a month before the transit, the letter from the King of Spain which he had requested for the governor of the Philippines. We reprint this letter just as it is given, as an impressive example of the high regard in which astronomers and their enterprises were held in court circles two centuries ago.

In the month of May 1769 I received from the Court of Spain the letter of recommendation which had been promised to me in 1767 for the Governor of Manila. This letter had gone around the world except for about five hours or 75 degrees of longitude. It had indeed left Cadiz, had gone to Mexico, from Mexico to Acapulco, from Acapulco to Manila by galleon, from Manila to Canton, from Canton to Pondicherry. . . . Here is the letter addressed to the governor.

"De par le roi. Au gouverneur & capitaine générale des isles Philippines, & président de l'audience royale de la ville de Manille: Le roi se trouve informé que sur le vaisseau de guerre *le Bon Conseil,* qui est sorti de Cadix en droiture pour ces isles (Philippines) en mars de l'année passée, s'est embarqué M. Gentil, membre de l'académie royale des sciences de Paris, à l'effet d'observer le passage de Vénus sur le disque de soleil, phénomène très-rare & qui importe beaucoup à la perfection de l'astronomie, & qu'il désire rester dans ce continent jusqu'en juin 1769, que doit se répéter le même passage de Vénus. Sa Majesté veut que ce particulier soit traité par V.S. & tous les autres officiers de ces isles, avec la distinction qu'il mérite, *& qui convient à l'harmonie qui règne entre les deux couronnes,* & que pour les observations qu'il voudra faire, on lui facilite tous les secours nécessaires pendant le tems qu'il restera dans ces isles. A cette fin, V.S. fera tout ce qui sera convenable; & de son côté, elle aura grande attention que la volonté de S.M. s'accomplisse.

Que Dieu garde V.S. beaucoup d'années! A Saint Ildephonse, le 2 août 1766.
Signé, Don Julien de Arriaga."

Le Gentil gives a lengthy discription of the sky conditions at Pondicherry, especially of the time around the transit of June 3rd. The egress of this transit was all that was scheduled to be seen from Pondicherry, early on the morning of the 4th. Those astronomers, professional or amateur, who have spent months of work in preparation for some sky event, always with worry as to the arrival of clouds at the critical moment, can appreciate the description Le Gentil gives of the critical time of June 3-4.

The nights at Pondicherry are of the greatest beauty in January and in February; you cannot have any idea of the beautiful sky which these nights offer until you have seen them. I had nowhere seen Jupiter so well with my telescope of fifteen feet [focal length] as I did there; the stars had no twinkling: I have very often left my telescope exposed to the night air for several hours in a vertical position without the objective receiving the slightest dampness. The month of March is not so fine, in April the weather begins to grow dull; but June, July, August and September are not very suitable to astronomical observations; in these months you have scarcely anything except fine mornings. In October, November, and December you have the rainy season and the winter.

I was prepared for the transit of Venus on June 3, 1769. The English at Madras had sent me an excellent achromatic telescope three feet long and I was awaiting the moment of the observation with the greatest impatience.

During the whole month of May, until the third of June, the mornings were very beautiful; the weather was still of this same fineness the day before. At nine o'clock in the evening I observed with M. Law who was using the achromatic telescope, the emersion of the first satellite of Jupiter which we saw very well. . . .

Sunday the fourth, having awakened at two o'clock in the morning, I heard the sand-bar moaning in the south-east; which made me believe that the breeze was still from this direction, or at least that it would blow from there in the morning. I regarded this as a good omen, because I knew that the wind from the south-east is the broom of the coast and that it always brings serenity; but curiosity having led me to get up a moment afterwards, I saw with the greatest astonishment that the sky was covered everywhere, especially in the north and north-east, where it was brightening; besides there was a profound calm. From that moment on I felt doomed, I threw myself on my bed, without being able to close my eyes. I no longer heard the bar in the south-east, but in the north-east; it was another very bad omen for me. Indeed, when I got up a second time I saw the same weather still, the north-east was even more overcast.

At five o'clock the wind blew ever so little from the south-west: which gave me again a gleam of hope, all the more because the part of the sky from the south to the east was a little clear; I believed therefore that the breeze might turn in this direction, and that it might clear the sky. However, the north and the north-east were continually threatening; the clouds did not move, and I still heard the bar in the north-east, so that I was between hope and fear. But this state of uncertainty did not last for very long: little by little the winds passed to the west, to the north-west, and to the north; in less than seven or eight minutes the weather was obstructed, as it were, by the approach of a gust of wind; from the north the winds passed to north north-east, and north-east, where they were at about five thirty. Then they blew with fury; the great clouds which until then had been motionless in the north-east began to move. They soon spread out so that they formed a second curtain. Among several little gaps that they left between them could be perceived the upper layer of clouds which was pale and continuous, but quite sufficient to hide the sun if it had been the only layer. The ships which until then had not swung before the wind, were forced to do so: the sea was white with foam, and the air darkened by the eddies of sand and of dust which the force of the wind kept raising continually. This terrible squall lasted until about six o'clock. The wind died down, but the clouds remained. At three or four minutes before seven o'clock, almost the moment when Venus was to go off the sun, a light whiteness was seen in the sky which gave a suspicion of the position of the sun, nothing could be distinguished in the telescope.

Little by little the winds passed to the east and to the south-east where they were at nine o'clock for a little while; the clouds brightened, and the sun was seen quite brilliant; we did not cease to see it all the rest of the day although the base of the sky remained covered with a whitish cloud. . . .

It is indeed a very singular and very rare phenomenon on the coast of Coromandel to have experienced during the season of the monsoon from the south and of the land breezes, a turning of the winds to the north-east, and a sort of gale from this direction which lasted two hours at the most. . . .

There was the same thing at Madras, where M. Call, chief engineer of that place, had been commissioned by M. Maskelyne to make the observations. . . . The observers were sleeping tranquilly when they were awakened by a most abundant rain and by a gusty wind, which carried off the tent and upset a part of their instruments. . . . This whirlwind was felt along the whole coast of Coromandel for more than thirty leagues advancing along the land of the peninsula.

That is the fate which often awaits astronomers. I had gone more than ten thousand leagues; it seemed that I had crossed such a great expanse of seas, exiling myself from my native land, only to be the spectator of a fatal cloud which came to place itself before the sun at the precise moment of my observation, to carry off from me the fruits of my pains and of my fatigues. . . .

I was unable to recover from my astonishment, I had difficulty in realizing that the transit of Venus was finally over. . . . At length I was more than two weeks in a singular dejection and almost did not have the courage to take up my pen to continue my journal; and several times it fell from my hands, when the moment came to report to France the fate of my operations. . . .

While the sky was treating me thus at Pondicherry, it presented the most calm appearance at Manila, as I have learned since by letters which I received and through Don Estevan y Melo himself who sent me the observation which he had made along with Father Théatin. M. Dargelet took the trouble of calculating it; you will see that it is very exact, and that it should merit perhaps as much as many others to be used for the parallax of the sun.

Le Gentil had by no means seen the end of his misfortunes. He fell ill, and in addition had considerable difficulty in getting passage back to France. In the following extracts we read of his troubles, as well as of the sad fate of M. Veron, another astronomer imbued with zeal for making observations in remote lands, but who became a victim of the diseases rampant in the tropics.

During the month of September while observing the comet I was attacked by a daily fever of which I did not take very good care in the beginning, and which finally forced me to stay for several days in my room.

My plan had been to go back to France on the ship Villevault: it was to leave in the month of October and go past the Isle de France. I had several cases of natural history to take on there; hence this arrangement suited me very well. . . . I was in bed at the time that the vessel left. My recovery was only apparent. Toward the end of December I fell seriously ill of the same fever; it was accompanied by dysentery and very sharp pains in my stomach. This time I recovered from it only with very great difficulty, and I saw myself almost forced to remain always in Pondicherry; but I had so great a desire to go back to Europe that I embarked on March 1, 1770, still convalescent, on the ship the Dauphin. This ship was to pass by the Isle de France, remain there several days, and continue its route to France; we anchored at the Isle de France on the evening of the 16th of April.

The state of my health did not permit me to continue on the ship and to go around the Cape of Good Hope in the bad season. M. Law had very strongly assured me before I left, that the Indian, which had gone to the coast of Malabar, would pass by the Isle de France in the month of May, or in the month of June. I therefore decided to stop at this island, and to recover entirely while waiting for the ship Indian. I found on the Isle de France my acquaintances and my friends, among whom I recovered from the fatigues of the voyage and regained my health. . . .

I had seen in India M. Veron, who had just made the trip to the south seas with M. de Bougainville. This astronomer was then on the Vigilant, a ship of the King, and he was going to the Moluccas; this was in the month of June 1769, when I saw him during his respite at Pondicherry, I gave him a letter of recommendation for Don Estevan Roxas y Melo in Manila, through which he was to pass and where he proposed to observe the transit of Mercury across the sun on November 9 of the same year, 1769. He arrived at the Isle de France in extremity, from a fever which he had acquired by his great zeal to observe throughout the night on land when he was at the Moluccas; he died three or four days after disembarking from ship, July 1, 1770.

M. Veron was of a very gentle character, tireless in work, a good observer; he could be counted on when entrusted with some operation relative to astronomy. . . . hence he was very much mourned by the commissioner.

I asked the commissioner for the papers, maps and journals of this astronomer: they were handed over to me numbered and signed, under receipt; I made a copy of them which I took away with me. The original remained at the Isle de France, and my receipt was returned to me.

This same commissioner tried to persuade Le Gentil to take a trip to the island of Tahiti for further discoveries, but by this time, Le Gentil says, a disgust of travel was beginning to lay hold on him, and he was impatient to see his native land. He therefore embarked on the ship Indian, expecting that he was making his final departure from the Isle de France, and little knowing what was ahead for him.

The Indian arrived July 26; this ship belonged to the India Company; I therefore asked for my passage from the administrators of this company; they procured for me all the facilities which I needed. I took on board with me all my cases of natural history, to the number of eight, which I had left in the hands of a very reliable person on my departure for Manila. We were to stop at the Isle de Bourbon, at the Cape of Good Hope, and at Ascension Island.

I was impatient to leave. We were approaching the season of hurricanes, the plagues which so often afflict the Isles of France and of Bourbon, and I knew that ships which meet these hurricanes fare very badly. We left the port of the Isle de France November 19, 1770; we would have been able to leave a week sooner. On the afternoon of the 20th we anchored at the Isle of Bourbon, at the harbor of Saint-Denys. The too long and useless sojourn which we made there was fatal to us, and caused me all the obstacles and the delays which remain for me to describe. December 3rd we were attacked by a hurricane which forced us to weigh anchor on the broadside and gain the open sea; it was then noon. Towards evening the try-sail was put out under the fore. During the night the violence of the wind and sea was so great that the helm of the rudder broke in its mortise; while the carpenters were busy repairing the helm, the tide-wave broke the bowsprit mast from its gammoning: this fall pulled down the main top-mast and the mast of the top-gallant sail of the mizzen-top, which all came down in a single fall; our main-yard was badly damaged, and I regard it as a sort of miracle that our main mast did not fall; for our main-shrouds had then more than six inches of slack; besides that, we were leaking in all parts. We took six or seven days to get into shape to reach the Isle de France again; we arrived there January 1, 1771, to the great astonishment of all the colony, since the last thing they expected was to see us again.

This annoying disaster made me lose all my hopes, made all my plans vanish. However much desire I had to be in France, I saw myself separated from it by an immense barrier and probably for a long time. This delay caused me the greatest anxiety because I felt that it could hurt me very badly. I had received at Pondicherry letters from my procurer in lower Normandy which had informed me that my heirs had spread the rumour of my death; that they spoke of nothing less than of having him give a reckoning and of taking possession of my estate. They lacked a certificate; and it was the sole difficulty which had held them back until then.

(*To be concluded*)

OUT OF OLD BOOKS

BY HELEN SAWYER HOGG

LE GENTIL AND THE TRANSITS OF VENUS, 1761 AND 1769

(*Concluded from May-June* JOURNAL)

Le Gentil's despair at his unscheduled return to the Isle de France after the ship Indian had been damaged by a tempest, was heightened by the fact that he had great difficulty in getting passage on any other ship bound towards France. He had been assured passage on a French ship returning from China, but when the vessel arrived he was not allowed to embark on it, for no definite reason except apparently a hostility on the part of the French officials on the island. He states that this was the only disagreeable episode which he experienced in any of the colonies of France during his lengthy sojourns. In March 1771 however, a Spanish warship arrived, whose captain received Le Gentil most cordially, and it was on the Spanish frigate Astrée that Le Gentil was finally transported to Cadiz. Even on this ship, his passage around the Cape was not an easy one.

I had planned to re-embark on one of the ships returning from China, which passed by the Isle de France on their return, arrived there in the first days of March and left again in the same period. I was assured on the word of the commissioner who had promised that he would get me a place on one of these ships, and who had repeated the same promise to me more than once. But in actual fact I could find no place on these ships under the pretext that they were loaded with special cargo and that they no longer belonged to the India Company. Finally I experienced here on the part of the government of the Isle de France exactly the same difficulties which had been made in Manila when I wanted to go to Acapulco in 1767. It seemed that at that moment the same spirit animated the Philippines and the Isle de France. There had been a time when M. Desforges was in command of this island, when all possible ways had been opened for me. There was, very happily for me, at the Isle de France, the Astrée, warship of his Catholic Majesty. This frigate was returning from Manila, and was commanded by Don Joseph of Cordova, captain of the frigate; I had known him on my voyage on the Bon Conseil. The Astrée arrived March 7th. . . .

How impossible it is for me to find terms to depict the obliging air with which Don Joseph of Cordova received the proposition of M. de Modave [to take Le Gentil as a passenger], and to describe the pleasure which it appeared to give him. . . . As if Don Joseph of Cordova wanted to command a great ship solely from the desire of seeing me more comfortable there. . . .

Don Joseph of Cordova, with whom I dined on the 26th at the home of M. Estenaur, having assured me that he was leaving within two or three days, I had carried on board the greater portion of my belongings on the 27th. . . .

On the afternoon of the 29th I carried aboard the rest of my belongings, even my bed. I returned to land however to spend the evening with some of my friends, and I slept at the home of one of them. I trusted in the word which Don Joseph of Cordova gave me that in case he was leaving the next day he would warn me of it by a cannon shot.

Indeed, the next day, the morning of the 30th, at ten o'clock a loud cannon shot which I heard made me rush on board. It was in such haste that I left the Isle de France. . . .

We sailed March 30, 1771, and I left the Isle de France three months after my return on the Indian: at the time when I should, without the adventure of the tempest of Dec. 3, 1770, have been in France, or at least near its shores.

We found ourselves towards the end of April around the Cape of Good Hope where new troubles awaited me for we remained almost two weeks struggling before being able to round this cape. During this time we experienced tempests on tempests, some of which were almost as bad as those which I had seen on the Indian. My sole worry in the midst of all these storms was the fear of being forced to see again the Isle de France, that island which I had nevertheless loved very much; but the sight of it had become unbearable to me since the misfortunes which I had finally had to experience. I told Don Joseph of Cordova my worry about the bad weather which we were having; he assured me that he would go back only as a last resort. He was an excellent officer, very intelligent and very active. He kept his eye on everything: he had a great deal of confidence in his frigate which was indeed an excellent ship. I learned during this hard passage what a good ship well commanded can do on the sea. Our frigate carried twenty-six twelve-inch cannon, armed, the gun ports open, and with only our port lids. Thus we went around the Cape in the middle of the worst weather; the sea was horrible, as I had never before seen it; I admired how this little ship balanced in the middle of this frightful sea; we received only a single wave over us, which did not do us the least harm. We continually manoeuvred; I believe that more manoeuvres were made on board the Astrée during the two weeks in the seas of the Cape of Good Hope than I had seen made during six years of travel. . . .

At this point in his narrative Le Gentil gives an interesting description of the visibility of Canopus and Sirius before sunset. Most of Le Gentil's scientific observations were published in different memoirs of the French Academy of Sciences, and are not included in the two volumes of his "Voyage" from which we are quoting.

At the Banc des Eguilles, before the tempest of which I have just spoken, the sky was of the greatest transparency and I witnessed a phenomenon which had never been seen in France, at least as far as I know, even in the finest and most serene weather. April 10th, when we were at 30 degrees 45 minutes of south latitude and about 16 degrees east of the Cape of Good Hope, a quarter of an hour before sunset, I perceived a star which was bordering upon the yard of the top gallant sail: I recognized that it was Canopus; then I looked for Sirius which I saw much more distinctly than Canopus. Indeed, Sirius in its splendour triumphs over this latter, and it is probable that I would have seen these stars still sooner if I had looked for them; but I did not know that they could be seen when the sun was above the horizon. I informed M. de Cordova and his office staff; they all saw, as I did, Canopus and Sirius before sunset. The night which came on resembled those beautiful nights of India of which I have spoken, and the stars had no twinkling.

After the Astrée had rounded the Cape, it met the French vessels on which Le Gentil had been refused passage, and acted as escort for them. There was also an interesting meeting with an English vessel, from whose captain Don Joseph extracted the information that England was not at war with Spain. This encounter ended happily when the English captain presented Don Joseph with the treat of a sack of potatoes. Le Gentil appears to condemn this latter with faint praise when he remarks, "At sea everything seems good."

Finally we went around this Cape quite luckily on May 11th: I say very luckily, to have come to the end of it without the slightest mishap, except for the loss of a great forestay which was carried off contrary to our expectations, because it was quite new and Don Joseph of Cordova had had it made expressly before arriving at the Cape to maintain us at the Cape.

When we had arrived at 11 degrees of north latitude and near the 36th meridian west of Paris, we met the French vessels from China on which I had not been able to embark at the Isle de France: they had left about ten days after us.

We had all left this island with news of the preparations of war between Spain, France and England: consequently the vessels from China which had forty-five millions of riches asked M. de Cordova to escort them as far as the neighbourhood of the tropics because they hoped to find there some ships which would give us news of Europe, the ships of England which are going to America going ordinarily as far as the tropic of Cancer. M. de Cordova answered quite obligingly to the captain of the Duras commanding the little division, "that he could not ask better than to escort them; but that he was afraid of making him lose some time because his frigate did not go as well as the two French vessels; however he would go as fast as he could". M. Dordelin commanding the Duras having agreed that he would suit his pace to that of the Astrée, we went together from June 11 until the 24th of the same month. That day, which was the second after we had left the tropics, we met an English vessel with three masts. We stopped it: the ship's boat was put to sea and sent to look for the captain and the supercargo with an order to bring back the invoice of the ship. The English captain was not too sure what they wanted him to say: he did not imagine that we had come from so far and that we were as little informed about the affairs of Europe as we were. When he had arrived on board the Astrée Don Joseph of Cordova, the better to draw out from him the information we desired, declared to him that he was his prisoner "because he had", he told him, "met only two days ago a despatch boat from Spain which was carrying to America the news of the declaration of war between Spain and England." The English captain appeared much surprised at the statement: he answered "that he knew nothing of this news; that it was indeed true that there had been a great deal of preparation in England; that they had armed; but that on his departure they were busy in disarming because the differences which had arisen among the three powers, England, France, and Spain, were settled." He was asked if he could give us some proof of what he said. He offered to show us the London *Gazette* for which he sent from his ship, and in which we saw the truth of what he had told us. Don Joseph of Cordova had brought some Spanish wine of several kinds, some biscuits, macaroons, etc., and we drank to his bon voyage. Potatoes were, without doubt, in fashion in England as I found them in France when I arrived; for when the English captain had returned to his ship he sent us a great bag of them and butter in proportion. At sea everything seems good; this sort of refreshment gave us great pleasure.

The ships from China, better sailors than the Astrée, thanked Don Joseph of Cordova and left us.

The captain of one of these ships wrote me to ask me to go on to his ship. It was too late to profit from his offer, he had refused me at the Isle de France; Don Joseph of Cordova had on the contrary received me with a great deal of kindness; I had received from him until then all sorts of good treatment; was it possible for me not to recognize so great a service by abandoning my benefactor at the moment when we were reaching Cadiz? . . .

We were delayed still again by contrary winds for eight to ten days; this was after having passed the Azores. At last I arrived at Cadiz the first of August, four months and two days after having left the Isle de France.

Le Gentil, who lingered nearly a month at Cadiz to rest and escape the heat, describes his overland journey to France in some detail. He had left the Isle de France so precipitously that he had not procured piastres, and had only paper money with him, so he had to be indebted to his friends to furnish him with money in Spain. One of his great moments was his actual reentry into France on October 8, 1771.

The eighth, at sun rise, we passed the crest of the Pyrenees, and at last I set foot on France at nine o'clock in the morning, after eleven years, six months and thirteen days of absence.

Unfortunately several disagreeable episodes greeted his return. One was that his heirs and creditors, believing him dead, were all ready to divide his estate. Another was that his procurer had been careless in keeping the estate funds and had been robbed of a large sum, which loss, after an acrimonious court fight, Le Gentil had to suffer himself. Probably his most bitter reward was to learn that he had been superseded in the Academy of Sciences, on whose behalf he had undertaken the journeys! This last indignity was rectified on February 28, 1772, four months after his return, by a letter from the faithful Duc de la Vrilliere informing him that His Majesty begged him to take his place again in the Academy.

Le Gentil must have derived a certain amount of sardonic satisfaction from people's astonishment at his appearance in the flesh, when he was finally able to drive in his native region. He says,

> I was very well received by everybody; people went to their windows and doors when I passed through the streets, and I had many times the satisfaction of hearing people recognize me and attest loudly that I was really alive.

One of the most bitter disappointments of his voyage was the loss of his precious cases of natural history, to the number of eight. These had left the Isle de France with him on the vessel Indian on its ill-fated attempt to round the Cape. After the return of the vessel to the Isle de France Le Gentil was not able to locate the whereabouts of the cases. He spent much time after his arrival in France trying to trace them, but he never saw them again, even though people with diplomatic powers made vigorous efforts to discover what had happened to them.

A happier aspect of his return was his marriage to Mlle. Potier, of Cotentin; and the arrival of a daughter that he adored provided him with a happy family circle after his years of lonely wanderings. He spent much time in writing his memoirs and the papers from his expedition. He died from an illness he contracted in October 1792, and was thereby spared many of the horrors of the revolution. Cassini in the concluding paragraphs of his Eulogy (*op. cit.*) sums up Le Gentil's personality.

> A healthy constitution which the voyages had strengthened rather than weakened, exempted M. Le Gentil from any sickness and would have brought him a longer life if an acute illness had not carried him off in the month of October 1792. He was only sixty-seven years old; but death, by cutting his

life short like that, spared him at least the spectacle of the great storm which was going to break, and of the destruction of the Academy of Sciences which certainly would have troubled the peace and happiness which he prided himself on enjoying. His face did not prejudice one in his favour; but animated by conversation, it took on an expression of wit and of originality which was pleasing. In his sea voyages he had contracted a little unsociability and brusqueness, but without rudeness; because to his intimates he was gay, pleasant, and agreeable. Finally to finish picturing him we shall say that he was a good fellow member of the Academy, a very good husband, and an excellent father.

His place in the Academy was not filled. In 1793 it was no longer a question of naming persons to academies, people busied themselves in suppressing them.

Thus at the age of sixty-seven, death came to Le Gentil, whose zeal for astronomy had carried him on eleven years of voyages and absence from his native land. Contrary to the impression given by Flammarion in his volume "Popular Astronomy" (Gore's translation, 1894, page 233), his life did not end soon after his return to France, for he enjoyed twenty-one years of life there after his voyages.

In his attempts to observe the transits of Venus he has set a record for astronomical persistence which it would be hard to equal. Nowadays we have astronomical equipment and facilities of living which it would have been impossible even to imagine two centuries ago. We can hardly hope to surpass some of the early astronomers in zeal, however.

For some centuries now the transits of Venus have been occurring in pairs eight years apart, with more than a century between the pairs, and this will continue for some time to come. Before and after these long periods of pairs of transits, there are long intervals of time during which the transits occur singly with an interval of more than a hundred years between each one. It is unusual when, in a given century, no transit of Venus occurs, as happens in the twentieth century. The last pair of transits occurred on December 8, 1874, and December 6, 1882. Few of our JOURNAL readers at present will have the opportunity to see a pair of transits as the next pair will not occur till June 8, 2004, and June 6, 2012.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Pieter Dircksz Keijser, Delineator of the Southern Constellations

A year ago in this Journal (Sept.-Oct. 1950) we published a summary of the origin of the eighty-eight constellations accepted to-day. The account that we reprinted from Chambers' "A Handbook of Descriptive Astronomy" attributed the naming of the southern constellations to Bayer. Actually Bayer's material for the southern stars came from early Dutch navigators, and the historical problem has been to identify which one of several was really responsible for first charting the southern sky.

For several centuries the credit was given to Frederick de Houtman who published in 1603 a "Catalogue of the Southern Stars" which he said was made from his own observations. In 1917, however, E. B. Knobel expressed grave doubt concerning de Houtman's claim. In a paper entitled "On Frederick de Houtman's Catalogue of Southern Stars, and the Origin of the Southern Constellations" (*Monthly Notices*, vol. 77, p. 414) Knobel reached the conclusion that, contrary to his own word, Frederick de Houtman was not the first to observe the positions of the southern stars, but rather that Pieter Dircksz Keijser was actually the first observer. Any proofs that Knobel could find for this assumption were of a somewhat negative character,—indications, rather than proofs.

Recently a decisive paper has been received from Dr. J. W. Stein, S.J., of the Vatican Observatory. Following the lead given by Knobel, Dr. Stein has done a clever bit of historical research and has proved, once and for all, that Pieter Dircksz Keijser first mapped the southern stars.

We quote from Knobel's account of the first study of the stars around the south celestial pole.

The origin of the constellations surrounding the South Pole is involved in much obscurity. They have all been attributed to various Dutch, Spanish, Portuguese, and Italian navigators, but the original records of the observation and formation of the different constellations appear to be lost.

The earliest existing map of the southern heavens is found in the map of the world made by Peter Plancius in 1594, entitled "Orbis terrarum typus de integro multis in locis emendatus Petro Plancio, 1594", given by Linschoten (*Navigatio ac Itinerarium,* 1599). In this map, besides the Ptolemy constellations, are shown Columba (the stars of which are in Ptolemy), Crux as a separate constellation (also in Ptolemy), Eridanus continued from Ptolemy's 34th star to α Eridani, Triangulum Australe, and a large constellation in the figure of a man called Polophilax, consisting of seven stars which I have not identified. The two Nubeculae are shown, but nothing else. It is tolerably clear that at this date no other southern constellations were known. Thomas Hood (*Celestial Map,* 1590) states that no more stars than those in Ptolemy have been observed.

The earliest publications containing the constellations surrounding the South Pole as we know them, are in Frederick de Houtman's *Catalogue of Stars,* Bayer's *Uranometria,* and Blaeu's *Celestial Globe,* which all appeared in the same year, 1603.

In the 49th map of Bayer's *Uranometria,* published at Augsburg, 1603 September, are shown the constellations Phoenix, Hydrus, Tucana, Grus, Indus, Pavo, Apus, Triangulum Australe, Musca, Chamaeleon, Volans, and Doradus. His map of Eridanus shows the continuation beyond the last of Ptolemy's stars to α Eridani; it also gives some stars in Phoenix and Doradus; that of Canis Major gives Columba, "recentioribus Columba"; Argo shows some stars in Volans; Centaurus includes Crux as a figure, and Triangulum Australe; Ara shows some stars in Triangulum, Grus, and Pavo; Piscis Austrinus includes stars in Grus. Bayer states that these constellations were observed partly by Amerigo Vespucci, partly by Andrea Corsali and Pedro de Medina, but their places were determined by that "most learned" seaman Petrus Theodorus.

Blaeu in his *Celestial Globe,* published in 1603, ascribes all the above constellations to Frederick de Houtman. Merula (*Cosmographia Generalis,* 1605) attributes all the observations of the "longitudes, latitudes, declinations, etc.," of the stars in question to Petrus Theodorus.

Knobel gives other references to the subject in early seventeenth century literature, concluding that the claim to the first charting of the southern constellations rests between Frederick de Houtman and Petrus Theodorus. But just who was Petrus Theodorus, and is it possible to decide which of the two men actually made the first positional measures of the southern stars? Knobel states:

Much uncertainty has existed as to the identity of Petrus Theodorus, and of the extent of his contributions to our knowledge of the southern constellations. His name is given in Latin as Petrus Theodorus F. Embdanus, and in German, Peter Theodors Sohn; but in 1825 Moll, in his work on the early naval expeditions of the Dutch, identified him as Pieter Dircksz, or (as the final z stands for zoon) as Pieter Dirckszoon; but he was unable to find any information about him, and he inquires whether this is the same person mentioned by Blaeu as Frederick de Houtman. I find in a Dutch dictionary that the English equivalent of the Dutch name Dirk is "Theodoric"; hence, in translating the Dutch name Pieter Dircksz(oon) into Latin, it became Petrus Theodorus

F(ilius). From Olbers (*Schumacher Jahrbuch für* 1840) and De Jonge (*De opkomst van het Nederlandsch Gezag in Oost-Indie,* 1862) we learn that his full name was Pieter Dircksz Keyzer. This man was instructed in mathematics and astronomy by Petrus Plancius, who was celebrated as a geographer and for his knowledge of astronomy and nautical matters, and who was one of the principal promoters of the Dutch expeditions to the Indies.

The first expedition of the Dutch to the East Indies sailed from the Texel 1595 April 2. It consisted of four ships, the second of which, the *Hollandia,* commanded by Jan Dignumz, carried Pieter Dircksz Keyzer as chief pilot and head of the steersmen, and Frederick de Houtman as sub-commissioner in the mercantile part of the expedition, with whom we here first become acquainted. From the journals and log of this ship and De Jonge we learn that the *Hollandia* arrived at Madagascar about 1595 September 3, and stayed there some time. It was here that Pieter Dircksz Keyzer "sought comfort in science, and enriched his knowledge of astronomy by improving the position of old and the observation of new constellations." It must have been at this time that Frederick de Houtman prepared that portion of his vocabulary, referred to below, which deals with the dialect of Madagascar. The ship went on to the Malay Archipelago, and though it is difficult to be certain about dates, it apparently anchored at Achin in Sumatra about 1596 June 5. After three months there it went on to the Straits of Sunda, where on 1596 September 13 Pieter Dircksz Keyzer died. Frederick de Houtman returned in the *Hollandia,* arriving in Holland about 1597 August 10. In the following year, on 1598 March 15, he went out again with a fleet, in command of the ship *De Leeuwin.* The chief pilot of the fleet was John Davis (not to be confounded with John Davis of Dartmouth, the inventor of the Davis Quadrant), and from Davis's journal we learn that the fleet arrived at Achin in Sumatra 1599 June 21. Tiele (*Mémoires sur les voyages Néerlandais*) says that Frederick de Houtman was taken prisoner by the King of Achin in 1598, and kept so for twenty-six months. This can hardly be correct, as he did not arrive there till sometime in 1599. He was, however, for a long time prisoner, and only escaped on 1600 December 31, when he went on board a ship commanded by van Caerden, which was then at Achin. He then returned to Holland. To conclude his career, in 1603 he went out on another voyage as mercantile commissioner, and two years after became governor of Amboyna. I have thus endeavoured to account for all Frederick de Houtman's time, which bears upon the authenticity of his *Catalogue of Stars.*

In 1603 Frederick de Houtman published at Amsterdam a Malay and Madagascan vocabulary, entitled *Spraeckende woordboeck Inde Maleysche ende Madagaskarche Talen met vele Arabische ende Turksche woorden.* At the end of this work he gives a catalogue of southern stars, the title-page of which is thus translated: "Here follow several fixed stars (observed), with efficient instruments, by Frederick de Houtman, in the island of Sumatra, (their positions) corrected and their numbers increased. For the use and service of those who navigate south of the equinoctial line, and also for all amateurs and those who have occasion for the best. . . ." In a dedicatory letter to the vocabulary Houtman says: "There will be found at the end the declinations of several fixed stars in the region of the South Pole which I had observed on my first voyage, and which on my second voyage I revised and corrected with more care and brought up to the number of 300, as may be seen on the Celestial Globe published by William Jansen (Blaeu)".

This work is extremely rare; every effort that I have made for many years to obtain a copy in Germany, France, Italy, and elsewhere, and by advertising, has failed. Four copies are certainly known, at the British Museum, the Bodleian, the Bibliothèque Nationale, and at Leiden. Thanks, however, to the enterprise of Professor Turner, he has had the copy in the Bodleian photographed, and thus this interesting catalogue—the first catalogue of southern stars ever made—is available.

The catalogue consists of the right ascensions, declinations, and magnitudes of 303 stars, of which 107 stars are found in Ptolemy, leaving 196 newly discovered. The right ascensions are too rough to allow of identification, but by plotting the whole catalogue in a map, and comparing with the maps of the *Uranometria Argentina,* it has been easy to identify nearly every star. . . .

Was this catalogue made by Frederick de Houtman at Sumatra on the occasions of his two voyages there, as he states?

On his first voyage, then twenty-four years of age, he was in a purely commercial capacity, and there is no record of his being an astronomer or of his having any mathematical ability. Most of his time at Sumatra on both occasions he was at Achin, which is in latitude 5° 34′ North; and, as we infer from the ship's journals of the first voyage, he was there on that occasion only some three or four months. The journal of his second voyage is not published, but we have enough information to infer that while at Achin, he had but a limited time at his disposal, as he was early taken prisoner by the king and kept in confinement for many months; and unless we assume that during his imprisonment he was able to make astronomical observations, there would have been no sufficient time for an inexperienced man to make the catalogue in question. How did he determine the right ascensions, rough as they are? In his catalogue he gives the positions of twenty-four stars, 7 of the 4th, 15 of the 5th, and 2 of the 6th magnitude, whose zenith distances at Achin ranged from 80° to 89° 15′; *obviously he could not have seen them at all. . . .*

Pieter Dircksz Keyzer, who was versed in mathematics and astronomy, and was held in high estimation by the Dutch East India Company, would have a favourable opportunity during the long stay of the *Hollandia* at Madagascar, from 1595 September to April or May of 1596, to make observations and to chart the new constellations. As Merula says: "sedens ibi in corbe, vel ut vocant nonnulli, galea." In that latitude he could quite well observe 5th and 6th magnitude stars near the South Pole. For the right ascensions we may surmise that Plancius would furnish him with some of Tycho Brahe's observations, from which they might be roughly derived; certainly there was no other material available for the purpose. I have failed to find any information as to relations between Tycho Brahe and Plancius.

Frederick de Houtman was in Pieter Dircksz's company all the time, but as commissioner he would be engaged in the development of trade, and, moreover, he was then collecting materials for his Malay and Madagascan vocabulary. Though all the evidence is circumstantial, the following inferences are irresistible: that the whole catalogue and the formation of the new twelve constellations must be attributed to Pieter Dircksz Keyzer, and not in any way to Frederick de Houtman: that the catalogue was sent back by the *Hollandia,* and after the return of that ship on 1597 August 10 it would go to Plancius, who probably communicated it to Bayer: and that Frederick de Houtman obtained an imperfect copy of it which, as Pieter Dircksz was dead, he published as his own work in the above-mentioned vocabulary.

Knobel in concluding his article reprinted eleven pages of the star catalogue as given by Frederick de Houtman in his "Malay and Madagascar Vocabulary." But it has remained for Dr. J. W. Stein, S.J., to find the proof of Knobel's surmise that Pieter Dircksz Keijser was the originator of the southern constellations, and publish the evidence in his fascinating paper, "The Celestial Globe of Jodocus Hondius of 1600," in the Miscellanea Astronomica of the Vatican Observatory, vol. III, No. 101, 1950, which shows three photographs of the globe. Dr. Stein states:

Knobel knew of no document antedating 1603 which made mention of the new constellations. In particular, Knobel was unaware of the existence of the celestial globe which Hondius produced in the year 1600, and which even by itself is quite sufficient to decide the question of priority. A description of this precious globe, one specimen of which is in the possession of the Nautical Museum of Amsterdam, was given by the present author (at the request of the Director of the Museum, Mr. W. Voorbeijtel Cannenburg) in the 13th Annual Report of the Museum. (1929).

The Hondius globe is very remarkable, since it is the first one on which the new southern constellations were depicted. The assigned names of the constellations (clearly painted in vivid colours) are: Phoenix, Watersnake (Hydrus), Goldfish (Dorado), Fly (Musca), Flying Fish (Volans), Chameleon, Southern Triangle (Triangulum Australe), Bird of Paradise (Apus), Peacock (Pavo), Indian (Indus), Heron (Grus), Toucan (Tucana). In addition to a representation of Tycho Brahe, the globe bears two inscriptions.

The first inscription contains the maker's dedication of the globe to the Academies of Leiden and Franeker: (Translation from the Latin). "To the Academies of Leiden and Franeker, most illustrious luminaries of Belgium and sources of wisdom, learning and true piety, Jodocus Hondius in the year 1600 with the greatest of pleasure dedicates and consecrates these globes engraved by his own hand for the advancement of the mathematical arts." From the use of the plural, "globos", we surmise that a terrestrial globe was constructed at the same time, as was usually the case.

The second inscription mentions the names of Tycho Brahe who observed the northern constellations, and of Pieter Dircksz, who observed the southern constellations. . . . Translation: "A celestial globe on which are most accurately marked all of the fixed stars which the nobleman Tycho Brahe observed so industriously and carefully, as well as the southern circumpolar stars which were observed by the most learned navigator, Peter Son of Theodore, student of mathematics."

Jodocus Hondius was born in 1567 at Wacken in West Flanders. Even in his early youth he revealed unusual talent for drawing and engraving, and so he was apprenticed by his father Oliver to an engraver in Antwerp. About 1585 he went to England, where he devoted his talents to the service of the geographers, Richard Hakluyt and Edward Wright. In 1592 he established himself at Amsterdam, and became a specialist in publishing geographical charts and constructing celestial and terrestrial globes. As far as we know, his first celestial globe was dated 1592. Of his next globe, built in 1600, only two certainly identified specimens remain; one is in the Nautical Museum of Amsterdam and the other is in the collection of the Henry E. Huntington Library in Pasadena, California. Both of these globes are illustrated in the previously mentioned Annual Report of 1929.

Dr. Stein corroborates and repeats the conclusions previously reached by Knobel, that Keijser made the observations during his stay at Madagascar:

The *Hollandia* arrived at Madagascar on September 3, 1595, and remained anchored there until April or May, 1596. In the meantime both Keijser and Houtman transferred to the ship *Mauritius*. It was during this long stay at Madagascar that Keijser sought comfort in science by improving his knowledge of the positions of the old constellations and by securing his observations of the new ones—"sedens ibi in corbe, sitting in the crow's nest", as Professor Merula of Leiden tells us in his *Cosmographia Generalis* (1605). About June 5, 1596, the fleet arrived at Atchin, Sumatra, and after three months they anchored at Bantam in the Straits of Sunda, which lie between Java and Sumatra. There Keijser died on September 13th; "mortuus est in coelo lustrando," while scanning the heavens, as Merula says. Houtman returned to Holland on August 10, 1597. The ship's journals of Keijser were turned over to Plancius, who then communicated the observed star positions to Merula, as attested by the latter. Hondius also must have had access to a copy, which enabled him to delineate the new constellations on his globe of the year 1600. . . .

That Houtman has misappropriated the honour of having *first* observed and delineated the southern constellations is definitely proved by the inscription on the globe of Hondius of the year 1600, in confirmation of the statement of Professor Merula, who received the original observations of Keijser directly from Plancius. The honor of having first measured the positions of the southern circumpolar stars and of having formed from them twelve new constellations is due exclusively to Pieter Dircksz Keijser.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Recognition of Uranus as a Planet

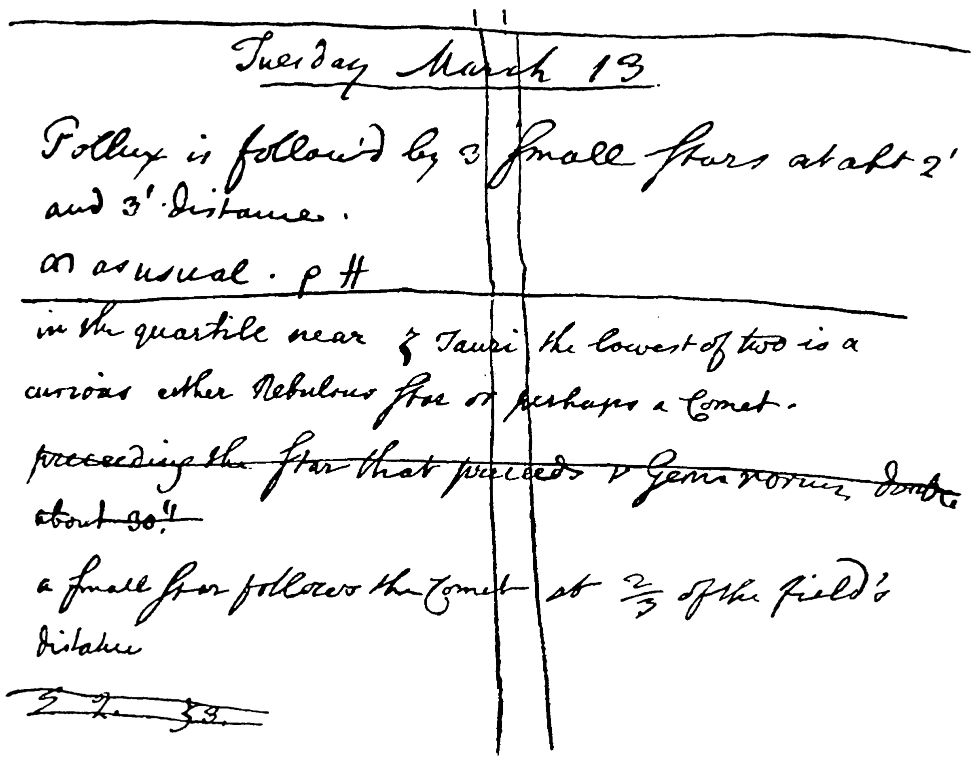
As time goes on the details of the events whereby the planet Uranus was added to the known bodies of the solar system have become more and more obscure, even in the minds of astronomers. Since scientific knowledge accumulates so rapidly, it is a real problem to keep up with the present, let alone search out the previous history of events.

It is common knowledge that William Herschel discovered the planet Uranus, and great credit is due him for his diligence in singling out this little speck in the heavens as something unusual. Records show that other watchers of the sky had actually observed Uranus some twenty times, but had not troubled to follow up the observations.

The fact lost sight of in many modern references to this discovery is that Herschel did not realize he had found a new *planet*. He and other astronomers followed it for weeks thinking it was a new comet. The recognition of the fact that it was a new planet, and not just another comet, is due as much to the mathematical as to the observational astronomers of the period. As observations on the object accumulated the mathematicians computed one orbit after another, finding with successive approximations that the body was definitely not moving in a cometary orbit since it never came nearer the sun than the outer planets. Probably the person most responsible for the final identification of a new planet was Anders John Lexell, but the better known Pierre Simon Laplace, as well as President de Saron of the French Academy, also made important contributions to the problem.

Unfortunately the original papers of the above are rather difficult to procure. Herschel's papers, on the contrary, are much more accessible on this continent. Excerpts from Herschel's paper "Account of a Comet" in the *Philosophical Transactions*, vol. 71, 1781, have already been published in Shapley and Howarth's "Source Book in Astronomy". All Herschel's works have been collected and published in the two volumes of "The Scientific Papers of Sir William Herschel", 1912. From this source we quote the remarks which Herschel entered in his Journal and reproduce a facsimile of the original record of the "comet" on March 13, 1781. The authors of this volume have also given a valuable discussion of the identification of the comet as a new planet.

The original entry in Herschel's Journal is as follows:— (the two vertical lines down the middle mean that the observations have been copied into separate books for fixed stars, planets, etc.)


Tuesday March 13

Pollux is follow'd by 3 small stars at abt 2' and 3' distance.

as usual.

in the quartile near ζ Tauri the lowest of two is a curious either Nebulous star or perhaps a Comet.

~~preceding the star that preceeds ν Geminorum, double about 30".~~

a small star follows the Comet at 2/3 of the field's distance

~~2. 7. 33.~~

FROM HERSCHEL'S JOURNAL.
Slightly reduced in size.

After this original entry it was some days before Herschel could observe his comet again. On Thursday morning between five and six o'clock he observed Mars and Saturn, but apparently the sky was not clear enough for him to hunt for the comet. He first saw it again on Saturday, March 17th, and we quote the following account from "Collected Scientific Papers," p. xxx. It shows that not only did Herschel fail to realize that he had found a new planet, but also, because of inaccuracy in his micrometers, he mistakenly thought the object was much nearer to us than it proved to be.

"I looked for the Comet or Nebulous Star and found that it is a Comet, for it has changed its place. I took a superficial measure 1 Rev. 6 parts and found also that the small star ran along the other wire." . . .

Saturday, 17th March 1781. 11^h.

This is the first determination of the "Comet's" position made by Herschel. He now followed it regularly, and his observations are given in the paper which was read before the Royal Society on the 26th April.

The discovery was very soon communicated to the only public observatories then existing in England, at Greenwich and Oxford. Maskelyne wrote on the 4th April to Dr. Watson that he had for the last three nights observed stars near the position pointed out by Mr. Herschel, whereby he was enabled on the 3rd to discern a motion in one of them, which convinced him that "it is a comet or new planet, but very different from any comet I ever read any description of or saw." On the 23rd April he wrote to Herschel: "It is as likely to be a regular planet moving in an orbit nearly circular round the sun as a comet moving in a very excentric ellipsis. I have not yet seen any coma or tail to it." Hornsby wrote on the 24th that he had searched for the comet, but could not find anything like a comet except an object which turned out to be a small cluster; he asked for further particulars. A week later he had not yet found it, but on the 14th April he wrote that he had found it immediately after receiving Herschel's last letter, and had in fact observed it on the 29th and 30th March "unknowingly". He adds: "I do not in the least question but this is the comet of 1770, but whether it has passed its perihelion or has not yet come to it, is more than I can say at present. I will very soon try to construct its orbit."

