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In This Issue

ANNOUNCEMENTS ... PAGE 101

THE RADIAL MARKINGS OF VENUS: FORTISSIMO ... PAGE 102

MODERN SELENOGRAPHY ... PAGE 106

SOME SUGGESTIONS ABOUT WHAT CAN BE DONE ON JUPITER DURING THE 1957 APPARITION ... PAGE 108

THE MARSITRON HYPOTHESIS: THE APPEARANCE OF LUCUS LUNAE AND BIBLIS FONS AS EVIDENCE OF A MOBILE SPECIES ON MARS ... PAGE 110

A NOTE ON THE S.E.B. DISTURBANCES OF JUPITER ... PAGE 115

BOOK REVIEWS ... PAGE 116

VISUAL OBSERVATIONS OF COMET AREND-ROLAND 1956h AND OTHERS ... PAGE 116

A COMING OCCULTATION OF A STAR BY SATURN AND ITS RINGS ... PAGE 122

ANNOUNCEMENTS

A. L. P. O. Emblem. We point with pride to the attractive emblem on the front outside cover of this issue. It was kindly drawn for us by Mr. Edgar Paulton, the Chairman of the active Observing Group of the Amateur Astronomers Association in New York City. We understand that Mr. Paulton is a commercial artist by profession, and we thank him for supplying our Association with this fine emblem. We shall be glad to hear from our readers how they think that it may most suitably be used - on future A. L. P. O. Convention programs, in reduced size on our front cover, on letterheads, or where else.

Concerning Drawings for Publication. We feel that The Strolling Astronomer owes much of its value to its ability to publish lunar and planetary drawings by amateur observers. It must be confessed, however, that many of the drawings we receive are so faint and lacking in contrast that publishing them is a matter of extreme difficulty. Our publishers do their best. However, it will help greatly if drawings, whenever practical, will be made in India ink on a white background and will be neatly labelled. It may be impossible to represent the variety of shading on a planetary disc in this fashion, but it is urged that contrasts of tone be exaggerated for ease in reproduction. We thank you for your assistance in this matter.

Spaceflight. The British Interplanetary Society has found over the years that their bimonthly Journal is criticized about equally much by those who think it too technical and by those who think it not technical enough. They accordingly are beginning a new, popular magazine called Spaceflight; Volume I, Number 1 appeared in October, 1956. The price is three shillings per issue - at least we find no subscription rates. We certainly recommend Spaceflight unhesitatingly to all who have any interest in space flight or in the numerous sciences which will eventually make it a reality. Volume I, Number 1 includes two discussions of the basic principles of interplanetary flight, an article on the Russian rocket pioneer Tsiolkovskii, a long and illustrated discussion of the Vanguard Project by K. W. Gatland, and an article on the colors of Martian "vegetation" by A. E. Slater. There is even a bit of humor in the way of space cartoons and some amusing, if maybe also embarrassing historical quotations on new developments in science and technology - thus in 1838, "Men might as well project a voyage to the Moon as attempt to employ steam navigation across the stormy North Atlantic Ocean." We are sure that our readers would not only enjoy Spaceflight but would learn much from it. The Editor is our member and colleague, Mr. Patrick Moore, Glencathara, Worsted Lane, East Grinstead, Sussex, England.

Assistant Jupiter Recorder. We now have appointed as our Assistant Jupiter Recorder to help Mr. Squyres:

Chester J. Smith
9775 Burgos Avenue
Oakland 5, Calif.

Mr. Smith is one of the most active lunar and planetary observers in northern California, and we thank him for accepting this post to help the work of our Jupiter Section.

Proposed Radio Communication Among Jupiter Observers. In the last few years there has been some talk, but little or no action, about using radio "hams" to achieve rapid communication among lunar and planetary observers. Jupiter Recorder Henry P. Squyres now urges us to attempt to use radio communication to advance our current study of Jupiter. A friend of his, Mr. Robert Fuller (W6KHK), is willing to supply radio communication for the A. L. P. O. in the Los Angeles area. Mr. Fuller, under good conditions, can reach other "hams" anywhere in the United States, occasionally anywhere in the world. He can operate on these amateur bands: 160, 80, 75, 40, 20, 15, and 10 meters. We strongly request A. L. P. O. members to try to find "hams" where they live and then to write to Henry P. Squyres, 3608 N. Durfee, El Monte, Calif., who will arrange talks between such A. L. P. O. members and either himself or other members living in the Los Angeles area.

Corrections to "Some Basic Procedures in Planetary Photography" in July-August, 1956 Issue. Author P. R. Lichtman has pointed out a few errors - strangely not our own - as follows:

Pg. 75 paragraph 3, line 6. Should read "inversely proportional to the square of its radius."

Pg. 77, table of filter factors. Should read "Filter Factors. (Kodak Royal Pan and Tri-X Pan emulsions.)"

Pg. 75, paragraph 1, equation (1). The equation is a simplification; it takes no account of reciprocity effect. If great accuracy is required, replace the term "It" in this equation by one that corresponds more closely to the particular emulsion being used.

Two Useful Tables. We acknowledge with thanks the receipt of a "Star Brightness Magnitude Table" and of a "Parallax Table" from Mr. Lawrence F. Smith, 979 Barrington Road, Grosse Pointe Park 30, Michigan. The former table lists brightness-ratios as a function of differences in stellar magnitude. The latter lists stellar parallax as a function of distance. Mr. Smith kindly offers these tables for the cost of printing and distribution, about 15 cents for the two of them - a generous offer and worth accepting by persons working in these fields.

THE RADIAL MARKINGS OF VENUS: FORTISSIMO

by James C. Bartlett, Jr.

In the May-June 1955 issue of The Strolling Astronomer my good friend, Mr. Patrick Moore, had at me with a shillelagh of formidable weight in regard to a paper I had published in the same journal for January-February of the same year. This left me in something of a pother, not because of Mr. Moore's objections but because I was rather put to it to find time for a suitable reply. I was in fact heavily engaged with Aristarchus and also in trying to determine whether the microorganism, paramecium, did or did not exhibit anything properly resembling intelligence. Paramecium will require further investigation, but I may have something to say about Aristarchus before too long. At any rate I come late to the fray.