It is not necessary in this place to give an account of the attempts made to calculate a parabolic orbit for the new "comet". That the new star was at a very great distance, became evident after some time, though Méchain had at first computed a parabolic orbit with a perihelion-distance of 0.46, the comet being about to pass the perihelion on the 23rd May. These elements were communicated to "Monsieur Hertsthel à Bath" by Messier, who wrote to the discoverer to express his wonder as to how he had found this stellar object, the motion of which could not be recognised in the course of a night. Herschel replied at once, explaining that it was by its appearance that he had distinguished the comet from a star. Later on Lexell computed a perihelion distance of 16, and announced that the perihelion would not be reached till April 10, 1789. It is difficult to decide who was the first to announce publicly, that the star moved in an orbit of small excentricity and at a distance about twice that of Saturn; but it appears that Saron, Lexell, and Laplace found this independently. Herschel had himself imagined that his observations made in March and April showed a considerable parallax. His paper as printed in the *Philosophical Transactions* contains a paragraph "Remarks on the path of the comet", in which he points out that the apparent distortion in his figures of the comet's path were simply caused by the inevitable errors of his measures. But this had not been his opinion at first. His paper as originally presented to the Royal Society did not contain the paragraph just mentioned, but instead of it another and longer one entitled "Remarks on the diurnal parallax of the Comet". Looking over the delineations he had made of the comet's path, Herschel says he had found a certain irregularity for which he could not account. He shows first that it could not arrive from refraction and next considers whether it might be caused by parallax. . . . He therefore took great care on the 16th April to obtain accurate measures as early and as late as possible, and the result was in his opinion decisive. "I cannot hesitate to say that no other cause but parallax could give that evident deviation from a regular path which in this observation amounted to no less than 11″." Finally he says that the observed diameters of the comet agree with the observations of its place in pointing to its having a considerable parallax.

In a letter to Hornsby of May 21, 1781, Herschel writes that, without having entered into calculations, he supposes that a parallax of not less than 10″ nor much more than 20″ would follow from his observations. This was very soon disproved by more accurate observations made elsewhere, the cause of the discrepancies was found, and the paragraph in question was not printed.

In a letter of February 26, 1782, Hornsby expressed his admiration of Herschel's diligence in measuring double stars, but threw some doubt on the value of his micrometers, since his measures had led him to place the new comet below the orbit of Mars.

Another good summary of the discovery of Uranus and of events leading up to its identification as a planet is given in "A History of Physical Astronomy" by Robert Grant, 1852. Grant points out that Herschel recognized that his object was not a star because when he switched from his magnifying power of 227 which he was using on his seven feet reflector, to higher powers of 460 and 932 he found that the comet's image was magnified much more than that of the stars. Herschel continued to observe the comet until the 19th of April, and his paper on it was read before the Royal Society on April 26th, 1781. Then Grant proceeds with the discussion of the identification (page 273):

Previous to transmitting the above-mentioned communication to the Royal Society, Herschel had taken an opportunity of announcing his discovery to Dr. Maskelyne, the Astronomer Royal, who in his turn gave due notice of it to the astronomers of France. Messier commenced his observations of the supposed comet on the 16th of April, 1781, and his example was speedily followed by Lalande, Lemonnier, Méchain, and D'Agelet, as well as by Reggio, De Cesaris, Bode, Wargentin, and various other astronomers on the Continent. As soon as a few observations of it were obtained at Paris, an attempt was made by means of them to determine the elements of the parabolic orbit in which it was presumed to revolve. A serious difficulty, however, soon presented itself to those engaged in this enquiry. It was found that although a parabola might be assigned, which would represent with tolerable fidelity a limited number of observations of the comet, yet in a few days afterwards, the positions of the body, when calculated upon the same hypothesis, appeared to be totally irreconcilable with the actual motion. Various attempts to discover an orbit which would *permanently* represent the motion of the body were made by Méchain, the President de Saron, Laplace, Boscovich, and others, but in all instances they proved to be equally unavailing for this purpose. Nor is their failure at all to be surprised at, for, since the body was universally supposed to be a comet, it was concluded by reasoning from analogy, that the perihelion, at the utmost, would not extend beyond the orbit of Jupiter, and that in all probability it was situated far within the terrestrial orbit. The attention of each calculator was therefore constantly directed towards constraining the body to move in an orbit the perihelion distance of which, even upon the most extravagant supposition, was imagined not to amount to four times the radius of the terrestrial orbit. It was never suspected all the while, that the nearest distance of the body from the sun exceeded the same standard of measurement at least eighteen times.

The President de Saron appears to have been the person who threw the first glimmering of light on this perplexing subject. On the 8th of May, 1781, he announced that the comet was in reality much more remote from the sun than astronomers had hitherto supposed it to be. He estimated its perihelion distance to be equal to at least twelve times the radius of the terrestrial orbit. (*Mém. Acad. des Sciences*, 1779, p. 529). This was an important suggestion, for it had the effect of directing the attention of enquirers to the region of the heavens in which the body was actually revolving. By adopting it, the observations were represented with greater precision than they had been on any previous hypothesis, and hopes began to be entertained of arriving at a determination of the real orbit of the body.

The next step in the enquiry was made by Lexell, who happened to be in England at the time of Herschel's discovery. In an account of his researches which he communicated to the Academy of St. Petersburg, he mentions that Dr. Maskelyne and the other English astronomers who observed the body, agreed with him in supposing that in all probability it was a planet. (*Nov. Act. Acad. Petrop.*, tom. i., p. 69, et seq.) Various circumstances, he remarks, concurred in suggesting this view of its nature. In the first place, observation shewed it to be a well-defined object, whereas comets generally have a nebulous appearance. Again, although very small, it was not difficult to discern a difference in its light from that of the fixed stars. Lastly, its slow motion in latitude (indicating that its inclination to the ecliptic was very inconsiderable), and its motion in the zodiac according to the order of the signs, were two independent facts which both strongly supported the hypothesis of its being a planet. Taking two extreme observations of the body, one by Herschel, dated March 17, 1781, and the other by Maskelyne, dated May 11 of the same year, Lexell found that they might be both satisfied by a circular orbit, whose radius was equal to 18.93, the mean distance of the earth from the sun being supposed equal to unity. In the month of June or July, while still residing in England, he wrote a letter to one of his friends in Paris, in which he stated, that the motion of the body which formed the subject of so much anxious investigation might be represented by a circular orbit, whose radius was equal to eighteen times the mean distance of the sun from the earth. "From that time," says Lalande, "it appeared to me that the body ought to be called the new planet." (*Mém. Acad. des Sciences*, 1779, p. 530.) Lexell soon afterwards found that on account of the slow motion of the body, and the consequent smallness of the arc described by it within a limited interval of time, the observations included between March 17 and May 28 might be satisfied by an infinite number of parabolas, whose perihelion distances varied from 6 to 22 times the radius of the terrestrial orbit. From this circumstance it appeared evident to astronomers, that until the planet had described a larger arc, it would be impossible to arrive at an accurate knowledge of the elements of its orbit.

After the lapse of a few months, when the motion of the planet began to be developed more clearly, its distance from the sun, upon the supposition of a circular orbit, was determined with considerable precision; but from the discordances which still existed between the computed and the observed positions, it was plainly apparent that the real orbit was an ellipse of small eccentricity. Elliptic elements of the planet were first calculated by Laplace, and were communicated by him to the Academy of Sciences, in the month of January, 1783.

When it was ascertained beyond all doubt that the body discovered by Herschel was a planet, it became desirable to distinguish it by some special name. As the privilege of choosing a name in all such cases is the incontestable right of the discoverer, Herschel, urged by a feeling of gratitude towards his royal patron, George III., proposed to confer on the planet the appellation of the *Georgium Sidus*. Lalande, influenced by an equally honourable motive, suggested the name of *Herschel*. Both these names sounded incongruously with the prevailing nomenclature of the planetary system, and neither of them consequently met with much favour on the part of astronomers. The names of various heathen divinities were proposed as more appropriate for this purpose. After some time had been spent in discussing the rival claims of different deities, the name of Uranus, suggested by Bode, was finally adopted by astronomers, and has always since been employed to distinguish the planet.

I have found a very interesting contemporary account of the dilemma precipitated by Herschel's discovery of the object in Gemini, given by Pingré in his "Comètographie, ou Traité Historique et Théorique des Comètes", 1784. This classic work is well known, but actual copies are scarce. Through the kindness of Dr. Harlow Shapley, and the help of Miss Hanley, I have been able to use the second volume of this work from the Treasure Shelf of the Harvard Observatory. In this, Pingré gives a long catalogue of comets which closes with the year 1781. In this catalogue Uranus is listed as the first comet discovered in 1781. Earlier in the volume Pingré gives a discussion of this comet, probably written late in 1781 or early in 1782, which we reprint in its entirety. Pingré states that it is not yet decided whether it is a comet or a planet, but that Lexell has determined a circular orbit for it, with a mean distance from the sun of nearly 19 astronomical units and a period of 82 years. The accepted distance from the sun now is the same, and the period is 84 years, so that Lexell's elements were amazingly close to the true value. Pingré explains that many people still regard the object as a comet, and hence in his table he publishes two sets of cometary elements, one by Boscowich and one by Laplace.

From Pingré's "Comètographie"

1781 Première Comète.

Cette Comète ou Planète (car il n'est pas encore décidé si elle est l'une ou l'autre) fut découverte en Angleterre par M. Herschel, Astrophile, dit-on, plutôt qu'Astronome, le 17 Mars 1781. Elle a été observée à Greenwich par M. Maskelyne, depuis le 1.^{er} Avril; à Paris par M. Messier, depuis le 16 Avril de la même année, & ensuite par M. Méchain & par plusieurs autres Astronomes. M. Lexell ayant tenté d'expliquer le mouvement apparent de cet astre dans une orbite parabolique ou elliptique, & n'ayant pu y réussir, conjectura que le nouvel astre étoit une Planète; & en effet les observations faites jusqu'alors, se prêtèrent passablement bien à l'hypothèse d'une orbite circulaire, dont le rayon seroit de 18,9283, la distance moyenne de la Terre au Soleil étant toujours prise pour l'unité: la révolution de la Planète seroit alors de 82,350718 ans. Nous avons pareillement trouvé que dans la supposition d'un mouvement circulaire, la distance de la Planète au Soleil seroit presque dix-neuf fois plus grande que celle du Soleil à la Terre, & que la révolution périodique excéderoit quatre-vingt-deux ans. Ce résultat a été pareillement confirmé par les calculs de M. de la Lande; suivant cet astronome, le rayon de l'orbite est 18,931; le temps de la révolution excède quatre-vingt-deux ans, le lieu du noeud ascendant est environ en $2^{f}\ 13^{d}$; il y avoit environ quatre ans que la planète y avoit passé; enfin l'inclinaison de son orbite à l'Ecliptique est de $0^{d}\ 46'$, & le mouvement est direct, ainsi que celui des autres Planètes. Le lieu du noeud seroit selon M. Lexell en $2^{f}\ 12^{d}\ 26'$, l'inclinaison de $0^{d}\ 45'\ 28''$.

On demandera peut-être comment ce nouvel Astre, s'il est une vraie Planète, a pu échapper si long-temps aux observations des Astronomers? On peut répondre qu'il est fort petit, presque invisible à la simple vue, & que son mouvement apparent est très-lent, nul même en quelques circonstances; il ne traîne pas de queue après lui, il n'est point environné d'une atmosphère assez sensible pour le faire reconnoître. Des Astronomes l'auront sans doute rencontré quelques fois dans le champ de leur lunette; mais ils l'auront pris pour une étoile télescopique, & s'ils l'ont observé plusieurs jours de suite, son immobilité apparente les aura confirmés dans leur première opinion.

Au reste comme nous l'avons déjà dit, il n'est pas irrévocablement décidé que cet Astre soit une Planète; plusieurs le regardent comme une Comète, & ont même essayé d'ébaucher la théorie de l'orbite qu'elle décrit. M. l'Abbé Boscowich en a donné une théorie complète, c'est la première de la Table générale. La seconde est dûe à M. de la Place; il l'a fondée sur une théorie neuve & très-ingénieuse, dont nous parlerons dans la quatrième partie de cet ouvrage; ce savant Géometre travaille actuellement à perfectionner cette méthode, & par conséquent la théorie même de l'orbite de la Comète, celle que nous donnons d'après lui ne devant passer que pour une ébauche. Il n'a pas cru que les observations faites jusqu'a présent fussent suffisantes pour calculer le lieu du noeud & l'inclinaison de l'orbite de la Comète.

Nous rapportons cette Comète à l'an 1781, parce qu'elle a commencé a être observée en 1781; elle ne sera cependant périhélie qu'en 1790; mais notre Table générale ne peut s'étendre jusqu'à cette année.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

Galileo and the Surface of the Moon

During the past few years much progress has been made in the interpretation of the remarkable surface features of the moon. Long neglected, probably because of the lack of noticeable change in its surface, the moon once more has come into importance as a source of fruitful astronomical study and thought.

The appearance of the excellent book, *The Face of the Moon*, by Dr. Ralph B. Baldwin in 1949 has done a great deal to crystallize current thought on the origin of the lunar maria and craters. This book is a masterly discussion of the correlation of the various surface features of the moon. Baldwin shows that not only all the craters could have been caused by impact of meteorites, but that the large maria probably resulted from direct hits of large bodies. In particular, various grooves on the moon's surface, radiating outward from the centre of Mare Imbrium, indicate that this region of the moon was once the scene of a terrific explosion. This catastrophe not merely formed Mare Imbrium itself, with its surrounding high mountain ranges, but also touched off a flow of lava which spread over a large section of the lunar surface.

At the winter meeting of the American Astronomical Society held in December 1951 at the Case Institute of Technology in Cleveland, one of the most outstanding papers was presented by the well-known physicist and chemist, Dr. Harold C. Urey of the University of Chicago. Following the lines suggested by Baldwin, Dr. Urey computed the tensile strength of objects which, when hurled across the moon's surface with speed of a mile a second by the Mare Imbrium explosion, would be able to cut great grooves across the surface and plow chasms through the mountains. He found that these missiles needed to have a high tensile strength such as that of nickel iron, whereas the strength of the resisting lunar surface must be low, comparable to pumice.

Because of the revival of interest in the lunar surface, it is interesting to read the beginnings of the subject when Galileo began to study the lunar features with his first telescope. Long before the invention of the telescope, however, some of the early Greek philosophers had very lucid ideas of the moon's source of illumination and the ruggedness of its surface. In particular, Anaxagoras, born about 500 B.C., realized that the sun was responsible for the moon's light, as he declared

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"The sun places the brightness in the moon." Further, he stated that "The moon is an incandescent solid, having in it plains, mountains, and ravines," and said also that "It has a surface in some places lofty, in others low, in others hollow." Only when telescopes became available, however, could the host of craters and other details of the lunar surface be properly seen.

An excellent translation of *The Sidereal Messenger* of Galileo Galilei, 1610, containing the original account of Galileo's first astronomical discoveries, was published in 1880 by E. S. Carlos of Christ's Hospital, England. We quote some excerpts from this work giving Galileo's first impressions of the moon. Some, but not all, of this has already been republished in the splendid *Source Book of Astronomy* by Shapley and Howarth, 1929.

From Galileo's *Sidereus Nuncius,* 1610

The Moon. Ruggedness of its Surface

Let me speak first of the surface of the Moon, which is turned towards us. For the sake of being understood more easily, I distinguish two parts in it, which I call respectively the brighter and the darker. The brighter part seems to surround and pervade the whole hemisphere; but the darker part, like a sort of cloud, discolours the Moon's surface and makes it appear covered with spots. Now these spots, as they are somewhat dark and of considerable size, are plain to every one, and every age has seen them, wherefore I shall call them *great* or *ancient* spots, to distinguish them from other spots, smaller in size, but so thickly scattered that they sprinkle the whole surface of the Moon, but especially the brighter portion of it. These spots have never been observed by any one before me; and from my observations of them, often repeated, I have been led to that opinion which I have expressed, namely, that I feel sure that the surface of the Moon is not perfectly smooth, free from inequalities and exactly spherical, as a large school of philosophers considers with regard to the Moon and the other heavenly bodies, but that, on the contrary, it is full of inequalities, uneven, full of hollows and protuberances, just like the surface of the Earth itself, which is varied everywhere by lofty mountains and deep valleys.

The appearances from which we may gather these conclusions are of the following nature:—On the fourth or fifth day after new-moon, when the moon presents itself to us with bright horns, the boundary which divides the part in shadow from the enlightened part does not extend continuously in an ellipse, as would happen in the case of a perfectly spherical body, but it is marked out by an irregular, uneven, and very wavy line, as represented in the figure given, for several bright excrescences, as they may be called, extend beyond the boundary of light and shadow into the dark part, and on the other hand pieces of shadow encroach upon the light:—nay, even a great quantity of small blackish spots, altogether separated from the dark part, sprinkle everywhere almost the whole space which is at the time flooded with the Sun's light, with the exception of that part alone which is occupied by the great and ancient spots. I have noticed that the small spots just mentioned have this common characteristic always and in every case, that they have the dark part towards the Sun's position, and on the side away from the Sun they have brighter boundaries, as if they were crowned with shining summits. Now we have an appearance quite similar on the Earth about sunrise, when we behold the valleys, not yet flooded with light, but the mountains surrounding them on the side opposite to the Sun already ablaze with the splendour of his beams; and just as the shadows in the hollows of the Earth diminish in size as the Sun rises higher, so also these spots on the Moon lose their blackness as the illuminated part grows larger and larger. Again, not only are the boundaries of light and shadow in the Moon seen to be uneven and sinuous, but—and this produces still greater astonishment—there appear very many bright points within the darkened portion of the Moon, altogether divided and broken off from the illuminated tract, and separated from it by no inconsiderable interval, which, after a little while, gradually increase in size and brightness, and after an hour or two become joined on to the rest of the bright portion, now become somewhat larger; but in the meantime others, one here and another there, shooting up as if growing, are lighted up within the shaded portion, increase in size, and at last are linked on to the same luminous surface, now still more extended. . . . Now is it not the case on the Earth before sunrise, that while the level plain is still in shadow, the peaks of the most lofty mountains are illuminated by the Sun's rays? After a little while does not the light spread further, while the middle and larger parts of these mountains are becoming illuminated; and at length, when the Sun has risen, do not the illuminated parts of the plains and hills join together? The grandeur, however, of such prominences and depressions in the Moon seems to surpass both in magnitude and extent the ruggedness of the Earth's surface, as I shall hereafter show. . . .

This part of the surface of the Moon, where it is marked with spots like a peacock's tail with its azure eyes, is rendered like those glass vases which, through being plunged while still hot from the kiln into cold water, acquire a crackled and wavy surface, from which circumstance they are commonly called *frosted glasses*.

[Specimens of *frosted or crackled Venetian glass* are to be seen in the Slade Collection, British Museum, and fully justify Galileo's comparison.]

Then Galileo goes on to suggest that the lunar spots may be seas bordered by ranges of mountains. Modern ideas are that they are geological seas, but the material which filled them was lava, and not water, for water is completely absent from the surface of the moon at present.

Now the great spots of the Moon observed at the same time are not seen to be at all similarly broken, or full of depressions and prominences, but rather to be even and uniform; for only here and there some spaces, rather brighter than the rest, crop up; so that if any one wishes to revive the old opinion of the Pythagoreans, that the Moon is another Earth, so to say, the brighter portion may very fitly represent the surface of the land, and the darker the expanse of water. Indeed, I have never doubted that if the sphere of the Earth were seen from a distance, when flooded with the Sun's rays, that part of the surface which is land would present itself to view as brighter, and that which is water as darker in comparison. Moreover, the great spots in the Moon are seen to be more depressed than the brighter tracts; for in the Moon, both when crescent and when waning, on the boundary between the light and shadow, which projects in some places round the great spots, the adjacent regions are always brighter, as I have noticed in drawing my illustrations, and the edges of the spots referred to are not only more depressed than the brighter parts, but are more even, and are not broken by ridges or ruggednesses. . . .

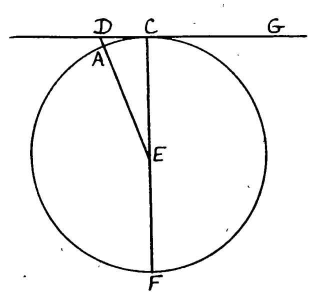
These phenomena which we have reviewed are observed in the bright tracts of the Moon. In the great spots we do not see such differences of depressions and prominences as we are compelled to recognise in the brighter parts, owing to the change of their shapes under different degrees of illumination by the Sun's rays according to the manifold variety of the Sun's position with regard to the Moon. Still, in the great spots there do exist some spaces rather less dark than the rest, as I have noted in the illustrations, but these spaces always have the same appearance, and the depth of their shadow is neither intensified nor diminished; they do appear indeed sometimes a little more shaded, sometimes a little less, but the change of colour is very slight, according as the Sun's rays fall upon them more or less obliquely; and besides, they are joined to the adjacent parts of the spots with a very gradual connection, so that their boundaries mingle and melt into the surrounding region. But it is quite different with the spots which occupy the brighter parts of the Moon's surface, for, just as if they were precipitous crags with numerous rugged and jagged peaks, they have well-defined boundaries through the sharp contrast of light and shade. Moreover, inside those great spots certain other tracts are seen brighter than the surrounding region, and some of them very bright indeed, but the appearance of these, as well as of the darker parts, is always the same; there is no change of shape or brightness or depth of shadow, so that it becomes a matter of certainty and beyond doubt that their appearance is owing to real dissimilarity of parts, and not to unevenness only in their configuration, changing in different ways the shadows of the same parts according to the variations of their illumination by the Sun, which really happens in the case of the other smaller spots occupying the brighter portion of the Moon, for day by day they change, increase, decrease, or disappear, inasmuch as they derive their origin only from the shadows of prominences.

Galileo also explains the faint illumination of the moon's disk at new moon as due to earthlight, an explanation which had already been advanced a century earlier by Leonardo da Vinci, whose statements we reprinted in this JOURNAL, October 1946.

Galileo very cleverly determined the height of some lunar mountains by measuring the distance from the terminator, or illuminated portion of the moon, at which the mountain peak on the night side of the moon was visible because it had sufficient altitude to be in sunlight. His determination is expressed in the Italian mile which is 0.925 of an English mile, and he concludes that there are mountains on the moon four miles high, whereas the mountains on earth do not reach above one mile. Accurate determinations of lunar mountain height by Beer and Maedler two centuries later showed six mountains which exceeded 19,000 British feet in height. Although Baldwin says that Galileo has somewhat overestimated the heights of any mountains that could be determined by his method, actually Galileo's understanding of the heights of lunar mountains was closer to reality than his ideas of the heights of mountains right here on earth!

CALCULATION TO SHOW THAT THE HEIGHT OF SOME LUNAR MOUNTAINS EXCEEDS FOUR ITALIAN MILES

I think that it has been sufficiently made clear, from the explanation of phenomena which have been given, that the brighter part of the Moon's surface is dotted everywhere with protuberances and cavities; it only remains for me to speak about their size, and to show that the ruggednesses of the Earth's surface are far smaller than those of the Moon's; smaller, I mean, absolutely, so to say, and not only smaller in proportion to the size of the orbs on which they are. And this is plainly shown thus:—As I often observed in various positions of the Moon with reference to the Sun, that some summits within the portion of the Moon in shadow appeared illumined, although at some distance from the boundary of the light (the terminator), by comparing their distance with the complete diameter of the Moon, I learnt that it sometimes exceeded the one-twentieth (1/20th) part of the diameter. Suppose the distance to be exactly 1/20th part of the diameter, and let the diagram represent the Moon's orb, of which CAF is a great circle, E its centre, and CF a diameter, which consequently bears to the diameter of the Earth the ratio 2:7; and since the diameter of the Earth, according to the most exact observations, contains 7000 Italian miles. CF will be 2000, and CE 1000; and the 1/20th part of the whole CF, 100 miles. Also let CF be a diameter of the great circle which divides the bright part of the Moon from the dark part (for, owing to the very great distance of the Sun from the Moon this circle does not differ sensibly from a great one), and let the distance of A from the point C be 1/20th part of that diameter; let the radius EA be drawn, and let it be produced to cut the tangent line GCD, which represents the ray that illumines the summit, in the point D.



Then the arc CA or the straight line CD will be 100 of such units, as CE contains 1,000. The sum of the squares of DC, CE is therefore 1,010,000, and the square of DE is equal to this; therefore the whole ED will be more than 1,004; and AD will be more than 4 of such units, as CE contained 1,000. Therefore the height of AD in the Moon, which represents a summit reaching up to the Sun's ray, GCD, and separated from the extremity C by the distance CD, is more than 4 Italian miles; but in the Earth there are no mountains which reach to the perpendicular height even of one mile. We are therefore left to conclude that it is clear that the prominences of the Moon are loftier than those of the Earth.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Introduction of the Copernican System to England

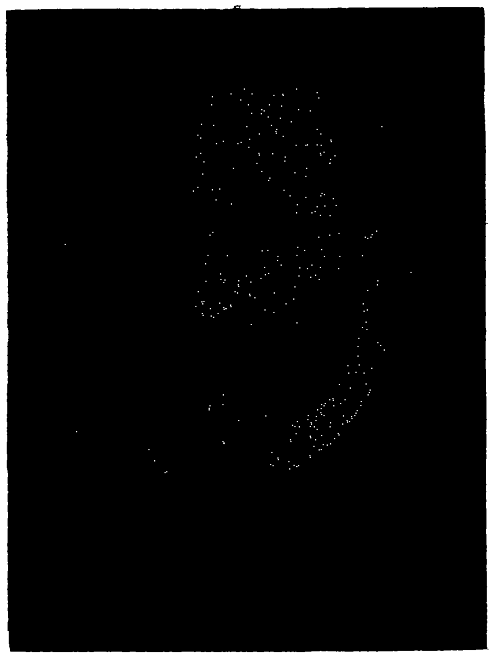
I. Robert Recorde and the *Castle of Knowledge*

A great amount has been written about the impact of the Copernican ideas on Italy, and the violent upheavals, even persecutions, which resulted there. This is natural because Galileo alone stands out as a dramatic figure representing a new observational era in astronomy, with its conflict with existing ideas. It is however unfortunate that in our astronomical literature the reception of the Copernican ideas in one country should almost completely overshadow the reactions which they received elsewhere. It is natural that histories of astronomy concern themselves almost entirely with the men who have actually added new knowledge to the subject. However, the astronomical torch can be carried by teachers as well as by research scientists, and it is a pity that, when centuries elapse, these astronomical torch-bearers tend to be forgotten. For example, Berry in his excellent *A Short History of Astronomy* dismisses the English work of this period with a single paragraph.

We have already mentioned in this section (this Journal, May-June, 1949) that the name of Jean Sylvain Bailly is more familiar to students of history than to students of astronomy. Similarly in the case of several sixteenth-century English astronomers, the students of English history and literature know them far better than the twentieth-century astronomers do. For this reason we propose to print a series of articles on these men who deserve much wider recognition by astronomers to-day.

For this material we are largely indebted to a masterly volume by Francis Rarick Johnson, *Astronomical Thought in Renaissance England,* published by the Johns Hopkins Press, Baltimore, 1937, pp. 357. This volume is one which every student of the history of astronomy should read. It represents a vast amount of research on the part of the author. Many extracts from the early works of the sixteenth century are given, and some of these, along with Johnson's comments, we shall reprint, by kind permission of the Johns Hopkins Press, in order that our readers may see for themselves the intelligent thinking of English scientists.

According to Johnson, the earliest known discussion of the Copernican system in English was published by Robert Recorde in 1556 in his volume, the *Castle of Knowledge*. Robert Recorde is best known and usually referred to as a mathematician—it was he who invented the sign of equality, =, which we use to-day. However, a brief survey of his life as given in the *Dictionary of National Biography* indicates that he dipped into practically all fields of learning, and was well qualified in many of them. Since the publication of this biography more recent research (for example, by Frances Marguerite Clarke in *Isis*, vol. 8, 1926) has unearthed new facts, and rendered obsolete some of the information in it. A hitherto unknown portrait of Recorde has also come to light in recent times. Purchased at a sale at Harrow by Rev. Done Bushell, it was first published by David Eugene Smith in the *American Mathematical Monthly*, vol. 28, 1921, from which it is reproduced here. The original is now in the National Portrait Gallery in London.



ROBERT RECORDE, 1510(?)–1558

Born about 1510, of a good family in Tenby in Pembroke, Recorde was admitted to Oxford in 1525, and received a B.A. there, and perhaps an M.A. as well. In 1531 he was made a Fellow of All Souls'. Then he proceeded to Cambridge where he achieved a high reputation as a teacher. He read and taught mathematics and medicine, "which he rendered clear to all capacities to an extent wholly unprecedented." At Cambridge he received the degree of M.D. in 1545. After this he returned to Oxford where he taught arithmetic and mathematics, rhetoric, anatomy, music, astrology, and cosmography. There is no record that he ever married. In 1547 he was a practising physician in London, and became physician to King Edward VI and Queen Mary.

In 1549 he was appointed Comptroller of the mint at Bristol, and in 1551 Surveyor of the Mines and Monies of Ireland. This latter seems to have been a difficult post. At any rate, after some years of it Recorde was imprisoned, and died in King's bench prison at Southwark in 1558. The exact reason for his imprisonment remains unknown. It may have been connected with his management of the mines in Ireland; or it may have been of a political or religious nature. It appears probable that whatever the offence, it was not the sort which leads to imprisonment now in a democratic country.

He was conversant with every branch of natural history and with the coinage; he knew Anglo-Saxon, was "no mean divine," and was acquainted with law. As a zealous antiquary he made a large collection of historical and other ancient manuscripts. He was the founder of the English school of mathematical writers, the first to introduce algebra into England, and the first to write in English on arithmetic and astronomy. In addition to the *Castle of Knowledge,* he published other excellent books including the *Grounde of Artes,* the *Pathway to knowledg,* and the *Whetstone of Witte.*

Johnson describes the *Castle of Knowledge* as follows (page 125):

> *The Castle of Knowledge* is a handsomely printed small folio of three hundred pages, containing many well-designed illustrations and geometrical diagrams. The book was dedicated to Queen Mary, and had a prefatory epistle addressed to Cardinal Pole. The plan and arrangement of the work demonstrate Recorde's skill and insight as a teacher. He particularly emphasizes the importance of the proper method and order in instruction, insisting, in the first place, that the student must be well grounded in arithmetic and geometry before attempting to learn astronomy. For this reason, Recorde expects his two earlier books, the *Grounde of Artes* and the *Pathway to knowledg* to have been thoroughly mastered before beginning the *Castle.* Assuming this preparation, Recorde proceeds, in accord with his ideas of the proper order in teaching, to present the fundamental principles and concepts underlying the science of astronomy, having the student construct his own celestial sphere to assist him in grasping these basic ideas.

Like the two previous books, the *Castle* took the form of dialogues between master and pupil, a popular method of presenting knowledge in those days. We quote a passage, *Castle,* pp. 164–5, which shows that Recorde considered that Aristotelian and Ptolemaic arguments against the earth's rotation were false, and in which he suggests that he will propound the revolution of the earth when the student has become intelligent enough to understand it. Though this passage is in early English spelling, it should not give the reader much difficulty; the most noticeable change is that our present letter v is a u in this period.

> Master. But as for the quietnes of the earth, I neede not to spende anye tyme in proouing of it, syth that opinion is so firmelye fixed in moste mennes headdes, that they accopt it mere madnes to bring the question in doubt. And therfore it is as muche follye to trauaile to proue that which no man denieth, as it were with great study to diswade that thinge, which no man doth couette, nother any manne alloweth: or to blame that which no manne praiseth, nother anye manne lyketh.

Scholar. Yet sometime it chaunceth, that the opinion most generally receaued, is not moste true.

Master. And so doo some men iudge of this matter, for not only Eraclides Ponticus, a great Philosopher, and two great clerkes of Pythagoras schole, Philolaus and Ecphantus, were of the contrary opinion, but also Nicias Syracusius, and Aristarchus Samius, seeme with strong arguments to approue it: but the reasons are to difficulte for this firste Introduction, & therfore I wil omit them till an other time. And so will I do the reasons that Ptolemy, Theon & others doo alleage, to prooue the earthe to bee without motion: and the rather, bycause those reasons doo not proceede so demonstrablye, but they may be answered fully, of him that holdeth the contrarye. I meane, concerning circularre motion: marye direct motion out of the centre of the world, seemeth more easy to be confuted, and that by the same reasons, whiche were before alleaged for prouing the earthe to be in the middle and centre of the worlde.

Scholar. I perceaue it well: for as if the earthe were alwayes oute of the centre of the worlde, those former absurdities woulde at all tymes appeare: so if at anye tyme the earthe shoulde mooue oute of his place, those inconueniences would then appeare.

Master. That is trulye to be gathered: howe bee it, Copernicus, a man of greate learninge, of muche experience, and of wondrefull diligence in obseruation, hathe renewed the opinion of Aristarchus Samius, and affirmeth that the earthe not only moueth circularlye about his owne centre, but also may be, yea and is, continually out of the precise cetre of the world 38 hundreth thousand miles: but bicause the vnderstanding of that controuersy dependeth of profounder knowledg then in this Introduction may be vttered conueniently, I will let it passe tyll some other time.

Scholar. Nay syr in good faith, I desire not to heare such vaine phantasies, so farre againste common reason, and repugnante to the consente of all the learned multitude of Wryters, and therefore lette it passe for euer, and a daye longer.

Master. You are to yonge to be a good iudge in so great a matter: it passeth farre your learninge, and theirs also that are muche better learned then you, to improue his supposition by good argumentes, and therefore you were best to condemne no thinge that you do not well vnderstand: but an other time, as I sayd, I will so declare his supposition, that you shall not only wonder to hear it, but also peraduenture be as earnest then to credite it, as you are now to condemne it.

Recorde's learning was of wide range and his scholarship was thorough. He had studied the Greek ideas of science and philosophy as well as those of Copernicus. Toward them all he preserved an intelligent and unbiased attitude. This is exemplified in a remark concerning Ptolemy (*Castle*, p. 127) which places him well ahead of many men of his, and earlier times, who accepted the voice of authority without question.

No man can worthely praise Ptolemye, his trauell being so great, his diligence so exacte in obseruations, and conference with all nations, and all ages, and his reasonable examination of all opinions, with demonstrable confirmation of his owne assertion, yet muste you and all men take heed, that both in him and in al mennes workes, you be not abused by their autoritye, but euermore attend to their reasons, and examine them well, euer regarding more what is saide, and how it is proued, then who saieth it: for autoritie often times deceaueth many menne.

In conformance with these beliefs, Recorde pointed out errors in the texts of ancient Greek authors, and proceeded to make corrections in them. For example, Proclus states that the star Canopus which can barely be seen above the horizon at Rhodes was invisible at Alexandria, further south. Recorde (*Castle,* p. 164) says that this is "contrary to common sense," and emends the statement to read "of good appearance," i.e. plainly visible, since Canopus rises several degrees above the horizon at Alexandria.

Recorde accepted the highest knowledge of the Greeks, however, when he adopted the Platonic idea that the secrets of the universe will be unfolded by mathematics. Introductions to his works contain loud praises of the mathematical sciences, showing the reverence in which he held them. An example is from the preface to the *Whetstone of Witte,* where Recorde says:

> It is confessed emongeste all men, that knowe what learnyng meaneth, that besides the Mathematicalle arts, there is noe vnfallible knoweledge, except it bee borrowed of them.

In summary, Johnson ranks the *Castle of Knowledge* as the outstanding astronomical textbook published anywhere in the sixteenth century. This places Recorde as one of the great astronomical torch-bearers. Johnson evaluates the *Castle of Knowledge* as follows:

> Enough has been said of Recorde's textbook on astronomy to show how misleading is the statement in the *Cambridge History of English Literature* that Francis Bacon "was the first to write an important treatise on science or philosophy in English." The *Castle of Knowledge* deserves to rank as the outstanding introduction to the science of astronomy published during the sixteenth century. No other book of its type, either in Latin or in one of the European vernaculars, rivals it in its combination of intelligent pedagogical method, sound scholarship, literary style, and truly scientific attitude toward ancient authorities. Its value was recognized by Recorde's fellow countrymen, and it became the standard work on the subject, holding this place for over half a century. The copyright was jealously guarded, all transfers being entered in the Stationers' Register, and a new edition printed as late as 1596. Recorde's book was imitated, three years after its first publication, by William Cuningham's *Cosmographical Glasse,* and copies of both of these works were included in the ship's library on Frobisher's first voyage, in 1576.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Introduction of the Coppernican System to England

II. John Dee and the Circle at Mortlake

John Dee (1527–1608), ardent mathematician and astrologer, was Robert Recorde's successor in the dispersal of knowledge of the Copernican system through England. During the third quarter of the sixteenth century, John Dee was the guiding spirit of the English school of mathematics. In fact, after Robert Recorde's death in 1558, there was no one to equal him. As a writer in English he was inferior both to Recorde and to his own pupil Thomas Digges, who was to follow him. In learning and scholarship, however, Dee was probably superior to the very remarkable Recorde. Most of Dee's important printed works were in Latin, and he was therefore better known on the Continent than was Recorde.

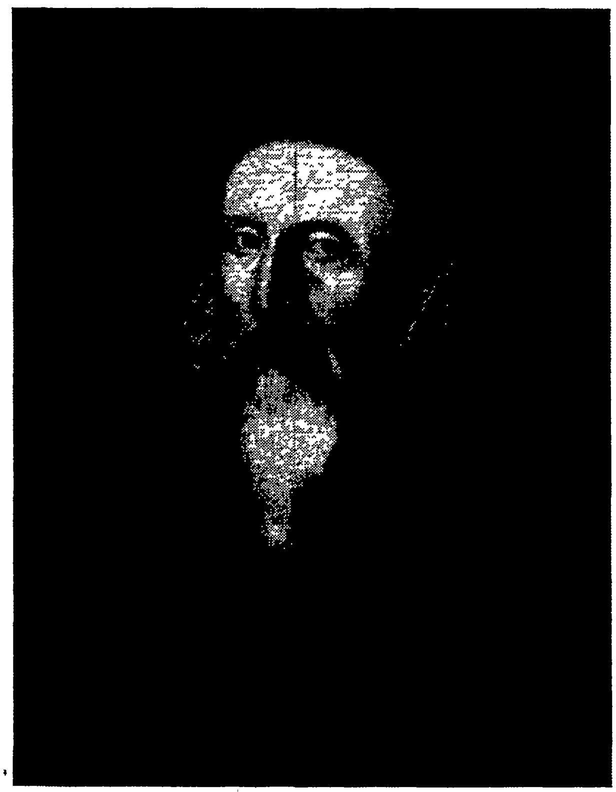
In the same year, 1556, in which Robert Recorde published the first discussion of the Copernican system in English, John Dee wrote a Latin preface to a revision in Latin of Reinhold's Prutenic Tables, done by John Feild. Dee stated that he had persuaded his friend Feild to compile these tables on the basis of the Copernican theory because the old tables would not be satisfactory under the new system. He also expressed the hope in this preface that the "Herculean labours" of Copernicus, Rheticus and Reinhold had become known to the English people. Because Dee spoke with the voice of authority, his opinion was a great boost for the Copernican system among the educated people of England.

On the authority of Francis Johnson, from whose valuable book *Astronomical Thought in Renaissance England* we have been given permission to quote by The Johns Hopkins Press, for the two decades after Recorde's death in 1558, Dee was the supreme scientific authority in England. He was chief advisor to the principal English voyages of the period, beginning in 1553 with the first Muscovy venture, on which Recorde also gave advice. A good account of his career has been published by Charlotte Fell Smith, *John Dee*, London, 1909, from which we also quote.

John Dee was born in London, the son of Rowland Dee. The exact date of his birth is known because of his interest in astrology, from a horoscope of his own drawing, as July 13, 1527. He had a Welsh descent of great genealogical antiquity. His father held a court appointment as gentleman server to Henry VIII. He was taught Latin at an early age, and attended the Grammar School at Chelmsford, which has always claimed him as one of its most famous alumni. In November 1542, at the age of fifteen, he left Chelmsford and entered St. John's College, Cambridge. In his autobiography, *Compendious Rehearsall,* he gives a vivid account of his diligence as a student. It would appear that he had both an iron constitution and an iron will.

> In the years 1543, 1544, 1545, I was so vehemently bent to studie, that for those years I did inviolably keep this order: only to sleep four houres every night; to allow to meate and drink (and some refreshing after) two houres every day; and of the other eighteen houres all (except the tyme of going to and being at divine service) was spent in my studies and learning.

In 1546 he received his B.A. degree from St. John's College. Later this same year Trinity College was founded by Henry VIII, and Dee was appointed one of the original fellows, and under reader in Greek. Here he



JOHN DEE, 1527–1608

created quite a sensation by the mechanical arrangements he made in staging a Greek play when a scarabaeus carrying an actor was made to mount to the ceiling. Indeed, his reputation as a sorcerer apparently dates from this incident. He also engaged in sterner studies, however, turning to astronomy where he made "thousands of observations (very many to the hour and minute) of the heavenly influences and operation actuall in this elementall portion of the world." These were later published in various Ephemerides.

His first journey abroad was made in May 1547 to confer with the learned men of the Dutch Universities in mathematics. He spent several months in the Low Countries, forming close friendships with noted men such as Gerard Mercator. Indeed, he brought back to Cambridge two large globes made by Mercator, and an armillary ring and staff of brass. These he later gave to the Fellows and students of Trinity College, but they are not now to be found.

In 1548 he entered the University of Louvain as a student, and acquired a great reputation for learning there. Many distinguished people came to Louvain from various countries in Europe, to see Dee and learn from him. Among the more interesting names we read one of special astronomical interest, Sir William Pickering of England, afterwards ambassador to France, who came to study astronomy "by the light of Mercator's globes, the astrolabe, and the astronomer's rings of brass that Frisius had invented." It is presumed that Dee graduated from Louvain as Doctor, since that title was used for him afterwards.

In 1550 he moved on to Paris where he gave a course of free public lectures and readings on Euclid. Apparently his lectures were an astonishing success. His audience was so large that the mathematical schools would not hold them, and many eager students were forced to climb up outside the windows. From this vantage point, if they could not hear the speaker they could at least see him! Few modern lecturers have the satisfaction of seeing their audience rise to such a height of enthusiasm. From these years spent in Louvain and Paris, Dee for the rest of his life maintained a lively friendship and correspondence with professors and doctors in almost every important university on the Continent.

Although he was invited to become a King's Reader in mathematics at Paris University, he had made up his mind to return to England, and did so in 1551. Upon his return he obtained an introduction to Secretary Cecil and to King Edward VI. He had already written and dedicated to the young King two manuscripts: *De usi Globi Coelestis,* 1550, and *De nubium, solis, lunae, ac reliquorum planetarum,* 1551. A happy result of the dedication was the gift to Dee of an annual royal pension of a hundred crowns, afterwards exchanged to his disadvantage for a rectory in Worcestershire. From this time on Dee proved to be a close friend and advisor of the royal family. When Mary Tudor succeeded her brother as queen in 1553, Dee was asked to calculate her nativity. He began a correspondence with Princess Elizabeth and cast her horoscope. Apparently by now Dee's powers of "magic" had become so well known that he was accused by one George Ferrys of striking one of his children blind and killing another by magic, and working a spell against the Queen. Dee was put into prison on these charges, but finally was cleared in 1555. His astrological influence was very great, and when Elizabeth succeeded her sister on the throne, Dee was commissioned to name an auspicious day for the coronation. Though nowadays we have no faith in astrology, we have to admit that his selection of January 14, 1559 was certainly the start of an era of glory for England and Queen Elizabeth.

His friendship with Queen Elizabeth continued throughout her lifetime, and she relied upon him in moments of stress. When a wax image of the Queen was found lying in Lincoln's Inn Fields with a huge pin stuck through the breast (an omen of impending death), messengers were hurriedly dispatched to Dee who hastened to solace her Majesty, and assure her she would come to no harm. When the famous comet of 1576 appeared, throwing the Queen and the Court into a perturbed state, Dee was summoned to give his interpretation of the phenomenon. His home at Mortlake on the Thames was near the favourite residences of Elizabeth, and the journey to and from London was made by water. When Dee became dangerously ill at Mortlake in 1571, Elizabeth sent two of her own physicians to attend him. She would sometimes call on him when she went out riding in Richmond Park. One windy day in March 1576 she seems to have chosen an inauspicious moment for her visit, as Dee's first wife (whose identity is unknown) had been buried just four hours previously. This unfortunate circumstance, however, did not prevent the Queen from seeing and admiring Dee's wonderful convex mirror, after which she rode away "amused and merry, leaving the philosopher's distress at his recent bereavement assuaged for the moment by such gracious marks of royal interest and favour." Incidentally one of Dee's mirrors, possibly this one, a solid pink-tinted glass the size of an orange, is now in the British Museum.