In the meantime Mr. Richard Baum has already effectively replied to many of Mr. Moore's criticisms, which leaves me with little ammunition. Still, I shall venture a shot or two.

But first let me say that I fully reciprocate the kind sentiments so courteously expressed by Mr. Moore. We may disagree now and then in some detail or other, but such disagreement in no way lessens the respect and friendship I feel for our gifted colleague. As to Mr. Richard Baum, it is no secret that I regard him as a particularly acute and talented observer certainly among the foremost of the present planetary workers. I think therefore that however we are to understand his recovery of Lowell's"spoke system" on Venus, we must not dismiss it as being in some way merely illusionary.

Returning now to Moore's paper contra, it may be noticed that while conceding Lowell's complete sincerity and integrity our critic simply feels that Lowell's observations of Venus were not accurate. In support of this conclusion, Moore quotes one of Lowell's descriptions (was it from The Evolution of Worlds?) of the Venusian streaks and of his interpretation of same.

Now so far as this goes I would certainly agree. As pointed out in my original paper, I have never seen the complete streak system as reported by Lowell; nor would I think his picture of a barren sun-baked desert as truly representing the facts. But this has no bearing whatever on his accuracy of observation within the limited meaning I gave to that word in my original paper. I did not base my claim to accuracy for Lowell on the basis of his "spoke system", nor was it implied that his system was a faithful description of reality - as may be gathered from the penultimate paragraph of page 4 of the January-February issue of The Strolling Astronomer.[1]

What I did was to show that in the main features of the planet, as described by Lowell, his descriptions were so faithful that the appearances he recorded can be recognized today. One thinks of the south cusp cap, the cusp band, terminator irregularities, etc. Therefore - and that was the whole point of the argument - Lowell's <u>general</u> accuracy having been established, we would not be justified in rejecting his streak system out of hand.

It may also be noticed at this point that a clear distinction should be drawn between the credibility of observational evidence and the interpretation which the observer places upon the same. The one may be completely accurate while the other may be completely fallacious. Thus the latter can hardly be used to measure the accuracy of the former.

Moore points out that Lowell used his "spoke system" to sustain"conclusively" the 225 day rotation period, now generally abandoned. The inference is that the observational data must have been completely erroneous, yet such an inference is by no means necessary. It is not necessarily implied that the observations upon which the erroneous conclusion was based were themselves in error.

Such an inference might be valid if the streaks were regarded as physical surface features; but Lowell himself did not so regard them. If atmospheric in nature it is entirely possible that an atmospheric pattern might be established which would remain relatively stationary for some days or even weeks with respect to a selected point of reference, say the bright limb. Measurements made upon such a system would obviously yield entirely erroneous results when used to establish a period of rotation. Yet the system itself would be real enough. To understand how this might be, we may consider a rapidly rotating sphere upon which a spotlight is thrown. To an outside observer the spot of light would not rotate with the sphere, but would remain in one position relative to the limbs. If the sphere were otherwise featureless, the spot of light would give no information as to the rotation of the sphere.

Of course in the case of Venus the matter is not so simple, since the planet is not only rotating but also revolving around the sun, which may be considered as the spotlight. Moreover the observing station, which would be the earth, is also moving in relation to both. Hence the angle between the observer, planet, and sun is not constant and for this reason alone any spot upon the planet produced by the sun would eventually show a displacement. It would in fact show a slow drift towards the west, or bright limb between superior conjunction and greatest elongation east as Baum observes. Yet if the displacement of the central spot depended solely or even mostly upon the change of angle between the planet, the sun, and the observer on the earth, then it would be sufficiently gradual to suggest a very slow rate of rotation for the planet if used to measure that quantity. Hence I do not see that Lowell's deduction of a 225 day rotation invalidates his accuracy as <u>an observer</u> in any way. Nor do I see that because he may have erroneously interpreted his data, the data themselves must be rejected.

All of which is by way of denying that Lowell's conclusions are proper measures of his accuracy in recording details. As to whether I suppose that Lowell's streak system is precisely as he depicted it, the reader is respectfully invited to consult page 4 of my original paper.[2]

Moore concedes that streak markings may exist upon Venus and even mentions certain photographs which would support such a view. Here again I find myself in agreement with our colleague.

If the reader will carefully review my original paper he will establish certain facts as follows:

a) No claim is made that either Lowell's or Baum's representations correspond exactly to the reality; b) a claim <u>is</u> made that there is much evidence from various quarters - some much before and some much after Lowell's time - for the existence of streak markings upon Venus; c) from which it follows that Lowell's Venus observations, while perhaps erring in detail, cannot be wholly dismissed; d) that the independent recovery by Baum of what appears to be substantially the same system furnishes important support to the theory of its objective existence, though it is recognized that the true organization of the system may not correspond exactly to the drawings of either observer.

What seems to bother Mr. Moore most of all - and this I regard as the crux of his criticism - is that Lowell's streak system was not seen by Barnard and other first rate observers equipped with superior - or at least larger - instruments than that commanded by Lowell. Ergo, to Mr. Moore, this seems sufficient reason for questioning the existence of the system. Mr. Baum has so effectively answered the aperture argument in his recent rejoinder that there is nothing I could add to it. But as touching the subtler argument, implicit in such a view, that because men of equal and perhaps greater stature failed to confirm Lowell's observations then the observations themselves must be invalid, why this I deny.

Would any one care to argue that Virchow was not a more eminent and a more experienced man than Koch? Yet Koch was right and Virchow was wrong in their respective views upon the nature of disease. One may also recall that the great Liebig was still maintaining his chemical theory of maladies while Pasteur was demonstrating beyond reasonable doubt the relation between microorganisms and infection. Leverrier, "the French Newton", certified the existence of "Vulcan" and even calculated the orbit. But where is "Vulcan" today? I once knew of an eminent geologist who identified a distorted staurolite crystal as a scale from the skin of a dinosaur.

Therefore to my iconoclastic mind authority is no guarantee of infallibility. The fact that Barnard et al were never able to confirm Lowell's streaks on Venus does not in any way imply that the streaks never had any real existence. Barnard was never able to see the Martian canals either, as pointed out in my original paper, notwithstanding that a few of the more prominent of them have since been photographed.