The letter that Dee wrote the Queen from the Continent in 1588, to congratulate her upon the defeat of the Spanish Armada has become a classic example of the prose and erudition of the day, and is reprinted by Ellis in his *Letters of Eminent Men,* and in facsimile by Charlotte Smith. It reads as follows:

Most Gratious Soueraine Lady, The God of heauen and earth, (Who hath mightilie, and evidently, given vnto your most excellent Royall Maiestie, this Wunderfull Triumphant Victorie, against your mortall enemies) be allwaies, thanked, praysed, and glorified; And the same God Almightie, euermore direct and defend your most Royall Highnes from all evill and encumbrance: and finish and confirme in your most excellent Maiestie Royall, the blessings, long since, both decreed and offred: yea, euen into your most gratious Royall bosom, and Lap. Happy are they, that can perceyue, and so obey the pleasant call, of the mightie Ladie, OPPORTVNITIE. And, Therfore, finding our duetie concurrent With a most secret beck, of the said Gratious Princess. Ladie OPPORTVNITIE, NOW to embrace, and enioye, your most excellent Royall Maiesties high favor, and gratious great Clemencie, of CALLING me, Mr. Kelley, and our families, hoame, into your Brytish Earthly Paradise, and Monarchie incomparable: (and, that, abowt an yere since: by Master Customer Yong, his Letters,) I, and myne, (by God his fauor and help, and after the most convenient manner, We can,) Will, from hencefurth, endeuour our selues, faithfully, loyally, carefully, warily, and diligently, to ryd and vntangle our selues from hence: And so, very devowtely, and Sowndlie, at your Sacred Maiesties feet, to offer our selues, and all, Wherein, we are, or may be hable, to serve God, and your most Excellent Royall Maiestie. The Lord of Hoasts, be our help, and gwyde, therein: and graunt vnto your most excellent Royall Maiestie, the Incomparablest Triumphant Raigne, and Monarchie, that euer was, since mans creation. Amen.

Trebon. in the kingdome of Boemia
the 10th. of Nouebre: A. dm: 1588: style Weser
Your Sacred and most excellent
Royall Maiesties
most humble and dutifull
subiect, and Servant
John Dee

In 1581 Dee had become acquainted with a charlatan Edward Kelley. From then on Dee's philosophical researches were concerned entirely with necromancy. Dee and Kelley followed the lure of the philosopher's stone around Europe, for some years in company with Albert Laski. Shortly after writing his famous letter to the Queen, Dee returned to England in 1589, and in 1595 was made Warden of Manchester College. In this position he had to make official visits of inspection to the Grammar School. Dee's remarks upon his visit of August 5, 1600 are interesting in that they show that official displeasure at students' knowledge is not confined to the twentieth century.

I visited the Grammar Schole, and fownd great imperfections in all and every of the scholers, to my great grief.

His last years were spent in great poverty, and failing health of body and spirit. When he needed funds, he sold pieces of his silver plate which he had acquired in more auspicious times. His last entry in his diary was of April 6, 1601, but he did not pass away until December 1608. He must have been a striking figure. His cousin Aubrey describes him as "of a very fair clear sanguine complexion, with a long beard as white as milk—a very handsome man—tall and slender. He wore a goune like an artist's goune with hanging sleeves."

It is unfortunate that in the later years Dee's interest in astrology and spiritualism overshadowed, both for his contemporaries and his biographers, his knowledge and love of sound science. The populace regarded Dee, by 1564, not as a highly equipped mathematician, geographer and astronomer, but rather as a conjuror and magician of doubtful reputation. One of Dee's finest works is the English preface to Henry Billingsley's first English translation of Euclid's *Elements* in 1570. Even in this preface

Dee feels called upon to defend himself from charges of having dealings with the devil. All critics agree that this preface was an outstanding piece of work, actually a treatise on the proficiency and advancement of scientific learning written 35 years before Francis Bacon's English work. According to Charlotte Smith:

A study of this preface alone must convince any reader that the author was no charlatan or pretender, but a true devotee of learning, gifted with a far insight into human progress. He covers in review every art and science then known, and some "until these daies greatly missed" (his comments on music and harmony are truly remarkable), and comes back to his own predilection—arithmetic, "which next to theologie is most divine, most pure, most ample and generall, most profound, most subtele, most commodious and most necessary". He quotes Plato to show how "it lifts the heart above the heavens by invisible lines, and by its immortal beams melteth the reflection of light incomprehensible, and so procureth joy and perfection unspeakable." Speaking of the refraction of light, he foreshadows the telescope as he describes how the captain of either foot or horsemen should employ "an astronomical staffe commodiously framed for carriage and use, and may wonderfully help himself by perspective glasses; in which I trust our posterity will prove more skilfull and expert and to greater purpose than in these days can almost be credited to be possible." Then he alludes to a wonderful glass belonging to Sir William P., famous for his skill in mathematics, who will let the glass be seen. The passage seems to show that looking-glasses were not common, or that this particular one was a convex mirror.

Dee's greatest influence, however, was probably not from his writings but from the circle of people that he gathered about him at his home in Mortlake, and the correspondence which he sent out all over the Continent. When he was over forty, Dee established himself at his mother's house at Mortlake on the Thames. It was an old rambling place, situated between the church and the river. Dee added to it bit by bit, so that eventually it had laboratories for experiments, libraries and rooms for workers. Johnson considers that John Dee and the people associated with him at Mortlake constituted the scientific academy of England.

One of the works undertaken by Dee and his pupil Thomas Digges was observation of the historical new star of 1572, which Tycho Brahe (with whom Dee was acquainted) also observed. This new star, representing as it did, change in what Aristotle considered a changeless region of the heavens, struck a great blow to Aristotelian theory. Dee and Digges ranked next to Tycho in their parallax measures establishing that this star was in the "celestial, not in the elementary region" of the sphere.

Of the importance of Mortlake, Johnson says (page 137):

The center of Dee's activities was his home at Mortlake, near London, where his library, his laboratory, and his astronomical instruments were located. London, and not the universities, became the scene of scientific study and scholarship in this period.

During the third quarter of the century, John Dee and his friends and pupils constituted the scientific academy of England. Through Dee's intimate acquaintance and correspondence with all the most eminent scientists on the Continent, the English group was kept in constant touch with all the latest ideas and discoveries originating abroad. Dee's unheeded plea to Queen Mary that the ancient books and manuscripts dispersed with the destruction of the monasteries be recovered, in order to found therewith a great National Library, is well known. When he perceived that nothing was going to come of this suggestion, he set about forming his own library of scientific books and manuscripts, which, by 1583, had grown to over four thousand volumes. It was undoubtedly the greatest scientific library in England, and probably not surpassed in Europe, for Dee not only had collected a vast store of important medieval manuscripts on science (which he could get the more readily because they were little valued by the plunderers of the monastic houses), but he had also seen to it that all the latest printed works on the mathematical sciences should be found on his shelves.

This great library was always at the disposal of Dee's fellow scientists among his friends and pupils. If one believes that the first essential and the true center of any university is its library, Dee's circle might truly be termed the scientific university of England during the period from about 1560 to 1583. Mention may be made, in passing, of the presence of two copies of Copernicus' *De revolutionibus* on Dee's shelves, along with the works of Nicholas of Cusa and the first Greek edition of Ptolemy's *Almagest.*

Dee also realised the importance of the technical arts in scientific work and their value in developing and perfecting new and better instruments for scientific experiment and observation. Working with Richard Chancellor, who was an expert mechanician as well as a scholar, he constructed a number of astronomical instruments with specially devised transverse divisions on the scale, so as to make possible finer and more accurate readings. His most prized instrument was a huge radius astronomicus or cross-staff, ten feet long, "having the staff and cross very curiously divided into parts equall, after Richard Chancellour's Quadrante-manner. The great instrument was in such a frame placed and layd, that it might most easily be weilded of any man to any position for practise in heavenly observation or mensurations on earth."

The position of Recorde and Dee as the outstanding and most influential English scientists at the beginning of the second half of the sixteenth century was universally conceded by their contemporaries among their countrymen. Consequently, their support of the Copernican theory has a special significance in the history of English astronomy.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Introduction of the Copernican System to England

III. Thomas Digges and *A Perfit Description of the Caelestiall Orbes.*

Pupil and close associate of John Dee, Thomas Digges made a lasting contribution to English astronomy, though the period during which he did most of his work was only a decade. His most famous publication, however, one on the Copernican system, was printed in at least seven editions, from 1576 down to 1605, thereby exerting a great influence on English thought during the last quarter of the sixteenth century. A full description of the work of Thomas Digges is given by Francis R. Johnson in his volume, *Astronomical Thought in Renaissance England*, from which we quote by kind permission of The Johns Hopkins Press.

Thomas Digges was born about 1546, the son of the famous mathematician Leonard Digges, who died when Thomas was thirteen. After his father's death Thomas became closely associated with John Dee, whom he calls his "second parent" in mathematics and astronomy, stating that Dee had "sown many seeds of those most sweet sciences" in his mind during "his most tender years" and had "nurtured and increased others which previously had been sown in a most loving and faithful manner by his father."

By 1573, when Digges was only twenty-seven, he ranked with Dee as one of the two leading mathematicians and astronomers in England. In February of that year he published his book on the new star of 1572, the *Alae seu Scalae Mathematicae*. Next to Tycho's this book contains the best published observations of this famous star. Tycho himself thought so highly of Digges' observations that in his *Progymnasmata*, which was a summary of all the treatises written on this star, he devoted over thirty pages to Digges' work, about twice as many as any other single work received. Johnson thinks that the instrument Digges used was the famous ten-foot cross staff designed by Chancellor and Dee, and mentioned in our article on the latter. Dee's own work on this star was published a little later.

Tycho, Dee and Digges all eagerly made measures of the new star in an attempt to determine its shift in parallax, and thereby prove the Copernican theory of the revolution of the earth. It is one of the ironies of early science that they selected a star of the highest possible absolute magnitude, a supernova, hence the one kind of star visible to the ends of the universe, and therefore on an average having the smallest parallactic shift of any! Even with our modern instruments we could not measure a direct trigonometric parallax for this star.

Camden, in his annals of the reign of Elizabeth, says (Johnson, p. 156):

I know not whether it be worth the labour to mention that which all historians of our time have recorded, to wit, that in the month of November, a new star, or if you will, a phenomenon was seen in the constellation of Cassiopeia, which (as I my self observed) in brightness excelled Jupiter in the Perigee or nearest point of the excentric and epicycle: and in the same place it continued full sixteen months, being carried about with the daily motion of the heaven.

Thomas Digges, and John Dee, Gentlemen, and Mathematicians amongst us, have learnedly proved by paralactic doctrine, that it was in the celestial, not in the elementary region: and they were of opinion that it vanished little by little in ascending. Certainly after the eighth month all men perceived it to grow less and less.

The importance of this new star in scientific thought can hardly be over-estimated. Once its lack of parallax was established, it had to be admitted that the star was farther away than the moon. This automatically meant a rejection of the iron-clad Aristotelian idea that there could be no change in the fixed realm of the stars.

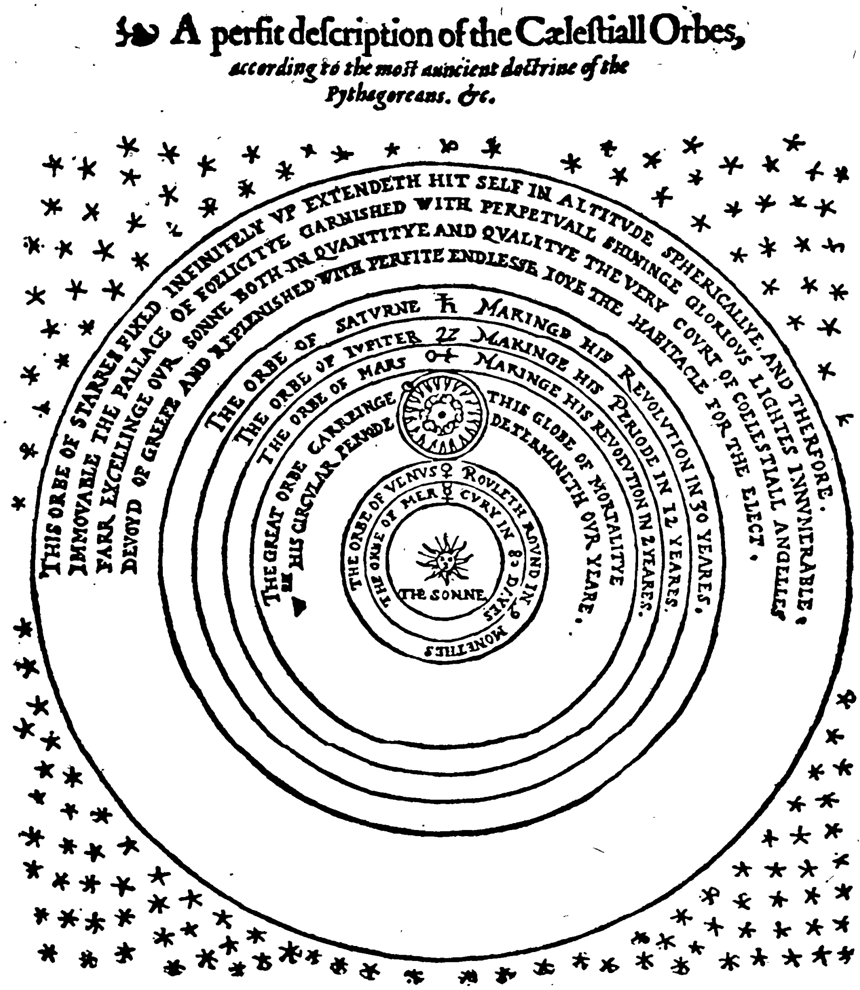
The *Alae* consists of new trigonometric theorems for determining parallax, Digges' observations of the new star, and a discussion of the value of the observational approach in science as opposed to a theoretical one. Digges states (as translated by Johnson, page 159):

I have perceived that the Ancients progressed in reverse order from Theories, which were clearly false, to seek after true Parallaxes and distances, when they ought rather to have proceeded in inverse order, and from Parallaxes, which have been observed and are known, they ought to have examined Theories. By this method it would not be at all difficult, if this remarkable Phenomenon should persist for a long time, to discern by exact judgment whether the Earth lies quiet and immovable in the center of the World, and whether that huge mass of moving and fixed Orbs rotates in a circle by a most rapid course in the space of 24 hours, or rather, that that immense sphere of fixed stars remains truly fixed and that apparent motion occurs only from the circular rotation of the Earth with reference to the celestial Poles themselves. . . .

Therefore, I have thought not only that a treatment of this subject is necessary, but also that Mathematics has rules for measuring the location, distance and magnitude of this stupendous star, and for manifesting the wonderful work of God to the whole race of mortals (who strive to understand something Celestial and lie not wholly buried in the earth); also for examining Theories and establishing the true System of the Universe, as well as for measuring most accurately the Parallaxes of Celestial Phenomena.

This book made Thomas Digges one of the leaders of scientific thought in England, but it was written in Latin and understood by scholars only.

In 1576 a new edition of the famous work by Leonard Digges, *Prognostication euerlasting*, was being prepared and Thomas seized the opportunity to add a supplement to his father's work. This supplement gave an English translation of the principal sections of Book I of Copernicus' *De revolutionibus*, and a diagram of the heliocentric system which we reproduce here. It is entitled "A Perfit Description of the Caelestiall Orbes



according to the most aunciente doctrine of the Pythagoreans, latelye reuiued by Copernicus and by Geometricall Demonstrations approued."

The translation of Copernicus' work is an excellent one, done phrase by phrase in the Elizabethan style rather than word by word, and various interpretations by Digges are worked into it. The most noteworthy addition is Digges' conception that the stars are at varying distances from us, not fixed to one finite outer wall. Johnson says that Digges "was the first modern astronomer of note to portray an infinite, heliocentric universe, with the stars scattered at varying distances throughout infinite space."

Digges' diagram of the universe, first printed in 1576 and then in all later editions of the *Prognostication*, was the same as the diagram in *De revolutionibus* for the planetary system, but instead of representing a sphere for the stars, it scattered them around at varying distances to the edge, and inserted the legend:

This orbe of starres fixed infinitely vp extendeth hit self in altitude sphericallye, and therfore immouable; the pallace of foelicitye garnished with perpetuall shininge glorious lightes innumerable, farr excellinge our sonne both in quantitye and qualitye . . .

Into the fabric of the translation of Copernicus, Digges wove the following paragraph of his own, appraising the position of the earth, "this litle darcke starre", in the vast universe (Johnson, page 165):

Heerein can wee neuer sufficiently admire thys wonderfull & incomprehensible huge frame of goddes woorke proponed to our senses, seinge fyrst thys baull of y[e] earth wherein we moue, to the common sorte seemeth greate, and yet in respecte of the Moones Orbe is very small, but compared with *Orbis magnus* wherein it is caried, it scarcely retayneth any sensible proportion, so merueilously is that Orbe of Annuall motion greater than this litle darcke starre wherein we liue. But that *Orbus magnus* beinge as is before declared but as a poynct in respect of the immesity of that immoueable heauen, we may easily consider what litle portion of gods frame, our Elementare corruptible worlde is, but neuer sufficiently be able to admire the immensity of the Rest. Especially of that fixed Orbe garnished with lightes innumerable and reaching vp in *Sphaericall altitude* without ende. Of whiche lightes Celestiall it is to bee thoughte that we onely behoulde sutch as are in the inferioure partes of the same Orbe, and as they are hygher, so seeme they of lesse and lesser quantity, euen tyll our sighte beinge not able farder to reache or conceyue, the greatest part rest by reason of their wonderfull distance inuisible vnto vs. And this may wel be thought of vs to be the gloriouse court of y[e] great god, whose vnsercheable worcks inuisible we may partly by these his visible coiecture, to whose infinit power and maiesty such an infinit place surmountinge all other both in quantity and quality only is conueniente. But because the world hath so longe a tyme bin carryed with an opinion of the earths stabilitye, as the contrary cannot but be nowe very imperswasible, I haue thought good out of *Copernicus* also to geue a taste of the reasons philosophicall alledged for the earthes stabilitye, and their solutions, that sutch as are not able with *Geometricall* eyes to beehoulde the secrete perfection of *Copernicus Theoricke*, maye yet by these familiar, naturall reasons be induced to serche farther, and not rashly to condempne for phantasticall, so auncient doctrine reuiued, and by *Copernicus* so demonstratiuely approued.

Among the more important ideas in other published works of Digges are those on "perspective glasses". These remarks show Digges' familiarity with the essential optics of a telescope. Almost certainly Digges had something resembling a telescope. In 1571 in his *Pantometria*, a book setting forth geometrical methods in surveying, Digges wrote, at the end of Chapter 21 of the first book (Johnson, page 175):

Thus much I thought good to open concerning the effects of a plaine Glasse, very pleasant to practise, yea most exactly seruing for the description of a plaine champion countrey. But marueilous are the conclusions that may be performed by glasses concaue and conuex of Circulare and parabolicall formes, vsing for multiplication of beames sometime the aide of Glasses transparent, which by fraction should vnite or dissipate the images or figures presented by the reflection of other. By these kinde of Glasses or rather frames of them, placed in due Angles, yee may not onely set out the proportion of an whole region, yea represent before your eye the liuely image of euery Towne, Village, &c. and that in as little or great space or place as ye will prescribe, but also augment and dilate any parcell thereof, so that whereas at the first apparance an whole Towne shall present it selfe so small and compact together that yee shall not discerne anye difference of streates, yee may by application of Glasses in due proportion cause any peculiare house, or roume thereof dilate and shew it selfe in as ample forme as the whole towne first appeared, so that ye shall discerne any trifle, or reade any letter lying there open, especially if the sunne beames may come vnto it, as plainely as if you were corporally present, although it be distante from you as farre as eye can discrie: But of these conclusions I minde not here more to intreate, hauing at large in a volume by it selfe opened the miraculous effects of perspective glasses.

Unfortunately there is no known copy of the volume on perspective glasses mentioned in this paragraph. The above quotation, however, should show beyond doubt that the telescope was not the invention of one person, as for example the most commonly cited Jan Lippershey, but was the product of the accumulated ideas of a number of people. And in placing credit for the invention of the telescope, far too little acknowledgement is given to the English group of scientists who worked before 1600. Digges must have had a fairly strong lens or mirror if he was able to read letters lying on tables in his neighbours' houses!

The existence and power of Digges' lenses and mirrors are corroborated in a treatise by William Bourne. This treatise was written about 1580, at the request of Lord Burghley, and discusses the manufacture and properties of convex and concave mirrors, and convex lenses. We read that in those days too the expense of instruments was a hindrance to their development (Johnson, page 177):

For that the habillity of my purse ys not able for to reache, or beare the charges, for to seeke thorowly what may bee done with these two sortes of Glasses, that ys to say, the hollowe or concave glasse: and allso that glasse, that ys grounde and polysshed rounde, and thickest on the myddle, and thynnest towardes the sydes or edges, Therefore I can say the lesse vnto the matter. For that there ys dyvers in this Lande, that can say and dothe know muche more, in thes causes, then I: and specially Mr. Dee, and allso Mr. Thomas Digges, for that by theyre Learninge, they have reade and seene many moo auctors in those causes: And allso, theyre ability ys suche, that they may the better mayntayne the charges: And also they have more leysure and better tyme to practyze those matters, which ys not possible for mee, for to knowe in a nombre of causes, that thinge that they doo knowe. But notwithstanding upon the smalle proofe and experyence those that bee but vnto small purpose, of the skylles and knowlledge of these causes, yet I am assured that the glasse that ys grounde, beynge of very cleare stuffe, and of a good largenes, and placed so, that the beame dothe come thorowe, and so reseaved into a very large concave lookinge glasse, That yt will shew the thinge of a marvellous largeness, in a manner vncredable to bee beleeved of the common people. Wherefore yt ys to bee supposed, and allso, I am of that opinyon, that havinge dyvers, and sondry sortes of these concave lookinge glasses, made of a great largeness, That suche the beame, or forme and facyon of any thinge beeynge of greate distance, from the place, and so reseaved fyrste into one glasse: and so the beame reseaved into another of these concave glasses: and so reseaved from one glasse into another, beeynge so placed at suche a distance, that every glasse dothe make his largest beame. And so yt is possible, that yt may bee helpped and furdered the one glasse with the other, as the concave lookinge glasse with the other grounde and polysshed glasse. That yt ys lykely yt ys true to see a smalle thinge, of very greate distance. For that the one glasse dothe rayse and enlarge, the beame of the other so wonderfully. So that those things that Mr. Thomas Digges hathe written that his father hathe done, may bee accomplisshed very well, withowte any dowbte of the matter: But that the greatest impediment ys, that yow can not beholde, and see, but the smaller quantitye at a tyme.

Is it possible, that Digges, with such perspective glasses in his hands, and with his keen love and understanding of astronomy, never turned the glasses toward the heavens? There are no extant references which state that he did, but two things suggest that he might have done this. One is his steadfast clinging to the Copernican theory in spite of the disappointing failure to measure a parallax for the new star of 1572. Was it because his crude lenses or mirrors had shown him the depth of stars in the Milky Way that Digges was such a stalwart supporter of Copernicus? Was it observations of fainter stars that caused him to scatter the stars at varying distances?

A second suggestion that possibly Digges had looked at the heavens with perspective glasses comes from a list he printed of "Bookes Begon by the Author, heerafter to be published", in 1579, among which was

Commentaries vpon the *Reuolutions* of *Copernicus*, by euidente Demonstrations grounded vpon late *Obseruations*, to ratifye and confirme hys *Theorikes* and *Hypothesis*, wherein also Demonstratiuelie shall be discussed, whether it bee possible vpon the vulgare *Thesis* of the Earthes *Stabilitie*, to delyuer any true *Theorike* voyde of such irregular Motions, and other absurdities, as repugne the whole *Principles* of *Philosophie* naturall, and apparant groundes of common *Reason*.

The reference to "late observations" is both intriguing and important. What these observations were remains unknown. There is however still hope that more references to them may be found. According to Johnson there are numerous letters written by Digges, now in the Public Record Office, the British Museum, and elsewhere, which, if thoroughly studied may yield a clue. It appears obvious that the English astronomers of this period get too little credit for their work because they did not get into print as much knowledge as they possessed.

After 1580 Digges had little opportunity to pursue his scientific investigations before his death in 1595. As so often happens in later years, his various talents made him much in demand for other things, such as the supervision of military fortifications. Furthermore, in 1583 Dee's establishment at Mortlake was sacked by a mob, which destroyed the instruments and laboratories. There were other English scientists, however, who were carrying on experiments with lenses and mirrors, in particular Thomas Harriot, of whose accomplishments we shall write in a later JOURNAL.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Introduction of the Copernican System to England

IV. Thomas Harriot, Sir Walter Raleigh's Surveyor and Topographer.

One of the most remarkable scientists in England at the end of the sixteenth century was Thomas Harriot. He was indeed a worthy successor to the ideas and skill possessed earlier in the century by Robert Recorde, John Dee, and Leonard and Thomas Digges. Unfortunately Harriot, in the three intervening centuries, has been little known as a scientist because he had the failing, common in the English school of this period, of not publishing his knowledge. Only one of his works was published during his lifetime. This was *A Briefe and true report of the new found land of Virginia,* in 1588. Some ten years after his death friends and pupils published his great work on algebra, *Artis Analyticae Praxis.*

Among their contemporaries, these English scientists whom we are discussing in this series of articles, had an outstanding reputation. Francis R. Johnson, in his valuable volume, *Astronomical Thought in Renaissance England,* (from which we quote by kind permission of The Johns Hopkins Press) cites the opinion of Captain Davis, the famous navigator, who listed Harriot, Digges, and Dee as the three English scientists living in 1595 who could surpass all foreign rivals in knowledge and skill. In the dedication of this book, *The Seamans Secrets,* Captain Davis wrote:

> For I think there be many hundreds in England that can in a farre greater measure and more excellent methode expresse the noble art of Nauigation, and I am fully perswaded that our Countrie is not inferiour to any for men of rare knowledge, singular explication, and exquisite execution of Artes Mathematicke, for what Strangers may be copared with M. Thomas Digges Esquire, our Countryman the great Archmastric, and for Theoricall speculations to most cunning calculation, M. Dee and M. Thomas Heriotts are hardly to be matched:

During these intervening centuries, however, little has been heard of Harriot except that he was the author of the Virginia report, previously mentioned, and of a work on algebra. In 1877 a learned gentleman, Henry Stevens of Vermont, founded the Hercules Club, whose purpose was research into early Anglo-American history and literature. Stevens became deeply impressed with the greatness of Harriot, and his undeserved oblivion. Accordingly he gathered together a volume of material which, after Stevens' death was published by his son; *Thomas Hariot: The Mathematician, the Philosopher, and the Scholar,* London, 1900. This volume is now so rare that it can be of little help in spreading information about Harriot unless parts of it are reprinted. Only 195 copies of this book were printed, of which we have been privileged to use Copy no. 45 from the John Crerar Library, Chicago.

Stevens' admiration and respect for Harriot apparently grew enormously as he unearthed rare material by him and about him, including Harriot's will, missing for centuries. He calls Harriot "a man who, though long neglected and half forgotten, must eventually shine as the morning star of the mathematical science in England, as well as that of the history of her Empire in the West." Stevens sums up Harriot's obscurity as follows, (pages 128 and 160):

> The truth seems to be that Hariot was unlucky and fell into oblivion accidentally. He was a man of immense industry and great mental power, but perhaps careless of his scientific and literary reputation. As has been seen, he always had many irons in the fire, and was overtaken by death in the prime of life, leaving, as his will shows, many things unfinished, and none of his papers in a state ready for publication. He was surrounded by the best of friends, but time and opportunity, as so often happens in the affairs of busy men, worked against him, and he was well nigh consigned to forgetfulness. . . .
>
> The Hariot manuscripts, of which there are thousands of folio pages all in his own handwriting, seem to be still in the same confused state in which he left them. He directed that the 'waste' should be weeded out of his mathematical papers and destroyed. But this duty seems, fortunately for us, to have been neglected by his executors.

For an account of Harriot's life, about which not a great deal is known, we will quote liberally from Henry Stevens, as he appears to have made a thorough study. Stevens thinks that Harriot's previous biographer, Anthony à Wood, has not done him justice (page 74).

> Hitherto dear quaint old positive antiquarianly slippery Anthony à Wood in his *Athenae Oxoniensis* embodies nearly all of our accepted notions of this great English mathematician and philosopher. Anthony was indefatigable in his researches into the biography of Hariot who was both an Oxford man and an Oxford scholar. He happily succeeded in mousing out a goodly number of recondite and particular occurrences of Hariot's life. He managed, however, to state very many of them erroneously; and he drew hence some important inferences, the reverse, as it now appears of historical truth. . . .
>
> Thomas Hariot was born at Oxford . . . in 1560. He was a 'batelor or commoner of St. Mary's hall. He 'took the degree of bachelor of arts in 1579, and in the latter end of that year did compleat it by determination in Schoolstreet.' Nothing of his boyhood, or of his family, except a few hints in his will, has come to light.

It is not known precisely at what time Hariot joined Walter Raleigh, who was only eight years his senior. From what their friend Hakluyt says of them both, their intimate friendship and mutually serviceable connection were already an old story as early as 1587. . . . On the 22d of February 1588, present reckoning, Hakluyt wrote from Paris to Raleigh in London, . . .

'But since by your skill in the art of navigation you clearly saw that the chief glory of an insular kingdom would obtain its greatest splendor among us by the firm support of the mathematical sciences, you have trained up and supported *now a long time,* with a most liberal salary, Thomas Hariot, a young man well versed in those studies, in order that you might acquire in your spare hours by his instruction a knowledge of these noble sciences; and your own numerous Sea Captains might unite profitably theory with practice.' . . .

From this extract one might perhaps reasonably infer that Hariot went directly from the University in 1580 at the age of twenty into Raleigh's service, or at latest in 1582 when Raleigh returned from Flanders. As our translation of this important passage is rather a free one the old geographer's words are here added, in his own peculiar Latin. . . .

From this early time for nearly forty years, till the morning of the 29th of October 1618, when Raleigh was beheaded, these two friends are found inseparable. Whether in prosperity or in adversity, in the Tower or on the scaffold, Sir Walter always had his Fidus Achates to look after him and watch his interests. With a sharp wit, close mouth, and ready pen Hariot was of inestimable service to his liberal patron. With rare attainments in the Greek and Latin Classics, and all branches of the abstract sciences, he combined that perfect fidelity and honesty of character which placed him always above suspicion even of the enemies of Sir Walter. He was neither a politician nor statesman, and therefore could be even in those times a faithful guide, philosopher, and friend to Raleigh.

In the year 1585, as has already been stated above, Hariot at the age of twenty-five, went out to Virginia in Raleigh's 'first Colonie' as surveyor and historiographer with Sir Richard Grenville, and remained there one year under Governor Ralph Lane, returning in July 1586, in Sir Francis Drake's home-bound fleet from the West Indies. . . . Hariot is known to have spent some time in Ireland on Raleigh's estates there during the reign of Elizabeth, but it is uncertain when. It may have been between the autumn of 1586 and the autumn of 1588. He was in London in the winter of 1588–89 in time to get out hurriedly his report in February 1589. It is possible, however, that he went to Ireland after his book was out. He was probably the manager of one of the estates there as Governor John White was of another in 1591–93.

Of this Virginia report, Stevens says it "must ever serve as the cornerstone of English American History". Johnson (page 178) quotes a sentence from the original report to show that apparently Harriot had a telescope with him on the Virginia expedition. Amongst the instruments listed on the expedition is "a perspectiue glasse whereby was shewed manie strange sightes." This affords another corroboration of the fact that the English group of scientists were familiar with at least some of the principles of telescopes years before the telescope was "invented" in some other country.

In addition to being companion and adviser to Raleigh, Harriot seems to have had much the same standing with Henry Percy, Earl of Northumberland. Stevens says, on page 93:

At what precise time Hariot, who never deserted Raleigh, became acquainted with Henry Percy, Earl of Northumberland, with whose honored name, next to that of Sir Walter's, his must ever be associated, does not as yet appear. It is known, however, that there was an intimacy between Raleigh and Percy as early as 1586. . . . From this time to the day of Raleigh's triumph on the scaffold there exists plenty of evidence of their continued intimacy.

When therefore the Earl and Raleigh were finally caged together in the Tower for life in 1606 their friendship was of more than twenty years' standing. From this we infer that Hariot also knew Percy almost from the time of his joining Raleigh; but the earliest mention of his name in connection with that of the Earl which we have met with is this of 1596, in the Earl's pay-rolls, still preserved at Sion, and described in the Sixth Report of the Royal Commission of Historical Manuscripts, page 227, 'To Mr. Herytt for a book of the Turk's pictures, 7*s*.' It appears from the same rolls that from Michaelmas 1597 to 1610, if not earlier and later, an annual pension of £80 (not £120, or £150, or £300, as variously stated) was paid to Hariot by the Earl. This pension was probably continued as long as Hariot lived; and besides there are not wanting many marks of the Earl's liberality, friendship, and love for his companion and pensioner, who was long known as 'Hariot of Sion on Thames,' as expressed on his monument. On the Earl's accounts for 1608 there is this entry, 'Payment for repairing and finishing Mr Heriotts house at Sion'.

At what time exactly Hariot took up his residence at Sion the Earl's new seat (purchased of James in 1604) is not known, but probably soon after the Earl was sent to the Tower in 1606. There is preserved a Letter from Sir William Lower addressed to Hariot at Sion dated the 30th of September 1607, and other letters or papers exist showing his continued residence there until near the time of his death in 1621. Wood and many subsequent writers to the present time have confused Sion near Isleworth with Sion College in London. They are totally distinct. Hariot had nothing to do with Sion College, which was not founded until 1630, nine years after his death. The error arose out of the coincidence of Torporley's taking chambers at Sion College on retiring from his clerical profession, and dying there in April 1632, leaving his mathematical books and manuscripts to the College Library. He had been appointed by Hariot to look over, arrange, and 'pen out the doctrine' of his mathematical writings. Torporley's abstracts of Hariot's papers are still preserved in Sion College Library. . . .

It is manifest, however, from many considerations that the noble Earl took a lively and almost officious interest in the public honor and character of his friend, for Hariot appears to have been as careless of his own scientific reputation as his contemporary Shakspeare is said to have been of his literary eminence.

On the other hand, Hariot's interest in the Earl's affairs and family at Sion redound greatly to his credit. He was both an eminent scholar and a remarkable teacher. Earnest students flocked to him for higher education from all parts of the country. Besides the private scientific and professional instruction that from the first he gave to Raleigh, his captains and sea officers, he seems to have had under his scientific tuition and mathematical guidance many young men who afterwards became celebrated; among whom may be mentioned Robert Sidney, the brother of Sir Philip, afterwards Lord Lisle of Penshurst; Thomas Aylesbury of Windsor, afterwards Sir Thomas, the great-grandfather of two queens of England; the late Lord Harrington; Sir William Protheroe and Sir William Lower of South Wales; Nathaniel Torporley of Shropshire; Sir Ferdinando Gorges of Devonshire; Captain Keymis; Captain Whiddon, and many others. Cordial and affectionate letters of most of these men to their venerated master are still preserved. . . .

The Earl had a large family to be educated, and there is reason to believe that in his absence from Sion Hariot was intrusted for many years with the confidential supervision of some of the Earl's personal affairs at Sion, including the education of his children. . . .

This responsible trust gave Hariot a good house and home of his own at Sion, with independence and an observatory. He had a library in his own house, and seems to have been the Earl's librarian and book selector or purchaser for the library of Sion House, as well as for the use of the Earl in the Tower. The Earl was a great book-collector, as appears by his pay-rolls. Books were carried from Sion to the Tower and back again, probably not only for the Earl's own use, but for Raleigh's in his History of the World. Many of these books, it is understood, are still preserved at Petworth, then and subsequently one of the Earl's seats, but now occupied by the Earl of Leconsfield.

Harriot must have achieved a remarkable feat to keep free of the intrigues of the time and visit both Raleigh and Percy. Indeed, Stevens feels very strongly that Harriot and his knowledge were behind Raleigh's volume, the *History of the World.* Stevens describes these years of Harriot's life as follows, when he was "the ears, the eyes, and the hands of these two noble captives" (page 105):

Here (at the tower) Raleigh dragged out his long imprisonment, and (as tersely & truly expressed by his son) was, after thirteen years, beheaded for opposing the very thing he was condemned and sentenced for favouring. The whole story is a bundle of inconsistencies, like that of Henry Percy, the 9th Earl of Northumberland, committed to the Tower in 1606, and his fifteen years' imprisonment. The stories of these two celebrated men are inseparably connected with that of Hariot. But it is not our purpose to trace either Raleigh's or Percy's progress through these long and dreary years any further than is necessary to illustrate the life of Hariot, who was the light of the outer world to them both. Incarcerated and watched as they were, Hariot was the ears, the eyes, and the hands of these two noble captives.

The depth of variety of Hariot's intellectual and scientific resources, his honesty of purpose, his fidelity of character, his eminent scholarship, his unswerving integrity, and his command of tongue, rendered him alike invulnerable to politicians and to royal minions. He was with Raleigh at Winchester and in the Tower, off and on, as required, from 1604 to 1618, except during the last voyage to Guiana. He was at the same time a pensioner, a companion, and confidential factotum of his old friend the Earl of Northumberland both in the Tower and at Sion for fifteen years. Watched as these two prisoners were, ensnared, entrapped, and entangled for new evidence against them, it was necessary for Hariot to pursue a delicate and cautious course, to eschew politics, statecraft and treason, and to devote himself to pure science (almost the only pure commodity that was then a safeguard) metaphysics, natural philosophy, mathematics, history, and literature. He was their jackal, their book of reference, their guide, their teacher, and their friend.

It is against this background that Harriot lived and worked. Small wonder perhaps, that he never got around to publishing his ideas and findings. Even to-day the bulk of the information we possess about him comes from published statements of other people—not from Harriot himself. In our next article we shall enumerate how Harriot had figured that planets were moving in ellipses, not circles, before Kepler announced his laws; why Halley's comet might have been named Harriot's instead, because Harriot had figured an elliptical course for the comet of 1607; how in June 1609, there was a recorded series of telescopic observations of the moon made by Harriot, which may not have been his first observations, and how in 1609 and 1610 he was selling telescopes, constructed by his mechanic Christopher Tooke under his supervision, to his friends.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

The Introduction of the Copernican System to England

V. Thomas Harriot and the Beginnings of Telescopic Astronomy in England.*

In our first article on Harriot we described the background of his life. At the time that the telescope came into astronomical importance, Harriot was living on the estate of the Earl of Northumberland at Sion, and making frequent trips to visit the Earl and Sir Walter Raleigh who were both imprisoned in the Tower. At Sion he had at his disposal an observatory and a large library. Henry Stevens (*op. cit.*, page 113) feels that Harriot's work in England was no less remarkable than that of Galileo in Italy. Stevens mentions his work on the comet of 1607 (now known as Halley's comet, which might have been named Harriot's) and also describes his manufacture of telescopes:

> Whether in the Tower, administering new scientific delicacies and delights to the prisoners; or at Sion, unlocking the secrets of the starry firmament by night, in his observatory; or floating between Sion and the Tower by day on the broad bosom of the Thames, prying into the optical secrets of lenses, and inventing his perspective trunks by which he could bring distant objects near, Hariot in foggy England of the north was working out almost the same brilliant series of discoveries that Galileo was making in Italy. To this day, with our undated and indefinite material, even with the new and much more precise evidence now for the first time herewith produced, it is difficult to decide which of them first invented the telescope, or first by actual observation with that marvellous instrument confirmed the truth of the Copernican System by revealing the spots on the Sun, the orbit of Mars, the horns of Venus, the satellites of Jupiter, the mountains in the Moon, the elliptical orbits of comets, etc. It is manifest, however, that they were both working in the same groove and at the same time.
>
> Hariot was undoubtedly as great a mathematician and astronomer as Galileo. In 1607 at Ilfracombe and in South Wales, he had taken by hand and Jacob's staff, the old patriarchal method, valuable observations of the comet of that year, and compared notes with his astronomical pupil William Lower, and afterwards with Kepler. This comet, now known as Halley's, ought perhaps to have been named Hariot's, for it confirmed his notions that the motions of the planets were not perfect circles and afforded probably the germ of his reasoning out the elliptical orbits of comets, especially after his friend and correspondent [see *infra*, pages 178–180] Kepler's book *de Motibus Stellae Martis* came out in 1609, and he had invented and improved his telescope or perspective 'truncke' or cylinder in 1609–10.

*This is the final article in a series on "The Introduction of the Copernican System to England."

It is not positively stated that Hariot held direct correspondence with Galileo in 1609 and 1610 or even later, but the evidence is strong that he was promptly kept informed of what was going on in Italy in astronomical and mathematical discovery, as well as in Germany and elsewhere. That he was using a 'perspective truncke' or telescope as early as the winter of 1609–10, and that his 'servaunte' Christopher Tooke (or as Lower in 1611 familiarly called him 'Kitt') made lenses for him and fitted them into his 'trunckes' for sale by himself, is known.

From this circumstance, and from the fact that he disposed of many 'trunckes' by his will, and left a considerable stock of them to Tooke, it is manifest that he manufactured and traded in telescopes from 1609 to 1621. With his invention of the telescope then it required no correspondence with Galileo to induce him to rake the heavens and sweep our planetary system for new astronomical discoveries. To an astronomer of his activity and mathematical acumen these discoveries followed as a matter of course. Like Galileo he may have borrowed from the Dutch (or quite as likely they of him) the idea that by a combination of lenses it was possible to bring distant objects near, but that he worked out the idea independently of Galileo admits hardly of a doubt. But he seems to have been less ambitious than Galileo to claim priority in either the invention or the discoveries that immediately followed.

It is from the very significant remarks in a letter written to Harriot by his ardent pupil Sir William Lower that we are able to gain an idea of the amount of original work Harriot had been doing. Lower chides Harriot for not publishing his findings, saying that Harriot will not get the credit for his discoveries unless he does. This, of course, is exactly what has happened. There is still a great mass of Harriot's manuscripts and records, which apparently no one has worked over carefully, at least since Harriot's time. However, it appears that much of this material is undated, and even a lengthy study of it might not show what knowledge Harriot had acquired before other people had published it.

According to Stevens (page 168), Harriot's papers are in two different collections. At Petworth, as described in the *Sixth Report of Historical Manuscript Commission* for 1877, page 319, folio, is:

A black leather box containing several hundred leaves of figures and calculations by Hariot.

A large bundle of Hariot's papers. They are arranged in packets by Professor Rigaud. Spots on the Sun. Comets of 1607 and 1618. The Moon. Jupiter's Satellites. Projectiles, Centre of Gravity, Reflection of bodies. Triangles. Snell's Eratosthenes Batavus. Geometry. Calendar. Conic Sections. De Stella Martis. Drawings of Constellations, papers on Chemistry and Miscellaneous Calculations. Collections from Observations of Hannelius, Warner, Copernicus, Tycho Brahe. On the vernal and autumnal equinoxes, the solstices, orbit of the Earth, length of the year, &c. Algebra.

A similar collection not yet arranged, catalogued, numbered, or bound is in the manuscript department of the British Museum, in eight thick Solander cases, as much bulk as the Petworth papers. This was presented to the Museum by the Earl of Egmont in 1810. Why Harriot's papers were split into two collections remains unknown.

A list of Harriot's papers was given by S. P. Rigaud in his *Supplement to Dr. Bradley's Miscellaneous Works: With an Account of Harriot's Astronomical Papers* (Oxford, 1833), in which two of the most valuable letters from Lower to Harriot are reprinted. Stevens also reprints them, but since both of these works are so scarce as to be almost inaccessible, we are again publishing these valuable letters to give them wider circulation.

Letter from Sir William Lower Mathematician and Astronomer to Thomas Hariot at Sion

I have receeved the perspective Cylinder that you promised me and am sorrie that my man gave you not more warning, that I might have had also the 2 or 3 more that you mentioned to chuse for me. Hence forward he shall have order to attend you better and to defray the charge of this and others, that he forgot to pay the worke man. According as you wished I have observed the Mone in all his changes. In the new I discover manifestlie the earthshine, a little before the Dichotomie, that spot which represents unto me the Man in the Moone (but without a head) is first to be seene. a little after neare the brimme of the gibbous parts towards the upper corner appeare luminous parts like starres much brighter then the rest and the whole brimme along, lookes like unto the Description of Coasts in the dutch bookes of voyages. in the full she appeares like a tarte that my Cooke made me the last Weeke. here a vaine of bright stuffe, and there of darke, and so confusedlie al over. I must confesse I can see none of this without my cylinder. Yet an ingenious younge man that accompanies me here often, and loves you, and these studies much, sees manie of these things even without the helpe of the instrument, but with it sees them most plainielie. I meane the younge Mr. Protheröe.

Kepler I read diligentlie. but therein I find what it is to be so far away from you. For as himself, he hath almost put me out of my wits. his Aequanes, his sections of excentricities, librations in the diameters of Epicycles, revolutions in Ellipses, have so thoroughlie seased upon my imagination as I do not onlie ever dreame of them, but oftentimes awake lose my selfe, and power of thinkinge with to much wantinge to it. not of his causes for I cannot phansie those magnetical natures. but aboute his theorie which me thinks (although I cannot yet overmaster manie of his particulars) he establisheth soundlie and as you say overthrowes the circular Astronomie.

Do you not here startle, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the planets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it in print. a little before Vieta prevented (anticipated) you of the gharland of the greate Invention of Algebra. al these were your deues and manie others that I could mention; and yet to great reservednesse had robd you of these glories. but although the inventions be greate, the first and last I meane, yet when I survei your storehouse, I see they are the smallest things and such as in comparison of manie others are of smal or no value. Onlie let this remember you, that it is possible by to much procrastination to be prevented in the honor of some of your rarest inventions and speculations. Let your Countrie and frinds injoye the comforts they would have in the true and greate honor you would purchase your selfe by publishing some of your choise workes, but you know best what you have to doe. Onlie I, because I wish you all good, wish this, and sometimes the more longinglie, because in one of your letters you gave me some kind of hope thereof.

But againe to Kepler I have read him twice over cursoridlie. I read him now with Calculation. Some times I find a difference of minutes, sometimes false prints, and sometimes an utter confusion in his accounts. these difficulties are so manie, and often as here againe I want your conference, for I know an hower with you, would advance my studies more than a yeare here, to give you a tast of some of thes difficulties that you may judge of my capacitie, I will send you onlie this one [upon the *Locum Martis* out of Kepler's Astronomy, de motibus Stellae Martis, etc. Pragae, 1609, folio Ch. XXVI, page 137]. . . .

Let me intreate you to advise and direct this bearer Mr. Vaughan wher and how to prouide himselfe of a fit sphere; that by the contemplation of that our imaginations here may be releued in manie speculations that perplexe our vnderstandings with diagrammes in plano. He hath monie to prouide doe you but tell him wher the are to be had and what manner of sphere (I meane with what and how manie circles) wilbe most vsefull for vs to thes studies. After all this I must needs tell you my sorrowes. God that gaue him, hath taken from me my onlie sun, by continual and strange fits of Epelepsie or Apoplexie, when in apparence, as he was most pleasant and goodlie, he was most healthie, but amongst other things, I haue learnt of you to setle and submit my desires to the will of god; onlie my wife with more griefe beares this affliction, yet now againe she begins to be comforted. Let me heare frō you and according to your leasure and frindshippe haue directions in the course of studie I am in. Aboue al things take care of your health, keepe correspondence with Kepler and wherinsoeuer you can haue vse of me, require it with all libertie. Soe I rest ever,

Your assured and true friend to be vsed in all things that you please.

Wllm Lowër

Tra'vent on Mount Martin (in South Wales)
6 February, 1610.

Addressed: to his especiall good friend
Mr. Tho: Harryot at Sion neere London

The second letter from Lower shows with what respect and excitement Galileo's discoveries were received by the English scientists. Lower certainly reaches a peak of astronomical enthusiasm when he considers, even in that exciting era of geographical exploration, that exploration of the heavens is more worthwhile! He states "Me thinkes my diligent Galileus hath done more in this three fold discouerie then Magellane in openinge the streightes to the South sea or the dutch men that weare eaten by beares in Noua Zembla." The letter shows that Lower himself had apparently observed the moon, the sword of Orion, and the Pleiades through a "cylinder" before he heard of Galileo's discoveries.

Letter of Sir William Lower in South Wales to Thomas Hariot at Sion 21 June 1610

I gaue your letter a double welcome, both because it come frō you and contained newes of that strange nature; although that wch I craued, you haue deserved till another time. Me thinkes my diligent Galileus hath done more in this three fold discouerie then Magellane in openinge the streightes to the South sea or the dutch men that weare eaten by beares in Noua Zembla. I am sure with more ease and saftie to him selfe and more pleasure to mee. I am so affected with this newes as I wish sommer were past that I mighte obserue these phenomenes also. in the moone I had formerlie observed a strange spottednesse al ouer, but had no conceite that anie parte therof mighte be shadowes; since I haue obserued three degrees in the darke partes, of w^{ch} the lighter sorte hath some resemblance of shadinesse but that they grow shorter or longer I cannot yet pceaue. ther are three starres in Orion below the three in his girdle so neere togeather as they appeared vnto me alwayes like a longe starre, insomuch as aboute 4 yeares since I was a writing you newes out of Cornwall of a view a strange phenomenon but asking some that had better eyes then my selfe they told me, they were three starres lying close togeather in a right line. thes starres with my cylinder this last winter I often observed, and it was longe er I beleued that I saw them, they appearinge through the Cylinder so farre and distinctlie asunder that without I can not yet disseuer, the discouerie of thes made me then obserue the 7 starres also in, ♉, w^{ch} before I alwayes rather beleued to be, 7, then euer could nomber them. through my Cylinder I saw thes also plainelie and far asunder, and more then, 7. to, but because I was prejugd with that number, I beleved not myne eyes nor was carefull to obserue how manie; the next winter now that you have opened mine eyes you shall heare much frõ me of this argument. of the third and greatest (that I confesse pleased me most) I have least to say, sauing that just at the instante that I receaved your letters wee Traventane Philosophers were a consideringe of Kepler's reasons by w^{ch} he indeauors to ouerthrow Nolanus and Gilberts opinions concerninge the immensitie of the Spheare of the starres and that opinion particularly of Nolanus by w^{ch} he affirmed that the eye beinge placed in anie parte of the Univers the apparence would be still all one as vnto us here. When I was a sayinge that although Kepler had sayd something to moste that mighte be vrged for that opinion of Nolanus, yet of one principall thinge hee had not thought; for although it may be true that to the ey placed in anie starre of, ♋ the starres in Capricorne will vanish, yet he hath not therfore so soundlie concluded (as he thinkes) that therfore towards that parte of the world ther wilbe a voidnesse or thin scattering of little starres wheras els round about ther will appeare huge starres close thruste togeather: for sayd I (hauinge heard you say often as much) what is in that huge space betweene the starres and Saturne, ther remaine euer fixed infinite nombers w^{ch} may supplie the apparence to the eye that shalbe placed in ♋, w^{ch} by reason of ther lesser magnitudes doe flie our sighte what is about ♄.♃.♂. etc. ther moue other planets also w^{ch} appeare not. just as I was a saying this comes your letter, w^{ch} when I had redd, loe qd I, what I spoke probablie experience hath made good; so that we both with wonder and delighte fell a consideringe your letter, we are here so on fire with thes thinges that I must renew my request and your promise to send mee of all sortes of thes Cylinders. my man shal deliuer you monie for anie charge requisite, and contente your man for his paines and skill. Send me so manie as you thinke needfull vnto thes obseruations, and in requitall, I will send you store of observations. Send me also one of Galileus bookes if anie yet be come ouer and you can get them. Concerning my doubte in Kepler, you see what it is to bee so far frõ you. What troubled me a month you satisfyed in a minute. I have supplied verie fitlie my wante of a spheare, in the desolution of a hogshead, for the hopes thereof haue framed me a verie fine one. I pray also at your leasure answere the other pointes of my last letter concerning Vieta, Kepler and your selfe. I have nothinge to presente you in counter, but gratitude with a will in act to be vsefull vnto you and a power in proxima potentia; wch I will not leaue also till I haue broughte ad actum. If you in the meane time can further it, tell wher in I may doe you seruice, and see how wholie you shall dispose of me.

Your most assured and louing friend

Tra'uenti the longest day of, 1610 Willm Lowër

Addressed: To his espesial good frind

Mr. Thomas Harriot

Seal B.M.Add. 6789 at Sion neere London

[Tra'venti or Trafenty, near Lower Court, is eight or nine miles south-west of Caermarthen, near the confluence of the rivers Taf and Cywyn.]

From 1615 on there are evidences of Harriot's failing health, from a cancerous ulcer on the lip. In the summer of 1621 he went to stay with his old Friend Thomas Buckner, in Threadneedle Street. Buckner had been one of Raleigh's First Colonie to Virginia with Harriot in 1585, and Harriot spent his last days with him. He died at Buckner's home on July 2, 1621, three days after he had made his Will, one of the important documents which Henry Stevens helped locate.

He was buried in the parish church at St. Christopher in Threadneedle Street, and the Earl of Northumberland erected a fine marble monument to his memory. Harriot's monument perished when the church was burned in the great fire of 1666, but the inscription was preserved by Stow. The church ground is now the site of the Bank of England, but Harriot still lies in a small garden plot there. As Stevens says, "The Bank of England is built round his bones, but it cannot cover his memory."

No portrait of Harriot is known to exist, but the inscription on his monument (as translated from the Latin by Stevens) gives a word picture of him.

Stay, traveller, tread lightly;
near this spot lies what was mortal
of that most celebrated man
THOMAS HARRIOT
He was the very learned Harriot
of Sion on Thames
by birth and education
an Oxonian,
who cultivated all the sciences,
and excelled in all.
In Mathematics, Natural Philosophy, Theology.
A most studious investigator of truth,
A most pious worshipper of the Triune God,
at the age of sixty, or thereabouts,
He bade farewell to mortality, not to life,
July 2d A.D. 1621.

OUT OF OLD BOOKS

BY HELEN SAWYER HOGG

PONS-BROOKS COMET

Announcements of the return of Pons-Brooks comet, 1884I, are appearing in various astronomical periodicals and the Harvard Announcement Cards. Our readers may be interested in some of the past history of this comet. The present return is only the second one since its discovery in 1812.

This comet is one of the 36 or 37 which were found by Jean Louis Pons, who broke all records for comet discovery. Many of his comets were of course found independently by other observers. Pons seems to have had more skill in discovering comets than in measuring accurate positions for them, and for some of his comets good orbits could not be derived.

However, for this comet, which Pons found on the 20th of July in Lynx, Encke worked out an excellent orbit, with elliptical elements and a period of 70.68 years. The comet passed perihelion Sept. 15, 1812 and was visible to the unaided eye for three months. It is a member of Neptune's family, of which at least nine members are known, including the famous Halley's.

After its 1812 appearance, the next person to see it was Prof. William R. Brooks of Phelps, N.Y. Brooks was one of the great comet observers on this continent during the latter part of the last century, along with Barnard, Perrine and Swift, each of whom discovered more than ten comets. We have had occasion previously in this column to refer to the work of Brooks on telescopic meteors (this JOURNAL, May-June, 1946). We are indebted to Mr. Howard A. Scheer of Newark, N.Y. for sending us pictures of the sites where Brooks' comet work was done. Of his earlier observing station, nothing remains, but this work led to the erection of the Smith Observatory at Hobart College, Geneva, N.Y., of which he was director until his death, and the picture of which is reproduced here. A New York State highway marker commemorates Brooks' work.

According to Chambers, "Handbook of Astronomy," 4th edition, vol. I, page 435, Brooks was looking for the return of Pons' comet by the aid of an ephemeris computed by Schulhof and Bossert. The announcement of the new comet in the *Sidereal Messenger*, vol. II, 1883, page 220 does not give this impression, and the first four sets of elements accompanying

FIG. 1—Smith Observatory, Hobart College, Geneva, N.Y., as seen from residence of Prof. William R. Brooks.

the announcement, from Harvard, Palissa, Boss, and Wilson of Cincinnati, are not the same as those of Pons' comet.

We quote from the *Sidereal Messenger*, op. cit.

EDITORIAL NOTES

A very faint telescopic comet was discovered September 1st by Prof. William R. Brooks, Phelps, N.Y. It has the appearance of a nebulous mass, a little brighter at the center and with indefinite outlines. It is now about three hundred millions of miles from the *Earth* and about the same distance from the *Sun*, and slowly approaching both. The discovery is a remarkable one in view of its great distance and faintness when first seen. We are not aware that the history of astronomy furnishes a parallel in these particulars. If computation at hand be trusted, the comet will not reach the perihelion of its orbit before May, 1884, at which time its brightness will be about one hundred times as great as it now is.

By a *Science Observer* circular we are favored with the following elements and ephemeris:

The four different sets of elements are followed by a subsequent communication in which Lewis Boss identifies Brooks' comet with that of Pons.

Under date of September 21, Professor Lewis Boss, Director of Dudley Observatory, Albany, N.Y., kindly sends the following:

Comet *b* 1883 (Brooks).

By means of observations secured at the Dudley Observatory on September 5th, 9th and 18th, I derived on the 19th the following parabolic elements marked I. The remarkable similarity of these elements to those given by Schulhof and Bossert for the Pons comet of 1812 pointed unmistakably to their identity. The elliptic elements of the Pons comet (here marked II) are transcribed from the memoir of Schulhof and Bossert (p. 150), except that they are reduced to the mean ecliptic and equinox of 1883.0, and a value of T derived from the observations of the present apparition.

	I			II
T	1884 January 25.788 (G.M.T.)	T		1884 January 25.699 (G.M.T.)
Node	254° 13′.6			254° 8′.8
Node to Perihelion	199 14.4			199 12.9
Inclination	74 47.1			74 03.3
log. q	9.87944	log. q		9.88930
		Eccentricity 0.95527		

It is remarkable that the present comet should have been picked up when its light ratio was six times as small as it was at discovery in 1812. It was then regarded as a faint object. Were it not for the overwhelming testimony from other sources, one might doubt, on the ground of brightness, the identity between the present comet and that of 1812. — — —

The identity of the Pons comet of 1812 with comet *b*, 1883, was announced in an "Associated press" dispatch from the Dudley Observatory on the evening of September 19.

The astronomical periodicals of late 1883 and early 1884 contain many interesting descriptions, accounts and drawings of this comet, which was remarkable for its changes in brightness and shape. Among other observations are those of its transit over a bright star, in *Sidereal Messenger*, vol. III, 1884, page 118, from which we quote.

TRANSIT OF THE PONS' COMET OVER A BRIGHT STAR

The passage or transit of a large comet over a large star is an event which very rarely occurs, and if, as in the present case, the comet be moving rapidly, the chances of any one observing it during the very brief interval of transit (in this case only two or three minutes) are extremely small. It is therefore a matter of some interest to preserve a carefully recorded instance of the kind. — — —

Early in the evening of the 24th inst. I found Pons' comet close to a star in *Cygnus* of the seventh magnitude, and from its relative position could see, at a glance, that a transit was inevitable. As I had been for three months watching this very comet for such an opportunity, I hurried my preparations, putting on a power of 100 and getting an assistant to mark time. The following are the results: — — — Star began to fade at $6^h31^m42^s$ local time; star began to brighten at $6^h35^m22^s$; interval 3^m40^s; — — —

I am very sensible that it would be unsafe to state, in positive terms, that the star and the centre of the nucleus coincided; but to all appearances, the central line of passage was through the star; yet I found it impossible to see the stellar centre of the comet in contact with the star on either side, and there was an interval corresponding with the waning of the star's light, when the nucleus was invisible. The star is an orange-tinted star, about 30^m south following ϵ *Cygni*, and faded by estimation half a magnitude.

If Pons comet, then, has a solid or liquid core, it cannot well be over two hundred miles in diameter, while the inference is, that it is, like many other comets, a gaseous body throughout, although to-night (December 27th), it shows a well-defined planetary disc, which, I must think, would obliterate an ordinary star. The tail (about 2° long) however, is diaphanous to stars down to the 13th and 14th magnitudes, even up to the circular nebulosity which surrounds the head. — — — *Beta, in Times-Union* (Ill).

The present return of Comet Pons-Brooks was heralded by Dr. Paul Herget of the Cincinnati Observatory in February 1952 (Harvard Announcement Card 1166). The comet was apparently not found however until June 20, 1953 when it was recovered by Miss Elizabeth Roemer on Lick photographs, as a 17th magnitude object in Draco. (We may be pardoned for remarking that Aquarius would have been more appropriate). The comet will come to perihelion May 22 this year, five days earlier than had been expected. It is east of the sun and north of it through April and part of May, when it may have naked eye visibility. Unfortunately it is so close to the sun that this is classed as a poor return of this particular comet.

NOTES AND QUERIES

New Publication on Meteoritics

From the time of the inception of the Meteoritical Society in 1933, its *Notes* and *Contributions* were published regularly in the monthly magazine, *Popular Astronomy*, until December 1951, when that periodical was discontinued on the completion of its 59th volume. By arrangement with the University of New Mexico and by unanimous vote of the Council of the Society, a new publication entitled *Meteoritics: The Journal of the Meteoritical Society and the Institute of Meteoritics of the University of New Mexico* was established, and its premier issue, consisting of 25 items and 123 pages, appeared in December 1953, as volume 1, number 1, whole number 1, 1953.

Meteoritics is to be issued at least once but not more than four times a year. It is expected that eventually the journal will become a quarterly. Each volume is intended to contain from 240 to 360 pages. The Editor of the Meteoritical Society, Dr. Frederick C. Leonard of the University of California, Los Angeles, is the Editor of *Meteoritics*, and the Director of the Institute of Meteoritics of the University of New Mexico, Dr. Lincoln LaPaz, is the Associate Editor.

The annual subscription price of *Meteoritics*, to both members and non-members of the Meteoritical Society, is $4.00 (or, to student members, $2.00), regardless of the number of issues published. Orders for subscriptions should be sent to the Secretary of the Meteoritical Society, Dr. John A. Russell, Department of Astronomy, University of Southern California, Los Angeles 7, California.

Frederick C. Leonard

OUT OF OLD BOOKS

By Helen Sawyer Hogg

David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

The Tunguska Meteoric Event

A recent paper by the Soviet astronomer V. G. Fesenkov on the Tunguska meteorite increases the interest in that remarkable object. Fesenkov's paper, "On the Cometary Nature of the Tunguska Meteorite", appeared in the *Astronomical Journal*, U.S.S.R., July–August 1961, vol. 38, p. 577, with a translation in *Soviet Astronomy AJ*, January–February, 1962, vol. 5, p. 441. The purpose of the paper is to set forth the reasons why Fesenkov and some other astronomers consider that this was not the fall of an ordinary meteorite, but actually the collision of the earth with a small comet.

This great event occurred in the basin of the Podkamennaya Tunguska river in Central Siberia on June 30, 1908. Though it has been extensively investigated, much of the information has been published in Russian, and even the translations published in English are not easily available to many of our readers. One of the best early review articles is by W. H. Christie in the *Griffith Observer*, April 1942, from which the diagram is taken. The book by E. L. Krinov, *Principles of Meteoritics*, recently translated into English by Irene Vidziunas, Pergamon Press, 1960, contains an excellent summary of the circumstances of this fall.

A number of prominent Soviet astronomers, including L. Kulik, E. L. Krinov, and I. S. Astapovich have spent many years gathering and correlating information about this event. First to start a real investigation was Kulik, who went to Central Siberia in 1921–22, but because of the isolation of the place, it was not until 1927 that he reached the actual region of the fall. Subsequently he returned on a number of expeditions, on one of which he wintered in this inclement place, whose winter temperatures are frequently −60° F.

Because the event occurred in such a remote and sparsely inhabited region, it took many years to piece together the story. The meteorite fell at 7:17 a.m. local time, near the trading post of Vanovara, at latitude 60° 55′ N., long. 101° 57′ E. Witnesses early that morning saw flying across the sky a bolide so blindingly bright that the day sky appeared dark by comparison. It went from SE to NW or from SSW to NNE, according to different investigators. The fall could be seen in a cloudless sky in Central Siberia over an area about 1,500 km. in diameter. A thick

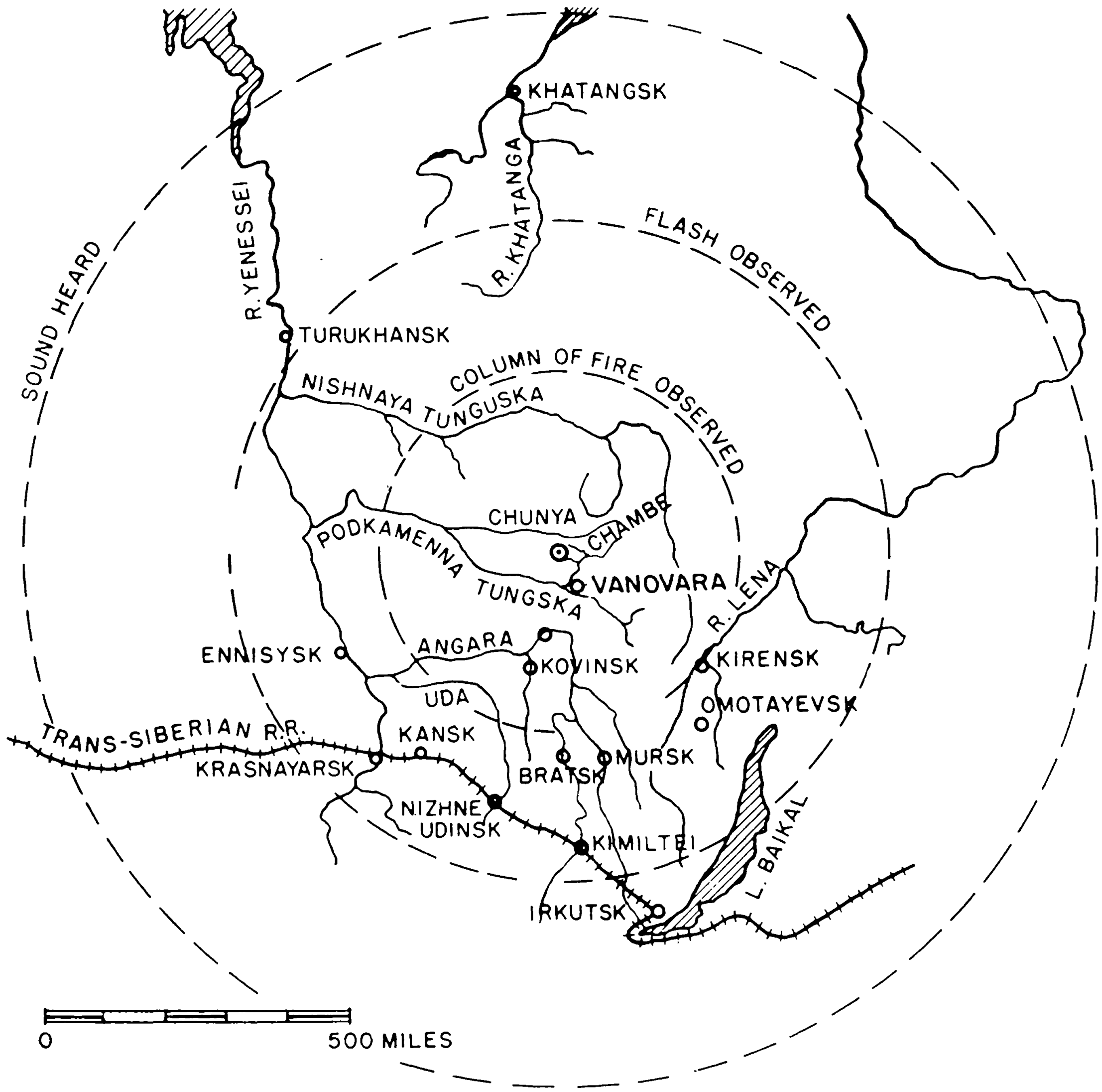


FIG. 1—Region of the Podkamennaya Tunguska meteoric event in Central Siberia. (Courtesy of *The Griffith Observer*.)

dust trail remained along the path, and over the place of fall, fire and a cloud of smoke were seen. Strong detonations, thunder, crackling and rumbling were heard after the fireball itself had disappeared. The sound phenomena were heard up to 1,000 km. from the place of fall.

Over the area where light phenomena were seen, ground tremors were felt, buildings shook, and windowpanes broke. A tent of Evenkians on the taiga about 40 km. from the fall was lifted into the air, along with people inside. The forest was charred and flattened over an area of 250 square kilometres. The central uprooted region had a radius of 12–15 km. From the size of the area over which sound was heard, and the force necessary to uproot the forest, Astapovich calculated the energy of the Tunguska meteorite to be 10^{21} ergs/sec.

It was some years before it was realized that certain effects observed in other parts of the world were connected with this fall. These effects include the bright night sky glow of June 30 that year, the seismic waves registered at various places, the aerial shock registered on the micro-baragraphs of six English meteorological stations, such as South Kensington, Westminster, and Cambridge, and finally the diminution in transparency of the earth's atmosphere from studies of C. G. Abbot in California in the summer of 1908. Fesenkov believes that the observed atmospheric turbidity was caused by an immense quantity—millions of tons—of dust hurled into the atmosphere over Vanovara by the break-up of the body.

Reference is frequently made to the early accounts of the Siberian peasants who witnessed the great explosion. We reprint now excerpts from these accounts taken from *Popular Astronomy*, vol. 43, 1935, pp. 596–599, a translation by Lincoln LaPaz and Gerhardt Wien of the article by L. Kulik in the *Journal of the Russian Academy of Sciences*, 1927A, pp. 399–402.

ON THE FALL OF THE PODKAMENNAYA TUNGUSKA METEORITE IN 1908.—In the month of February, 1927, an expedition, with the author as leader, was equipped by the Academy of Sciences of the U.S.S.R. for the investigation of the place of fall of the meteorite of June 30, 1908, in the basin of the upper part of the Podkamennaya Tunguska river.

In the latter part of March, 1927, I began my investigations to the north of the Podkamennaya Tunguska, having my base at the factory [trading post] Vanovara, lying near this stream in approximately the 72° of longitude east of Pulkovo. After repeated attempts to penetrate the marshy forests north of this river, advancing to the northwest on small rivers by means of a raft, I reached the central part of the area of the fall in the month of June and made a hurried survey of the place and its neighbourhood. . . .

The central part of the fall, lying on the plateau which forms the watershed between the basin of the river Chunya and the Podkamennaya Tunguska proper, consists of an area several kilometers in diameter which has the appearance of a huge crater surrounded by an amphitheatre composed of chains of hills and isolated peaks. To the south, along a tangent to this circle of mountains, the river Khushmo, the right hand tributary of the river Chamba, which enters the Podkamennaya Tunguska about 30 km. below Vanovara, flows from west to east. This system of tributaries was for the most part my road from Vanovara to the place of fall and back. In the previously mentioned crater, there are in turn chains of hills and isolated peaks, marshy plains, swamps, lakes, and small streams. Very recently, according to the testimony of local residents, there was here a typical marshy woods. At present the entire marshy woods inside and outside of the crater is practically destroyed, being altogether blown to the ground, where it lies in generally parallel rows of trunks (stripped of branches and bark), the tops of the trunks pointing in the direction away from the center of the fall; this peculiar "fan" of broken-down woods can be especially well seen from the summits of the chains of hills and individual heights which form a peripheral ring about the basin. However, here and there the marshy woods remained in the form of trunks standing on their roots (usually without bark and branches). Similarly, in spots, unimportant strips and small copses of green trees remained. But these exceptions are a rarity and can be easily explained in every individual case. The whole former vegetation of both the crater and the surrounding hills, as well as the zones for several kilometers around them, carries the characteristic traces of a uniform and continuous burning not resembling the effects of an ordinary [forest] fire; moreover, this burning is shown on the broken-down trees as well as on the standing trees [and] on the remnants of bushes and moss on the summits and the sides of the hills as well as in the marshy plains and on the isolated islands of dry land in the midst of the swamps covered with water. The area with traces of burning is several tens of kilometers in diameter. The central region of this "burned" area, which measures several kilometers in diameter, exhibits in that part of it which is occupied by marshy plains covered with bushes and woods, traces of something like a sidewise pressure which gathered up the soil and vegetation in flat folds with depressions several meters deep, drawn out on the whole perpendicularly to the northeast direction. Moreover, this region is strewn with dozens of freshly formed flat "craterlets" [funnels], which have various diameters, ranging from several meters to tens of meters, with a depth also of several meters; the walls of these "craterlets" are usually steep, although there are also some sloping ones; the bottom of the craters is flat, swampy, and mossy, and carries sometimes the traces of a central eminence. On the northeast end of one of these marshy areas, the moss cover seems to be pushed several tens of meters away from the foot of a hill and replaced by a bog.

To the preceding, it is necessary to add that the collection of testimonies from local eyewitnesses of the fall, made by myself, gave me a number of interesting narratives from which I quote the following.

The peasant S. B. Semenov related to me in a letter:

"It was in 1908 in the month of June about 8 o'clock in the morning; I lived at that time on the Podkamennaya Tunguska at the factory Anovara (Vanovara, L.K.) and was occupied with work around my hut. I sat on the open porch with my face toward the north and at that time there arose, in a moment, a conflagration which gave off such heat that it was impossible to remain sitting—it almost burned the shirt off me. And it was such a flaming wonder that I noticed that it occupied a space of not less than two versts [one verst = 0.663 mile]. But to make up for that, this conflagration endured only a very short time; I had time only to cast my eyes in that direction and see how large it was, when in a moment it vanished. . . . After this vanishing it grew dark, and at the same time there was an explosion which threw me off the open porch about seven feet or more; but I did not remain unconscious for very long; I came to myself and there was such a crashing sound that all the houses shook and seemed to move from their foundations. It broke the window panes and window frames in the houses, and in the center of the square, near the huts, a strip of earth was torn out, and at the same time the so-called shore [bracing strip] of iron on the door of the barn was broken, but the lock remained whole."

Another peasant, P. P. Kossolapov, personally informed me on March 30, 1927, that in June, 1908 at 8 o'clock in the morning, he was getting ready at the same factory to go hay-cutting; he needed a nail; not finding one in the room, he went out into the yard and began pulling a nail out of a window with his pincers. Suddenly something very strongly scorched his ears. Reaching for them and thinking that the roof was burning, he raised his head and asked S. B. Semenov, who sat on the open porch at his house, "Say, did you see anything?" "How could one help seeing it?" answered the other; "It seems to me too as if heat embraced me." P. P. Kossolapov there and then went into his house, but scarcely had he entered the room and got ready to sit down on the floor to his work when an explosion occurred, the sod fell from the ceiling, the oven door of the Russian stove broke loose and flew on to a bed standing opposite the stove, and one window pane was broken, falling into the room. After this, there was a sound similar to the rolling of thunder, vanishing gradually to the north. When it became somewhat quieter, Kossolapov rushed out into the yard but did not notice anything more.

Finally, on the 16th of April, 1927, the Tungus Luchetkan told me that the whole central region of the wind-felled trees was occupied before this event by his relative, the Tungus Vasiliy Ilyich, who used it as a pasture for reindeer. Vasiliy Ilyich was a rich Tungus; he called up to fifteen hundred reindeer his own; he had in this region many sheds in which he kept clothes, utensils, reindeer equipment, etc. With the exception of several dozen tame ones, the reindeer were permitted to roam at will in the hills in the region of the river Khushmo. But down flew the fire and broke down the woods; the reindeer and the sheds were gone. Thereupon the Tunguses went in search of them. Of some reindeer they found the charred carcasses; the others they did not find at all. Of the sheds nothing remained; everything was burned up and melted to pieces—clothes, utensils, reindeer equipment, dishes, and samovars; they found only a few "kettles" [buckets] intact.

Fesenkov cites three reasons for his belief in the comet hypothesis. The first is that, despite extensive searches, no primary meteoritic fragments have ever been found. The numerous craters found are not now considered to be places of fall of parts of the meteorite. It is easier to visualize a "dirty iceberg" exploding to nothing, than huge rocks. There is certainly no doubt that a massive body entered the atmosphere and exploded. Its mass is variously estimated at a million tons or greater. It exploded at a height of 5–6 kilometres above the earth. This height is well established from the measured velocity of the shock wave, both direct and reverse, which travelled around the earth. Since both waves were received at the same place, for example at the Potsdam Geodesic Institute, the speed could be accurately determined entirely independent of any timing at the original source. A few microscopic spherules, partly magnetite and partly silicate, have been found, which may be connected with the Tunguska meteorite since they are found only in the region of its "fall". They must have been formed by the hardening of the meteorite dust in the atmosphere.

The second reason is that the best interpretation finds that the Tunguska meteorite came in on a retrograde orbit. It was moving from south to north at a time when the earth was moving generally north to south. Now asteroids—the presumed parents of meteorites—are not travelling in retrograde orbit, but are following the normal preferential counterclockwise direction (as seen from the north) of the regular planets. The type of body which is likely to be in a retrograde orbit is a comet. About as many comets pursue retrograde motion as direct.

A third bit of evidence is the curious and unusual night sky glow seen over Europe. Fesenkov computes that if this glow were caused by the tail of a comet, it would have appeared over western Europe in just the latitudes and longitudes where it was observed. The particles causing this glow were at a height of several hundred kilometres above the earth's surface and did not behave as ordinary meteors in giving a display of shooting stars. The sky glow was not caused by the gigantic break-up of the body near Vanovara. The fragments from that break-up actually went round the world in the northern hemisphere in a period of weeks. The resulting attenuation of sunlight in California was measured by C. G. Abbot, who did not then realise the cause. This dilution of sunlight did not reach its maximum until early August 1908.

One of the vivid accounts of the sky glow over Europe is to be found in the *Observatory*, vol. 31, p. 324, August 1908, an unsigned article.

A PROTRACTED TWILIGHT.—On the night of June 30, and to some extent on the nights succeeding, brilliant sky-glows were observed in various parts of the country, and lasted throughout the night. At 9:30 p.m. at Greenwich, on June 30, the sky along the north-west and north horizon was of a brilliant red, in fact there was what is usually termed a "brilliant sunset", the only peculiarity being that the brightness stretched more to the north than is usual, and endured, so that at one o'clock in the morning it extended well across the north of the horizon, and the northern sky above was of a brightness approaching that of the southern sky at the time of Full Moon. The light, indeed, was sufficient to take photographs of terrestrial objects. Mr. Evans, of the Royal Observatory, secured excellent photographs of the domes of the Naval College with fifteen minutes' exposure. It has been suggested that this was an appearance of the aurora borealis, but at Greenwich no shooting streamers were seen. It happened that the Sun was in a state of activity at the time, as shown by a large prominence on the south-west limb, and this gave strength to the suggestion that it was an auroral display, but spectroscopic observations fail to give any evidence of this. A long-lasting solar halo was seen in the forenoon and afternoon of June 30, and another on July 1.

The comet theory is by no means a new one. It was first suggested many years ago by Professor Kohl of the Carina Observatory in Denmark to explain the strange night sky glow. Possibly in the years ahead further evidence will be found which will strengthen or weaken the theory. For example, the suggestion has been made that the hunt for the giant meteorite has not been conducted in the region where the pieces might have fallen, but rather in the region which received the greatest shock wave from the explosion. Would further search disclose a rocky mass? Meanwhile we are interested in speculating on this awe-inspiring event, and we are grateful that it occurred in a desolate region of the earth, not in a populous valley or a large city.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

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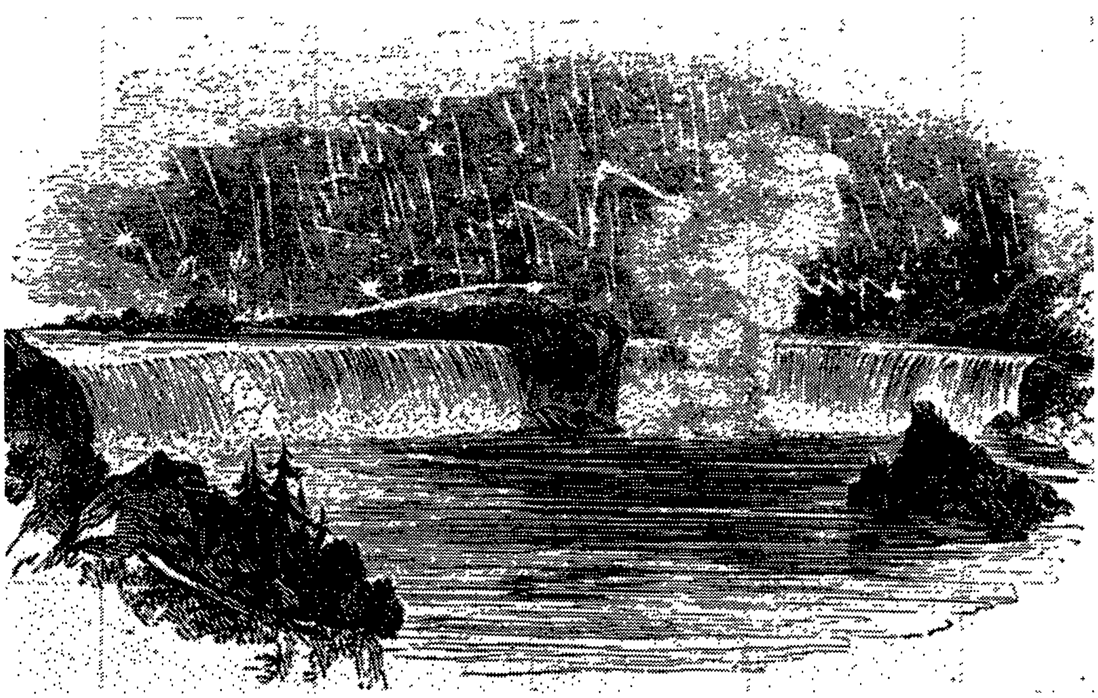
THE LEONID SHOWER OF 1833

WITH the return of substantial numbers of Leonids in the autumn of 1961, as described in *Sky and Telescope*, February 1962, our interest in the dramatic showers of previous centuries is quickened, as we wonder if we may be building up to a good display in the next few years.

Much has been written about the Leonid showers, but for lack of space, many of the eyewitness accounts have had to be edited and condensed. For example, in his important volume *Meteors* (Williams and Wilkins Co. Baltimore, 1925) Charles P. Olivier devotes about five pages to the discussion of the Leonids of 1833 (pages 24–28). He digests the most important facts published by various observers and professors of American Colleges, including Denison Olmsted of Yale in *Silliman's Journal*, otherwise known as the *American Journal of Science*, series 1, vol. 25, pages 354–411; vol. 26, pages 132–173, 1834. Our readers can see at once that many items of interest had to be omitted in Olivier's condensation. So we will reprint some of the less well known accounts. It is a pleasure



"Shower of Falling Stars at Niagara in November 1833"—(From an old newspaper).

to acknowledge our debt to the Ontario Legislative Library and its librarians for the use of these scarce books.

Many pages of the accounts are devoted to discussion of the height above earth from which the meteors came, the discovery of a radiant point, its position, and whether it was moving or stationary. Olivier has summarized the facts pertaining to the radiant. Some of the contemporary discussion seems childish to us, a hundred and thirty years later, and some of the conclusions reached by witnesses were contradictory. The descriptions of the sights that the eyewitnesses *saw*, however, make just as fascinating reading now as they did a century ago, and whet our appetite for a return of the great shower.

Although nowadays there are frequent references to the rate of fall of the Leonids—so many thousand an hour—actually very few of the accounts give figures sufficient to determine the rate. This is quite understandable. If you are suddenly, without warning, confronted with a spectacle of this nature you would want to do many other things than just count numbers!

One of the best accounts, also interesting in its reflection of life in New England a century ago, is by an anonymous writer at Boston, published in the *Columbian Centinel*. This account we reprint practically in its entirety (*op. cit.*, vol. 25, p. 366).

2. Phenomena as observed at *Boston* (Lat. 42° 21′ N. Lon. 71° 4′ W.) and published in the Columbian Centinel.

"This morning there was the appearance of a thick shower of fire. It was occasioned by the incessant falling of innumerable meteors commonly called falling or shooting stars.

Having risen as usual at 4 o'clock, I thought I observed several very bright falling stars, but as the window was covered with steam, in consequence of the change of temperature out of doors during the night, I saw but indistinctly, and took my box to strike a light without seeing or thinking more of the phenomenon, until I went down to the parlor twenty minutes before six o'clock; except that twice I saw a very sudden and bright glare of light, which, at the time, I supposed must have come from some lamp or fire in the house opposite. On opening one of the parlor shutters, I was surprised at seeing innumerable meteors similar to those commonly called shooting stars. They were moving in a direction downward, and I should say, according to the best judgment I could form, were falling about half as thick as the flakes of snow in one of our common snow falls, with intervals of a few seconds, when there was not so many.

I stood observing the phenomenon till fifteen minutes before six, at which time, the meteors being fewer, I attempted to count a portion of them. In the part to which my attention was confined, and which was perhaps a tenth part or rather less of the horizon, I counted 650 stars, during the fifteen minutes before six o'clock. They fell so fast and thick, however, that I supposed I was not able to enumerate thus distinctly, more than two-thirds the number of those which actually fell during that time in the space to which my attention was directed. If I am correct in my estimation, this would show the number of meteors falling during the fifteen minutes, to have been more than 8,660. At 6 o'clock, I went to the top of the house. The phenomenon was now beginning to cease.

During the first fifteen minutes after six, the number which fell in the southern half of the heavens from the zenith downward was 98. The last fell ten minutes before sunrise.

In the course of this time there were two exceedingly bright meteors. I did not see them, as they did not make their appearance in the part to which my attention was directed; but the steeple of the neighboring church was reddened by the light of them; and I then supposed that the glare of light in my chamber, which I had before attributed to a lamp or fire in the house opposite, must have proceeded from meteors.

The sky was clear excepting on the verge of the horizon, where in the east, there were a few thin streaks and small specks of clouds, and in the south and southeast, the round heads of a range of dark heavy clouds were just visible above the horizon. There was however, a vapor in the atmosphere, visible round the horizon, which in the southeast assumed a very beautiful appearance during ten minutes, about half an hour before sunrise. The thermometer yesterday at 2 p.m. was 63 deg.; this morning at 4 o'clock, it was 39 deg. There was but little wind, and this from the west. The direction in which the meteors moved was almost directly downward, and not oblique as usually seen, except in two instances, when the course was horizontal, nearly in a straight line, and from northeast to southwest, and these two meteors were high and small. Generally the meteors appeared to be very low in the atmosphere, some to come down apparently almost to the house tops. They all had a distinct nucleus, generally about half the size of Jupiter, some were larger than this, some smaller, and a few were larger than the apparent disc of Jupiter. They all left luminous white traces, bands, or tails, which generally appeared to be, in popular language, about a yard in length; a few were three times and some not more than half this apparent length. Their appearance continued in most cases from three to four seconds, some five, not many, if any, longer than this. There was no appearance of explosion or bursting to the nucleus of any of them.

I have never met with any account of such a phenomenon having been observed in this country before. Similar phenomena have occasionally been presented elsewhere, and have been spoken of as Showers of Fire, to which indeed this bore a perfect resemblance."

In some of the descriptions, the writer has attempted to convey in a literary way to the reader the impression of the heavens on this momentous occasion, rather than placing the emphasis on the scientific enumeration. One of the most vivid of these accounts is from Missouri (*op. cit.*, vol. 25, p. 381).

Phenomena as observed at *Bowling Green*, Missouri, Lat. 39° 20′ N., Lon. 91° W., as published in the Salt River Journal of Nov. 29th. (Communicated to Prof. Silliman.)

"On Wednesday morning the 13th inst., from four o'clock until daylight, a most sublime Phenomenon continued to present itself in the sky, and was beheld by most of our citizens. We were awakened, and told that the stars were falling, and flying in all directions of the heavens; and knowing that the individual who awakened us, was a person of observation and science, we instantly hurried from our room for the purpose of witnessing a spectacle so extraordinary, and found what had been told to us, had the full appearance of being a reality.

This place, situated on an elevated point of an extensive prairie, presents an unbroken view of the horizon, and afforded an excellent opportunity of beholding this Phenomenon in all its various aspects, and impressive sublimity. The most perfect master of language would fail of conveying to others a full picture of this extraordinary and uncommon appearance, and vain would be his attempt to express the sensations of its beholders.

Above all, around the firmament—thicker than the stars themselves, which were uncommonly bright, large and beautiful—we beheld innumerable fire-balls of a whitish pallid color, rushing down, and to appearance across the sky—drawing after them, long luminous traces, which clothed the whole heaven in awful majesty, and gave to the air, and earth, a pale and death like appearance.

Our first look, after a common glance, was directly above to the zenith, and at that instant, an inconceivable number of meteors, or falling stars, as though the sky had just received a mighty shock, burst from the blue and cloudless arch, which never appeared more clear, and shot like so many burning arrows, towards every part of the horizon. . . .

They continued till near day light when they gradually disappeared, but we are informed that some were seen shortly after sunrise.

Though there was no moon, when we first beheld them, their brilliancy was so great, that we could, at times, read common sized print, without much difficulty, and the light which they afforded was much whiter than that of the moon, in the clearest and coldest night, when the ground is covered with snow. . . .

There was a grand, peculiar, and indescribable, gloom on all around—an awe inspiring sublimity on all above, while

> 'the sanguine flood
> Rolled a broad slaughter o'er the plains of Heaven,
> And Nature's self did seem to totter on the brink of time!'

Forcibly were we reminded of that remarkable passage in Revelations, which speaks of the great red dragon, as drawing the third part of the stars of heaven, and casting them to the earth; and if it be a figurative expression, that figure appeared to be fully painted on the broad canopy of the sky, spread over with sheets of light, and thick with streams of rolling fire. There was scarcely a space in the firmament which was not filled at every instant with these falling stars, nor on it, could you in general perceive any particular difference, in appearance; still at times they would shower down in groups, calling to mind the 'fig tree, casting her untimely figs when shaken by a mighty wind', and their phosphorescent burning flashed around you like the mighty flash of lightning on the expanse of water, though more light and pallid.

The long luminous traces which they left behind, would last for several seconds; and at times, when the nucleus had entirely disappeared, those traces or streams, varying from ten to a hundred yards in length, would linger on the sky and continue to shine in all their brilliancy for two or three minutes, and then expire in a twinkling of an eye.

Their size was about the same as that of the morning star,—they moved somewhat higher, and their velocity was much faster than that of the common meteors, and from the place of their starting to where they seemed to expire, it was, we would suppose, from ten to forty degrees.

You would now and then see some solitary ones, resembling balls of livid fire, like burning rockets shooting towards the earth, and emitting numerous sparks, as they boldly rushed into the more dense and vaporous atmosphere—acquiring as they fell, a more baleful and murderous aspect, and like incendiary spies, portending ruin and destruction.

We are also informed, that from the beginning of that Phenomenon, there was not a space on the firmament equal in extent to three diameters of the moon, which was not filled at every instant with falling stars; all of which left luminous traces from five to ten degrees in length, that lasted seven or eight seconds; and that many of them had a very distinct nucleus as large as the disk of Jupiter, from which darted sparks of vivid light. The light of those meteors was white, which is attributed to the absence of vapors, and the extreme transparency of the atmosphere; and we think, that those of a reddish and fiery aspect, which we beheld, had fallen from the rest, and that this appearance was the effect of the vapors which had risen from the earth, or of the thin clouds of smoke which had ascended from the burning prairies, into which they had wandered."

Descriptions from two of the frontier settlements of this continent at the time have a certain charm to them (*op. cit.*, vol. 26, pp. 138–139), as related by Denison Olmsted.