The rub is, as Moore himself has shown in his excellent Guide to the Planets,[3] that the human eye may differ so significantly as between different individuals that merely negative evidence from this quarter can hardly be used to invalidate positive evidence to the contrary. Thanks also to individual differences in color sensitivity, as I have shown elsewhere,[4] it is quite possible for two dissenting observers to be equally right though on the superficial evidence one may appear to be completely wrong. I contend therefore that it is improper to suppose either Lowell or Baum must have been deceived by illusion, merely because they recorded phenomena not attainable by others. Moreover, as remarked above, it is not necessary to go back to the 225 day period in order to believe in the permanency of at least some of the linear markings.

Mr. Moore also draws a distinction between radial markings and mere streaks; but the difficulty here is chiefly semantic. Given a system of radial markings seen only in part it would manifest as an unsystematic collection of streaks. I myself, as previously stated, have never seen anything resembling the integrated streak systems of either Lowell or Baum. Yet I have seen well-defined streaks which if integrated and prolonged might well produce a system resembling that reported by Lowell and Baum. Whence I conclude that such a system may have a real existence, but inaccessible to me as a system for various reasons including unfavorable seeing conditions.

It is here necessary to notice certain statements made in Mr. Baum's excellent rejoinder in which, among other things, he does me far too much honor.

Under head of "Some Important Errors in the Bartlett Paper", Mr. Baum calls attention to the matter of Young's statement to the effect that in 1902 Percival Lowell repudiated his own previous views with respect to the radial markings of Venus. If I erred in this I erred in good company, for as Mr. Baum himself shows in quoting me the facts were not known to the Lowell Observatory authorities themselves whom I consulted when unable to run down the original reference in materials available to me. On the other hand we are all much indebted to Mr. Baum for having recovered the source of Young's remark.

The second apparent faux pas discovered by Mr. Baum, and which he charitably describes as a "misinterpretation" has to do with a statement concerning the bisecting of the central spot of the spoke system by the terminator.

Now as Mr. Baum properly observes, this central spot, the hub of the system, considered as marking the subsolar point would occupy the center of the disc only at superior conjunction. From thence to eastern elongation it would show a slow displacement toward the bright limb, theoretically lying on the limb at dichotomy. At any rate a mere moment of thought will show that it could <u>never</u> lie on the terminator <u>as a subsolar point</u>.

Nevertheless it is indubitably true that in my original paper I did indeed speak of this spot as being bi-sected by the terminator and - even worse - as having passed to the <u>night</u> side of the terminator, a <u>gaucherie</u> which Mr. Baum was kind enough not to notice.

Whence then did I derive such odd statements? From Mr. Baum's own drawings, unless I have in fact misinterpreted them. Perhaps the reproduction was at fault - if so, my humble apologies - but if one will consult the series running from March 23, 1951, to July 7, 1951, as published in <u>Urania</u> , he will see what I mean. In the first drawing, taken soon after superior conjunction, the hub of the spoke system is seen to occupy a nearly central position on the gibbous disc, and in subsequent sketches through April 24th the westward drift toward the bright limb is apparent. But now when we come to the figure for June 13th, 1951, we see a very different aspect. There appears to be a crescentic band running near the limb, on which occur knots at more or less regular intervals. From two of these there appear to run two streaks converging on a large, dusky, circular spot which apparently forms the center of the system - and this spot <u>is</u> cut by the terminator in the drawing. In the subsequent drawing for July 7, the terminator has apparently advanced <u>past</u> this spot (which would then indeed lie on the night side) and streaks are observed apparently also passing into the night side. As a matter of fact Prof. Haas, noticing this apparent error, thought that I had inadvertently used "terminator" for "limb" and wished to change the text accordingly. But, as I advised him, regardless of the difficulty the drawings seemed to show differently and thus I contended that the statement should stand.

Why then did I not draw attention to this peculiarity at the time? Well, <u>mea culpa</u>, because I had a sly desire to see if anyone would catch such an odd,apparent error. No one did - not even Mr. Moore - excepting Mr. Baum himself who was properly grieved. Still, this is what the drawings seemed to indicate to me - a central spot, the apparent hub of a spoke system, which certainly lay upon the terminator.

Of course the necessary inference is that if <u>this</u> spot is the center of a spoke system, then it is not a subsolar spot at all and so some other explanation for the system must be sought. But inasmuch as Mr. Baum correctly speaks of the limbward drift of the central, subsolar hub of the system, and inasmuch as his pre-June 13th drawings show the same, it may be either that the engraving process produced a spurious hub, or that this was an independent spot unfortunately occurring in such a way as to suggest a relationship which did not exist.

At any rate it is quite clear that Mr. Baum understands very well the mechanics and geometry of a spoke system generated from a subsolar point, so whatever the explanation for the drawings cited above no inadequacy in this department can be charged against him. Indeed it may be that he understands the limitations of such a system, used to measure rotation, better than did Lowell; for in his rejoinder, Mr. Baum specifically recognizes that a rotation period derived from the system really measures only the rotation period of an atmospheric system, and not that of the solid planet at all.

One more thing remains to be considered. Mr. Baum, in his rejoinder, reproaches me for having selected "extremist" views of other observers, "completely confirming those of Lowell". It is to be feared that any misinterpretation is here on the other side, for in truth I quoted Espin to show how the streaks recorded by Gruithuisen and Fournier "were alotgether different in arrangement from those depicted by Lowell".[5] The point was <u>not</u> to show that earlier or later observers completely confirmed Lowell, but only to show that Lowell had been neither the first nor the last observer to record streak markings upon Venus.

To return to the mainstream of argument. Mr. Moore - perhaps with his tongue in his cheek - invites me to show him where he is wrong. This would be a formidable assignment since, as you know, "He who is convinced against his will ......" But I have sought to show why I believe that his arguments contra are not necessarily sound.