From *Professor Thomson,* formerly of the University of Nashville, Tenn., and from *Henry R. Schoolcraft, Esq.* of Michilimackinac, I have received communications, which are valuable, not only on account of the well known competency of the witnesses, but as relating to observations made at points remote from each other. Professor Thomson, remarks as follows:

"Having been engaged in running the standard lines for the general survey of the Chickasaw Nation in Mississippi, I was at the house of Major Allen, on the night of the 'falling stars'. Major Allen is the government agent, and resides nearly in the center of the Nation. About an hour before daylight, I was called up to see the falling meteors. It was the most sublime and brilliant sight, I had ever witnessed. The largest of the falling bodies, appeared about the size of Jupiter or Venus, when brightest. Some persons present, affirmed that they heard a hissing noise on the fall of some of the largest. The sky presented the appearance of a shower of stars, which many thought were real stars, and omens of dreadful events.

"I noticed the appearance of a *radiating point,* which I conceived to be the vanishing point of straight lines as seen in perspective. *This point appeared to be stationary.* The meteors fell towards the earth at an angle of about 75°, with the horizon, moving from the east towards the west. There was not sufficient wind to account for the above inclination in the fall of the meteors. . . ."

Mr. Schoolcraft writes thus:

Michilimackinac, Jan. 6, 1834.

"*To Prof. Olmsted.*—SIR—The meteoric display described in your letter of the 13th November, was observed, at the same time, on this island, and the adjacent shores of lake Huron. The appearances coincided, generally, with those you mention. The sentinels at post in the garrison, which is situated on a cliff, saw the lake illuminated, as it were, with falling stars. Indians, at point St. Ignace, describe the stars as falling into the water. Persons, at the fishing grounds, forty miles south, speak in admiration of the brilliancy of the meteoric phenomenon. Two persons encamped at Point Detour, forty five miles N.E. who passed the night in the open air, describe the scene as presenting an assemblage of dancing or shooting stars, which continued to be visible until day light. No similar scene is recollected to have occurred by white, or red men.

"It may be added, that the weather has been uncommonly mild, subsequent to this occurrence. This fact, was first noticed the latter part of November, when the existence of severe cold is expected. All December was characterized by a comparatively high range of the thermometer, with prevalent winds from the southward and westward. So striking were the effects, that maple sugar was made by the Indians, during that month. But little snow fell, until the first inst. Floating ice appeared in the lake on the 3d.; but the lake and harbor are still without any fixed body of ice.

"I am Sir, very respectfully your obedient servant,

HENRY R. SCHOOLCRAFT."

A picturesque description comes from a doctor in North Carolina who was travelling his rounds all night, and was rewarded in his diligence by a fine view of the celestial spectacle (*op. cit.*, vol. 25, p. 378).

9. Phenomena as observed at Salisbury, N. Carolina, Lat. 35° 39′ N., Lon. 80° 25′ W., by Ashbel Smith, M.D. (Communicated to Prof. Olmsted.)

"Travelling on a professional visit, I was in the open air, without any intermission from night fall till the day dawned. In the early part of the night, the atmosphere was uncommonly bright and even glittering. A few meteors of inferior brightness, in remote regions of the atmosphere, were seen by me previously to midnight; some as early, I feel pretty confident, as 10 o'clock. After midnight, they rapidly increased in number and brilliancy till 4 o'clock. The display was then in the highest degree magnificent and imposing, and continued without diminution till the dawn of day, every region of the atmosphere all the while presenting the sublime spectacle of a shower of fire. The meteors varied greatly in the degree of splendour, some being an obliquely luminous line, while others resembled a rushing ball of liquid fire, with a splendid train or tail, bathing the surrounding objects in a flood of most gorgeous but mellow light. . . .

By far the most magnificent meteor seen on the morning of the 13th, in this vicinity, crossed the vertical meridian about 3 o'clock a.m. Its course was nearly due west, in length by conjecture, about 45°, and at a distance of about 25° south from the zenith. In size, it appeared somewhat larger than the full moon rising. I was startled by the splendid light in which the surrounding scene was exhibited, rendering even small objects quite visible; but I heard no noise, though every sense seemed to be suddenly aroused, if I may so speak, in sympathy with the violent impression on the sight. Nor did I at any time hear any aerial noises. It is proper, however, to add, that although I was very attentive, the movement of my sulkey would have rendered slight sounds inaudible. The track of the meteor adverted to, was visible at least twenty minutes, or, forming my estimate from the distance I travelled the while I should rather say, half an hour. I greatly regretted my want of instruments for taking the altitude of this track . . . I concluded it was probably several miles high."

The conclusions concerning numbers and kinds of meteors were summed up by Olmsted as follows (*op. cit.*, vol. 25, p. 389).

3. NUMBER.—The whole number of meteors that fell towards the earth cannot be accurately estimated, but it must have been *immensely great*. Few accurate attempts appear to have been made to estimate the number of meteors that fell within a given time. It is well known that the number of the stars is, by most people, greatly overrated; and, for a similar reason, the number of the meteors was doubtless generally estimated much too high, some describing them as descending by "thousands" at a time, and some even by "millions".

The writer in the Boston Centinel, whose description we have inserted at length on page 366, appears to have made as exact an estimate as any we have met with, although we think it considerably too low. He supposes the number of meteors which fell during the fifteen minutes before 6 o'clock to have been 8660. Consequently they must have fallen at the rate of 34,640 an hour, making for three hours, 103,920. The observer mentions that the number had become fewer at the time of counting, in consequence, probably of the advancing light of day. Reckoning, therefore, from 12, till 7 o'clock, we may safely double the foregoing amount, making the aggregate number of meteors 207,840,—an estimate which probably does not exceed, though it may fall very far short of the whole number which were visible at Boston. On the supposition that the meteors seen at places remote from each other, were not the same, the entire number that descended towards the earth, must have been indefinitely great.

The meteors however, were not uniformly distributed over the sky, but appear, at some places of observation, to have been peculiarly abundant in particular parts of the heavens. . . .

4. VARIETIES. The meteors exhibited three distinct varieties: the first consisting of *phosphoric lines*, apparently described by a point; the second, of large *fire balls*, that at intervals, darted along the sky, leaving trains that occasionally remained for some time; the third, of *luminous bodies that continued for a long time in view.*

You will notice that, in addition to the enormous number of ordinary falling stars, Olmsted draws attention to those which were outstandingly brighter than the rest, fireballs, and trains that continued a long time in view. In the next issue of the JOURNAL we will reprint some more reports of these exceedingly bright objects.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

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THE LEONID SHOWER OF 1833 (continued from the October issue)

IN reading through many pages of contemporary accounts of the great Leonid shower of 1833 (in *American Journal of Science*, series 1, vols. 25 and 26, 1834), I was surprised at the number of times that truly spectacular fireballs are mentioned with this shower. This aspect of the great shower tends to be overlooked in the discussion of the enormous numbers of more ordinary meteors. These fireballs certainly sound bright enough to be stone-dropping meteors. Why, then, is there a scarcity of meteorites recovered from great showers? Is the chemical composition of shower fireballs different from that of meteorites presumed to come from the asteroid belt? If these fireballs were similar to cometary ices they might completely volatilize in their passage through the earth's atmosphere. Here are some more of the accounts which specially describe fireballs, along with other phenomena of the shower.

From Annapolis comes a vivid description of the "silent and simultaneous dance of the stars" with reference to one fireball "as large as the moon."

4. Phenomena as observed at *Annapclis*, (Lat. 39° N., Lon. 76° 43′ W.). Communicated to Professor Olmsted by the writer, Rev. Dr. HUMPHREYS, President of St. John's College.

"A remarkable phenomenon of *shooting stars* was seen at Annapolis, about 4 or 5 o'clock, on the morning of Wednesday, the 13th instant; the number of the meteors was far greater than in any former instance ever observed by the writer. They all appeared to move from a common centre, at or near the zenith; and at times, they completely filled the whole heavens, particularly towards the East, with beautiful brilliant streams of light, extending to the horizon. It is not meant that all the trains actually extended from the zenith to the horizon; but that the lines of light were *so directed*, that if *produced*, they would all converge to a point in the zenith. Their appearance was so incessant during some part of the phenomenon, that all the stars of the firmament, seemed to be darting from their places. Many persons thought a shower of fire was falling, and became exceedingly alarmed. The light was so intense, that apartments, where persons were sleeping, were strongly illuminated, and some were aroused under the apprehensions that their dwellings were in flames. It prevailed most for about an hour before the dawn of day. It is known to the writer, that numbers of shooting stars were seen as early as 2 o'clock, in the morning. The phenomenon must have continued therefore more or less vividly, for four or five hours. During the period just previous to the dawn, it was observed by many intelligent persons in this city, whose statements coincide most perfectly, as to the *almost infinite number* of the meteors. In the words of most, they fell, *like flakes of snow*. . . . the accounts differ, as to *the size* of some of the meteors. One, in particular, is stated by several, to have been as large as the moon, while to others it appeared considerably smaller. So also, the most brilliant of them, was said by some to have been visible for more than a minute, though it could not, probably have continued longer than a few seconds. . . . It is certain that one of the *trains* remained faintly for about thirty seconds. No audible explosion so far as we can learn attended any of the meteors. It was as it were, a perfectly *silent and simultaneous dance of the stars*. It is probable, that the phenomenon was seen over a wide range of the country. . . .

Notwithstanding the strong persuasion of several observers, that the meteors fell upon the ground, the writer is convinced that their paths were in the upper and rarer strata of the atmosphere, since optical principles show that in darting away to the horizon, they would *appear* to descend and to strike into the earth."

A fine summary of the contemporary accounts has been given by Alexander C. Twining, vol. 26, page 320, with especial attention to some of the bright fireballs.

Art. VIII. Investigations respecting the Meteors of Nov. 13, 1833.—Remarks upon Prof. Olmsted's theory respecting the cause; by Alexander C. Twining, Civil Engineer and late Tutor in Yale College.

The writer of this article had the privilege of witnessing the meteoric display, on the morning of Nov. 13, 1833, from a few minutes past 5, by the watch, till day. Since that time, he has had opportunity to collect well authenticated facts from observers in different places, and, by reflection upon the facts which he himself witnessed and those which he has learned from other observers, has arrived at conclusions satisfactory, in some measure, to his own mind, respecting both the *facts* and their *cause*. . . .

FIRST. *The observed facts.*

It is established upon the witness of Prof. Thompson and other authority specified in Mr. Olmsted's memoir, and upon a statement to be given at large in a succeeding paragraph, that the first meteors showed themselves as early as nine o'clock in the evening of Nov. 12, in parts of the United States the most distant from each other—for example, in the state of Mississippi, at Augusta in Georgia, at Charleston, S.C. and on the Hudson river above the city of New York. . . . The following statements, which have not appeared before, throw light upon the commencement and progress of the phenomena as they developed themselves in the region of New York.

Capt. Isaac Faurot, known as an intelligent man who sails constantly between West Point and New York, was on the Hudson, the whole night, on the downward passage. His account is; that, being in Tappan bay at 8 or 9 p.m. and the wind being south west, he saw, as frequently as once in every minute, common shooting stars coursing against the wind to the S.W. by W. or W.S.S. They rose mostly in the N.E.; and those which started down in the S.E. were very short; about midnight the stars became too numerous to be counted; but maintained the same general course, shooting on every side to the west of S.W.,—some were very short and others long and brilliant, and many of them were seen coursing over head. Between twelve and one o'clock the trains were longest and most brilliant; but though less brilliant afterwards, the meteors continued progressively to become more numerous till about day break when they seemed like rain drops beginning to fall from a shower. They still kept the same direction as before but "dropped down", as it were, instead of coursing across the sky as they seemed to do at first. Capt. Seymour, of the steam boat De Witt Clinton, was upon the Hudson in one of the North river boats, on the downward passage, and witnessed the display entire. . . . During the night he observed as many as fifty brilliant rocket-like meteors of large size and long trains; the most remarkable instance of which occurred just before dawn, in the case of a meteor which left a vivid train that remained straight four minutes, by the watch, then wavered in the middle and progressively towards the extremities, and finally coiled up into a cloud as bright, at first, as the train itself. The cloud continued in sight two minutes, and left a shade still visible. . . .

In fact the most brilliant meteor which the writer saw on the morning of the 13th, blazed into view as far as 90° from the radiant, having an altitude of 20°, or perhaps a little more, and an azimuth of N.15° W.; falling vertically down to the tops of the hills which were about 4° elevated, where its train tapered to a point. It was a fiery ball of a deep red color, and perhaps 6′ in diameter when divested of the glare which made its appearance full as large as 10′, and it travelled down the sky with majestic rapidity, carrying an impression of united force and splendor. Its track was marked by a train of uniform breadth, but little exceeding that of the ball, and of the same deep color, excepting that it was prismatic in its tints—certainly near the point where the meteor seemed to be expiring. The general aspect of the phenomenon was the same as if a column of glowing melted metal had been poured down from the spot whence the meteor issued.

Twining gathers together some of the fireball data as follows:

Some of the trains left by the meteors were so peculiar in their aspects or remarkable in their changes, *that a meteor seen at different places might in some cases be known as one by means of its train and thus data be obtained for calculation of the meteor's height.*

Four meteors at least—one in each quarter of the United States—and which must have been distinct one from another, are known to have given origin to a train which curled up into a luminous cloud and remained in sight several minutes. One of these which was borne east as if by the wind, was seen at New Haven in the northwest by Prof. Olmsted and is described by him (vol. xxv, p. 366;) another certainly, and perhaps more than one, also borne east by the wind, is described as seen in Ohio by Mr. Riddel (p. 377) a third, in N. Carolina, was seen in the south by Dr. Smith (p. 379) and a fourth was seen east of south in Louisiana, near Fort Jesup, by Mr. Peter Peterson, and is described by Dr. Leavenworth, in a letter to Prof. Olmsted. . . .

The following is extracted from a letter written by Mr. E. Wade, dated Union Ville, Geauga Co. Ohio, Dec. 24, 1833. "Another peculiarity was the luminous train left behind the larger meteors for some seconds, and in one (and I presume the largest which could have been seen during the time of my observation) for a space of from five to ten minutes. I was particular in the observation of this. It started from near the zenith and shot off nearly north west to within perhaps fifteen degrees of the horizon, illumining the whole heavens "above the brightness of the sun" for a few seconds. The luminous train of this meteor (distinctly visible for five minutes) was probably not less than forty degrees in length. It began gradually to bend from the point towards the zenith eastwardly until, while it was distinctly to be seen, the upper half (for the angle was formed about the middle) formed a right angle with the lower half of the train, and in this position gradually vanished."

Mr. James Sperry, in a letter dated Henrietta, Monroe Co. N.Y. writes—"The large ball which you describe as having shot off in a N.W. direction, resembled one which I saw at fifteen minutes past five, moving to the S.W. This left a streak of light apparently as broad as the moon, and extending at least 30° of the arch of the heavens, that was visible three minutes, shining at first with such splendour that small objects on the earth could be as easily distinguished as at the full of the moon: it was straight at first; but, after continuing about one minute, contracted and crooked in the middle—the bend forming nearly a right angle with the other part, and then gradually grew more dim until it disappeared."

Twining drew the following conclusions from the observations of the fireball seen by both Capt. Seymour and Prof. Olmsted (page 342).

The result is, *that this meteor exploded, or expired, at the height of twenty nine miles and a half, above the surface of the earth.* . . . As there is no reason to suppose this meteor to have been luminous before its entrance into the atmosphere, we see, in these facts, a presumption *that the atmosphere itself must have its limit much higher than is generally supposed.* . . . The cloud must therefore, when it vanished, have held an elevation of about twenty one miles—having descended probably more than seventeen miles in addition to its horizontal motion. The distance of twenty three miles and a half—which appears to have been the effect of a current in the atmosphere—was passed in less than four minutes. . . .

Our conclusions come to this; *that this particular meteor, and probably all the meteors, entered the atmosphere with a velocity not less, but perhaps greater, than fourteen miles in a second; that they became luminous many miles from the earth—in this case over eighty miles; and became extinct high above the surface—in this case nearly thirty miles.*

Twining also summarizes the shower in general (page 334).

1st. The shower commenced feebly about 9 p.m. Nov. 12 throughout the United States, and every where came to its greatest intensity at about the same hour,—probably half past 4 of the morning following.

2nd. The general phenomena, exhibited in the body of the shower, were every where alike; particularly that remarkable one of the existence of a point of apparent radiation.

In his final summation, Twining leans heavily on the philosophical implications of the great event (page 351).

Final Obs.—When we glance back upon all the known circumstances of the meteoric shower, the extent and magnitude of the powers in exercise fill the mind with wonder. The one body whose flight we have been able to calculate, and which moved with such inconceivable rapidity, could not have been less—after allowing for the deceitful glare of surrounding and enveloping flame—than a hundred feet in diameter; and doubtless very many others were as large. The multitude of bodies was such as no man can venture with confidence to limit by numbers; and, had they held on their course unabated for three seconds longer, half a continent must, to all appearance, have been involved in unheard of calamity. But that Almighty Being who made the world, and knew its dangers, gave it also its armature—endowing the atmospheric medium around it with *protecting*, no less than with life sustaining properties; and, considered as one of the rare and wonderful displays of the Creator's preserving care, as well as the terrible magnitude and power of his agencies, it is not meet that such occurrences as those of Nov. 13th, should leave no more solid and permanent effect, upon the human mind, than the impression of a splendid scene. To return, however, to our immediate subject and its connexion with the cause of human knowledge:—we may well regret that, with so much to be known apparently within our reach, so little that is definite should be, as yet, disclosed.

These eyewitness accounts convey an unforgettable impression of the night when "all the stars of the firmament seemed to be darting from their places", when the meteors fell "like flakes of snow", and when a fireball illumined the whole heavens "above the brightness of the sun for a few seconds". We can only hope that in the twentieth century we may be treated to a similar display, from either the Leonids or some other meteor shower.

(Concluded)

OUT OF OLD BOOKS

By Helen Sawyer Hogg

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Peary and the Cape York Meteorites (the Saviksue)

One of the unfortunate aspects of the rapid growth of astronomy in recent years is that the volume of new knowledge seems effectively to displace much of the old knowledge. After all, young astronomers have only a finite number of hours for studying the literature.

The Cape York meteorites illustrate this point sharply. They are among the most important meteorites in the world since one of them, the Ahnighito or Tent, is the largest known meteorite readily accessible to human view. Text-books commonly mention them as the meteorites which Peary found near Cape York, Greenland and brought back to New York City—a phrase which conveys nothing of the hardships Peary went through to discover and capture these masses of iron.

In point of fact, Robert E. Peary, Civil Engineer, U.S.N., made three separate voyages to the Arctic largely for the purpose of locating and collecting these meteorites. Not merely did he undergo the strain of the rigorous conditions of Arctic exploration, but also he and his wife personally raised the funds to carry on this work, and contributed their own money. Peary states that fully two-thirds of the total amount he expended in Arctic work over twelve years was from his own personal earnings, and that no other single contribution exceeded $1,000 except in one case when President Jesup of the American Museum of Natural History sent a ship to rescue him in 1895. Peary published the full account of these explorations in two volumes, "Northward Over The Great Ice", Frederick A. Stokes Company, Cambridge, Mass., 1898, with the account of the "Saviksue" or Cape York meteorites in Volume II. So unreservedly had Peary put his earnings into the cause that at that time he was several thousands of dollars in debt.

The first expedition in which Peary systematically hunted for the Saviksue (Great Irons) mentioned by the Eskimos, was the North Greenland Expedition of 1893–94, setting out in June 1893 in the *Falcon*. Peary and his party wintered in Greenland. It was in May 1894 that Peary first located the meteorites. After some years of trying to get specific information about the location of the "Iron Mountain" of the Eskimos of the desolate Cape York region, he won the confidence of the

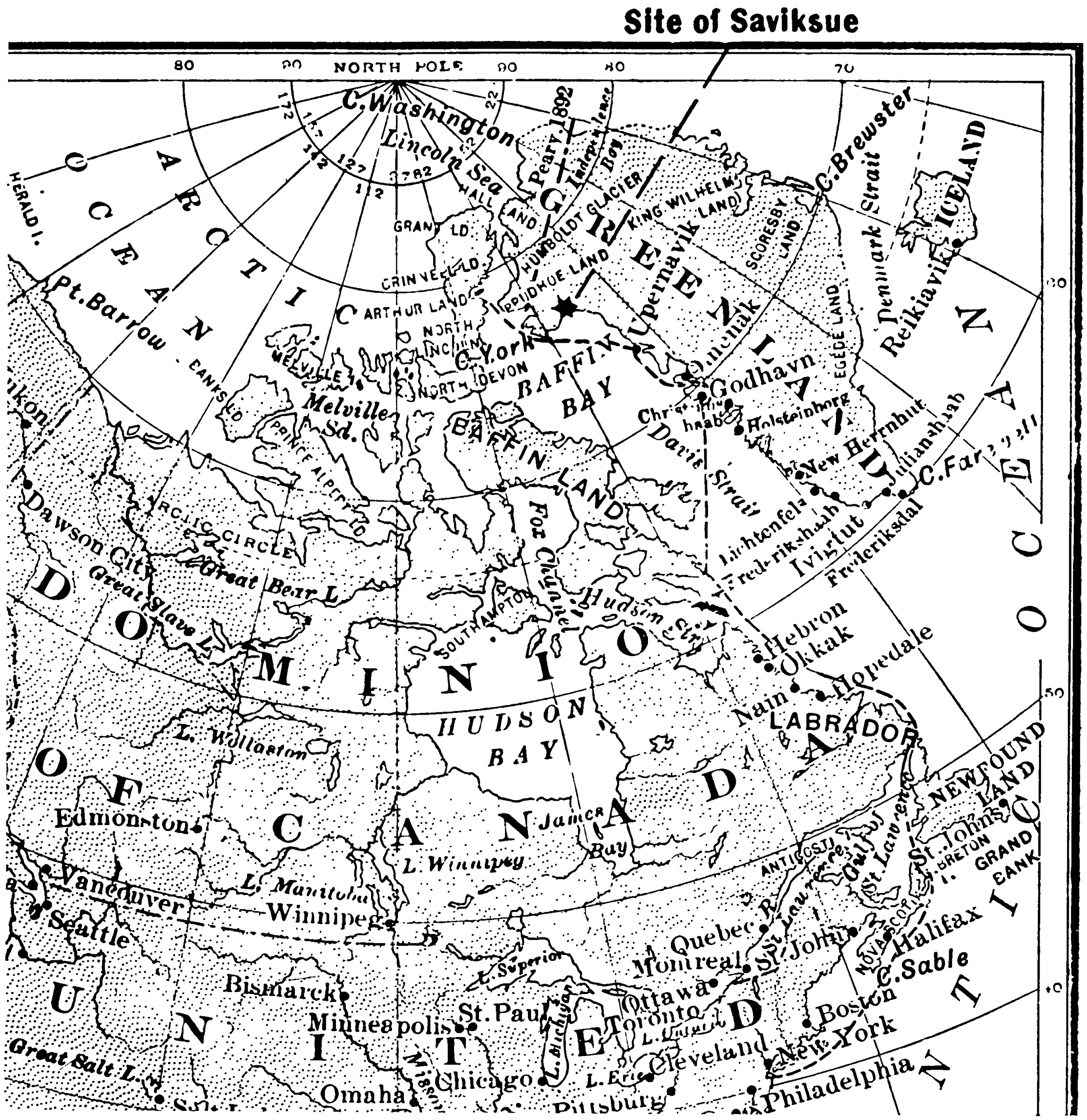

FIG. 1—Map of Peary's route to Cape York and the location of the Saviksue.

tribe of Smith-Sound Eskimos, and one of them, Tallakoteah, guided him to the site. Thus ended a search pursued by white men since 1818 when Captain Ross found the Eskimos of that region using knives of iron, whose source he could not locate.

The *Falcon* returned in 1894 and brought back most of the party, including Mrs. Peary and her daughter, but Peary remained behind with two companions. While returning to St. John's after landing the party in Philadelphia, the *Falcon* was tragically lost with all hands. This meant that a replacement ship had to be found to return for Peary the following summer. Though Mrs. Peary managed to raise a substantial sum, it was

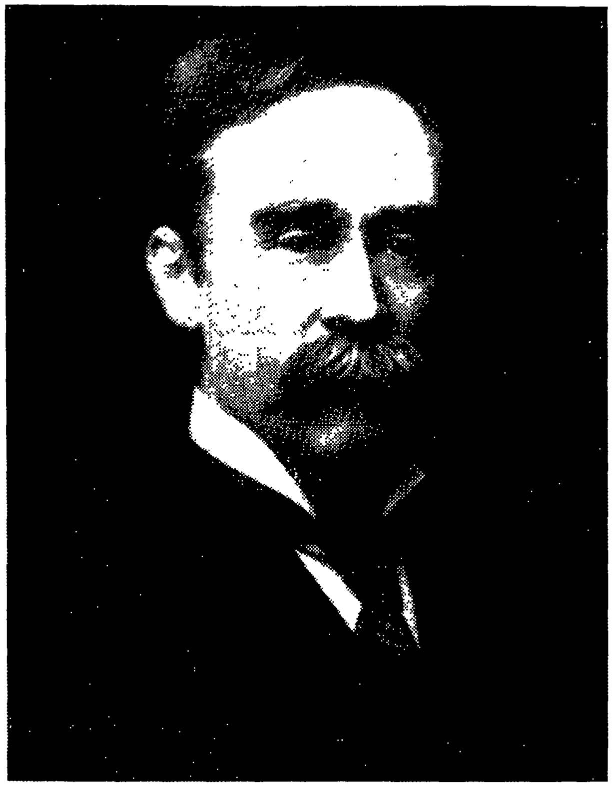

FIG. 2—Robert E. Peary, Civil Engineer, U.S.N. at the time of the Cape York expeditions. In 1902 he was promoted to the rank of commander, and in 1911, to rear admiral. (Courtesy Hayden Planetarium, American Museum of Natural History.)

only through the help of Morris Jesup that the ship *Kite* was obtained and sent north in 1895. With the help of the *Kite* on this voyage, Peary first began his work of removal of the meteorites from the icy wastes of northern Greenland.

Peary describes their history and the attempts to locate these meteorites:

HISTORY, AND EFFORTS TO SECURE

Of all the great meteorites of the world's collections, as well as the more or less legendary and mysterious celestial visitors, the "heaven stones", "thunderbolts", "abaddirs", Palladium, etc., which have elicited the awe and veneration of man since remote antiquity, the "Saviksue" or Cape-York meteorites, must, from their exceptional size, their purity and homogeneousness of composition, the extreme northern latitude in which they were found, their incontrovertibly celestial origin, and their human associations, be conceded to rank first.

The history of these meteorites up to the time of their discovery by me is comprised in the statement that, when Capt. Ross in 1818 discovered the existence, in the vicinity of Cape York, of a previously unknown tribe of Eskimos, he found in their possession rude knives and harpoon points with cutting edges of iron. The metal in these implements, as well as could be determined from the imperfect communication with these people, had been obtained by them from an "Iron Mountain" on the northern shore of Melville Bay.

An analysis of the metal showed the presence of nickel, and led to the inference that the source of iron supply of these northern people was meteoric. For a full account of this, and for various papers bearing upon the subject, the reader is referred to Capt. Ross's narrative and to the *Arctic Manual*.

Nordenskjöld's discovery of the famous Ovifak irons on Disco Island, and the ultimate determination of their telluric rather than extra-terrestrial origin, gave rise to doubts as to the meteoric character of the more northern and semi-mythical Cape-York iron, and it was assumed that this iron was also telluric.

One of the objects of almost every expedition which has gone north in that region since 1818 has been the solution of the mystery of the "Iron Mountain".

Peary then gives a lengthy description of the region where the meteorites lay, "on the northern shore of that great icy fastness, Melville Bay, some thirty-five miles east of Cape York". The two smaller meteorites, the "woman" and the "dog" were a few yards north of the end of an isthmus of land dividing Glacier Bay to the east from Saviksoah Bay to the west, about eighty feet above sea level. The two smaller of the "Saviksue" were lying loosely on gneissose rocks which cover the ground.

Close to the northern shore of Melville Bay, and previously mistaken for part of the mainland, is an island. Midway on its eastern shore, about six miles south of the site of the "woman" and the "dog" the third and largest of the Saviksue was lying nearly buried in rocks and soil. The "Ahnighito" or "tent" meteorite was about eighty feet above high-water mark, and about a hundred yards from the shore.

The Arctic severity of Melville Bay is vividly described by Peary:

In winter this region is the desolation of Arctic desolations, constantly harassed by biting winds, and every rock deep buried beneath the snow, swept in by these winds throughout the long dark night, from the broad expanse of Melville Bay, and piled in drifts, which in many places are hundreds of feet deep. Even in summer, only the directly southward-facing slopes of the mountains are free from snow for a few weeks, while in the valleys and on the northward slopes the drifts remain eternally. A large portion of the ice and bergs of Melville Bay pass close along this coast in their slow drift westward toward the southerly current of Smith Sound. Consequently the shore is beset with ice during about eleven months of even the most favourable years, and the slightest increase in the severity of a season beyond the normal, results in the coast being completely blockaded and rendered inaccessible throughout the entire year.

Peary's description of his trip in 1894 and his first glimpse of the "Iron Mountain" is well worth the reading.

On the 16th of May I left the lodge again with Lee, my iron-runner sledge, and ten dogs, in search of the "Iron Mountain" of Melville Bay....

At Netiulumi, we occupied the tupik of one-eyed Merktoshar and his kindly wife Ahma....

Among the natives here was Tallakoteah, who at Red Cliff, two years ago, had acted as my mail carrier, taking letters to Cape York to deliver to a whaler . . . .

This man was thoroughly conversant with the region about Cape York, having lived there several seasons, and professed to be well acquainted with the location of the "Iron Mountain", which he said he had seen repeatedly. He told me that there were three *saviksue* (great irons) of varying sizes, the smallest about the size of a *mikkie* (dog), indicating a dog curled up, the second considerably larger, and the third still larger than the second.

He also said that one of them was neither very high above the water-level nor very far from the water, while the other two were upon the side of the mountain. He agreed to go with us to Cape York and guide me to them.

He would take his own sledge and four dogs, and for the consideration of a knife I obtained from Ahngeenyah five more fine animals, which would give me sixteen dogs in all. . . .

At one a.m. of the 19th, we left Netiulumi, Tallakoteah and myself on one sledge drawn by ten dogs, Lee following with the second sledge drawn by six.

. . . At last we had reached "Imnaminomen" (Cape York) after ten days of struggle with the difficulties of Arctic-spring travelling, but even now the outlook was not encouraging for a termination of our troubles, and there was every possibility that we might be storm-bound here for several days.

Three days later, the storm had abated sufficiently for us to start. . . .

Skirting along the shore, we passed round the south-east point of Cape York with its numerous deserted igloos, to the village beside the glacier where the *Falcon* stopped last summer. From this point our course lay straight across the bay to the islands on the eastern side, where there were said to be four igloos, and where we thought to find my old acquaintance "little" Kessuh, the same youth that I had expected would be my guide two years ago. The entire circuit of this bay, which is certainly large enough to deserve a name on the charts, from the Eskimo village which we had just left, round to the islands ahead of us, is a glacier face broken by a few nunataks. Arrived at the island igloos, we found them deserted, but a fresh sledge track led from them round the end of the island, and following this we soon came to a cave in the rocks, and in the cave was our little friend fast asleep upon a luxurious bed of bearskins with a deerskin thrown over him. . . .

He jumped at the opportunity of accompanying us, and in a few moments was dressed and had his dogs fastened to his sledge. Six of my dogs were added to his four. Lee got on the sledge with him, and with this arrangement of loads, fresh dogs, and hard snow we left the cave at a gallop, which speed was kept up past the outer island and eastward along the shore till after midnight, when we reached the western point of the double-armed bay, running into the land north of Bushnan Island. . . .

At 4:15 in the morning, we had reached the head of the bay, the dogs were fast to the ice-foot, and Tallakoteah and myself were climbing over it in search of the "Iron Mountain".

After passing some five hundred yards up a narrow valley, Tallakoteah began looking about until a bit of blue trap-rock, projecting above the snow, caught his eye. Kicking aside the snow, he exposed more pieces, saying this was a pile of the stones used in pounding fragments from the "iron mountain". He then indicated a spot four or five feet distant as the location of the long-sought object. Returning to the sledge for the saw-knife, he began excavating the snow, and at last, after digging a pit some three feet deep and five feet in diameter, just at 5:30 Sunday morning, May 27, 1894, the brown

FIG. 3—The Ahnighito meteorite *in situ*, almost entirely buried in the ground, with Eskimo excavator. (Courtesy Hayden Planetarium, American Museum of Natural History.)

mass, rudely awakened from its winter's sleep, found for the first time in its cycles of existence the eyes of a white man gazing upon it.

I kept Tallakoteah at work enlarging the pit and excavating about the meteorite until Lee and Kessuh arrived, when he was relieved by the latter. In addition to the thick blanket of snow, the meteorite was completely covered with a half-inch-thick coating of ice. The work of excavation satisfactorily completed, I spent the remainder of the perfect, cloudless day of Sunday, until four o'clock in the afternoon, in measuring, sketching, and photographing the heavenly visitor and taking angles for a rough map of the vicinity, and then descended to the sledge for a little needed sleep.

Tallakoteah tells me that the Innuits call the meteorite a woman in a sitting position, and says it used to be much larger and higher than it is now, but that his people have gradually worn it down, and that years ago natives from Peterahwik broke off the head and carried it away. He also voluntarily told how the ancient knives of his people used to be made, namely, by inserting several small flattened pieces of the metal in a bone or ivory back, and then with a piece of trap lying near, showed me how the flakes of iron were detached. Nothing could be more interesting than his re-enacting of this ancient practice.

I scratched a rough "P" on the surface of the metal, as an indisputable proof of my having found the meteorite, in case I should not be able, later on, to reach it with my ship; and built a small cairn upon the top of a big gneissose boulder, 112 yards distant, in which I placed a brief record:

"Sunday, May 27, 1894."

"This record is deposited to show that on the above date R. E. Peary, U.S. Navy, and Hugh J. Lee of the North-Greenland Expedition of 1893–94, with Tallakoteah, an Eskimo guide, discovered the famous 'Iron Mountain', first mentioned by Capt. Ross, and have carefully examined the same."

Signed "R. E. Peary, U.S.N.
"*Comd'g Expedition.*"

Then, after a last look at the celestial straggler, I descended to the sledge where Lee had already preceded me, and stretching myself upon it immediately fell asleep. Two hours later, I awoke to find the entire sky overcast and a chill wind blowing up the bay. The weather demon had given us just one perfect day in which to learn the secret of the "Iron Mountain" and was now resuming his baleful sway.

Supper, breakfast, or dinner, just as one chooses to call it, over, the dogs were hitched up and we started to locate the second and largest mass, which my guide told me was on the island at the entrance to the bay. Passing at a good pace down the bay, we soon reached the site of this second meteorite, some seven miles distant on the eastern end of the island. Its location was pointed out to me, but the depth of snow covering the entire island was so great that I made no attempt to dig for it, satisfied to know where it was. At midnight we started on our return, and ten days later, on the 6th of June, were back at the lodge.

Peary describes his first attempts to get the meteorites on board a ship for home:

In the latter part of August of the same year I attempted in the *Falcon* to penetrate Melville Bay to the site of the meteorites, and embark them for the purpose of sending them home. The summer of 1894, however, was an unusually severe one in this portion of the Arctic regions, and the ice of Melville Bay did not move out at all, but remained cemented to the shore throughout the entire season, rendering it impossible for me to get my ship within thirty or forty miles of my prizes.

In December of the same year (the midnight of the Arctic winter night) I made a second attempt to revisit the meteorites, sledging from the lodge in Bowdoin Bay, but bad weather combined with the darkness to close the ever inhospitable door of Melville Bay to me, and I was unable to get beyond Cape York, where I was storm-bound for several days, and then returned to the lodge, narrowly escaping the loss of my dogs and sledge by the breaking up of the ice about me while rounding Cape Parry.

The story of an earlier attempt by the Eskimos to carry away the head of the "woman" is told very vividly by Peary.

The historical data to be obtained from the natives in regard to the meteorites is rather scanty. According to them the "Saviksue" (great iron) have been where I discovered them from time immemorial; but they were originally an Innuit woman and her dog and tent hurled from the sky by Tornarsuk (the Evil Spirit). They say that at first the "woman" was in shape like a woman seated and sewing, but that the constant chipping off of fragments through successive ages has gradually removed the upper portion of her body and reduced her size one-half or one-third. Years ago her head became detached and a party of Eskimos from Peterahwik or Etah (settlements north of Whale Sound) attempted to carry it away, actuated probably by the desire to have a supply of the precious metal more convenient, and save themselves the long and arduous journey to Cape York and into Melville Bay, when they needed to replenish their stock of iron. The head was lashed upon a sledge and the party started for their home, but when well out from the shore the sea ice suddenly broke up with a loud noise, and the head disappeared beneath the water, dragging down with it the sledge and dogs. The Eskimos themselves narrowly escaped with their lives, and since that time no attempt has been made to carry away any but the smallest fragments of the heavenly woman.

This mass is the one from which all the ancient iron supply of this people was obtained. . . . For several generations, probably from the time of the wintering of the *North Star* or possibly earlier, no use has been made of the iron of these meteorites by the natives; they obtained their scant supply of knives from the whalers and expedition ships visiting their coast or beset in the ice off Cape York.

(*To be continued*)

OUT OF OLD BOOKS

By Helen Sawyer Hogg

David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

Peary and the Cape York Meteorites (the Saviksue) (*continued*)

In May 1894 when Peary succeeded in locating the "Iron Mountain" of the Eskimos of Melville Bay, he became the first man from the civilized world to gaze upon the Cape York meteorites. Ever since Captain Ross in 1818 had come upon the mystery of the iron which the Eskimos were using in their knives, various expeditions had tried and failed to solve the riddle of the Iron Mountain. In the 40's the King of Denmark authorized an expedition for the purpose, but nothing came of it. The officers of the Franklin search ship, the *North Star*, in the winter of 1849–50, tried and failed. So too did the English Arctic Expedition of 1875–76 and Baron Nordenskjold's ship in 1883. It remained for Peary to succeed in locating the iron.

Though to locate the meteorites was difficult, to remove them, particularly the largest, was even more so. However, Peary persevered, despite the gloomy Eskimo tale of the loss of the head of the "woman" and his own previous frustrations in trying to reach the desolate location of the meteorites with equipment adequate to remove them. Peary tells how in August 1895 he was successful in acquiring the two smaller Cape York meteorites.

SECURING "WOMAN" AND "DOG" IN 1895

In spite of my previous unsuccessful attempts to revisit the meteorites the effort was not given up, and finally late in August, 1895, I rounded Cape York in the Steamer *Kite*, which had been sent by Mrs. Peary to bring me and my two companions home. . . .

As we penetrated mile after mile into the icy fastnesses of Melville Bay without finding our progress barred by ice, my hopes began to rise, only to be dashed again when when we entered Saviksoah Bay and saw the previous winter's ice stretching entirely across it. It looked as if even after getting thus far I was yet to be stopped several miles away from the objects of my visit. From the masthead a narrow lead of open water was detected penetrating the bay, and following this lead to its end, then ramming the *Kite* her length into the edge of the floe, the ice-hooks were put out and the ship made fast a mile from the shore. . . .

I climbed over the side of the *Kite*, crossed the ice, reached the ice-foot at the head of the bay, and, passing up the little valley, stood once more beside the great heaven-born mass, from which a little more than a year before I had removed the deep covering of the winter's snows.

With the snow now melted away from the "woman" and her surroundings, it was possible to obtain a clear idea of the difficulties incident to transporting the mass to the ship. I was encouraged to find the meteorite was not larger than I had first estimated it to be (about 5500 lbs.), my excavation of the previous year having determined its maximum dimensions. The continued existence of a large drift of compacted snow and ice in the little valley between it and the head of the bay was also a valuable point in our favour. Yet the several hundred feet of distance intervening between the meteorite and the upper end of this drift, thickly covered with large gneissose boulders, and the wide lane of open water separating the ice in the bay from the shore at the mouth of the valley, presented difficulties which I could see would require all our resources to overcome.

The next day, Diebitsch [Emil Diebitsch, Mrs. Peary's brother] began work with the ship's crew and the Eskimos; the "woman" was lifted out of her bed with jacks, and a rough sledge of spruce poles made for the "dog". On the second day, the "woman" was blocked up ready for transportation, and the "dog" rolled upon its sledge and dragged by the combined force of the ship's crew and my native allies over the boulders and down the snow-drifts to the shore; then ferried across the open water upon a cake of ice, and finally hauled for a distance of about a mile over the surface of the ice in the bay to the ship's side, where it was hoisted on board and deposited in the hold.

On the third day a heavy timber drag was constructed for the "woman", upon which she was placed and secured, then slowly transported upon iron rollers over a plank tramway laid along a rude road-bed, roughly graded by my Eskimos with the abundance of stones in the vicinity. In this way the meteorite was brought to the upper end of the snow-drift. Then after midnight, when the surface of this drift was frozen firmly, it was moved down to the shore, where a huge cake of ice, 40 ft. long by 20 ft. wide by 7 ft. thick, had been securely moored to receive it. Upon this novel ferry-boat it was floated across the open water to the bay ice, and into a dock cut to receive it. . . . As soon as the prize was alongside [the ship], all possible speed was made in hooking on to it with the ship's tackles and purchases; but before this could be completed the ice gave way under the great weight leaving the meteorite only partially secured. Fortunately, the lines and chains already fastened to it were strong enough to hold, though insufficient to lift it, and finally, although nearly submerged by the listing of the *Kite* under the unbalanced load, additional lines were attached and the meteorite slowly warped up to the rail and swung inboard. Everyone breathed a sigh of relief when the sulky giant was safely deposited in the hold.

The work of transporting and embarking these two masses was engineered entirely by Diebitsch, and was accomplished by him in a most able and effective manner.

Peary then describes his first investigation of the Ahnighito, or Tent meteorite, to appraise the difficulties and problems of its removal. He tells also how the rapid formation of heavy ice caused the *Kite* hurriedly to head for home, with the two meteorites, the "woman" and the "dog" safely on board. The presence of so much iron on the ship, however, gave them compass troubles on their way home.

While this work on the two smaller meteorites was progressing, the big one out on the island was visited and partially excavated with a view to getting an idea of its size and weight.

A portion of it about four feet long by two feet high by one and one-half feet wide, projected above the scant turf and moss on the crest of a terrace on the eastern side of Meteorite Island, eighty feet above, and some three hundred yards distant from high-

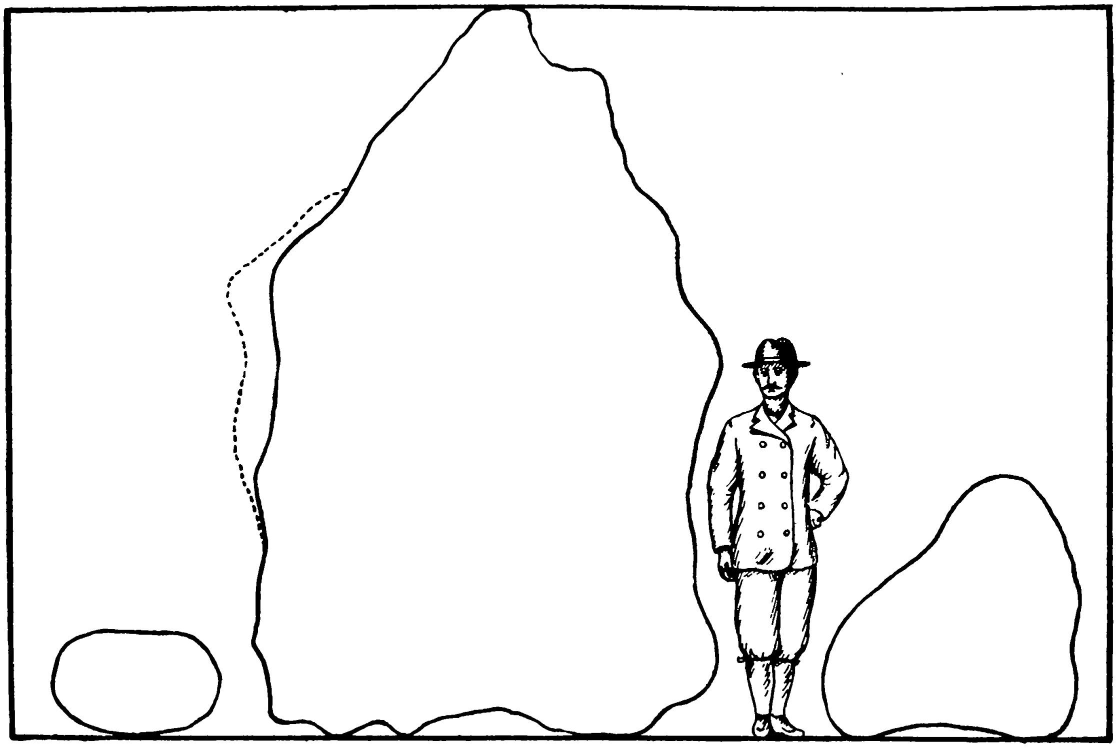

FIG. 1—Peary's sketch showing relative sizes of "Saviksue and Six-Foot Man".

water mark. The excavation developed that this projection was in the nature of a dorsal fin, rising from nearly a flat table about twelve feet long and eight feet wide, tapering at one end to a point or tail. The excavation, although carried down over three feet at this time, did not discover the depth of the mass, which was evidently considerable.

Two ten-ton screw-jacks which I applied together under one end and forced to the point of crippling without disturbing the monster, showed not only our appliances but the ship itself were entirely inadequate for handling and transporting such a huge mass and concentrated weight, which I estimated at one hundred tons. Four days were then devoted to an attempt to break off the point already noted, by drilling holes close together and driving in taper bolts. The toughness of the metal rendered the effort abortive, and the rapid formation of heavy young ice then compelled the retreat of the *Kite* to escape being frozen in for the winter.

With the two meteorites safely on board, the *Kite* proceeded to Cape York and thence to St. John's, Newfoundland, in safety, though the presence of these unusual masses of iron affected our compasses to such an extent that, whenever thick or stormy weather compelled us for any length of time to depend upon our dead reckoning, it was found impossible to keep on our course.

From St. John's, Newfoundland, the meteorites were transported by steamer to New York.

Peary returned to the north in 1896 in a larger ship, hoping to obtain the "Ahnighito".

Determined to secure the giant, I chartered a larger ship, the *Hope*, of 307 tons net register, and went north in July 1896 with more powerful appliances on board, reaching

Cape York August 9. The ice in Melville Bay being not broken up, I put in two weeks north of Cape York, returning there the 22d of August.

The stop at Cape York was only long enough for me to take on board all the able-bodied men of the village, when the *Hope* continued on her course eastward across Cape-York Bay, and so on to Saviksoah Bay and the eastern side of Meteorite Island, where we arrived shortly before noon. Before we reached the natural pier just below the meteorite, its dark-bronze crest could be seen on the top of the terrace, peering out from the debris of last year's excavation. A barrier of ice-pans packed close against the shore delayed us somewhat in getting in; but outside of this was a narrow lane of open water, and beyond this again a chain of grounded icebergs, holding the still unbroken ice of Melville Bay in check.

My full force of Eskimos was set to work at once with pick and shovel, clearing away about the meteorite, and by supper-time the brown monster stood out in all its immensity as to length and breadth, though its depth was still indeterminate. From this time on during ten days, the work on the meteorite was continued night and day. The Captain and the ship's complement took the day watch, and I, with Lee, Henson, and my Eskimos, took the night.

The first thing to be done was to tear the heavenly visitor from its frozen bed of centuries, and as it rose slowly inch by inch under the resistless lift of the hydraulic jacks, gradually displaying its ponderous sides, it grew upon us as Niagara grows upon the observer, and there was not one of us unimpressed by the enormousness of this

FIG. 2—"Ahnighito" starting toward ship in 1896. (Courtesy Hayden Planetarium, American Museum of Natural History.)

lump of metal. The expressions of the Eskimos about the "Saviksoah" (the great iron) were low but earnest, and it, and the other wonderful great irons (the jacks) which could tear it from its bed, awed them to the utmost.

Sliding the meteorite upon steel rails laid upon heavy timbers across the few yards intervening between it and the crest of the hill, it was then rolled down the slope to the natural rock-pier.

It was interesting, though irritating, to watch the stubbornness of the monster as it sulked and hung back to the last inch. Under the strain of the two powerful chain blocks which transformed the wire cable and the big chain straps into rigid bars of steel, and urged by the resistless lift of the jacks, the huge brown mass would slowly and stubbornly rise on its side, and be forced to a position of unstable equilibrium; then everyone, except the men at the chain blocks down at the foot of the hill, would stand aside. A few more pulls on these, then cable and the chain straps would slacken, the top of the meteorite would move almost imperceptibly forward, the stones under the edge of revolution would begin to splinter and crumble, then, amidst the shouts of the natives and our own suppressed breathing, the "Iron Mountain" would roll over. . . .

Arrived at the bottom of the slope, the meteorite was again lifted upon the rails and timbers, and slowly and laboriously pushed forward towards the edge of the pier.

Never have I had the terrific majesty of the force of gravity and the meaning of the terms "momentum" and "inertia" so powerfully brought home to me, as in handling this mountain of iron. No purchase or appliance which we could bring to bear upon it, outside of the jacks, made the slightest impression upon it. When lowered slowly upon heavy timber blocking by the jacks, it settled resistlessly into the wood until it seemed as if it would never stop. The timber creaked and groaned in every fibre, and in the immediate vicinity of the pressure its structure was entirely destroyed and it became a mass of incoherent fibres. . . . The inherent deviltry of inanimate objects was never more strikingly illustrated than in this monster. Had the matter been a subject of study for weeks by the celestial forge-master, I doubt if any shape could have been devised that would have been any more completely ill-suited for handling in any way, either rolling or sliding or lifting.

Peary recounts that the difficulty of handling the monster with the jacks was so great that his two thirty-ton jacks gradually gave out. The sixty-ton jack was already out of commission, and at last he had only an unwieldy hundred-ton one left.

Especially vivid is Peary's description of one particular night around the Ahnighito.

There were many incidents of the work to suggest the supernatural even to the most prosaic mind. The dogged sullen obstinacy and enormous inertia of the giant against being moved; its utter contempt and disregard of all attempts to guide or control it when once in motion; and the remorseless way in which it destroyed everything opposed to it, seemed demoniac.

I remember one particularly striking occasion. It was the last night of our stay at the island,—a night of such savage wildness as is possible only in the Arctic regions. In spite of the driving storm, it kept artist Operti running up out of the warmth and light of the cabin, upon the snow-covered deck, to feast his eyes upon the scene. The wild gale was howling out of the depth of Melville Bay through the *Hope's* rigging, and the snow was driving in horizontal lines. The white slopes of the hill down which the meteorite had been brought, showed a ghastly grey through the darkness; the fire, round which the fur-clad forms of the Eskimos were grouped, spread its bright red glare for a short distance; a little to one side was a faint glow of light through the skin wall of a solitary tupik. Working about the meteorite was my own little party, and in the foreground the central figure, the *raison d'être* of it all, the "Saviksoah", the "Iron Mountain", towering above the human figures about it, and standing out black and uncompromising. While everything else was buried in the snow, the "Saviksoah" was unaffected. The great flakes vanished as they touched it, and the effect was very impressive. It was as if the giant were saying: "I am apart from all this, I am heaven-born, and still carry in my heart some of the warmth of those long-gone days before I was hurled upon this frozen desert." To strengthen this fancy that the meteorite still held some of its celestial fire and feeling, if a sledge, ill-aimed in the darkness at wedge or block, chanced to strike it, a spouting jet of scintillating sparks lit the gloom, and a deep note, sonorous as a bell, a polar tocsin, or the half-pained, half-enraged bellow of a lost soul, answered the blow.

Through all this time of labour and exposure, my Eskimo allies worked faithfully and contentedly, sleeping between decks when they could find time. They assisted in every possible way, and never interposed the slightest objection to my removal of their heavenly guest,—in fact, seemed almost as disappointed as I when the insweeping ice compelled me to give up my prize till another time.

That was the fate of the 1896 expedition—the Ahnighito could not be boarded on ship. As Peary relates:

"Then progress became so slow that before I could get the meteorite close to the edge of

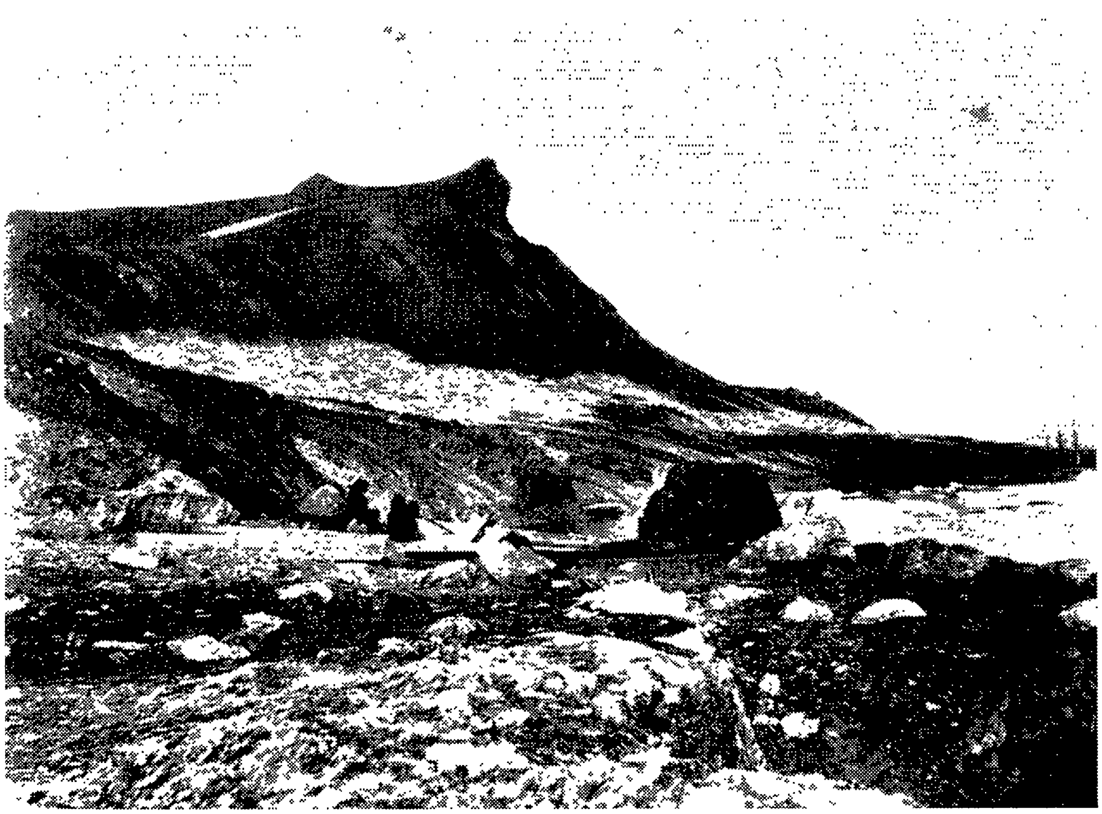

FIG. 3—The Saviksoah as left in 1896. Signal Mountain in the background, with masts of the *Hope*. (Courtesy Hayden Planetarium, American Museum of Natural History.)

FIG. 4—The *Hope* at Meteorite Island, August 17, 1897. (Courtesy Hayden Planetarium, American Museum of Natural History.)

the pier a furious south-easter broke up my iceberg barrier, and the pack ice of Melville Bay driving in upon the shore forced us to pull the ship out with haste to avoid having her crushed like an eggshell against the rocks.

During all this time it was an impressive sight to see the *Hope* lying quietly beside the natural rock-pier, with her mooring lines out, waiting for her cargo as if at home, yet everywhere about her a wilderness of ice and bergs and savage snow-capped mountains.

. . . As soon as the *Hope* was free of the ice, she steamed into the little bight where the *Kite* had lain to embark the two smaller meteorites the previous summer, and the anchor was dropped till daylight and the cessation of the storm should enable us to see our way back to Cape York. From Cape York the voyage was continued home and Sydney, C.B., reached late in September.

Peary did not abandon the capture of the meteorite at that point, however. He resumed the pursuit the following summer, in 1897.

Disappointed, but not discouraged by my non-success in embarking the meteorite, I again put on board the *Hope* in 1897, when I went north in her to communicate with my Eskimos, powerful appliances with the view of giving the giant another fight if the Melville-Bay ice would permit me to get near him.

Arriving at Cape York on 12th of August, the ice conditions of Melville Bay were found to be favourable to an immediate approach to the meteorite, and instant advantage was taken of these conditions to force the *Hope* again to her berth alongside the natural rock-pier on Meteorite Island.

My ten days' work on the "Saviksoah" in 1896 had given me a very thorough acquaintance with its peculiarities and perversities, and had emphasised to me the full meaning of its concentrated weight, its intractable shape, and its almost resistless inertia.

I felt, however, the utmost confidence that the equipment that I had brought with me, the powerful hydraulic jacks, the magnificent oak timbers (the best that could be bought), the heavy steel rails, the bolts, chains, and tools of various kinds, all of the best quality, would enable me to bring it safely on board, provided the hostile Arctic ice would allow me to get near it.

This year as I neared the locality again the outlook was at first disheartening. There was much less open water and double the number of bergs that I had found last year, but, much to my relief, by butting a passage through two or three icy barriers, and after grounding twice from being forced to the shore by the ice, the *Hope* was brought alongside the natural rock-pier where I had left the meteorite a year before.

In spite of this good fortune, the ship's position and surroundings were such as to cause disquietude even in the mind of a man who had seen some Arctic experience, and to a novice were discouraging to the verge of fear. The rocky shore to which the ship was made fast lay fully exposed and absolutely unprotected against the resistless pressure of the Melville-Bay ice-pack under the stress of south-east winds: the open water through which we had crept close along the shore was scarcely more than a ship's length in width, was already coated with young ice, and outside of it lay an indescribable labyrinth of icebergs, through which even the practised eye could not discover an opening. To add to the dismal outlook and the mental unrest of many on board, we forged alongside the meteorite in a driving snow-storm that twelve hours later had covered our little world a foot deep in snow, and formed upon the water a thick covering of slush, which forty-eight hours of severe cold would transform into unbreakable fetters for the *Hope*. No one who was not present can form any idea of the savageness and hostile aspect of the scene. There were good reasons for the belief that the Arctic winter had already set in.

Fortunately the natural features of the shore, at the site of the meteorite, were uniquely favourable for getting it on board the ship, and my previous summer's work had left the huge mass close to the edge of the natural rock-pier, with sufficient depth of water alongside to allow the ship to be brought within about eighteen feet of the shore.

(*To be concluded*)

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OUT OF OLD BOOKS

By Helen Sawyer Hogg

David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

Peary and the Cape York Meteorites (the Saviksue) *(concluded)*

In August 1897 the Ahnighito lay on the shore, close to the edge of a natural rock-pier, with deep water alongside so that the ship *Hope* could be brought within about eighteen feet of the shore. The problem was then to construct a bridge over which the great inert mass of the meteorite could be transported to the ship. Peary describes the process.

I proposed to construct a very strong bridge, reaching from the shore across the ship; lay the heaviest steel rails upon this, and then, after depositing the meteorite upon a massive timber car resting upon these rails, slide the huge mass across the bridge until it rested directly over the main hatch; remove the bridge; then lower the meteorite with my hydraulic jacks through the hatchway to the ship's hold.

This was simple enough in theory, yet when such an enormous and concentrated mass is concerned, every detail of construction must be of the most massive character, and every detail of manipulation studied with the utmost care.

The transferring of such an enormous weight from the unyielding support of the shore to the yielding and continuously changing support of the ship, with the shifting and complicated strains resulting from the rise and fall of the tide, the varying displacement of the ship with the increasing load, and her listing with the unbalanced weight as it came upon her rail, all demanded the most careful thought and study.

The first thing was to prepare the ship for receiving her ponderous and unusual freight, so as to insure against the possibility of any mishap, and cause as little strain and reduction of her stability as might be.

To accomplish this, all the coal remaining amidships was hoisted out and put in the bunkers; heavy oak timbers laid fore and aft on either side of the keelson; then the entire amidships space filled with coarse, heavy ballast up to the deck beams, and in the centre, directly under the main hatch, some two feet higher. The 'tween-deck beams were carefully wedged and blocked up upon this ballast, and the main deck throughout the ship's waist supported from them by a small forest of twelve-inch posts kept in position by systems of horizontal struts and braces.

The object of the ballast was to increase the inertia and stability of the ship; absorb and distribute the shock in case, through any mishap, the meteorite should be allowed to drop; and finally to serve as a firm bed and matrix for the enormous mass during the homeward journey. . . .

This work accomplished below decks, an almost continuous floor of heavy timber was laid on deck, so as to distribute the weight of the meteorite and bridge over some twenty-five feet of the ship's length. . . .

The backbone of my bridge consisted of two royal sticks of fourteen-inch by sixteen-inch white oak, sixty feet long, straight-grained, tough, and well seasoned, which were to span the gap between the ship and the shore, reach well under the meteorite at one end, and across the ship at the other.

A third stick of timber twelve by twelve inches and thirty inches long, re-enforced these in the span from the ship to the shore, and the whole was bound rigidly together by heavy timber cross-heads and spreaders, bolted through and through by powerful screw bolts of the best Swedish iron.

The inshore end of this bridge rested continuously upon the rocks and gravel. The shipboard end was almost continuously supported by the heavy timbers on deck. . . .

The assembling of the bridge had of necessity to be done in place, as the big oak timbers weighed some three tons each, and the completed structure would be too heavy for the ship's tackle to handle. These were launched separately under the meteorite, which had previously been raised for the purpose, and supported upon blocks at each extreme end.

Some anxious moments were encountered at this stage when the break-up of an iceberg sent swells of water against the ship and shore.

Scarcely had they [the big oak timbers] been so placed and the work of assembling commenced, when a huge iceberg in the labyrinth outside of us went to pieces, sending a succession of heavy swells in upon the shore. On these the *Hope* rolled and danced like a cork, jerking viciously at her moorings and keeping me in a fever of anxiety during minutes which seemed like hours, knowing as I did if one of the lines parted, the great timbers, with one end still resting upon the *Hope's* heaving deck, would act as irresistible levers to pry the blocks from under the meteorite and let it topple over the edge of the pier into the water. It was with the utmost relief that I saw the swells gradually subside, and yet the occurrence kept me in a state of apprehension for the next forty-eight hours, until I had the meteorite firmly mounted upon its car and resting its full weight upon the inshore end of the bridge. . . .

The assembling of the bridge, and the stringing of the cable truss completed, the thirty-foot standard steel rails of the New York, New Haven, and Hartford R.R., weighing one hundred pounds to the yard, were hoisted out and laid in pairs, side by side, on each of the oak timbers, with their inshore ends coming just through under the meteorite, and the other ends coming just inboard of the *Hope*'s starboard rail. Two fifteen-feet lengths of rail continued the track across the main hatch, and then all were fastened down with numerous spikes.

The massive timber car, clamped together like the bridge, by heavy screw bolts, and sheathed underneath with steel plates, was then hoisted upon the rails, and pushed out against the meteorite; some of the timbers were removed . . . the meteorite was finally lowered to its position on the car. . . .

Then the monster was lashed to the car by fathom after fathom and turn and after turn of steel chains, tightened by oak wedges, until it and the car were inseparable.

The next thing was to adjust the ship in precisely the right position, with the bridge centred, to an inch over the main hatch . . . the *Hope* was finally adjusted to a nicety, the shipboard end of the bridge lashed down to eye-bolts in the deck and down on the starboard side, then cables and mooring lines were all set taut and carefully stopped.

While this was being done, a ten- or fifteen-ton counterpoise was being loaded on the inshore end of the bridge behind the meteorite, with the old timbers and rails of last year for a platform, and big gneissose boulders for weights.

The ship's heaviest tackles were then attached to the car, and the ends carried to the drums of the steam winch. The hydraulic jacks were also placed in position behind the car, with their bases working against the heavy cross-head.

Nothing remained now but to clear the *Hope's* waist of everything, except tools and materials needed while bringing the meteorite on board, slush the rails with a thick mixture of tallow and soap, then await the proper stage of the tide, start the huge mass with the jacks, and warp it inboard with the tackles, if they could handle it, or, if not, jack it the entire distance.

This matter of the tide was an extremely important one, and I am indebted to my young assistants, Arthur Moore and Lansing Baldwin, for their assiduous, hourly readings of the tide through storm and darkness, and plotting the tidal curves from the time the *Hope* came alongside the meteorite, so that now I knew to a nicety at just what time the tide would serve me.

Then follows the dramatic account of the start of the meteorite toward the ship, draped in the Stars and Stripes, and christened with a bottle of wine by the baby, while every Eskimo, with superstitious fear, left the ship.

At last the tide was right, and while Mrs. Peary and Captain Bartlett, at the levers of the jacks, started the monster, draped in "Old Glory", toward the ship, the baby dashed a little bottle of wine against it and named it "Ahnighito". Then the jacks, manned by the engine-room force, pushed it steadily forward to the edge of the pier.

Every man on board had his station and knew his work. The Captain had charge of the winch and tackles, the chief engineer of the jacks, and men were stationed at the lashings to slush the rails, etc., while I kept an eye on everything.

As the jacks moved the meteorite to the edge of the pier, the winch started, setting the heavy tackles taut, and the huge monster, in a series of short jumps, crept out upon the bridge.

At this moment, every Eskimo on board went over the stern gangplank to the shore. With all their confidence in me, and their awe for the size and power of the ship, which they had repeatedly seen smashing her way through the pack ice, and even battering pieces off the bergs themselves when they opposed her, they could not overcome a superstitious fear that the mountainous weight of the "heaven stone" would crush the *oomiaksoah* (ship), and they preferred to say farewell to it from the shore.

When the meteorite reached the centre of the bridge, a master might have played a grand march with the tense strands of the steel cable for violin strings. When it reached the rail, the *Hope* began to careen, but not seriously, and the men stationed at the lashings took in every inch of slack the moment it appeared.

In an hour from the time it started, a motion of my hand stopped the winch with the meteorite precisely over the main hatch. Three cheers went up from everyone on ship and ashore, and the glorious Stars and Stripes and the ship's flags went flying to the mastheads. . . .

The next step was to get the bridge out of the way. . . . As the saw passed nearly through the last timber, a long crack split out into each part, and Mr. Figgins, the naturalist, seizing a broad axe, jumped upon the rail, and with a blow or two severed the last connection of the "great iron" with the land. After years of rest it was to resume its wanderings. . . .

As the saws went through, the *Hope* righted herself slowly and quietly to an even keel, and the heavy stone counterpoise ashore held the severed bridge projecting like a cantilever. The valves of the jacks were opened, and the portion of the bridge under the meteorite sank till it rested true and level across the ship's waist. It was now six P.M., of Friday, August 20th. We had been engaged upon the meteorite five days, working throughout the entire day and much of the night, and during this entire time, from the moment the *Hope* came alongside the meteorite in a blinding storm, it had been one

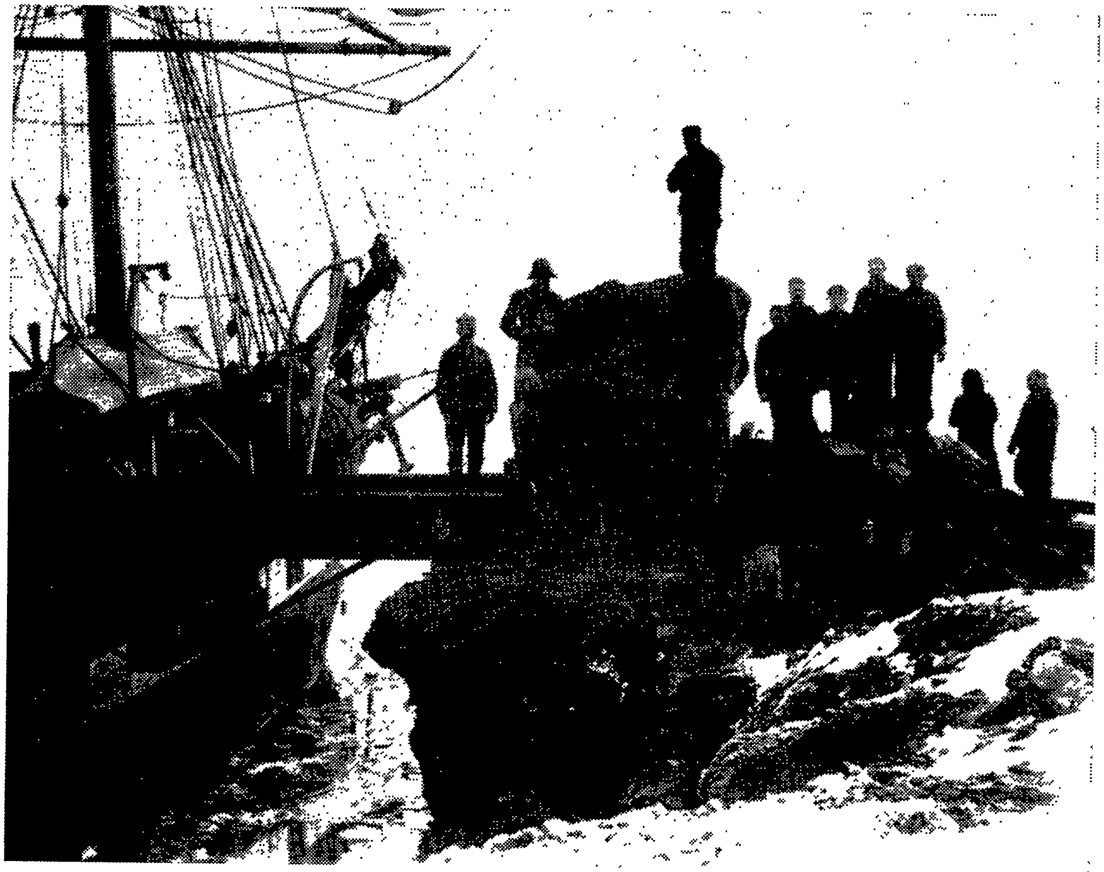


FIG. 1—The bridge from the shore to ship *Hope* for loading the Ahnighito, August 1897. (Courtesy Hayden Planetarium, American Museum of Natural History.)

constant succession of fog and driving snow. This not only retarded the work very seriously, but had a pronounced dampening effect upon the spirits of the men, particularly the superstitious sailors, some of whom had been with me last year and called this regular meteorite weather. They insisted that the brown monster was hoodooed, that I would never get it on board, or if I did we should never get it home, as it would surely take the ship to the bottom. . . .

Under the circumstances I could certainly almost forgive their associating supernatural agencies with the meteorite, and it was a strange but actual and unexaggerated fact that, as the great mass crept slowly over the bridge and across the ship's rail, patches of blue sky appeared overhead; and when at last it rested safely over the main hatch, the last tie which bound it to the land completely severed, the horizontal rays of the low midnight sun burst past the cliffs of Signal Mountain, fell upon the meteorite, changing it into molten bronze, flooded the countless icebergs east of us in light, and bathed the ragged black crests and flowing ice-domes of Imnahlooksoah and Nahgloktoo, the savage mountains of Prince Regent's Bay, in unspeakable tints of rose and yellow. It was as if the demon of the "Saviksoah" had fought a losing fight, accepted the result, and yielded gracefully.

The congratulations that evening in the cabin of the *Hope* were numerous and earnest.

By the middle of the next afternoon the car was lowered into the hatch combings. . . . At five o'clock, the last lines were cast off, and the *Hope* steamed away for the last time from the shore of Meteorite Island.

Then began a hazardous journey, and Peary tells of his dramatic passage through the icebergs.

Yet my risks and uncertainties were not yet ended. During our stay at Meteorite Island, the young ice had formed in every interval of calm, the last day's snow-storm had cemented everything with a thick leathery stratum of slush, and the almost continuous south-easterly wind had been steadily compacting the icebergs and forcing them nearer and nearer to the shore. Just before starting, Captain Bartlett and myself reconnoitred the bay from the top of the island, and saw that there was but one practicable route of escape, and even by that we should be obliged to force a barrier of bergs. . . . This barrier, though narrow, was formidable, made up entirely of bergs and heavy-berg fragments.... It was evident we must ram a passage in spite of our ugly load.... and it was with considerable anxiety that I watched the effect of the first blow, as the Captain from the foretop conned the rushing ship straight at the keystone of the barrier. As the bow struck the ice, it rose upon it with a harsh grating lift, and then with a crash and quiver the *Hope* came to a dead stop. The meteorite trembled, and the ballast underneath groaned and settled slightly, but no serious results followed, and as there was no alternative, the engines were reversed, and we backed out for another blow. Blow after blow was delivered, big pieces of ice were broken off and sucked out by the draught of the ship's backing, till at last the massive wedge of the *Hope's* iron-clad bow could be entered between the last two bergs of the barrier, and, with engines going at full speed, gradually forced them apart. The entire engine-room force was stoking like demons, black smoke poured in clouds from the *Hope's* funnel, the propeller was whirling at ninety revolutions per minute, and the *Hope* herself was pulsating like a human heart. Inch by inch we squeezed between the frozen blue rocks on each side, rasping the iron bark sheathing from stem to stern, and as the sternpost cleared the bergs, the flying propeller-blades struck once or twice, sending throughout the ship a resonant clangour, fierce as the bellow of fire bells on a winter's night. It was our paean of escape.

Looking back over the *Hope's* wake I saw the bergs between which we had squeezed swing slowly together again. The icy cordon of Meteorite Island had closed for the winter, but the treasure of the island, the celestial prisoner, had escaped. . . .

And then, in a few hours, they reached Cape York where Peary left his Eskimos, with suitable presents for their efforts.

Six hours later we were at Cape York, where I sent my faithful Eskimos ashore, accompanied by several barrels of biscuit, and loaded with guns, knives, ammunition, and numerous other articles which I had brought to reward them for their faithful service. . . .

From Cape York we steamed away for Cape Sabine; but the next morning, off Wolstenholm Island, a furious Arctic gale descended upon the ship, against which she was barely able to fight her way inch by inch to safety under the lee of the island, where for thirty-six hours she dodged back and forth, a phantom ship, her decks deep with snow, her spars, sails, and rigging crusted with frozen crystals, barely able with full head of steam to hold her own, while I, with four of my bravest Eskimos, worked like miners in our timber-cage under the meteorite, lowering it with the jacks, inch by inch and foot by foot, in order to get it low enough not to endanger the ship's safety. All this time the furious wind howled through the *Hope's* tense rigging, as if the demon of the "Saviksoah" were shrieking at us.

The superstitious ones on board were now more firmly convinced than ever that we should never reach home, and that this storm was but a warning from the devil of the meteorite.

After this Cape Sabine was visited. . . . Here the meteorite was lowered to within a few feet of the keelson, where it rested firmly upon the ballast, which was also packed solidly about it. Then twelve-inch by twelve-inch timbers were placed between it and the ship's side and wedged, blocked, and spiked in place until there was no possibility of the huge weight moving except as the ship moved. Every loose object on deck was also sent below, and the ship made snug for the mauling which the experience of the previous years had led us to expect in crossing Davis Strait.

And fortunate it was that every precaution was taken. Before we were across the strait a fierce north-wester descended upon the ship, and during the night of September 8th, she rolled and pitched dizzily upon the furious seas till the grey light of dawn began to filter through the tumult. Time after time the lee dead-eyes were under water, and as the *Hope* leaned and wavered and hesitated with her rail out of sight, and the boiling tumult to leeward seething up to the side of the companion-way, it seemed as if she would never right. . . .

Nowhere will such a mad sea be raised in such an incredibly short time as when the autumn boreal winds, marshalling in Baffin's Bay, charge southward, and, crowding through the narrows of Davis Strait, hurl every intruder out of the realm of night, foundering many a majestic berg, and driving others, foaming like battle-ships, through the water. It is the mighty besom of Kokoyah, the demon of the North, sweeping his domain clear and closing his realms for the winter. . . .

More than one anxious heart on board was certain at every wave shock that the demoniac iron had broken loose and was smashing a way for itself through the ship's side, and more than one gave up hope of ever seeing the morning light again.

Though the bulwarks of the starboard bow were smashed by a sea, and occasionally the waist filled with green water to the rail level, yet with everything . . . battened down, no serious damage was done, little water was taken in, and the meteorite never moved.

The next morning we were steaming under the lee of the Cape of God's Mercy, named by Davis centuries ago.

After this nothing of moment occurred, though the presence of such an enormous mass of iron on board rendered the compasses useless, and compelled us to make a coasting voyage all the way back to Sydney, where the ship arrived in safety on the 20th of September, burning her last ton of coal. The homeward voyage was hampered and delayed by almost constant fog and head-winds. The dangerous passage of the Straits of Belle Isle, with its rapid and erratic currents, was made in the night and in densest fog, and was one of the neatest pieces of navigation by Bartlett, who knows every inch of this coast, that I have ever seen. It was simply intuition on his part that brought us through.

Saturday, October 2, 1897, the hundred-ton floating crane at the New York Navy Yard, through the courtesy of the Navy Department, lifted the giant from the *Hope* and deposited it upon the quay wall, the largest known meteorite in the world, and a meteorite with human associations such as attach to no other.

Three years of persevering efforts had won. The great Star Stone of the North, traced to its icy matrix, and torn therefrom, had been brought safely out through the ice, the storms, and darkness of the Arctic seas.

This brief narrative would be incomplete without my acknowledgment of the invaluable assistance, of Capt. John Bartlett, one of the most reliable, conservative, and gentlemanly of that hardy company of Newfoundland ice navigators; of Emil Diebitsch, the able, cool-headed young engineer; of the officers and crews of the *Kite* and the *Hope*, who, though they availed themselves of the sailor's universal prerogative to grumble, still did yeomen's work; and of my faithful little band of Eskimos, who, handling heavy rails and timbers, working with pick and shovel and bar, and pumping on the jacks, did all they could to put into my possession the "Iron Mountain" of their forefathers.

To this dramatic story we will add only a short epilogue.

Peary speculated as to which arrived first on the Cape York peninsula—the Eskimos or the meteorites. He concluded that it was the former, for otherwise he saw no reason why Eskimo legends would consistently refer to the meteorites as being "hurled from the sky".

A fourth mass of meteoric iron, weighing 3401.7 kg., was found in 1913 on the Savik peninsula by the natives, 9 and 13 km. from the places where the other irons were found. It is a medium octahedrite with the same composition as the others, and was taken to Copenhagen in 1925.

The weight of the Ahnighito remained in doubt for many years. It measured 10 ft. 10 in. long, 7 ft. 2 in. high, and 5 ft. 6 in. thick. From computation, and from a model built to size, the weight was estimated at

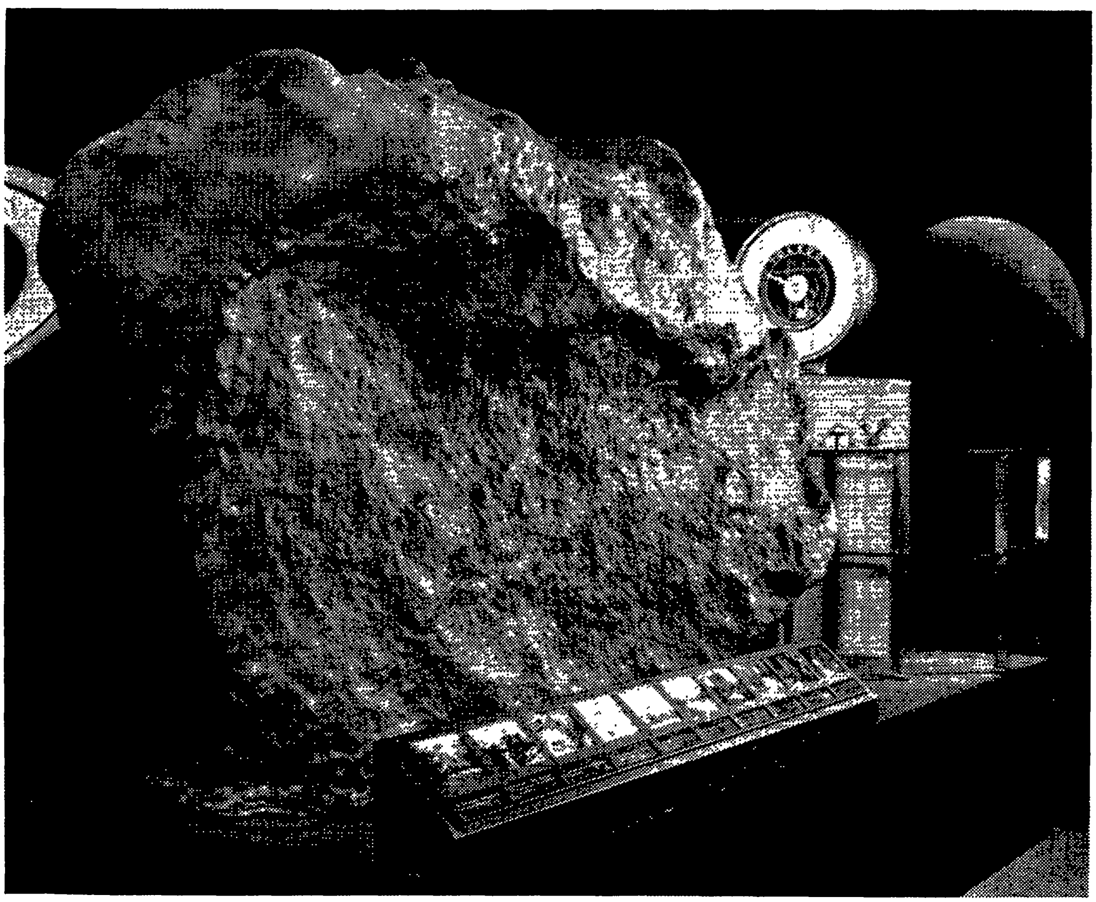


FIG. 2—The Ahnighito on Toledo scale, with picture sequence of its history. (Courtesy Hayden Planetarium, American Museum of Natural History.)

around 66 tons. Actually the true weight is only slightly more than half this amount. On February 14, 1956 the meteorite was placed on a special scale built by the Toledo Scale Company, on which it still rests in the Hayden Planetarium. It weighed 68,085 pounds, a little over 34 tons.

FIG. 3—The Woman meteorite mounted in corridor. (Courtesy Hayden Planetarium, American Museum of Natural History.)

The account of the weighing is given in *Sky and Telescope*, April 1956, page 261, and pictures Mrs. Marie Ahnighito Peary Stafford, the last surviving member of the expedition. Marie Ahnighito was born to Robert and Josephine Diebitsch-Peary on September 12, 1893 at Anniversary Lodge, four miles from the edge of the Greenland ice cap. She was the baby who christened the Ahnighito when it started toward the ship, as described in Peary's account that we have quoted.

As a result of the prodigious efforts of Robert Peary and his helpers, tens of thousands of people every year, including many school children, are privileged to see the great Star Stone of the North, as well as the Woman, which have been brought within reach of civilization. You do not have to brave the icy fastnesses of Melville Bay to see the great Ahnighito, but have easy access to it in the heart of one of the world's greatest cities.

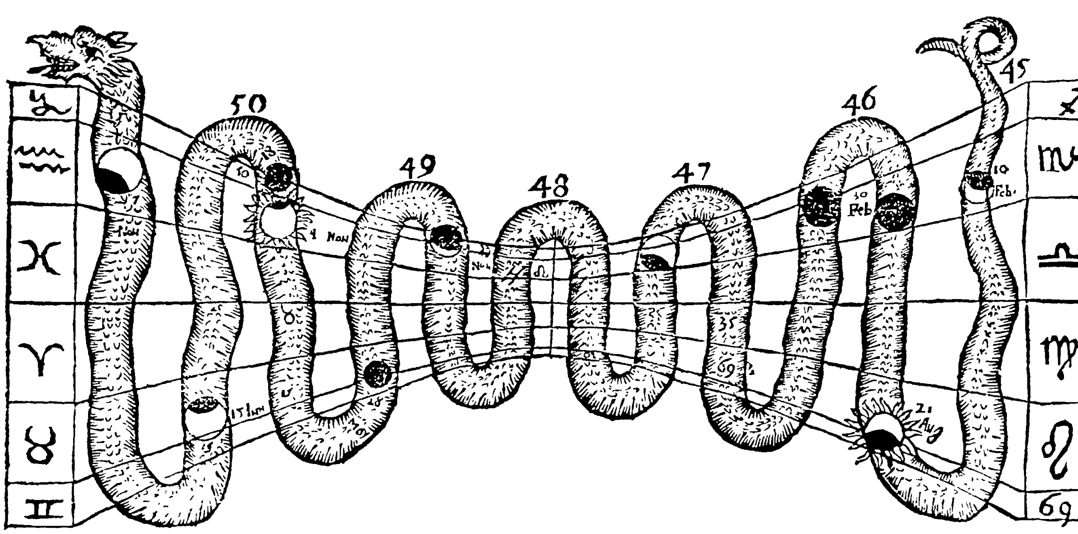
It is a pleasure to acknowledge the kindness of Dr. Joseph M. Chamberlain and Mr. James S. Pickering of the Hayden Planetarium for their help in locating original negatives of the Peary pictures, with which these articles have been illustrated.

OUT OF OLD BOOKS

A Seventeenth-Century Map

The eclipse of the sun on July 20, 1963, was anticipated with much interest throughout North America. In previous centuries, when the geometry of the eclipse was not understood, the event was popularly regarded as a portent of some catastrophe to befall mankind. A common interpretation of the event was that some celestial monster—such as the dragon—had swallowed the sun. This idea is presented in a seventeenth-century map of the sky which presents for the populace a number of eclipses which would occur during the years 1645 to 1650. Each of the folds of the snake's body represents a year in the seventeenth century as indicated by the numbers. The signs of the zodiac, six on one side and six on the other side, indicate the constellation in which the solar eclipse occurs and so indicates the month of occurrence. The various types of eclipse shown within the body of the celestial snake range from total to partial. It is interesting to note that the map has the form of the graduations found on sun dials. The map is taken from Kircher's "Ars Magna Lucis et Umbrae" published in Rome in 1646. Thus the eclipses shown for the years 1647 to 1650 are predictions.

Arthur E. Covington



OUT OF OLD BOOKS

By Helen Sawyer Hogg
David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

James Keeler and the Rings of Saturn

Recently Drs. Allan Cook and Fred Franklin of the Smithsonian Astrophysical Observatory have announced that they measure the thickness of Saturn's rings as only eight inches. This brings to mind the first spectroscopic proof that the rings are actually composed of meteoric particles. This observation was achieved in 1895 by James Keeler with a powerful new spectrograph built by J. A. Brashear and mounted on the 13-inch equatorial refractor of the Allegheny Observatory, Pittsburgh. The wooden tube of that instrument was specially replaced with a steel tube to bear the weight of the new spectrograph.

Every astronomer who teaches students mentions this spectroscopic proof—but how many, either professional astronomers or students—have actually read the original document in Keeler's own words? Some parts are to be found in the *Source Book of Astronomy*, by Shapley and Howarth, but the account there is greatly abbreviated.

The original article is published in volume 1, 1895 of a periodical which in the intervening years has become justly famous, namely, the *Astrophysical Journal.* The contributors to this first volume read like an astronomical Hall of Fame. The first paper is by Albert A. Michelson entitled "On the Conditions which Affect the Spectro-Photography of the Sun". Some of the splendid photographs of the Milky Way by E. E. Barnard adorn the volume. Edward C. Pickering describes the discovery of variable stars from their spectra. Henry A. Rowland gives a table of solar spectrum wave-lengths, and F. L. O. Wadsworth (of Wadsworth grating fame) and William Huggins, describe "The Modern Spectroscope", while George E. Hale details a method of mapping the solar corona.

Amongst such classic papers by outstanding authorities, it is therefore no surprise to find the original work of James E. Keeler, "A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings." Lucidly written, with just enough data and formulae to prove the point, the paper makes splendid reading. Since relatively few of the readers of this Journal have easy access to volume 1 of the *Astrophysical Journal*, pages 416–427, we will reprint most of Keeler's paper.

A SPECTROSCOPIC PROOF OF THE METEORIC CONSTITUTION OF SATURN'S RINGS

By James E. Keeler

The hypothesis that the rings of Saturn are composed of an immense multitude of comparatively small bodies, revolving around Saturn in circular orbits, has been firmly established since the publication of Maxwell's classical paper in 1859. The grounds on which the hypothesis is based are too well known to require special mention. All the observed phenomena of the rings are naturally and completely explained by it, and mathematical investigation shows that a solid or fluid ring could not exist under the circumstances in which the actual ring is placed.

I have recently obtained a spectroscopic proof of the meteoric constitution of the ring, which is of interest because it is the first *direct* proof of the correctness of the accepted hypothesis, and because it illustrates in a very beautiful manner (as I think) the fruitfulness of Doppler's principle, and the value of the spectroscope as an instrument for the measurement of celestial motions.

Since the relative velocities of different parts of the ring would be essentially different under the two hypotheses of rigid structure and meteoric constitution, it is possible to distinguish between these hypotheses by measuring the motion of different parts of the ring in the line of sight. The only difficulty is to find a method so delicate that the very small differences of velocity in question may not be masked by instrumental errors. Success in visual observations of the spectrum is hardly to be expected.

Soon after the large spectroscope of the Allegheny Observatory was completed, in 1893, I attempted to determine the relative motions of different parts of the system of Saturn, by photographing the spectrum with the slit parallel to the major axis of the ring, but failed to obtain satisfactory results. The unfavorable atmospheric conditions at Allegheny, the strong yellow color of the objective of the thirteen-inch equatorial, and the yellow color of Saturn itself so reduced the intensity of the violet part of the spectrum that the negatives obtained with a sufficiently high dispersion were too weak and granular to admit of measurement. Another unfavorable circumstance was the fact that I had to guide the practically invisible image corresponding to the $H\gamma$ line by means of the visual image, which was greatly out of focus on account of the chromatic aberration of the visually corrected telescope. Having recently obtained excellent results in other directions with orthochromatic plates, by the use of which the difficulties mentioned above are to a great extent obviated, I was induced to repeat my earlier attempts, and obtained two fine photographs of the lower spectrum of Saturn on April 9 and 10 of the present year. The exposure in each case was two hours, and the image of the planet was kept very accurately central on the slit-plate. After the exposure the spectrum of the Moon was photographed on each side of the spectrum of Saturn, and nearly in contact with it. Each part of the lunar spectrum has a width of about one millimeter, which is also nearly the extreme width of the planetary spectrum. On both sides of the spectrum of the ball of the planet are the narrow spectra of the ansae of the ring.

The length of the spectrum from *b* to D is 23 millimeters. The focus was adjusted on the line λ 5352, a little above the position of maximum sensitiveness of an orthochromatic plate, in the yellow green. On both plates the densities of the different spectra are very nearly equal, and the definition is excellent. It is hardly necessary to say that all the lenses used in the apparatus are visually corrected objectives.

These photographs not only show very clearly the relative displacement of the lines in the spectrum of the ring, due to the opposite motions of the ansae, but exhibit another peculiarity, which is of special importance in connection with the subject of the present paper. The planetary lines are strongly inclined, in consequence of the rotation of the ball, but the lines in the spectra of the ansae do not follow the direction of the lines in the central spectrum; they are nearly parallel to the lines of the comparison spectrum, and, in fact, as compared with the lines of the ball, have a slight tendency to incline in the opposite direction. Hence the outer ends of these lines are less displaced than the inner ends. Now it is evident that if the ring rotated as a whole the velocity of the outer edge would exceed that of the inner edge, and the lines of the ansae would be inclined in the same direction as those of the ball of the planet. If, on the other hand, the ring is an aggregation of satellites revolving around Saturn, the velocity would be greatest at the inner edge, and the inclination of lines in the spectra of the ansae would be reversed. The photographs are therefore a direct proof of the approximate correctness of the latter supposition.