It remains to ask one last question. Do I believe that the streak system of Baum exists? Yes, I do. I believe it because the recording of such a system, so strikingly similar to Lowell's, by a man who at the time was not familiar with Lowell's work on Venus would otherwise be an altogether too remarkable coincidence. I believe it also because there is nothing impossible in it, and because many other observers, myself included, have seen at least imperfect presentations of it. As to why it is not seen by others, I suggested a possible reason in my original paper - namely, difference in color sensitivity between different observers. Indeed Moore himself testifies to the striking effects of such differences, and any given observer may demonstrate for himself the significant differences in planetary detail when examined by light of different colors. Let him but examine Jupiter through a number of differing color filters.

I do not, however, believe that the streak system of Venus necessarily corresponds exactly to the delineations of either Lowell or Baum - anymore than I believe that my own drawings are photographic reproductions of reality. As Mr. Baum himself clearly recognizes, personal equation, the difficulty of observing faint detail at the limit of visibility, and the special difficulties attendant upon observations of Venus all must play a part in reducing drawings to approximations.

Nevertheless they may be very close approximations, and as accurate as the limitations of the eye and method will permit. Hence I do not see that we must reject as illusionary all observed detail which others have not been able to resolve ..... and there I rest my case.

<u>References</u>

1. Bartlett, J.C.; "<u>The Radial Markings of Venus and Their Modern Resurrection</u>" in <u>The Strolling Astronomer</u>; January-February, 1955.

2. Ibid.

3. Moore, Patrick; <u>A Guide to the Planets</u>; W. W. Norton & Co., Inc., New York; 1954; p. 62, footnote.

4. Bartlett, J.C.; "<u>The Radial Markings of Venus and Their Modern Resurrection</u>" in <u>The Strolling Astronomer</u>, January-February, 1955; p. 7.

5. Ibid; p. 4.

<u>MODERN SELENOGRAPHY</u>

by Dr. H. P. Wilkins, F.R.A.S.

(Paper read at the A.L.P.O. Convention at Flagstaff on September 1, 1956)

In recent years the study of the moon has attracted the attention of both professional and amateur astronomers. The former have contributed many fine photographs, of which those secured at Mt. Palomar, Mt. Wilson, Lick and Pic-du-Midi Observatories deserve special mention. Photometry and theoretical considerations as to the maximum density of a possible lunar atmosphere, of the origin of the moon and its surface features have occupied many eminent astronomers and physicists. Amateurs have contributed by the determination of positions by measurement of photographs, by ascertaining the heights of peaks or of crater walls from measurement of the length of the shadows under known solar altitudes and above all by painstaking observations of fine detail. Here the A. L. P. O. has done valuable work, particularly as many observations are secured under a high angle of solar illumination. On the other hand British selenographers have generally confined their observations to formations near

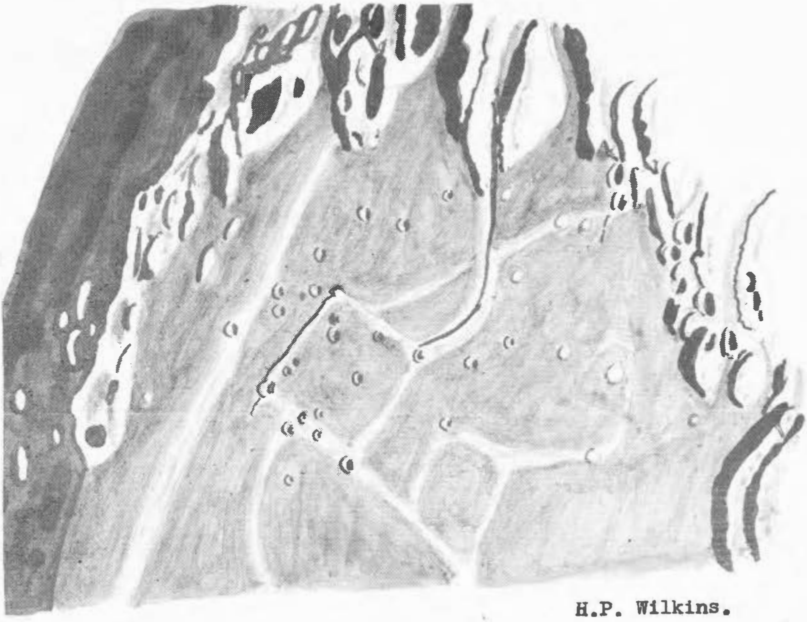

Figure I
Drawing of South Portion of Mare Crisium by H. P. Wilkins. October 5, 1952. 30-inch Reflector. 300X. This drawing shows for the first time, Dr. Wilkins reports, a cleft issuing from the mountains bordering the Mare. This cleft was traced to the "Trapezium."

the terminator, or when the angle of illumination is low.

In addition to these activities several amateurs have assiduously observed the moon for unusual phenomena, such as flashes, possibly indicative of meteoritic impact, glows, relative intensity of shadows, color effects, temporary cloud-like appearances or the study of variable dark areas. A few observers have scanned the neighborhood of the moon in an attempt to detect a possible lunar satellite but, so far, without success. Such a body would be unique in the Solar System, must be very small and, if it exists, will probably be detected by photography. I understand that Clyde Tombaugh favors such a search and everyone will hope that the illustrious discoverer of Pluto will also discover this little body, assuming its existence.

For the finest details visual observation with large apertures is superior to photography and selenography thus comes within the scope of amateurs who possess the necessary instrumental equipment. By this is meant GOOD telescopes of 12 inches aperture and upwards. We are here talking about the most minute details. This is especially true of the numerous fine clefts. It may be of interest to note that the writer has recently completed Part I of an extensive Catalogue of Clefts, 1217 in all. This is the first Cleft Catalogue since that of Schmidt. The great majority of the clefts are shown on the writer's large lunar map, revised edition as published in the recent text-book, The Moon , by Wilkins and Moore.

Unfortunately the work accomplished in one country is often unknown in others and the need has often been felt for some international organization to correlate the work of lunar observers all over the world without, in any way, clashing with existing societies. Such an organization was inaugurated on July 1, 1956, with the title of The International Lunar Society. It is a means by which observers of various nationalities can exchange information and ideas and it will show the kind of work being carried out in different countries.