To apply more precise reasoning to the subject under consideration, let us determine the form of a line in the spectrum of Saturn when the slit is in the major axis of the ring, on the assumption that the planet rotates as a solid body and the ring is a swarm of particles revolving in circular orbits according to Kepler's third law. At present the motion of the system as a whole is neglected. The upper part of Fig. 1 represents the image of Saturn on the slit of the spectroscope (the scale above it applies to the instrument used at Allegheny), and the narrow horizontal line in the lower part of the figure

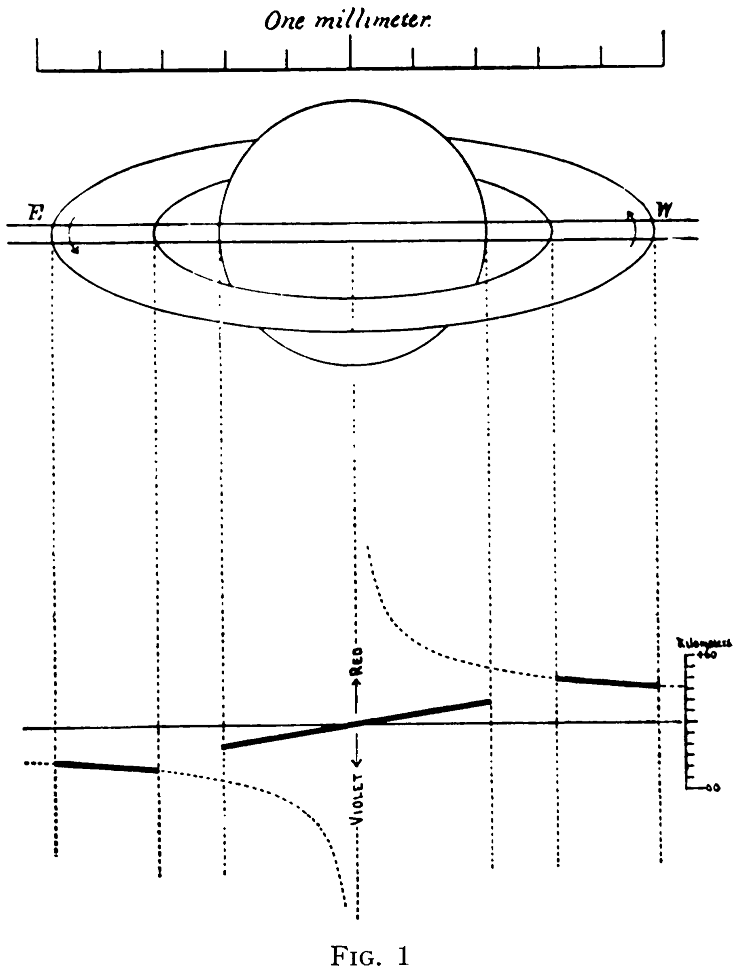


FIG. 1

represents an undisplaced line in the spectrum, or solar line. Let this line be taken as the axis of x, and the perpendicular line through its center as the axis of y. The red end of the spectrum is supposed to be in the direction of the positive axis of y, and the camera and collimator of the spectroscope are assumed to have the same focal length, so that the breadth of the spectrum is equal to the length of the illuminated part of the slit. Corresponding points in the slit and spectral line will then have the same value of x.

Now let x, y be the coördinates of a point on the displaced line,

v = velocity of point corresponding to x, y in the line of sight,
V' = velocity of a point on the equator of Saturn,
α = angle between the line of sight and the radius of Saturn which passes through the point corresponding to x, y,
2ρ = width of spectrum
β = elevation of Earth (and Sun) above the plane of the ring.

The displacement y is proportional to the velocity in the line of sight.
Then we have

$$x = \rho \sin \alpha$$
$$y = av = aV' \sin \alpha \cos \beta$$
$$\frac{y}{x} = \frac{aV'}{\rho} \cos \beta = \text{constant}$$

Hence the planetary line is straight, but inclined to the solar line at an angle

$$\varphi = \tan^{-1} \frac{aV'}{\rho} \cos \beta.$$

To determine the form of a line in the spectrum of the ring, regarded as a collection of satellites, we have, by Kepler's third law,

$$T^2 = cR^3,$$

or, since $TV = 2\pi R$,

$$V^2 = \frac{4\pi^2}{cR}.$$

Since x is proportional to R and y to v (where v = velocity in the line of sight = $V\cos\beta$), we may write

$$xy^2 = b,$$

which is the equation to the curve of which the lines in the spectrum of the ring are a part. The curve is represented by the dotted line in the figure; it is symmetrical with respect to the axis of x, but only the upper branch has a physical meaning, and the curve corresponding to the other half of the image is obtained by taking both x and y with negative values.

In the equation $V = \frac{k}{\sqrt{R}}$, $\log k = 3.7992$ for the Saturnian system, R being expressed in kilometers and V in kilometers per second. The computed motions of different parts of the system are given in the following table. The gauze ring is not considered, as its spectrum does not appear on the photographs; the rings *A* and *B* are not separately distinguishable.

Object	*R*	Period of a Satellite at Distance *R*	Velocity	Velocity in Line of Sight April 10, 1895
	Kilometers	Hours	Kilometers	Kilometers
Outer edge of ring	135,100	13.77	17.14	16.35
Middle of ring	112,500	10.46	18.78	17.91
Inner edge of ring	89,870	7.47	21.01	20.04
Limb of planet	60,340	4.11	25.64	24.46
		Rotation		
Limb of planet	60,340	$10^h.23$ (A. Hall)	10.29	9.82

With the values given in the above table, and others which do not correspond to actual points in the system, the dotted curves were plotted. For the ordinates, however, twice the values in the last column were taken, since the displacement of a line, due to motion in the line of sight, is doubled in a case of a body which shines by reflected and not by inherent light, provided (as in this case) the Sun and the Earth are in sensibly the same direction from the body. The planetary line is drawn to the same scale, and the heavy lines in the figure represent accurately the aspect of a line in the spectrum of Saturn, with the slit in the axis of the ring, as photographed with a spectroscope having about three times the dispersion of my own instrument.

The width of slit which I used ($0^{mm}.028$, or 7900^{km} on the surface of Saturn) is also represented in the figure.

If the whole system has a motion in the line of sight, the lines in the figure will be displaced toward the top or the bottom, as the case may be, but their relative positions will not be altered.

It is evident that in making a photograph of this kind the image must be kept very accurately in the same position on the slit plate, as otherwise the form of the lines shown in the figure would be lost by the superposition of points having different velocities. The second plate was made with special care, and as the air was steadier than on the first occasion, the definition is on the whole somewhat better than that of plate 1, although the difference is not great. On both plates the aspect of the spectrum is closely in accordance with that indicated by theory and represented in the figure. The planetary lines are inclined from 3° to 4°, and the lines in the spectra of the ansae have the appearance already described. The slight curvature of the latter indicated by theory is, of course, unrecognizable. On account of the extreme narrowness of the spectra (barely more than a tenth of a millimeter) it was useless to attempt anything like a measurement of the inclination of the lines. The direction of such short lines is frequently masked by irregularities in the grain of the plate, and occasionally a line is considerably distorted. However, in fifty of the sharpest lines, in the region of best definition, only five were inclined in the same direction as the lines of the ball, while the rest were inclined as required by the theory, or elsewhere apparently parallel to the undisplaced lines of the lunar spectrum.

If the ring revolved as a whole, the displacement of lines in its spectrum would follow the same law as for a rotating sphere; that is, the lines would be straight and inclined, their direction passing through the origin. If the ring rotated in the period of its mean radius, a glance at the figure shows that the lines would practically be continuations of the planetary lines. Such an aspect of the lines as this would be recognizable on my photographs at a glance.

It will be seen from the foregoing considerations that the photographs prove not only that the velocity of the inner edge of Saturn's ring exceeds the velocity of the outer edge, but that, within the limits of error of the method, the relative velocities at different parts are such as to satisfy Kepler's third law.

Besides (1) the proof of the meteoric constitution of the rings, explained above, each line of the photographs gives (2) the period of rotation of the planet, (3) the mean period of the rings, (4) the motion of the whole system in the line of sight. I have measured a number of lines on each plate and compared the results with the computed values of the corresponding quantities.

Keeler then after further discussion, gives tables of measurements of his two photographs, no. 1 taken on April 9, 1895, and no. 2 on the following night, April 10, each with two hours exposure. He tabulates his measures on the lines λλ 5324.3, 5328.4, 5371.6, 5383.5, 5429.9, and sums up his findings as follows:

The results from both photographs are
Velocity of limb = 10.3 ± 0.4 kilometers,
Mean velocity of ring = 18.0 ± 0.3 kilometers;
the computed values being 10.29 and 18.78 kilometers respectively.

Although there seems to be no systematic difference between the two plates, the results for each differ by more than the probable error. With photographs on so small a scale, distortions of the lines are produced by the irregular deposit of even a few particles of silver; hence it is advisable to measure a large number of lines instead of multiplying observations on a few of them.

The number of lines in the table is however sufficient for the present purpose.

As I have already pointed out, it is necessary to guide the telescope with extreme accuracy in making such photographs as those described in the present paper, and the method which I have used is so simple and effective that a short account of it may be of interest.

The spectroscope is fully described in *Astronomy and Astrophysics*, **12**, 40, January, 1893, and the prism-train used in these observations is there shown in Plate VII. The slit is observed during an exposure by a small "broken" telescope, which receives the rays reflected from the first surface of the prism nearest to the collimator.

To prepare for an observation of Saturn the slit is shortened until its length is equal to the computed length of the image (major axis of the ring). A small bar, which is wider at one end than at the other, is cut out of thin metal, and placed across the field of the diagonal telescope. If the bar is approximately of the right width, then, by throwing the image of the slit a little above or below the center, and by rotating the eyepiece, which carries the bar with it, the bar can be made to very nearly cover the image, leaving a very short length of slit uncovered at each end. When the telescope is directed to Saturn the extreme ends of the ring appear from behind the (invisible) bar as two minute points or stars, and the attention of the observer is concentrated on keeping these stars equally bright. Any displacement in declination is indicated by their disappearance or unusual faintness. The photographs show that the guiding by this method is quite accurate. The spectra of the ansae do not show any traces of the Cassini division, but it would probably be requiring too much to expect that they should do so, considering the small size of the image and the length of the exposure.

It is a question whether these observations could be better made with a larger telescope. If the same spectroscope were mounted on a large telescope, the width of the photographed spectrum would be greater, the lines would be longer, and their direction could be more definitely measured. On the other hand, the inclination of the lines would be diminished, since tan φ varies inversely with ρ, and it could not be increased by employing a greater dispersion, as the brightness of the spectrum, which would be the same for both telescopes, would hardly bear any further reduction. A material advantage would be that with the same slit-width a smaller area of the image would be included between the jaws, and hence at any part of the slit there would be fewer points having different velocities in the line of sight. On the whole, it seems to me that the advantage would lie with the large telescope. With a reflector, or a photographically corrected refractor, the photographs could be taken at the Hγ line, where the dispersion is more than twice as great as in the region near λ5350, and the only difficulty in that case would be found in the yellow color of Saturn.

I have given a somewhat full account of these observations, partly because of the interest inherent in everything that relates to the magnificent system of Saturn, and partly because the successful application of the spectroscope to the measurement of celestial motions depends largely upon details of appliances and methods.

Thus ends James Keeler's account, written sixty-eight years ago, describing his successful method of obtaining the Doppler shift in the spectrum of the rings of Saturn with the equipment at Allegheny Observatory. His measures proved that the inner parts of the ring were rotating more rapidly than the outer, and that therefore the rings must be composed of individual tiny moonlets, and could not be a solid body like a wheel.

OUT OF OLD BOOKS

Lunar Volcanic Activity

The recent observations of luminescence in or near the lunar craters Aristarchus, Copernicus and Kepler described by Zdenek Kopal and T. W. Rackham (1964) is reminiscent of the description of volcanic activity given by N. A. Kozyrev (1963) and by Kopal (1958) of a reddish cloud in the vicinity of the central peak of the crater Alphonsus. Kopal suggests that the observations of Kozyrev are unique.

Similar observations have since been reported from Flagstaff, Arizona, by J. A. Greenacre (1963) (see also *Sky and Telescope*, Jan. 1964). He describes his impressions of the phenomena as though he were "looking into a large polished gem ruby but could not see through it".

Since the invention of the telescope a number of instances of apparent changes on the surface of the moon have been recorded. One of these is reported in the *Scientific American*, December 1878, page 285. The observation was "supposed to be due to an eruption of a volcano" in the vicinity of Baco, Barocius and Nicolai and was seen on November 12, 1878. The fleeting characteristics of the phenomenon, which lasted only one-half hour, is similar to recent observations. The interesting report by John Hammes of Keokuk, Iowa, was considered sufficiently astonishing by John P. Rodgers, Rear Admiral, Superintendent of the U.S. Naval Observatory, Washington, that he asked for a signed statement from the mayor of Keokuk and other responsible persons as to the reliability of John Hammes.

In his survey, Hammes used a 6 1/2-inch telescope, while the recent observations have all been made with larger telescopes. In particular, the observers from Flagstaff noted that the phenomenon they reported was not seen in the 6-inch finder of their 24-inch refractor telescope. There may have been differences in seeing or the magnitude of the phenomena observed; the Flagstaff observers report seeing as ranging between 2–4 on a scale of 10 and that the image was unsteady.

AN ACTIVE VOLCANO IN THE MOON

For many years the opinion prevailed that the moon had long since arrived at the stage of physical quiescence: that it was, in short, a dead planet. Neison, in his admirable work on the Moon, takes strong ground against this opinion; and the drift of later observation has been to indicate at least the probability of the progress of active volcanic changes during recent years. In one instance, at least, a large crater has disappeared, apparently by filling from within; but hitherto no astronomer has been fortunate enough to witness anything like an eruption.

The correspondence below puts this question in a novel and most interesting light. The observation of Mr. Hammes seems to confirm the growing opinion of the best of our living selenographers that the moon is far from dead; and from the nature of his occupation the probability of his being misled by any error in the adjustment of his instrument, accidental reflection, or the like, would seem to be very slight. The fact that the supposed eruption lasted, or was watched, for half an hour, by at least two different observers, still more reduces the likelihood of error.

U. S. NAVAL OBSERVATORY,
Washington, December 4, 1878.

To the Editors of the Scientific American:

I inclose a correspondence in relation to a volcano in the moon.

Mr. Hammes writes to me that he travels around the country with his telescope, showing the moon and planets to colleges and schools, and that he is perfectly familiar with the appearance of the moon, and with the use of his instrument.

The crater seen by Mr. Hammes seems to be, from his drawing, in the vicinity of Baco, Barocius, and Nicolai, as these names are given on Beer and Mädler's Map of the Moon.

Very respectfully, yours,

JOHN RODGERS,
Rear-Admiral, Superintendent.

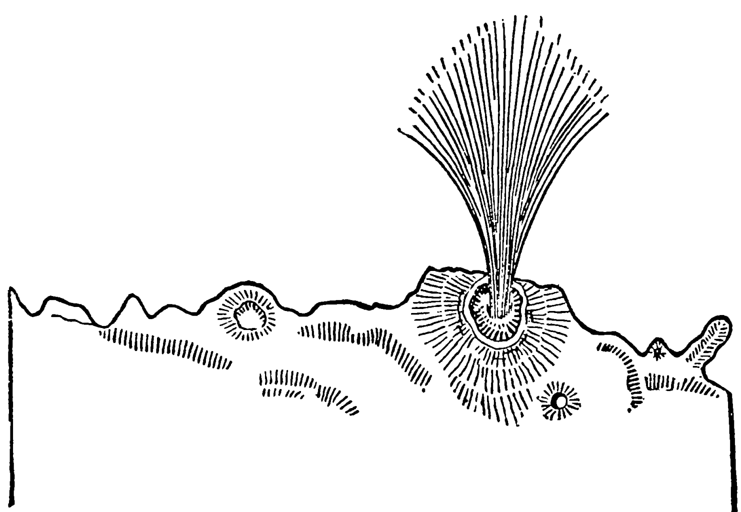


FIG. 1—Supposed volcanic eruption on the moon. (Seen by Mr. John Hammes, at Oskaloosa, Iowa, November 12, 1878, 8:30 p.m., civil time).

KEOKUK, Iowa, November 20, 1878.

ADMIRAL JOHN RODGERS:

I take the liberty to offer you a sketch of an observation on the moon, taken November 12, hour 8:30 evening, seen by me, my son, and several gents who were present, at the town of Oskaloosa, Iowa, about latitude 41° 30′—what I supposed to be an eruption of a volcano. It was only seen for one half hour through my 6½ inch telescope, as plain as any other mountain scenery in the moon is seen, and of the same color. I would like to hear what you think of it.

I remain, respectfully, your obed't servant,

JOHN HAMMES.

[Reply.]

U. S. NAVAL OBSERVATORY,
Washington, November 23, 1878.

DEAR SIR: Your report of what you saw on the 12th of this month in the moon is very interesting, so interesting that the fact will not be received by the astronomical world without the closest scrutiny. Your observation will be attributed to some chance dust on the glass, or to some error in adjustment, or to an accidental light reflected from some neighbor's window, or to some other source of error. Therefore please send your observation in full, with the signatures of the gentlemen who saw it with you; and also the certificate of some well known person, governor, mayor, U. S. senator, or other, stating something about the gentlemen who sign.

I presume you, as a man of the world, will understand this: that new facts are received in astronomy with extreme caution, and that in publishing such things, one must take care to give all his grounds for so doing, and leave no means of verification untried. Very truly yours,

JOHN RODGERS, R. Admiral, Superintendent.

Mr. John Hammes, Keokuk, Iowa.

[Testimonials.]

CITY OF KEOKUK, Mayor's Office, December 2, 1878.

John Hammes is well known in this city, and bears the reputation of an honest and reliable man.

JOHN N. IRWIN, Mayor.
J. C. PARROTT, P. M.
R. ROOT, Dept. U. S. M.
W. T. RANKIN, Ass't U. S. Attorney.

Earlier in the same year in the *Scientific American* for July 1878, a shorter note appeared reporting the observation of Hermann J. Klein of Köln on May 27, 1877, of an apparently new crater near Hyginus on Mare Vaporum.

AN ACTIVE VOLCANO IN THE MOON

When examining the surface of the moon, May 27, 1877, Dr. Hermann J. Klein of Köln, noticed what seemed to him to be a new crater on the Mare Vaporum, a little to the northwest of the well known crater of Hyginus. Being deep and dark, and about three miles in diameter, it formed a conspicuous object on the dark gray Mare Vaporum. Having frequently observed this region during the last twelve years, Dr. Klein felt certain that the crater was new. Communicating his observation to Dr. Schmidt, of Athens, he was assured by that veteran selenographer that no such crater appeared in any of his numerous drawings of that part of the lunar surface; nor is it shown by Schroter, Lohrmann, or Mädler, who carefully drew the same region with the fine refractor of Dorpat. In April, 1878, Dr. Klein laid the discovery before the Senelographical Society, and since then the new crater has been observed by several English students of the moon. The Mare Vaporum lies close to the center of the visible surface of the moon, so that objects in this region are very slightly affected by the lunar librations. The region has been closely studied by many, and as it contains several well known craters, some of them less than a mile in diameter, it is evident that the large crater described by Klein is new.

There have undoubtedly been other accounts of changes on the lunar surface published in the past; see, for example, a report of lunar observations by Sir William Herschel in 1783 of a reddish glow in the vicinity of Aristarchus discussed by B. M. Middlehurst (1964). A search of old records and journals may reveal useful information which could be important in view of the current interest in the moon.

L. R. McNarry

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OUT OF OLD BOOKS

By Helen Sawyer Hogg

David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

Halley's Comet and Unusual Atmospheric Phenomena

During the past year many of us have become acutely aware of the fact that certain specific events can affect the appearance of the sky above us even for long periods of time. Night after night, month after month we have experienced a series of brilliant sunsets, at times quite breath-taking in their magnificence. Furthermore these have had a pattern—they reached their most spectacular state half or three-quarters of an hour after sunset, indicating that their cause was a very high-level phenomenon in the earth's atmosphere.

The explanation for these coloured sunsets in 1963 and 1964 is universally accepted. It is the explosion of the Mount Agung volcano on the island of Bali on March 17, 1963 that hurled vast quantities of dust into the atmosphere. The effects of this dust as it circled the earth could be traced in both the southern and northern hemispheres. One of the most spectacular effects was the degree of darkness of the eclipsed moon on December 30, 1963. At that time the moon in total eclipse came closer to invisibility than for several decades.

Some of the dust hurled into the atmosphere has actually been collected from high altitudes by S. C. Mossup (1964) of the C.S.I.R.O. Division of Radiophysics, Sydney, from flights of a U-2 aircraft which had begun early in 1963 a survey of meteoric dust. After the Mount Agung explosion, the dust collected was of a different character, typical of volcanic ash and seemed to have a sulphurous coating.

The vivid descriptions of the brilliant sky effects following the eruption of Krakatoa in 1883 are familiar to us. However, as most astronomers of today, amateur and professional alike, were not alive at that time, our eye-witness experience with the coloured sunsets attributable to the Mount Agung explosion is even more vivid to us.

Another recent illustration of atmospheric effects caused by material hurled into the atmosphere was noctilucent clouds over the south-western United States. The normal type of noctilucent cloud has been excellently described by Balfour S. Currie in this Journal, vol. 56, p. 141, 1962. Two clouds over Arizona, one on Feb. 28, 1963 and another on June 15, 1963 have been discussed by J. E. McDonald (1963) and A. B. Meinel, B. Middlehurst, and E. Whitaker (1963). These clouds were at lower latitudes than customary for noctilucent clouds, and are attributed to the launching of space vehicles from the Pacific Missile Range at Vandenberg Air Force Base in southern California. Apparently an influx of tiny particles into certain levels of the earth's atmosphere (around 80 km.) as from a meteor swarm or other source, can trigger the mechanism of formation of these clouds at a particular level in the mesosphere. From rocket samplings of these clouds over Sweden, R. K. Soberman (1963) and C. L. Hemenway, R. K. Soberman and G. Witt have recently shown that the density of particles in them is 100 to 1000 times that of the normal density in the region when the clouds are absent. Brilliant displays of noctilucent clouds over western and northern Europe occurred some hours after the Tunguska fall in Siberia in 1908.

Because of these recent experiences with atmospheric events, it seems fitting to recall phenomena, somewhat overlooked and forgotten, associated with the day when the earth passed through the tail of Halley's comet, May 19, 1910. Shortly after that event the late W. J. Humphreys, well known for his work of many years in meteorology and for his book *Physics of the Air*, collected all unusual accounts of atmospheric phenomena recorded on that day over the United States. He gave a complete list of these at the eleventh meeting of the American Astronomical Society held at Harvard College Observatory, August 17–19, 1910 when Professor Edward C. Pickering, Director of the Harvard College Observatory was President of the Society.

Humphrey's paper (which he notes beside the title, is complete and not an abstract) appeared in the *Publications of the Astronomical and Astrophysical Society of America*, vol. 2, pp. 35–38, 1915. Since this volume is now fifty years old, and not readily available to most of our readers, we reprint the entire paper here, including the table of observed phenomena.

ON PASSING THROUGH THE TAIL OF HALLEY'S COMET

(Complete Paper)

By W. J. Humphreys

Because so little is known about comets it was both natural and proper that particular attention should be given to the great historic one, known as Halley's, during its recent near approach to the earth; and especially so since it was expected to transit the sun, and the earth to pass through its tail at no great astronomical distance from the head.

Careful preparations were made for the study of this comet not because disastrous or even spectacular phenomena were anticipated, but simply to note any minute effects, of whatever nature, that might be attributable to the comet itself, or to our passage through its tail, and in this way to add something to our scanty knowledge of the nature, origin and density of these strange celestial objects.

So far as the scientific world is concerned it may at once be said that nothing more than very slight terrestrial disturbances of any kind were expected, and that instead of being surprised at the magnitude of the results it does not feel certain that even any effect at all was observed.

The head of the comet seems to have been invisible as it passed across the face of the sun, and hence we can still say, as before, that the solid nucleus, if there is one, is small—not more than a few miles in diameter. Thus a rough upper limit to the size of anything like an opaque or solid head is set by the transit observations, though what the other, or lower, limit may be no one knows.

Particular attention, of course, was given to certain magnetic, electric and meteorological phenomena that conceivably might depend upon or be modified by the passage of the Earth through the substance of a comet's tail.

Many of these phenomena were briefly referred to in this connection in the journals with the hope of enlisting the assistance of numerous and widely scattered observers. In addition to this, and to be certain of securing many trustworthy meteorological observations, the Chief of the U.S. Weather Bureau, Professor Willis L. Moore, wisely sent out a circular letter to nearly two hundred meteorological stations in the United States and the West Indies, asking that on the 17th, 18th especially, and 19th of May particular observations be taken of auroral displays, meteoric trails, Bishop's ring, color of the sun and sky, twilight phenomena, luminous clouds, zodiacal light, gegenschein, solar and lunar halos and coronas, "and all other appearances that may seem unusual and worth noting."

At the time this letter was prepared it was known that the transit of the comet across the sun would take place almost certainly, some time on the 18th, and therefore, that the earth could not enter the tail before that date. Nevertheless it seemed advisable for the observations to begin at least one day ahead of time for the purpose of securing records of undisturbed conditions of about the same date with which to compare those obtained while the earth was actually in the tail.

According to reports from various parts of the world no magnetic nor electric phenomena were noted that appear necessarily, or even very reasonably, attributable to the comet; and the same statement would be more or less true of all other observed phenomena, except possibly certain halos and coronas seen on the 19th.

While the unusual appearances of some of these rings can not with certainty be referred to the comet, nevertheless they and one or two special conditions of the sky are recorded here for whatever they may be worth.

Similar phenomena have been reported from other places. Professor E. B. Frost, Director of the Yerkes Observatory, states that from noon till about 1 p.m. of the 19th there were seen from that Observatory iridescent clouds of unusual brilliancy, and also the rare phenomena of a halo of 15° radius.

Professor Max Wolf reports from the Königstuhl Observatory[1] that late in the afternoon of the 19th a Bishop's ring was seen around the sun, and that this was followed by a twilight of unexpected intensity, extension and duration; that three

[1]*Astronomische Nachrichten*, No. 4414.

PHENOMENA, POSSIBLY DUE IN PART TO HALLEY'S COMET, OBSERVED ON THE 19TH OF MAY, 1910

Place	Observer	Hour	Phenomena
Macon, Ga.	W. A. Mitchell	Much of the day. Most marked at 10 a.m.	Formless, motionless, pale-yellow haze over a zone 40° wide along the ecliptic.
Springfield, Mo.	J. S. Hazen	Forenoon	Solar halo.
Chicago, Ill.	H. J. Cox	11:45 a.m. to 12:15 p.m.	Double solar halo 45° and 90° diameter.
Lexington, Ky.	G. H. Noyes	—	Brilliant solar halo.
Grand Haven, Mich.	C. H. Eshleman	11 to 11:30 a.m.	Solar halo, colors diffused through the circle.
Grand Haven, Mich.	C. H. Eshleman	6:10 p.m.	Parhelia.
Lansing, Mich.	D. A. Seeley	Afternoon	Bright solar halo.
Reno, Nev.	H. F. Alps	11:05 a.m. to noon	Solar halo.
Moorestown, N.J.	J. C. Beans	9:30 a.m. 11:00 a.m. noon	Solar halo.
Moorestown, N.J.	J. C. Beans	11:15 p.m.	Lunar halo.
Asheville, N.C.	R. T. Lindley	8:43 a.m.	Solar halo 45° diameter, vivid coloring.
Charlotte, N.C.	W. V. Martin	10 to 11:30 a.m.	Solar halo 45° diameter, remarkably distinct and brilliant; rainbow colors easily distinguished. Light cirrus clouds near horizon, but none could be seen elsewhere.
Charlotte, N.C.	W. V. Martin	Early p.m.	Solar halo in upper clouds that gathered rapidly.
Waynesville, N.C.	Dr. G. B. Green	Noon	Solar halo, colored. Sky dark inside, clear outside.
Cincinnati, Ohio	M. E. Blystone	10:40 a.m.	Solar halo.
Grants Pass, Oreg.	J. B. Paddock	All day	Solar halo.
Roseburg, Oreg.	W. Bell	All day	Solar halo.
Roseburg, Oreg.	W. Bell	4:30 p.m.	Two parhelia with rainbow colors.
Scranton, Pa.	W. M. Dudley	Sunset	Unusually brilliant sunset. Sun deepest blood red.
Abilene, Tex.	W. H. Green	10 to 10:15 a.m.	Solar halo.
Trinidad, Wash.	J. C. Wheeler	11 a.m. to 1 p.m.	Most pronounced halo ever seen.
Basseterre, Guadeloupe, W.I.	D. H. Ross	10:40 a.m. to 1:45 p.m.	Solar halo, at times exceedingly bright.

twilight purple glows succeeded each other, and that all the particular sky-phenomena following the eruptions of Krakato and Mount Pelé appeared in an intensified manner. Also that a Bishop's ring more intense than any he had ever seen before, and of 28° radius, indicating particles of 1.5 μ diameter, appeared about the moon.

In the same number, 4414, of the Astronomische Nachrichten, W. Krebs, reports from Grossflottbek that from 4 to 4:30 p.m. a parhelium was seen, and that at the same time a peculiar shimmering white corona of 5° radius appeared around the sun.

It is reported by Émile Marchand[2] that observations made on Pic du Midi and at Bagnères-de-Bigorre showed, on the morning of the 20th, a lunar corona and a bright tinted sky such as was seen after the eruption of Mount Pelé. These phenomena were even more pronounced on the evening of the 20th. There was also an analogous solar corona of 3° to 4° diameter on the 20th, that was still seen on the 2nd of June.

The halos, coronas and other phenomena listed above were both widely scattered and, in some respects, distinctly unusual; and their occurrence coincident, as near as can be determined, with the passage of the earth through the tail of the comet suggests for them a cosmical origin. Still they certainly were far from universal, and besides they have all been seen before when there was no comet to which they could be attributed; and, therefore, while admitting the possibility, in this case, of a cometary influence, it would seem rash, without additional evidence, to conclude that the comet was the principal or even partial cause of any of the appearances mentioned above.

These phenomena are recorded here for the convenience of those who may have occasion to study them, and because, at present the possibility of the comet's influence in producing them can not be definitely excluded.

Some years later articles on noctilucent clouds which appeared in this JOURNAL prompted Frederick Slocum to print a description and photograph of some unusual clouds observed by the staff of the Yerkes Observatory on May 19, 1910. Slocum's account, with a beautiful picture is given in this JOURNAL, vol. 28, March 1934, page 145. As our members can borrow this volume from the Society library, we will quote from it only briefly.

Slocum notes that though he had been watching the skies since the early nineties he had seen only one display of iridescent clouds, and now deemed them worthy of record. Then he quotes from the record in his notebook of May 19, 1910.

"Between noon and 1 p.m. solar halos were observed and also streaks and patches of brilliant colours; some resembling fragments of a rainbow, others isolated bands and patches of iridescence or of monochromatic colours; rose-pink, green, blue, etc. These colours were sometimes apparently projected against cirrus clouds and sometimes against what appeared to be clear sky".

The iridescent clouds were at an altitude of from 15° to 20° above the south point of the horizon, the sun at the same time having an altitude of 65°.

Strictly speaking, of course, these were not noctilucent clouds since they appeared at high noon, but in their texture, and colouring they seem to bear a strong relation. None of the Yerkes staff, according to Slocum, had seen any clouds like them before.

[2]*C.R.*, 150, 1404; 1575.

These observations, by careful scientists of high repute, add further details to Humphreys' collection of data, which strongly suggests that unusual atmospheric phenomena were actually caused by the passage of the earth through the tail of Halley's comet. In view of the atmospheric effects which we have witnessed in recent years, some of which have been cited here, I think it time that we attribute the phenomena of May 19, 1910, more positively to Halley's comet than did Humphrey when he wrote that "the possibility of the comet's influence in producing them can not be definitely excluded".

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OUT OF OLD BOOKS

By Helen Sawyer Hogg

David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

Dark Days

Although atmospheric effects are properly part of the study of meteorology, nevertheless astronomers, professional and amateur alike, who are concerned with making observations *through* the atmosphere become acutely aware of the air! To us, it seems interwoven with actual astronomy.

In the last issue of this Journal we discussed the atmospheric effects observed at the passage of the earth through the tail of Halley's comet, on May 19, 1910, and referred also to the coloured sunsets from the recent Mount Agung explosion on March 17, 1963.

Anyone who witnessed, as I did, the great smoke pall of September 24–30, 1950 can never forget the eeriness of the occurrence and the extraordinary gloom, as I described it in this Journal, vol. 44, p. 241, 1950. The sun was turned to various shades of blue or violet over much of the eastern part of this continent. According to the late Dr. Harry Wexler (*Weatherwise*, Dec. 1950, p. 129) of the U.S. Weather Bureau, the insolation at Sault Ste. Marie on September 24 dropped to 4 langleys, or less than one per cent of the normal clear weather value.

Over most of the affected area, however, the darkness of that gloomy September day is hardly to be compared with some of the dark days recorded in the past. The most famous one, Friday, May 19, 1780 was described by Dr. Joseph Ashbrook in his column "Astronomical Scrapbook" in *Sky and Telescope*, April 1964, page 219. Since the investigation of these dark days falls under other areas of science than that of astronomy, the astronomical community may not be fully aware of published discussions of them.

In 1912 Fred G. Plummer, Geographer, U.S. Department of Agriculture, published a paper, "Forest Fires: Their Causes, Extent and Effects, with a summary of recorded Destruction and Loss," as Forest Service Bulletin 117, Washington. In this he compiled a list of dark days experienced over this continent in two centuries. He also gives a vivid description of the transportation of forest fire smoke by winds and air currents across the country, and its effects—a description so useful that we reprint large sections of the article (beginning with page 15).

SMOKE PHENOMENA OF FOREST FIRES

It has already been pointed out that the damage resulting from forest fires is usually estimated in acres, quantity of timber, or in dollars, that these estimates are always too low, and that they do not include other losses due to the interruption of business, destruction of young growth, loss of soil fertility, and damage to water-courses, with the attendant depreciation of property. Even this is not all.

A thrifty forest purifies the air we breathe, and it is an irony of nature that when it goes up in smoke it causes a pollution of the atmosphere. The mischief thus caused is by no means trivial, since a heavy pall of smoke interrupts business, interferes with navigation, and, turning day into night, compels the use of artificial light. Such conditions have obtained over an expanse of many thousands of miles, and the actual loss must be very great. In the vicinity of a great fire the atmosphere sometimes carries ashes and burning brands to a distance of several miles. The atmosphere in motion, flowing over the surface of the earth, has an enormous carrying capacity, very much greater than is generally supposed. It is analogous to flowing water in that matter can be carried to any distance, determined only by the sustained velocity of the wind and the size or weight of the particles of matter. Wind, like water, will deposit the particles "downstream", assorting them and carrying the lightest the greatest distance. It is probable that, for equal areas, the winds transport as much matter as the streams.

Forest fires are the most frequent causes of widespread pollution of the atmosphere, and the volume of the pollution is exceeded only in the case of violent volcanic eruptions. The forest fire not only causes an uprush of heated air, usually with a cyclonic movement, but it furnishes at the same time the material which is lifted to the higher atmospheric strata. Under such conditions it is not surprising that fires may jump several miles; in fact, as far as a live brand can be carried. The phenomena are analogous to those of local winds, which, sometimes assuming cyclonic form, can suck up water, sand, dust, and all kinds of small objects, which later "rain" down. The forms of wind which raise heavy matter are such centers of cyclonic action as whirlwinds, sand spouts, and waterspouts. These are the causes of many of the so-called prodigies, which have as surely been observed as they have reluctantly been believed. The shower of oranges which occurred near Naples in 1833 would have been discredited had it not been known that the fruit had been taken up by just such means. The waterspout is guilty, in a similar manner, of causing showers of living frogs, turtles, fish, and worms, and the sand spout or dust storm may fill the air with particles which must eventually fall.

A large forest fire has an appreciable effect upon the surrounding atmosphere, causing a movement of the air toward the fire. This effect is quite local, and is overbalanced if there is a strong wind blowing, which will drive the fire before it. Under such conditions a fire may advance with great rapidity, while blazing branches sail through the air and start other fires far ahead of the main conflagration. During the great forest and city fire at Fernie, British Columbia, August 1–8, 1908, which was accompanied by a high wind, flaming trees, timbers, lumber, and sections of buildings were carried. This fire burned a strip 3 miles wide for a distance of about 20 miles. . . .

Plummer then describes the formation of the phenomena of dark days, and illustrates the atmospheric sequence with a diagram (figure 1). He also firmly disposes of earlier astronomical theories of their cause.

The tendency is for smoke to spread out and to be dissipated, but if the volume is great it may be identified for hundreds of miles, even when the cause of it is unknown.

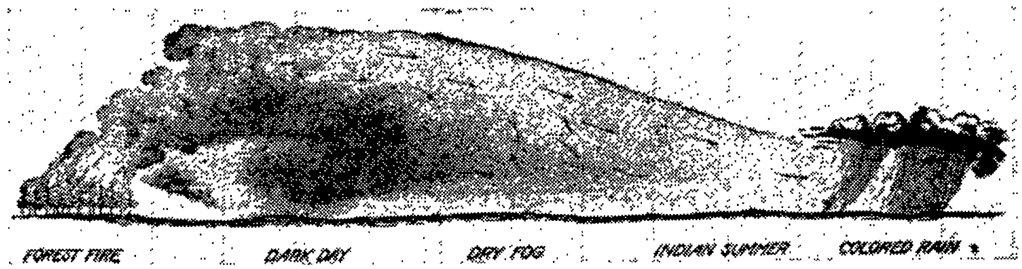

FIG. 1—Phenomena caused by smoke from a forest fire.

At greater distances, where the smoke is more attenuated there is only a slight obscuration of light, though if the smoke has descended to the earth it may interfere with vision. At still greater distances from the fire, when the smoke has been further mixed with clear air, its presence can only be noted by a yellow or pearly haze about the horizon or by the discoloration of rain. These phenomena, observed from time immemorial, have been known by various names—in this country as dark days, dry fogs, Indian summers, and colored rains.

Dark days have been recorded for centuries. Usually there is a gradually increasing gloom until it becomes so dark that artificial light is necessary. This darkness may last a few hours or several days and decrease as gradually as it came.

We are now able to show that dark days are due to dense smoke in the atmosphere, and that in this country forest and prairie fires have been the causes. In other countries peat fires and volcanic eruptions have also furnished smoke to produce dark days, but such cases are more rare. Theories advanced in olden times that dark days are caused by solar eclipses or by the transit of inferior planets across the solar disk are ridiculous, since a total solar eclipse seldom lasts over five minutes, and a transit of Venus, the largest and nearest of the inferior planets, is barely visible to the naked eye, and would not cause a diminution in light or heat that could be measured. If any consideration of such theories were necessary, it would be sufficient to point out that the dark days of modern history have not been coincident either with eclipses or transits.

Next Plummer gives a table of notable dark days, and comments on "yellow days." This table should be of use in checking any old local records for appearances of blue suns or moons, or uncommonly dark lunar eclipses. It is noteworthy, as figure 2 shows, that the atmospheric circulation across southern Canada and the northern United States is such that New England and the southern portions of eastern Canada are the regions which have experienced the greatest frequency of dark days in the past two centuries.

The record for dark days in the United States and Canada is as follows:

1706....May 12, 10 a.m., New England.
1716....October 21, 11 a.m. to 11.30 a.m., New England.
1732....August 9, New England.
1762....October 19, Detroit.
1780....May 19, New England. (Black Friday. The Dark Day.)

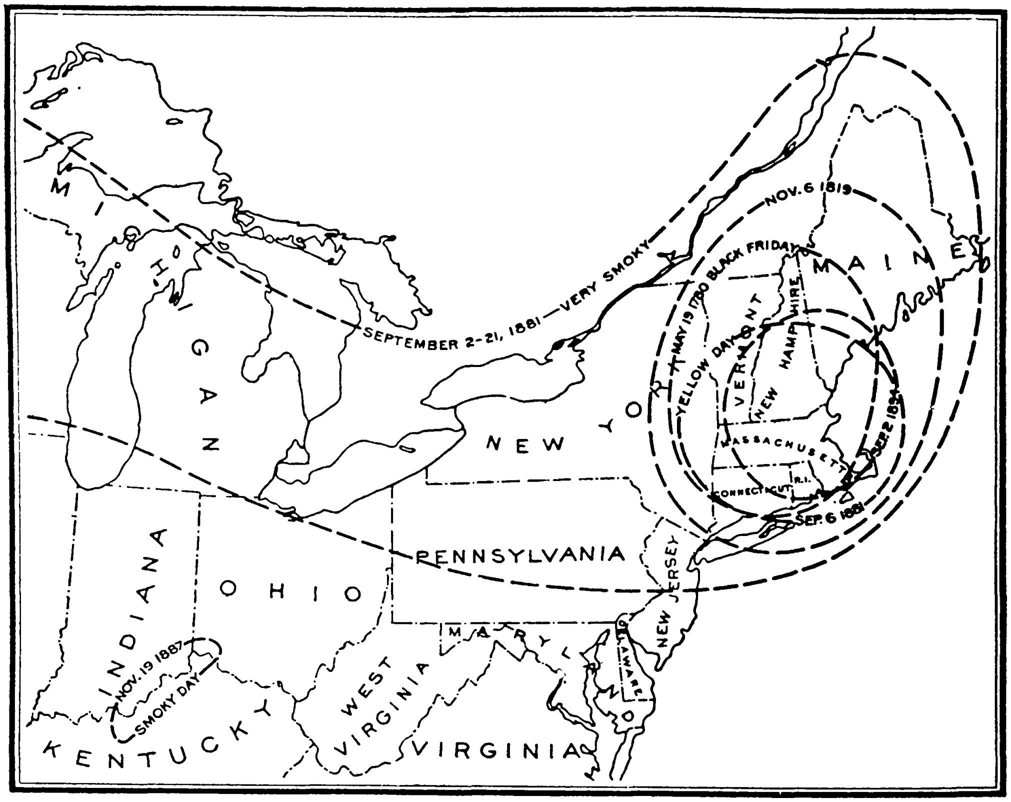

FIG. 2—Areas in the northeastern States in which occurred the most important dark days since 1780.

1785. . . .October 16, Canada.
1814. . . .July 3, New England to Newfoundland.
1819. . . .November 6–10, New England and Canada.
1836. . . .July 8, New England.
1863. . . .October 16, Canada. ("Brief duration.")
1868. . . .September 15–October 20, western Oregon and Washington.
1881. . . .September 6, New England. (The Yellow Day.)
1887. . . .November 19, Ohio River Valley. ("Smoky Day.")
1894. . . .September 2, New England.
1902. . . .September 12, western Washington.
1903. . . .June 5, Saratoga, N.Y.
1904. . . .December 2, 10 a.m., for 15 minutes, Memphis, Tenn.
1910. . . .August 20–25, northern United States, from Idaho and northern Utah eastward to St. Lawrence River.

Most dark days might more properly be called "yellow days." Even "Black Friday," May 19, 1780, which was the most memorable of all the dark days of modern times, was preceded by a gradually increasing yellowness and an odor. The same was true of the dark days of 1819, 1881, 1894, and 1903. September 6, 1881, was so distinctly yellow that it is known as "The Yellow Day".

The evidence that dark days result from fires may be briefed as follows: In 1716 the air was very full of smoke. During the dark day of 1780 ashes of burnt leaves, soot, and cinders fell in some sections from forest fires in New York and Canada. In 1785 black rain fell during a thunder shower in the darkened area. In 1814 ashes of burnt wood fell and there was a strong smell of smoke. In 1819 a shower in the darkened area was discolored as if the water were impregnated with soot. The fires near Wissitaquik, Me., probably caused the darkness in 1836. In 1868 the smoke from the Coos and St. Helens fires was encountered on the Pacific Ocean. In 1881 dense smoke was noticed over a large area (fig. 2), chiefly from the Michigan forest fires. In 1887 the smoke from forest fires to the westward interfered with navigation, became painful to the eyes, and rendered breathing disagreeable. In 1894 the smoke came chiefly from the Hinckley fire in Minnesota. In 1902 the smoke came from numerous fires, one of the largest being in the South Fork of Lewis River watershed. In 1903 the smoke was from fires in the Adirondacks. In 1910 the smoke was from the great Idaho fires. Indeed, the British ship *Dunfermline* reported that on the Pacific Ocean, 500 miles west of San Francisco, the smell of smoke was noticed, and the haze prevented observations for about 10 days. In connection with the 1910 phenomenon it was noted that a cool wave followed, passing eastwardly over the same area, but spreading farther southward, which gave the lowest temperatures, with frosts, for the month of August.

New England easily leads in the phenomena of very dark days, and several of the most pronounced have affected practically the same area which is shown in figure 2. The tracks of many air currents and storm centers converge toward this area from all over the United States, and sometimes meet an opposing storm from the east or northeast. It therefore seems that dark days are caused by the banking up of smoke-laden air. The greatest forest fires have occurred in the Northern States, and the winds, transporting the smoke eastward, flow over the New England States. At such a time, if a "nor'easter" flows in from the ocean and banks up a smoke-laden stratum, increasing its thickness and density, it is evident that obscurity, and perhaps darkness, will result.

"Dry fogs," "Indian summer," and "coloured rains" are described by Plummer, and their origin explained.