Officers hold office for two years and will then be replaced by others of different nationalities. There is also a Permanent Secretary. A Journal will be published every 6 months and will contain summaries of the most important items of lunar research, papers and illustrations. Each contributor will write in his own language; summaries will be given in German, English and French.

There will be NO subscription, the only expense being that involved in publishing the Journal. The following have been selected as the first officers:

President - H. P. Wilkins (the writer).
Vice President - Professor W. H. Haas.
General Secretary - Herr G. Roth, Germany.
Permanent Secretary - Prof. A. Paluzíe, Diputacion 337, Barcelona, Spain. (Secretarial Address).

Other well known friends who have enrolled include Dr. Gant, Barcroft, Bartlett and other members of the A.L.P.O.

Those good folk who have small telescopes can still do useful work. There is much still to be learnt about the variable intensity of the interiors of many formations, while near the limb several formations still await adequate mapping. By individual activity, by recording with localised organisations and then by letting the results become world known through the new society, our knowledge of the nearest and, in many ways, the most interesting, of celestial bodies, the nearest to us and the first to be actually reached if and when space-flight becomes a reality, will be greatly increased. But beyond all this there is a fascination and a real pleasure in observing the moon quietly but accurately and studying the fantastic details on this silvery globe.

On Making Lunar Drawings. The drawing of the southern portion of Mare Crisium here published as Figure I is taken as an example. This took forty-five minutes to do, first sketched in with pencil, then shaded with diluted Indian ink, the shadows inserted by ordinary, undiluted ink. The lighter ridges only had one wash in comparison with the three given to the surface of the Mare.

Use a board and paper attached, pencil fastened to the board by a piece of string passing through a hole. Then the pencil will not be lost during the darkness of the night.

Always use as little artificial light as possible; when the moon is past first quarter, the moonlight will often be found sufficient. Insert the prominent detail first, then the finer and finally the most delicate. Check with the original before inking in. Try to keep the correct proportion.

SOME SUGGESTIONS ABOUT WHAT CAN BE DONE ON JUPITER DURING THE 1957 APPARITION

by Henry P. Squyres

Jupiter comes to opposition on March 17 of this year and it is time to begin observing the Giant Planet.

The full disc drawings of Jupiter should be made at least 2 inches in diameter and any sectional drawings showing some particular belt or zone should also be at least 2 inches in diameter. Care should be taken to make the drawings as quickly as possible; otherwise the markings will have shifted so much due to the rapid rotation of Jupiter that the drawings will be worthless.

Central meridian transits are the most valuable work that the average amateur can do on Jupiter. All that has to be done is to estimate visually when a given marking is exactly on the central meridian and to record the time to the nearest minute. The central meridian is an imaginary line running north and south, midway between the east and west limbs. The observer should also give a brief description of the object, such as what belt it is in, the color of object, size of the object, etc. He should also try to secure as many transit observations as possible on the same night. These central meridian transit observations should be sent to Chester J. Smith, our Assistant Jupiter Recorder, at 9775 Burgos Ave., Oakland 5, Calif.

For any drawings of Jupiter, it is very important that the observer computes the central meridians. Because Jupiter does not rotate as a whole, it has been found convenient to use two systems. System I applies to all objects situated on, or between, the north component of the S. Equatorial Belt and the south component of the N. Equatorial Belt. System II applies to all objects situated north of the south component of the North Equatorial Belt or south of the north component of the South Equatorial Belt.

All one needs in computing the C.M. is a current annual volume of the American Ephemeris and Nautical Almanac and the time of the observation. Before the C.M. is computed, the observed time must be converted to Universal Time. This is a 24-hour system beginning with zero hours at midnight, Greenwich, England. Zero hours,which starts the new day, is at 7:00 p.m., E.S.T., 6:00 p.m., C.S.T., 5:00 p.m., M.S.T., and 4:00 p.m., P.S.T. of the previous day

If the drawing was made on March 3, 1957 at 8:15 p.m., P.S.T., the U.T. would be $4^h\ 15^m$ U.T., March 4, 1957. If the drawing was made on March 4, 1957 at 5:45 a.m., P.S.T., the U.T. would be $13^h\ 45^m$ on March 4, 1957. To find the longitude of C.M. I for $13^h\ 45^m$ U.T. on March 4, 1957, you first turn to page 402 of the 1957 Ephemeris. You will find that the longitude of C.M. I is 254.8° at 0^h U.T. March 4, 1957. From Table I you find that $13^h\ 45^m$ is equal to a change in longitude of 142.9° You add the 142.9° to the 254.8° and you get 397.7°. You have to subtract 360° from this answer, because there are only 360° to a complete revolution. The answer comes out to 37.7°. To find the longitude of C.M. II for $13^h\ 45^m$ U.T. on March 4, 1957, you turn to page 403 of the 1957 Ephemeris. You will find that the longitude of C.M.II is 71.2° at 0^h on March 4, 1957. From Table 2 you find that $13^h\ 45^m$ is equal to a change in longitude of 138.6°. The 71.2° is added to the 138.6° and you get an answer of 209.8°.

Amateurs who have filar micrometers can make accurate measurements of the latitudes of different belts and the longitudes of markings not on the central meridian. Observers can also make measurements of the size of individual features like the Red Spot.

Some detail can be seen on the four large moons. It takes a telescope of at least 12" aperture to do much work on these satellites. It also takes high powers of 300X or more, and it requires excellent seeing.

We hope that we can have a much larger amount of work done on Jupiter during this present apparition.

TABLE I

Increase of Longitude of C.M. of Jupiter in Intervals of Mean Time for System I.

Hours	Degrees	Minutes	Degrees
1	36.6°	1	0.6°
2	73.2	2	1.2
3	109.7	3	1.8
4	146.3	4	2.4
5	182.9	5	3.0
6	219.5	10	6.1
7	256.1	20	12.2
8	292.7	30	18.3
9	329.2	40	24.4
10	5.8	50	30.5

TABLE 2

Increase of Longitude of C.M. of Jupiter in Intervals of Mean Time for System II.