Dry fogs.—On portions of the Pacific coast, particularly in the Puget Sound country, the smoke from forest fires is often so dense as to interfere with navigation. The same is true on the Great Lakes and on navigable rivers. Under such circumstances captains prefer to run slow or tie up until the danger has passed. At the time of the Hinckley fire, in 1894, it was estimated that the losses to lake vessels on account of the smoke exceeded $40,000.

The smoke may last for days or weeks, until dispersed by a favorable wind. When there is no odor, it might be taken for fog, except that it is dry. Although it is simply "smoke," the phenomenon is also called "dry fog" in English-speaking countries. In Spain it is known as "callina"; in Germany as "hohrauch"; in Switzerland as "hale"; and in Russia as "mgla." Prairie fires and the burning of peat or turf beds produce the same phenomena.

Several of such dry fogs have been noted in history. In 1783 the phenomenon lasted from May 9 until the latter part of June. It was first observed at Copenhagen and extended over France, Germany, and the Alps. At the same time a similar phenomenon existed over a large part of North America. On several of the days the sun was so obscured that they were recorded as "dark days." In 1831 a dry fog extended generally over the Northern Hemisphere. In May and July, 1834, there was a dry fog in central Europe, caused by the burning of peat beds in Germany and forest fires near Berlin and in Sweden and Russia. In 1881, in the northeastern United States, a dry fog lasted from September 1 to September 10, culminating on September 6 in the "yellow day." Its limits were determined as between 40 and 45 degrees of latitude, and between 67 and 87 longitude. (See fig. 2.) This was caused by forest fires in Michigan, with contributions from fires in New Jersey, Pennsylvania, and Canada.

Indian summer is the name applied to periods during the months of October and November when the weather is unseasonably warm and there is a dry fog of longer duration but of less intensity than the average. The air during Indian summer is saturated with impurities so fine that they are not precipitated unless a rain occurs. It is characterized by a smoky haze, sometimes of pearly or phosphorescent appearance, so attenuated that the zenith may be very blue, though the smoky appearance is very pronounced near the horizon.

Under different names Indian summer is recognized all over the world. In England it is called "St. Luke's summer" when it occurs in October, and "St. Martin's summer" if in November; in Wales, "St. Michael's summer"; in France, the "summer of St. Martin" and the "summer of St. Denis"; in Italy, the "summer of Santa Teresa"; in Sweden, "St. Bridget's summer"; in Russia and Germany, "old woman's summer"; in Argentina, "St. John's summer."

The phenomena vary in intensity in different years, or may be wholly lacking in some locality. On an average of one year in three Indian summer is marked, and about one year in three it is wholly lacking.

Colored rains, being so little known and understood, are considered as prodigies; nevertheless, they have been observed for many centuries. The phenomena are not difficult to understand. We say that "a shower of rain has cleared the atmosphere," meaning that the drops of water have carried to the earth such impurities as were held in suspension or solution. It is to be expected that under certain conditions, depending upon the quantity and nature of these impurities, the falling rain should be colored, and that in some cases the color should be so marked as to attract attention. Forest fires, as has been shown, are one of the principal agents by which the atmosphere is polluted, and are therefore related to the phenomena of colored rains. Usually such rains, noted in the daily press, are better described as discolored, being gray or muddy. It is not probable, however, that the most notable or highly colored rains mentioned in history can be attributed to smoke.

Oct. 16, 1785, a black rain fell in Canada, and on Nov. 6, 1819, black rain and snow fell in the northern part of the United States and Canada. In the Middle Atlantic States, Apr. 11–13, 1902, there were showers of eolian dust, which, when accompanied by rain, fell as gray mud. Similar dust fell at Grantville, Pa., Feb. 10, 1896. On Apr. 12, 1902, a rain in the Middle Atlantic States was so loaded with dust from the dry western plains as to cause a mud shower. . . .

An exceptional occurrence of red rain was observed off the coast of Newfoundland in February, 1890.

Although forest fires are not particularly related to the science of astronomy, nevertheless since we are discussing the dark days which result from them, our readers may be interested in Plummer's tables of fires so great as to be considered historical. As our continent comes more and more under the control of modern fire fighting equipment, it becomes harder and harder for us to visualize the extent of some of the great conflagrations of the past.

HISTORIC FIRES

As a calamity a great forest fire ranks well with flood, pestilence, famine, or earthquake, and, like them, is as soon forgotten. Some fires, however, because of lives lost, property destroyed, or large areas burned, have become historic.

The first accounts of such great events are generally exaggerated and subsequent accounts are often conflicting. The best sources of information have been given preference in Table 3.

TABLE 3—*Historic Fires in the United States and Canada*

Date	Name of fire	Location	Area Burned Acres	Lives Lost
1825—October	Miramichi	Maine and New Brunswick	3,000,000	160
1837—(?)	Seboois	Maine	130,000	—
1846—(?)	Yaquina	Oregon	450,000	—
1853—May	Pontiac	Quebec	1,600,000	—
1860—(?)	Nestucca	Oregon	320,000	—
1868—September	Coos	Oregon	300,000	—
1868—September	St. Helen	Washington and Oregon	300,000	—
1871—October	Peshtigo	Wisconsin	1,280,000	1,500
1871—October	—	Michigan	2,000,000	—
1876—(?)	Big Horn	Wyoming	500,000	—
1880—September	Bagot	Quebec	288,000	—
1881—September	Michigan	Michigan	1,000,000	138
1891—May	Comstock	Wisconsin	64,000	—
1894—July	Phillips	Wisconsin	100,000	13
1894—September	Hinckley	Minnesota	160,000	418
1902—September	Columbia	Oregon and Washington	604,000	18
1903—April–June	Adirondack	New York	450,000	—
1908—August	Fernie	British Columbia	64,000	9
1908—September	Chisholm	Minnesota	20,000	—
1910—August	Great Idaho	Idaho and Montana	2,000,000	85
1910—October	Baudette	Minnesota and Ontario	300,000	42

"Black Friday," the dark day of May 19, 1780 over New England is the most famous of all the dark days on this continent. Some vivid eye-witness descriptions of it have been quoted by Joseph Ashbrook (op. cit.). It struck terror to the hearts of many of the beholders who were convinced that the end of the world was at hand. Of particular record was the effect on the Connecticut legislature, where at a session of the governor's council, Col. Abraham Davenport refused to be panicked by the alarming darkness. His courage was commemorated in a poem by John Greenleaf Whittier, which deserves to be better known, in which Abraham Davenport stands "Against the background of unnatural dark, A witness to the ages as they pass, That simple duty hath no place for fear."

ABRAHAM DAVENPORT

By John Greenleaf Whittier

In the old days (a custom laid aside
With breeches and cocked hats) the people sent
Their wisest men to make the public laws.
And so, from a brown homestead, where the Sound
Drinks the small tribute of the Mianas,
Waved over by the woods of Rippowams,
And hallowed by pure lives and tranquil deaths,
Stamford sent up to the councils of the State
Wisdom and grace in Abraham Davenport.

'T was on a May-day of the far old year
Seventeen hundred eighty, that there fell
Over the bloom and sweet life of the Spring,
Over the fresh earth and the heaven of noon,
A horror of great darkness, like the night
In day of which the Norland sagas tell,—
The Twilight of the Gods. The low-hung sky
Was black with ominous clouds, save where its rim
Was fringed with a dull glow, like that which climbs
The crater's sides from the red hell below.
Birds ceased to sing, and all the barn-yard fowls
Roosted; the cattle at the pasture bars
Lowed, and looked homeward; bats on leathern wings
Flitted abroad; the sounds of labor died;
Men prayed, and women wept; all ears grew sharp
To hear the doom-blast of the trumpet shatter
The black sky, that the dreadful face of Christ
Might look from the rent clouds, not as he looked
A loving guest at Bethany, but stern
As Justice and inexorable Law.

Meanwhile in the old State house, dim as ghosts,
Sat the lawgivers of Connecticut,
Trembling beneath their legislative robes.
"It is the Lord's Great Day! Let us adjourn,"
Some said; and then, as if with one accord,
All eyes were turned to Abraham Davenport.
He rose, slow cleaving with his steady voice
The intolerable hush. "This well may be
The Day of Judgment which the world awaits;
But be it so or not, I only know
My present duty, and my Lord's command
To occupy till he come. So at the post
Where he hath set me in his providence,
I choose, for one, to meet him face to face,—
No faithless servant frightened from my task
But ready when the Lord of the harvest calls;
And therefore, with all reverence, I would say,
Let God do his work, we will see to ours.
Bring in the candles." And they brought them in.

Then by the flaring lights the Speaker read,
Albeit with husky voice and shaking hands,
An act to amend an act to regulate
The shad and alewive fisheries. Whereupon
Wisely and well spake Abraham Davenport,
Straight to the question, with no figures of speech
Save the ten Arab signs, yet not without

The shrewd dry humor natural to the man:
His awe-struck colleagues listening all the while,
Between the pauses of his argument,
To hear the thunder of the wrath of God
Break from the hollow trumpet of the cloud.

And there he stands in memory to this day,
Erect, self-poised, a rugged face, half seen
Against the background of unnatural dark,
A witness to the ages as they pass,
That simple duty hath no place for fear.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

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Castles in the Air

The term "castles in the air", frequently used in application to daydreamers, actually has a fairly strict scientific connotation. It is used for a particular across-water phenomenon which is rarely seen by a casual observer, but may be often studied by the persistent. In Europe this phenomenon is referred to as the *Fata Morgana*, a term which is found in books on this side of the water, but is less well known than the English equivalent. This is not an astronomical phenomenon, but the writer of this section believes that students of the heavens should be aware of the queer tricks played by the air through which they observe the stars. Dr. M. Minnaert in his fascinating and comprehensive volume "The Nature of Light and Colour in the Open Air" (Dover Publications, 1954) has given a fine description and explanation of it.

The Fata Morgana should be especially pertinent for Canadians, many of whom live close to bodies of water with the necessary dimensions. A body of water, apparently seven to twenty miles across, is required, with a calm surface. The position of the observer's eye is crucial to seeing the phenomenon. It must be between six and twelve feet above water level, and experimentation is necessary to determine the best height. The time of day also enters in, and afternoons are the most favourable.

Apparently the first person to document the apparition to a complete degree was Professor F. A. Forel of Morges, Switzerland on the shore of beautiful Lake Geneva (also known as Lake Léman). He presented a description and summary of his four to five hundred observations representing fifty years of study, to the Royal Society of Edinburgh, and published it in the Proceedings of this Society, volume 32, 1911–12, pp. 175–182, from which we quote large sections. He begins with the historical account of the discovery and naming of the phenomenon, and proceeds to the more scientific understanding of it by Charles Dufour, his teacher, in 1854.

The Fata Morgana. By Professor **F. A. Forel,**
Morges, Switzerland

Abstract of an Address delivered before the Society on July 14, 1911; translated by Professor C. G. Knott.

Among optical phenomena which originate over the surface of water there is one so ill-defined and ill-observed as to be still mysterious; till now it has received no valid explanation. The Italians call it the Fata Morgana. Under conditions still lacking precise description, there appear on the far side of the Straits of Messina certain fantastic visions, fortresses and castles of unknown cities, which seem to emerge from the sea, soon to vanish again. These are the "palaces" of the "fairy Morgana", which appear and disappear at the capricious stroke of the magician's wand.

Most of the accounts of the phenomenon are founded on the extravagant description and the amazing picture published in 1773 by the Dominican friar Don Antonio Minasi, professor of botany at the Roman College of Sapienzia. This drawing, with its incoherent groupings of castles and boats, reflected and refracted at random in a manner quite inconsistent with physical possibilities, was largely the creation of the distorted imagination of an artist who did not understand in the least the wonderful illusion. . . .

In 1854 Charles Dufour rendered a signal service by his recognition of the phenomenon of the Fata Morgana on the Lake of Geneva; and his description and explanation were free from the exaggerations and contradictions of his predecessors. He showed the illusion to a pupil of his, a small schoolboy of thirteen; and since then, following the instructions of my beloved master, I have seen the Fata Morgana again and again every springtime, some four or five hundred times in all. I can therefore speak of it as a familiar friend. . . .

Forel then gives a precise description of the conditions which bring forth the appearance of the Fata Morgana over Lake Geneva, and a beautiful description of the apparition.

The physical conditions which determine the appearance of the Fata Morgana are always the same. When, under the bright skies of spring or early summer, the lake lies calm or slightly rippled by light and intermittent puffs of air, there appears at times during the afternoon when the air is warmer than the water a phantom-like transformation of the scenery of the opposite coast. Over a horizontal stretch of twenty or thirty degrees, the familiar details of the coastline, which may be from ten to thirty kilometres distant, become strangely transformed. Resting on the water horizon, and bounded above by another horizontal line at a height of a few minutes of arc, there comes into view a vertically striped band or striated zone (*zone striée*), seemingly composed of rectangles placed side by side, of varied tints and hues. It might be compared to the distant cliffs of Dover as seen by travellers crossing the Channel; or to a great city built on the shore, with blocks of houses ranged along the quays, a Genoa, a Naples, or a Constantinople, miraculously rearing itself in a region known to be occupied by a few scattered huts. Such are the palaces which the fairy Morgana creates before our wondering eyes.

The position of its first appearing varies with each occasion. It comes into view now here, now there. It does not remain steady, but moves more or less slowly in one or other direction, never lingering in the same neighbourhood for more than ten or twenty minutes. As the mirage passes on towards another part of the horizon, the coast-line recovers its ordinary aspect.

The phenomenon as a whole does not last long. Barely an hour will be consumed as it moves from one end to the other of the half circle of the opposite coast. I have never seen it before noon, and never after six o'clock in the evening.

An essential feature of the viewing of the fairy castles is the position of the eye of the observer, which Forel specifies as follows:

The Fata Morgana is visible only to an observer whose eye is a few metres above the level of the lake. The best height would seem to be from two to four metres (six to twelve feet). A shift of a foot or two above or below the best position in any given case is sufficient to make the phenomenon disappear. This limited range in the position of the eye which ensures the visibility of the mirage at once explains the astonishing scarcity of good observations.

In order to explain the appearance of the Fata Morgana Forel discusses the conditions of refraction over water which may cause it. It occurs between refraction over warm water and refraction over cold water. Forel first discusses the characteristics of refraction over warm water.

Following Dufour, we are in the habit of speaking of the Fata Morgana as being due to abnormal refraction, although we know perfectly well that there is nothing abnormal in natural phenomena. Given the conditions, the consequences necessarily follow.

"Abnormal" though we call it, the Fata Morgana comes between two phenomena which we shall call normal, because they are frequent and easily observed. It succeeds the one and precedes the other.

Under the category of optical refractions in air over the surface of a lake, I regard as "normal" phenomena those which accompany refraction in air over water whose temperature is either higher or lower than that of the superposed air. When the air is cooled by contact with cold water, the successive layers of air rise in temperature from below upwards. This might be called the *direct* thermal gradient. When, on the other hand, the lower layers are warmed by contact with warmer water, the thermal gradient is *inverse*, the temperature in the air falls as the height increases through a limited stratum of air.

The mirage phenomena which accompany *refraction over warm water* are the most frequent. This condition holds throughout the whole day during autumn and winter, and during the morning hours in springtime and part of summer. The air is then colder than the surface on which it rests; the thermal gradient is of the inverse type, and the curve of a refracted ray of light is concave above. The characteristic optical accompaniments are:

1. A depression of the plane of the apparent horizon of the lake below its normal position; the apparent horizon is lower than the true horizon.
2. The apparent exaggeration of the rotundity of the earth, which becomes evident to the eye, although normally it is unrecognisable.
3. The approach of the circle of the horizon, much less distant than it ought to be according to the height of the observer's eye above the surface of the water.
4. The apparent exaggeration of the crests of waves, which show like crenations along the line of the horizon.
5. The phenomenon of "mirage": "the mirage of the desert". Objects lying low over the surface of the water and situated beyond the circle of the horizon are seen as inverted images below Bravais' "ligne de partage". (This is the line which separates the erect and inverted images in the usual mirage.) These images lie in the zone which separates the "ligne de partage" from the apparent horizon of the lake.

These details become more marked as the difference of temperature between the cold air and the warm water is increased.

Next Forel discusses the phenomena which occur with refraction over cold water, and the conditions under which they appear.

The phenomena associated with *refraction over cold water* are rarer than the preceding. They appear only during the afternoon hours of warm days in spring and summer, and occasionally in the morning hours of very hot days in the height of summer. In this case the air is warmer than the water, and the lower layers of air cooled by contact with the water are characterized by a thermal gradient of the direct type. The curve of a refracted ray of light is concave below. The characteristic optical accompaniments are:

1. An apparent elevation of the plane of the horizon above the normal position; the apparent horizon is higher than the true horizon.
2. The apparent concavity of the surface of the lake, resembling a broad valley rising with gentle slopes towards the margin.
3. The apparent extension of the circle of the horizon; distant boats, which to the observer's eye should have been on the circle of the horizon or beyond it, appear to be at the bottom of the illusory valley of water well within the apparent circle of the horizon.
4. The visibility of the lower parts of the opposite coast, which normally should have been hidden by the rotundity of the water surface. For example, the Château de Chillon at 34 kilometres distance appeared to the spectator to be resting on the quay of Morges.
5. The reduction in relative height or dwarfing of the lower parts of the opposite coast. . . .

Forel then describes how the abrupt transition from the cold air over warmer water to the warmer air over cooler water is responsible for the Fata Morgana flashing suddenly into view at a given point.

Let us now study the sequence of phenomena on a fine day towards the end of spring or the beginning of summer, when, as hour follows hour, the air being at first colder than the water surface becomes warmer, while the temperature of the water remains comparatively constant. The temperature of the air, lower than that of the water in the morning, becomes equal to it towards midday, and exceeds it in the afternoon. Consequently the two normal types of refraction succeed each other in time. Let us suppose, for example, that the temperature of the morning air is 15° C., that of the water 18° C. The phenomena that are associated with refraction in air over warm water are in all their perfection; the apparent horizon of the lake is depressed below the true horizon. As the day progresses, the air heats rapidly under the powerful action of the solar radiation; the water also grows warmer, but at a slower rate because of its great thermal capacity. The temperature of the air soon equals that of the water, and is not long in passing it. In the afternoon the water may have risen in temperature to 20° C., and the air to 26° or 28° C. The phenomena associated with refraction in air over cold water are developed, the apparent horizon is elevated, the surface of the lake appears concave.

But the transformation from the one type to the other does not take place slowly or progressively. The depression of the horizon due to refraction over warm water does not diminish little by little until its value is zero; and the elevation of the horizon does not grow little by little, starting from zero when the temperatures of the two media are equal, and attaining a maximum when the difference of temperature is the greatest. The transformation does not occur simultaneously over the whole lake. The change takes place suddenly at each region, and successively from point to point. At a particular instant there may appear at different parts of the lake the two types of phenomena clearly recognisable.

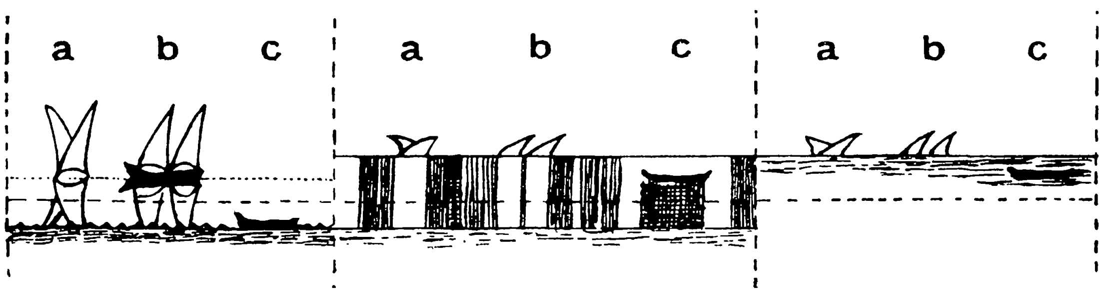
It is at such an instant that the Fata Morgana appears, represented, as I have already said, by a striated zone of rectangles in juxtaposition. Its features are at first quite disconcerting. When first recognised, it may appear in any azimuth. It moves slowly, in one or other direction, along the shores of the lake. It was from a study of the manner of shifting in position that I gained the key to the explanation of the phenomenon which may be summarised under four heads:—

I. The Fata Morgana has its origin in the region between the two regions where the opposite types of refraction rule; in the one region the morning conditions still hold, while in the other region the afternoon conditions have been established.

II. The lower limit of the striated or ribbed zone of the Fata Morgana is continuous with the depressed line of the horizon which is associated with refraction in air over warm water; the upper limit, with the elevated line of the horizon accompanying the refraction over cold water.

III. The Fata Morgana shifts always from the region where the refractions are over cold water towards that where the refractions over warm water still hold sway. The effect is due to the refraction over the cold water invading the scenery point after point.

IV. On the rare occasions on which I have observed the first appearance of the Fata Morgana, I have always seen it at one of the extremities of the circle of the horizon.



Refraction over warm water, Mirage. Fata Morgana. Refraction over cold water.

The horizontal lines in the figure in order top to bottom, represent in the *left* section: *ligne de partage*, true horizon, apparent horizon; the *centre* section: apparent horizon, true horizon, apparent horizon; the *right* section: apparent horizon, true horizon. *a* and *b*, vessels beyond the circle of the horizon; *c*, boat within the circle. Fata Morgana shifts from right to left.

Forel concludes by summing up briefly both the reasons for the appearance of the Fata Morgana and a description of its castles in air.

The general conclusions may be stated in these words:—

(*a*) The Fata Morgana is made manifest at the region where the morning type of refraction in air over warm water is being transformed into the afternoon type of refraction over cold water.

(*b*) At this region the eye of the observer placed at a convenient height sees simultaneously and in superposition both the depressed and the elevated horizons associated with the two types of refraction.

(*c*) Bright objects on the lower parts of the opposite coast are stretched and drawn out in height between the two momentarily coexistent false horizons of the lake, and, by forming rectangles in juxtaposition, give the appearance of the banded or ribbed structure of the striated zone. . . . The rapid transformation from this instability to the stability associated with the direct thermal gradient is the determining factor in the production of the Fata Morgana. The suddenness of its appearing and its brief transitory character are at once explained. . . .

As to the identity of the phenomenon here described with that observed at the Straits of Messina I could deduce several details. . . . For example, a ship sailing in the banded zone of the Fata Morgana, a steamer, or a house of known form, may be deformed so as to be absolutely unrecognisable. Although never bent into the impossible positions shown in Minasi's drawing, they may have their relative dimensions altered in an incredible manner. . . . The vertical extension of objects situated in the banded zone, the flattening of objects above the upper limit of this zone, the multiplication of images of the same object, the superposition of erect and inverted images, etc., etc., are so astonishing and so irrational that we may well forgive the superstitions of early observers, amazed and perturbed by these fantastic illusions.

The accompanying figure illustrates my hypothesis; it needs no further explanation.

It would be interesting to have reports of some Canadian castles in the air. Maybe some of our readers know of such observations, either published or unpublished. There would appear to be ample opportunity to observe the *Fata Morgana* in Canada with the many bodies of water available to us.

OUT OF OLD BOOKS

By Helen Sawyer Hogg

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THE CALLANISH STONES

The upsurge of interest in the ancient monument Stonehenge following Gerald Hawkins' investigation of it, has spilled over to include other megalithic monuments. Indeed Hawkins has also analyzed the positions of the stones at Callanish in the outer Hebrides, without achieving as definitive results as in the case of Stonehenge.

In the fervour accompanying the recent work, much of the colossal labour of the past dealing with these monuments tends to be buried or overlooked. In fact, that admirable periodical, *Science News Letter* (Dec. 18, 1965), in its summary of important astronomical events for 1965 states "A new pattern of huge standing rocks used by Stone Age man to mark the seasons, a Scottish 'Stonehenge', was found in Callanish." This could be misinterpreted to mean that this monument was actually discovered in 1965, which is very far from the truth. The misunderstanding may have originated from a footnote on page 87 of the fascinating book "Stonehenge Decoded" by Gerald S. Hawkins in collaboration with John B. White (Doubleday and Co. 1965), "A megalithic monument at Callanish in Scotland has recently been found to be of considerable interest, but the results of investigations of that site were discovered too late for inclusion here. They appear in an article in the appendix."

Hawkins' results may have been too late for inclusion in the main body of the text of his book, but those of Captain Boyle Somerville, R.N., certainly were not, because they were published in 1912 in the *Journal of the British Astronomical Association*, vol. XXIII, pp. 83–96, with plates and figures. This work was carried out by Captain Boyle Somerville, R.N., in 1908 and deserves to be much better known. Accordingly we reprint sections of his paper, beginning with Captain Somerville's explanation of how he became connected with the investigation.

Astronomical Indications in the Megalithic Monument at Callanish

By Captain BOYLE SOMERVILLE, R.N.

I wish, before entering upon the subject of my lecture, to express my thanks to your President for so kindly inviting me to lay before you some of the results of my investigations, on astronomical lines, among British prehistoric monuments; it is an honour that I greatly appreciate.

I should like at once to explain that the knowledge of Astronomy which I possess does not soar into the heights to which your Association is accustomed; nor does it reach to a greater knowledge of mathematics than is required by a sailor for the purposes of navigation. Primitive and practical as it is, however, it is sufficient for the investigation of ancient megalithic ruins, with a view to discovering whether there is or there is not any ground for supposing that they have been laid out by their founders with the purpose of connecting them with the heavenly bodies: whether, in fact, they have been orientated; and if so, to which of the heavenly bodies.

If this supposition can be sustained, we are at once led into profound ethnological considerations, derived from the why and the wherefore, and the origin of the custom—matters which belong more properly to the Royal Anthropological Institute, where I read a paper last November, from which a large portion of what I propose to say here is taken verbatim. I shall not, therefore, now go outside the astronomy of my subject; and refer to these further problems only in order to show that such a discussion does not in the least end with the question of orientation. But until this orientation receives official sanction at the hands of astronomers, all further discussion of the matter comes to little more than vain imaginings.

I was myself led into the investigation chiefly through reading Sir Norman Lockyer's well-known book "Stonehenge", at a time and place when I was within easy reach of several prehistoric monuments, being then engaged on a hydrographic survey of Lough Swilly, on the north coast of Ireland, the shores of which offer a rich field for archaeologists. This was in 1908; and since then, I have had opportunities of examining over 60 of these ancient ruins, of all descriptions, in Scotland and Ireland, with the result that I am now fully convinced of the reality of the existence of orientation. I will even go further, and say that in my belief no one with a technical knowledge of practical astronomy and surveying, who takes the trouble to go into the field to examine for himself, could long remain in doubt on the matter.

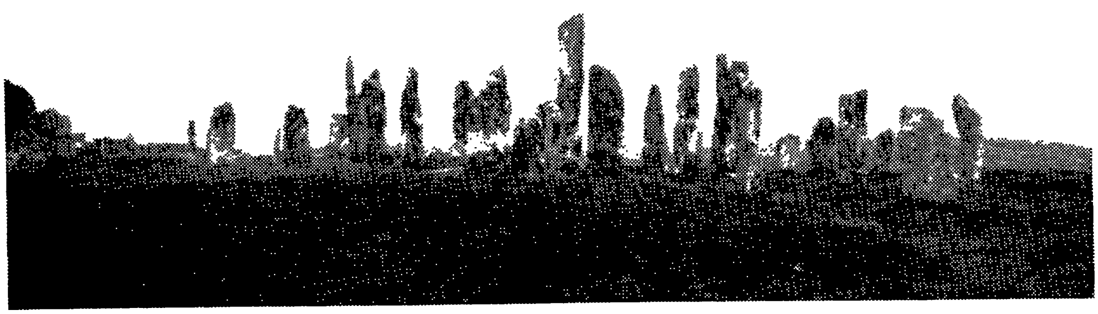

FIG. 1—The Callanish Stones. (Courtesy of Dr. and Mrs. Gerald Gold)

Captain Somerville then gives a clear description of the structure of stones on the Island of Lewis. His original figure 2 is a long fold-in. It has been reduced with some renumbering for reproduction in this JOURNAL.

After these prefatory remarks, I now beg to convey you to the N.W. coast of the Island of Lewis, the northernmost of the Outer Hebrides, and to the comparatively well preserved megalithic structure there, known as Tursachan Challanish, which contains in itself so remarkable a collection of orientational features that it forms quite a classic example for demonstration.

This imposing group of forty-eight megaliths stands on the ridgy crest of a small peninsula near the head of East Loch Roag. The surrounding view is of a hilly country, wild and treeless, covered with heather.

Though elevated 100 feet above sea level, the water of the loch is visible only in small glimpses as it winds inward from the outer sea, 7 miles distant; the sea horizon is also almost entirely obscured by the land.

Fig. 2 is a plan of the whole monument; and is produced from a careful survey, plotted originally on the scale of 1/10th inch to 1 foot.

The arrangement of the menhirs is carefully described by Captain Somerville. The term "menhir" is derived from the Breton, "men" for stone, and "hir" for long, and is applied to the large upright megalithic stones found in Europe and other parts of the world.

The remains consist, as will be seen, of the following chief features:—

A.—Two long lines of menhirs, running nearly parallel to each other, a little eastward of true north. (I shall refer to these as "A east" and "A west" respectively, when describing them.)

B.—A short line of menhirs, lying in an east and west direction.

C.—A longer line, lying in a north and south direction.

D.—A short line, lying in a direction slightly northward of east.

E.—A "circle" of 13 great menhirs.

Besides these, there are a chambered sepulchre; a very large single menhir (which dominates the whole group) lying within the perimeter of the "circle"; and three single menhirs without it, to the north-east, south-east, and south-west respectively, standing at no great distance from it.

The astronomical implications of the monument are discussed by Captain Somerville who explains why the sun is less satisfactory for dating alignments than conspicuous stars.

When a row of standing stones, or several rows, as at Callanish, have been set up in straight lines, it seems rational to suppose that it was done with some intention, dependent on the direction in which they lie, for it is clear that they can never have formed part of a house or roofed structure; they appear now, probably, as they have always appeared. This becomes even more obvious when it is found that in monuments differing widely in geographical position, the direction of these rows, or alignments, is towards the same points of the horizon. Occasionally the eye is directed by the line of stones to a hill-top, or a cairn on a hill summit; sometimes, as at Callanish, no object, natural or otherwise, lies along the lines of sight in either direction . . . .

The inference is that we must look beyond the terrestrial termination of the line for

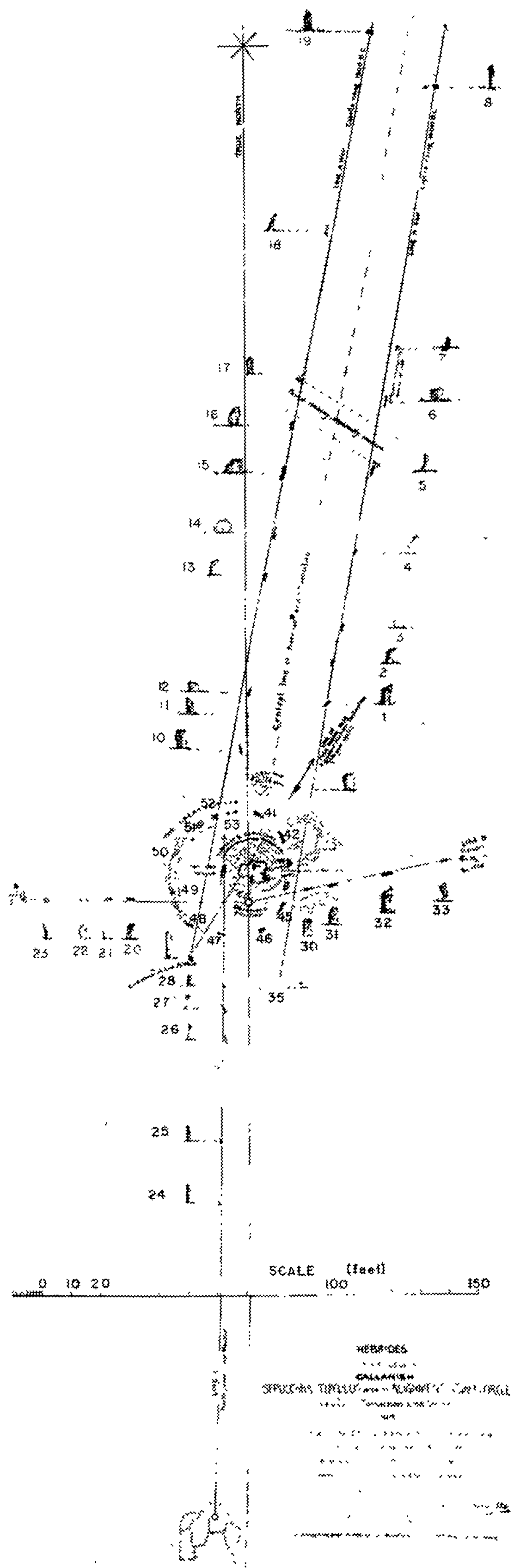

FIG. 2

some celestial body; and, if such, it must naturally be at that moment of its path when it is either rising or setting behind the horizon of the observer. As this land horizon is always elevated above the true, or sea horizon, the body must, at the time of observation, be itself elevated by the same amount.

On the presumption that these alignments originally directed to heavenly bodies, having carefully found the azimuth and the altitude, and knowing the latitude, it is of course easy to determine the corresponding declination, whether of Sun, Moon, or star.

If provided with a table of the declinations of stars in previous epochs, it is thus possible to produce a date, or rather dates, for the founding of the monument, when the star with the inferred declination could have been seen rising on the given alignment.

The same is true of the Sun, when the solstitial azimuth is indicated; but as the change in obliquity, and consequently the alteration of the Sun's tropical declination, has been so extremely small, compared with the change in any stellar declination, due to precessional movement, in an equal period of time, a solar date can only be looked on with proportional uncertainty. The alteration in the Sun's tropical declination, for example, between 2000 B. C. and the present day has been only 0° 28′ 27″, while in the case of the stars, that of the Pleiades (η Tauri) has changed by about 7° 30′, Capella by 14° 29′, and Arcturus by 23° 06′.

Captain Somerville points out that another difficulty with the usefulness of the sun in these computations is the uncertainty as to which part of the sun was used, the moment of emergence or disappearance of the upper limb, the centre, or the lower limb. As a specific instance, he describes the computations for a monument named Cloghbane on Lough Swilly. If the upper limb of the sun is used, the computed date would be many centuries in the future; the lower limb would indicate 1210 B.C., and the Sun's centre, 850 A.D.

Captain Somerville proceeds to give detailed descriptions of his measures of alignments of the various sections of the Callanish monument. From these descriptions which occupy seven pages of his paper, we excerpt the most important statements and conclusions.

Line "A East."

I shall first deal with the eastern of the two lines, which consists of eight stones. In order to determine its azimuth, I chose a spot to the southward, whence all the stones composing it came into alignment with the high menhir at its northern end, and there set up my theodolite. There seemed to be the foundations of a standing stone at this point, though no other vestige of it remained; but in any case, the azimuth was exactly obtainable from here, and is N. 9° 49′ 30″ E. This, with the hill-crest altitude seen along it of 1° 10′, produces a star with declination 32° 26′ 37″ N.

Line "A West."

The western line consists of ten stones. The seven northern ones, including the tall terminal menhir, are, with one exception still all in line; but the three nearest the circle are evidently either displaced or else never belonged to the line; for it is not now possible to get them into alignment with it . . . .

The azimuth of "A west" . . . produces a star with declination of 32° 28′ 12″ N.; a result so close to that derived from the "A east" alignment that there is no doubt that both directed to the same star, though, as will be seen, the two alignments are not parallel, differing as they do by 1 1/3° in bearing .... The same star would, in fact, be seen along each line, but at slightly differing positions of its path up the heavens.

The bright stars and dates to which this declination (32° 27′ N.) may refer are as follows:—

Capella in 1800 B.C.
Castor in 650 B.C.
Arcturus in 320 B.C.

For reasons to be discussed later, connected with the time of rising of this star, and also with the date to be derived from line D, it seems most likely that the star Capella, at its appropriate date, was the object of observation; but, astronomically speaking, each of these stars and dates has equal probabilities. I shall accordingly for convenience, refer to these lines in future as the Capella lines, remarking, in passing, that alignments for this star have been found at various dates in several other prehistoric monuments in Great Britain.

The last point of interest concerning the Capella lines to which I would direct attention is the connection between them and the outlying stone standing at about 15 feet to the north-east of the circle .... Standing at this south-western stone, the azimuth of the north-eastern, combined with the altitude of the horizon seen beyond it (at a somewhat marked dip in the distant hills), produces a declination of 28° 10′ 25″ N.

When first reaching this result I was somewhat puzzled as to its meaning, for I had fully expected that the direction given by the line would turn out to be for the solstitial sunrise, which would entail a declination of about 24° N. Obviously, therefore, it could not be a sun-line; nor does this declination belong to any *probable* star in "prehistoric" times except perhaps Pollux in 1200 B.C. The only heavenly body to which otherwise it could refer is the Moon, and in view of the fact that I have obtained a similar declination, along undoubted sightlines, in several other monuments in other parts, I venture to put forward the following suggestion.

The rising of full Moon, when it is at its northern tropical declination, occurs only at or near the date of the winter solstice, and if the azimuth of this event (full Moon rise) were marked when the Moon had reached its greatest possible declination of 28° N., this full Moon rise would indicate the beginnings of periods of (roughly) 19 solar years, and would also closely be connected with the rising of Capella; which, as I shall show presently, took place shortly after sunset at the winter solstice in 1800 B.C.

Line B.

This line, composed of four menhirs, is constructed in a similar fashion to the two Capella-pointing lines, namely, of flat slabs placed in the ground in the direction of the line and terminated by a taller menhir of nearly square section ....

The azimuth of this line, combined with the altitude of the horizon along it, produces a declination of 0° 35′ 19″ N.—that is to say, for sunset on the day of equinox. Of that there can be no doubt whatever.

Line C.

This line is composed at present of five menhirs, but, by the gaps in it, evidently comprised originally at least three more ....

I should add that near the southern end of line C there is a collection of enormous boulders, so enormous that their heaping together can scarcely be other than natural, though there is a sort of symmetry in their disposition which renders an opposite view permissible ... but, whatever method was employed, the fact remains that the line of stones under discussion does present a practically true north and south bearing.

The importance of the Pleiades and the 19-year lunar cycle seems to be emphasized by Line D.

Line D.

The stones of this line are somewhat disarranged from their original regularity, though not seriously, and it is not difficult to obtain a mean alignment which shall include all the four stones very fairly. The azimuth of this alignment, combined with the altitude of the horizon seen along it, produces a declination of 6° 43′ N., which refers to the following stars and dates, viz.:

Pleiades rising in 1750 B.C.
Spica in 1270 B.C.
α Arietis in 1130 B.C.
Aldebaran in 800 B.C.

The astronomical probabilities of each of these four stars is equal in degree, but it should be remarked that α Arietis is not at all conspicuous in the heavens, either by position or brilliancy; though it must not be forgotten, either, that this star, or rather the constellation to which it belongs, had great importance in the astronomy of eastern countries in early days, marking as it did the Sun's entry at springtime on a new year.

Taking the Pleiades' date of 1750 B.C., it would make the building of this line practically contemporaneous with the two long Capella lines, which, from the similarity in size and description of their stones, is in the highest degree probable, whatever their date. I would even go further, and say that the Pleiades star-date forms a valuable check on the accuracy of the Capella star-date for the founding of the monument. But the Spica date of 1250 B.C. and the Aldebaran date of 800 B.C. are just as astronomically probable, though neither is supported by the date given by another line in the same way as that of the Pleiades.

I would now call attention to the fact that line D, the Pleiades line—if I may so name it—and the Equinoctial line (line B), if produced towards each other, meet at a point on the central line of the tumulus covering the sepulchre, at its southern edge, which is *exactly* equidistant, namely, in each case, 69 feet 6 inches from the terminal stones of lines B and D. A single position is thus afforded from which an observation can be made along both these lines .... This connection between the equinox and the rising of the Pleiades is of particular interest in the light of the following well-known quotation from Diodorus Siculus concerning the Hyperboreans, and I would also call attention to the reference to the 19-year lunar cycle.

Quotation

(Diodorus Siculus, ii., 47 ed., Didot, p. 116).

"It is also said that in this island (i.e., that of the Hyperboreans*) the Moon appears very near to the Earth; that certain eminences of a terrestrial form are plainly seen upon it; that the god (Apollo) visits the island once in the course of 19 years, in which period the stars complete their revolutions, and for this reason the Greeks distinguish the cycle of 19 years by the name of the Great Year. During the season of his appearance the god

*It is, I understand, by no means certain that the island of the Hyperboreans refers definitely to Britain; but the quotation clearly points to some race inhabiting a country northward of Greece, where solar and stellar observations of a religious character were made in early days.

plays upon the harp, and dances every night from the Vernal Equinox to the rising of the Pleiades, pleased with his own successes."

. . . at Callanish, in 1750 B.C. (the date obtained from the Pleiades line), this remarkable group of stars rose heliacally, that is to say, so as to be *observable* in the morning twilight (or at about 40 minutes before sunrise) on April 10th. There would thus be at Callanish, at the obtained date, 20 nights after the Sun's passage of the Equinox for the "dancing", before the rising of the Pleiades became visible in the dawning of the day.

Concluding the discussion of the individual features is the alignment of the Great Menhir and the tumulus (sepulchral mound), and the great circle.

THE GREAT MENHIR

The only astronomical fact to which I would call attention in connection with the tumulus and its sepulchre is that the Great Menhir—standing within the "circle", though not at its centre—is erected due west from the centre of the tumulus; that is to say, that at sunset on the day of equinox the shadow of the great stone would lie exactly across the grave, and along the entrance alley that leads to it from the eastward. . . .

E.—THE "CIRCLE."

The great circle, which is at first sight the most striking feature of the whole group, being composed of 13 huge stone slabs 10 to 12 feet in height, carries to-day no actual evidence (astronomical) of its date or purpose. It is, in the first place, not strictly speaking a "circle" at all, but a sort of irregular oval, with its eastern side flattened; while the stones composing it are themselves not equidistant from one another.

It is hard to account for this shape, unless, perhaps, there were difficulties in providing foundations for such large and weighty stones at equal distances apart, . . .

It seems obvious in any case that the circle was not erected at the same time as the rest of the monument . . . .

The delicate balance in the position of Capella at this period as seen from Callanish is described by Somerville.

An interesting fact in connection with Capella, as seen from Callanish in 1800 B.C., must now be stated.

In this year, at that latitude, Capella performed its path round the pole at a distance from it of 57° 33′. At Callanish, the north pole of the heavens is elevated 58° 12′ above the horizon; so that Capella, when at the lowest point of its path, was some 39′ above the horizon, and thus never set below nor rose above it; it was circumpolar. But the skyline of the hills towards which the long lines of stones is directed, is elevated 1° 14′ 50″, so that Capella was obscured from sight by the hills, when at the lower part of its course, just as much as if it had sunk below the sea horizon; and thus its rising actually could have been observed, only it was above a hill horizon, instead of a sea horizon.

By about 1700 B.C., a hundred years after the date that I have assigned to the erection of these lines of stones, the declination of Capella would have altered sufficiently to cause this apparent rising to cease; and Capella would then always be in sight at night, circling round the pole. This fact gives additional weight to the date assigned for the erection of the stones (1800 B.C.).

Captain Somerville then summarises his conclusions.

SUMMARY

... We may summarize, therefore, the results of the preceding investigation as follows:—

(1) There is a single point, situated on the southern edge of the tumulus covering the now exposed sepulchre, from which an observer finds himself aligned by lines of megaliths for the equinoctial sunset along line D, for the rising of Capella along the central line of the avenue formed by lines "A east" and "A west," and for the rising of the Pleiades along line D; the two latter events during the epoch 1800 to 1750 B.C.

(2) Line C is laid out on a true north and south line with the Great Menhir erected at the western edge of the tumulus. All the alignments, therefore, are connected with the sepulchre as their point of origin.

(3) Evidence possibly exists, from the direction of the line joining the two outlying menhirs, of the observation of moonrise when it is full Moon at the extreme northern tropical point of its path, occurring every 19 years, thus marking that epoch.

(4) The Great Circle, besides being constructed of megaliths of a different type from the others, and much larger, is placed entirely asymmetrically to the remainder of the group, and is, therefore, probably a later, and possibly an alien construction, intended to invalidate or to mar the astrologically auspicious qualities of the alignments.

H.M.S. "Research",
Portsmouth.

It seems remarkable that over fifty years ago from his deductions on the alignments of the Pleiades and Capella, Captain Somerville gave dates for the monument in line with present thinking. Hawkins (op. cit.) rejects Somerville's interpretation of these alignments because he thinks the rising of these objects would not be observable, stating that a star at sea level is six magnitudes fainter than when higher in the sky. Hawkins does corroborate Somerville in the 19-year lunar cycle. He found ten alignments with the sun and moon at their extreme positions on the horizon. He takes as a definition of sunrise and sunset the time when the lower limb is tangential to the horizon, noting that this seems to have been the definition of sunrise used by the Stonehengers. Hawkins concludes that "the Callanish people were as precise as the Stonehengers in setting up their megalithic structure, but not as scientifically advanced".

After I had published in the *Toronto Star* an article on Hawkins' work on Callanish, two natives of the island of Lewis, now living in Toronto, got in touch with me. Mrs. Gerald Gold reported that she and her husband had taken colour photographs of the Callanish stones on a recent trip there. I am indebted to Dr. and Mrs. Gerald Gold for the picture accompanying this article; the plates of Captain Somerville would not reproduce well. Apropos of Dr. Hawkins' remarks on the visibility of the Pleiades and Capella at rising, Mrs. Gold comments that nowhere else has she seen the sky as transparent and the stars as brilliant as they appear at times over the Hebrides. From Mrs. Anne Macleod I received a clipping from the island's newspaper, the Stornaway Gazette, with a drawing of the stones as a heading.