Hours	Degrees	Minutes	Degrees
1	36.3°	1	.6°
2	72.5	2	1.2
3	108.8	3	1.8
4	145.1	4	2.4
5	181.3	5	3.0
6	217.6	10	6.0
7	253.8	20	12.1
8	290.1	30	18.1
9	326.4	40	24.2
10	2.6	50	30.2

THE MARSITRON HYPOTHESIS

THE APPEARANCE OF LUCUS LUNAE AND BIBLIS FONS AS EVIDENCE OF A MOBILE SPECIES ON MARS

by Donald Lee Cyr

(Paper read at the A.L.P.O. Convention at Flagstaff on Sept. 1, 1956)

To paraphrase the words of Percival Lowell, "We must learn to wait upon our opportunities and then no less to wait for mankind's acceptance of our results. * * * For in common with most explorers, we will encounter upon our return, the final penalty of penetration, the certainty at first of being disbelieved." (1). The scientific world has waited a long time to accept the observations of Lowell as being factual. Skepticism and outright disbelief are still common regarding observations of Mars, but nevertheless, the opportunity exists to vindicate the observations of Lowell. Let us now make the most of this opportunity.

The hypothesis about to be presented is a simple one, and one whose parts have been presented before by others, in some instances. The hypothesis consists of three main parts: first, an explanation is presented for the oases of Mars; second, an explanation is presented for the canals of Mars; and third, some unique evidence is presented regarding the existence of a mobile species on Mars.

The oases of Mars are those tiny round dots which were first seen at the intersections of canals. The oases were discovered by W. H. Pickering (2) who gave correct explanation of their nature, namely that they are craters. The fact that the oases are distributed at random over the surface of Mars, and the fact that the oases are circular and vary in diameter, indeed suggest that the oases are craters. That these craters resulted from the impact of meteorites, comets, or asteroids has been explained elsewhere (3).

The evidence for impact-craters follows the fact that the oases are darker than the surrounding areas, regardless of whether the oases occur in the Martian desert or in the blue-green regions. That the blue-green areas are vegetation will be assumed at this point. An impact-crater forms a basin below the surface of a planet, and as a basin provides a favorable environment for vegetation. The fact that the oases are darker argues that vegetation is more luxuriant therein. Crater basins provide a sheltering environment from wind, and probably provide a warmer environment than the normal surface of Mars. Since some of the oases are a hundred miles in diameter, a comparison can be readily made with sheltered valleys on the earth. For example, Imperial Valley is below sea level and is noted for having a mild climate.

If the observations of Mars allow us to estimate the diameter of the crater-oases, it then becomes possible to estimate their depth. Statistically, all craters, whether caused by impacting meteorites, by impacting bombs, or by explosion, are related. The

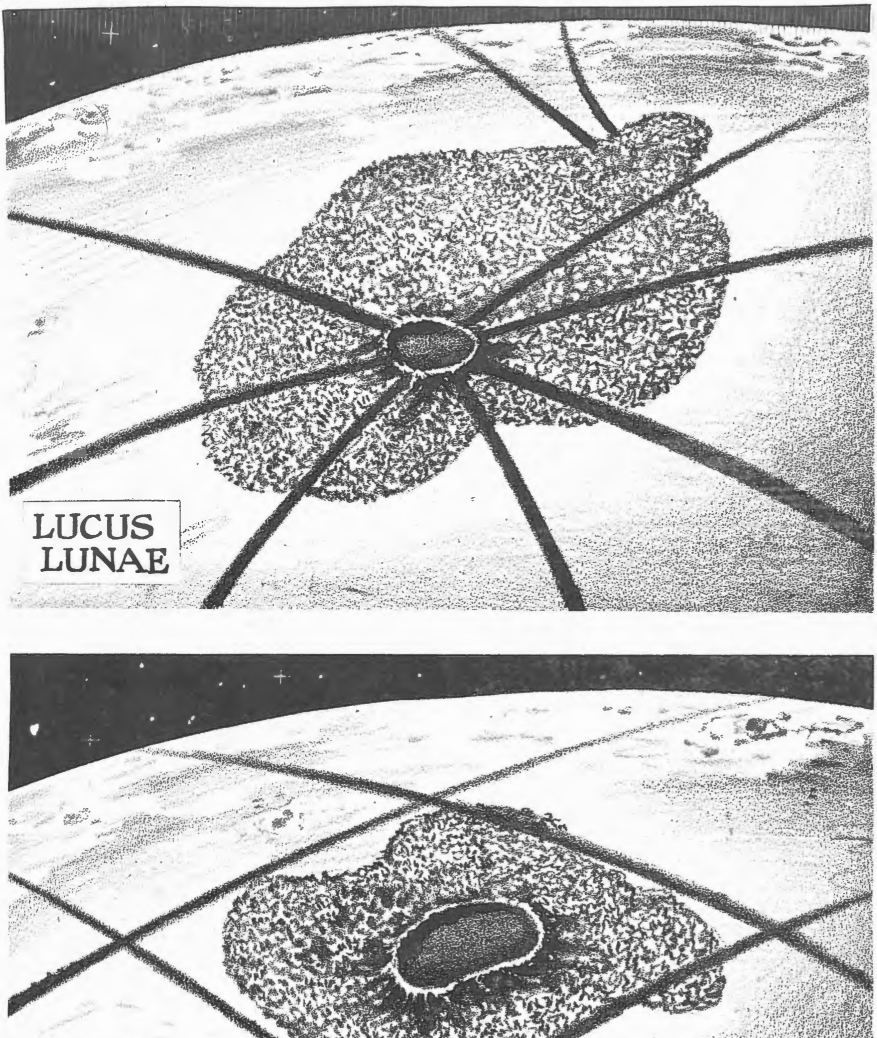

Figure 2.
Appearance of Lucus Lunae and Biblis Fons on Mars, according to Percival Lowell.
Refer to Article by Donald Lee Cyr in this Issue.

relationship of diameter to depth can be plotted as a single curve applicable to lunar craters, and explosion and bomb craters (4). By simple deduction, it seems reasonable to state that the Martian craters also follow this statistical curve. Neglecting erosion for the moment, the Martian craters may have once had depths as much as three miles in the instance of Pseboas and Ascraeus Lucus. Biblis Fons and Lucus Lunae, under similar circumstances, may originally have been over two miles deep. Admittedly, the wind-blown dust of Mars must have partially filled these craters during the eons of Martian history. If the accumulated wind-blown sand and dust deposits are a mile deep, the present depth of Biblis Fons and of Lucus Lunae might still be appreciable.

Now let us consider the explanation of the canals themselves. A number of persons have independently suggested that the canals are migratory paths which connect the oases (5). The existence of such paths would argue for the existence of a mobile species on Mars, but would not necessarily imply a highly intelligent species. The migratory path would be visible because of the effect of fertilizer dropped by the migratory species. The fertility-path idea seems to be a very worth-while approach to the problem of the markings on Mars.

The fertility-path idea first occurred to me some years ago when I saw the old abandoned wagon trails near Las Cruces, New Mexico. When the summer rains come in August, suddenly brilliant green trails appear across the desert. During the rest of the year, the paths are invisible. Apparently, the trails owe their sudden appearance, not only to the rain, but also to the fertility still lingering as a result of the passing horses and oxen of many years ago. The same principle may well be operative on the planet Mars.

The fact that trails of fertility exist across the otherwise barren desert of Mars indicates that some factor in the ecology of the "canal" causes plants to grow well there. Lowell thought this ecological factor was water. The fertility interpretation suggests that the factor may be nitrogen compounds, phosphorus or potassium minerals, or other substances, either singly or as a complex. Even the Ph factor, a simple acidity-alkalinity relationship, could be the determining factor. The suggestion is made that this fertility factor on Mars arises through the digestive process of an unknown species of MARSITRONS.

Without knowing the exact life processes on Mars, it is a reasonable assumption, by earth standards, that any vegetal species must coexist with an alternate "animal" species. The assignment of the animal species, ecologically speaking, is to reduce the energy impounded by the plants. This unknown animal species of MARSITRONS is therefore suggested to return the plant material to a primal state. The plants growing on Mars, be they lichen, or an unknown species, represent in the ecology of that planet, a part of the life cycle. Those who would understand the markings on the planet Mars would be well-advised to study carefully the principles of ecology. For with the fertility interpretation, a new break-through is given to the study of Mars.

The intensity of the vegetation therefore becomes not only a measure of available fertility, but is a measure of both past and future grazing potential. The MARSITRONS may be expected to populate those areas where vegetation is most intense. Furthermore, the plants are most apt to grow where the MARSITRON'S fertility elements have been deposited. Thus the vegetation of Mars indicates a "locus" of points visited by MARSITRONS.

A similar condition is known to occur on the earth, and many Oriental cities are said to be surrounded by a ring of green fertility. The distribution of fertilizer by "honey-pots", as they are jokingly described, is well-known to service men. The resulting green belt extends around a city, half the distance the "honey-pot" merchant can travel in a day.

The unimaginative investigator will be shocked to learn that there are at least two clear examples of green belts surrounding craters on Mars. Lowell's maps of many years ago (1), as well as the more recent map of the Association of Lunar and Planetary Observers (6), show green belts around Lucus Lunae and Biblis Fons.

The MARSITRON hypothesis explains the green belts surrounding the craters as being a statistical measure of the area travelled by MARSITRONS while grazing near a crater. As previously explained, a crater basin provides a wind-sheltering, warm environment. Even a stupid MARSITRON would be bright enough to return to the comparative warmth of the crater at nightfall. Since lazy MARSITRONS would perhaps rather stay within the crater even during the day, the assumption can be made that population pressure would motivate the MARSITRON to graze beyond the crater rim for self-preservation.

Since the days on Mars are roughly equivalent to our own, the MARSITRON may be imagined to have a commuter's schedule. He probably starts out at dawn, rushes across the floor of the crater, climbs the crater wall, and then grazes from plant to plant. The MARSITRONS travel about 185 miles (2 radii) during about 12 hours. The 185 mile round trip is the approximate diameter of the green belts of Lucus Lunae and Biblis Fons. During the summer season, the available daylight hours increase, and the MARSITRONS might make proportionally longer trips around the craters in higher latitudes.

The speed that the MARSITRON is capable of maintaining is an interesting, though speculative, question. In the example given, the 12 hour journey of 185 miles is equal to 15.4 miles per hour. Since the MARSITRON is likely to spend part of the time in fast travel, and part in slow grazing time, a number of solutions are evident:

The 15.4 MPH rate would allow no free grazing time.
A 30.8 MPH rate would allow 6 hours of grazing time.
A 46.2 MPH rate would allow an 8 hour shift of grazing time.

Is such a speed reasonable? On the earth, antelopes can achieve speeds in excess of 60 miles per hour. Furthermore, the condition of gravity on Mars means that each leap of equal force would propel a MARSITRON over a greater distance than would be the case for his antelope cousin.

Be that as it may, I am confident that the MARSITRON hypothesis, when developed in detail, will provide interesting explanations for many of the strange markings on Mars. The MARSITRON can then take his place in the world of science, along with other hypothetical particles such as the electron, the positron, and the neutrino. Electrons and positrons have never been "seen" and are only evident because of the "fertility" trail they leave in a Wilson cloud chamber. The MARSITRON is very much like the neutrino, which just has to exist, to make things come out right. In any case, I venture to predict that many of us will be convinced of the existence of the MARSITRON long before we have seen one.

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(4) R. B. Baldwin, THE FACE OF THE MOON, University of Chicago Press, (1949).

(5) Willy Ley and Wernher Von Braun, THE EXPLORATION OF MARS, The Viking Press, (1956). Both Elihu Thompson and Hermann Ganswindt are mentioned as having suggested that the canals are routes of animal migration.

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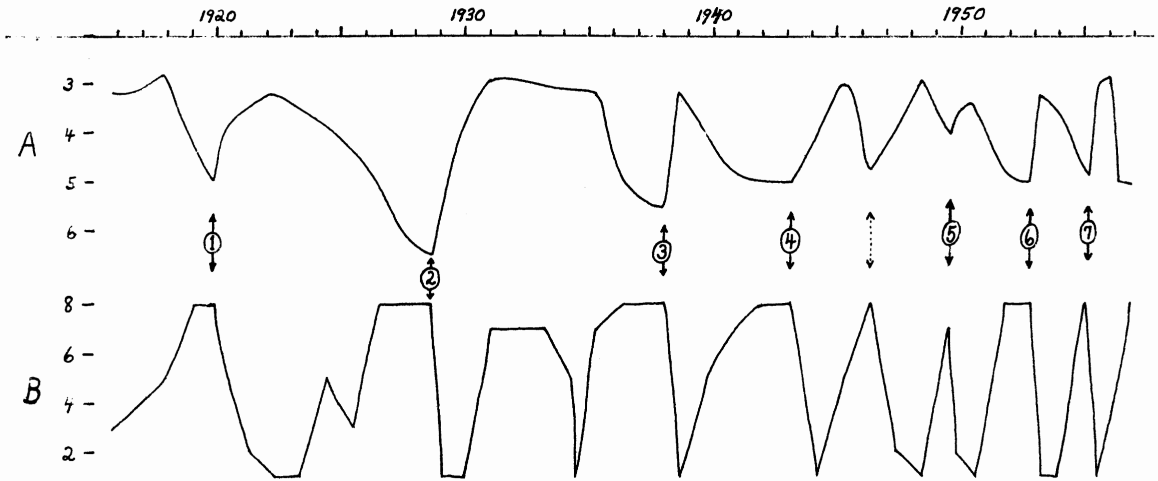

Figure 3.

Graphical Representation of an Apparent Relation Between the Intensity of the Red Spot on Jupiter, the Intensity of the South Equatorial Belt, and the Occurrence of Major South Equatorial Belt Disturbances. Graph Constructed and Contributed by Elmer J. Reese.

A NOTE ON THE S.E.B. DISTURBANCES OF JUPITER

Mr. Elmer J. Reese, R.D. 2, Box 396, Uniontown, Penna. has contributed an interesting and perhaps very important note about certain Jovian surface features. We should mention that Mr. Reese is probably our ace observer of Jupiter and has been a most persevering student of the Giant Planet for almost 20 years. Among the phenomena of the visible surface of Jupiter have been seven (so far) major South Equatorial Belt Disturbances. Each one begins with one or two very dark spots in the South Equatorial Belt and advances as a wave of darkening in the direction of decreasing longitude at a rate of several degrees per day. New spots or streaks meantime continue to appear at the longitude of the initial outbreak. Mr. Reese has found that the longitudes of all initial S.E.B. outbursts of this kind can be related to the longitudes of two hypothetical subsurface "volcanoes", each one having a constant rotation-period of 9 hrs., 55 mins., 42.66 seconds. (The term "volcano" should probably not be taken too literally here; we have in mind two sources of activity below the visible surface of the Giant Planet and capable of affecting the surface markings.) Readers further interested in S.E.B. Disturbances might refer to "A Possible Clue to the Rotation Period of the Solid Nucleus of Jupiter", Journal of the British Astronomical Association, Vol. 63, page 219, 1953 and to "Major S.E.B. Disturbances on Jupiter and an Apparent Clue to the True Rotation of the Giant Planet", The Strolling Astronomer, Vol. 9, page 64, 1955.

Figure 3 presents in graphical form Mr. Reese's present studies. Graph A is variations in the mean intensity of the South Equatorial Belt, where the smaller intensity numbers represent the greater darkness of this belt. The data for Graph A come from publications of the B.A.A. and the A.L.P.O. Graph B is variations in the aspect of the Red Spot region, where the larger numbers represent the greater darkness of the Red Spot. The scale for Graph B is explained fully in an article in The Strolling Astronomer, Vol. 7, page 88, 1953. Finally, the date of the initial outbreak of each major S.E.B. Disturbance is indicated on Figure 3. It will be noticed that the intensities of the S.E.B. and the Red Spot are inversely related - when the Spot is darkest, the belt is faintest, and conversely. It will further be noted that each S.E.B. Disturbance began when the intensity of the S.E.B. was at a minimum and the intensity of the Red Spot was simultaneously at a maximum. In fact, every minimum in the intensity of the S.E.B. since 1915 has been accompanied by a Disturbance with the single exception of the minimum in 1946. (Of course, a complete theory of this apparent relation among the Jovian features must explain this single exception as well as the seven conformances to the rule). It is finally of the greatest interest that the current 1956-7 apparition of Jupiter again finds the Red Spot near maximum intensity and the S.E.B. near minimum intensity. On this basis Mr. Reese in the autumn of 1956 tentatively predicted another imminent major S.E.B. Disturbance. It will presumably begin with one or two intensely dark spots in the South Equatorial Belt, or perhaps with a dark column connecting the two components of this belt. Therefore, observers of Jupiter should watch keenly for such features whenever they look at the Giant Planet; and if they do see any very dark S.E.B. spots or streaks, they should make every effort to record their longitudes. As far as the Editor knows, nothing has yet been seen of another major S.E.B. Disturbance in late February, 1957 - but everyone should be alert for such an event during the spring and early summer.

Mr. Reese suggests that his volcanic explanation of S.E.B. Disturbances (see above) is weakened by the correlations indicated in Figure 3, for it is difficult to see why the volcano should choose to be active only when certain Jovian atmospheric features assume a given aspect. He then says: "Sadao Murayama (Publications of the Astronomical Society of Japan, Vol. 1, No. 3, pp. 69-72) has called attention to the fact that the S.E.B. was always dark and wide prior to 1919 (the year of the first S.E.B. disturbance) and unstable thereafter. I suggest that continuous volcanic activity maintained the dark aspect of the S.E.B. prior to 1918. Since the volcanic vent was open during those years, no unusually great internal pressure could build up. However, beginning in about 1918, the volcanic vent became sealed from time to time. When this happened, the S.E.B. would fade away while internal pressure would mount. When a certain critical pressure was attained, an explosion would reopen the vent and a disturbance in the S.E.B. would be the visible result."

BOOK REVIEWS

Ancient Education, by William A. Smith. Philosophical Library, New York, 1955. 309 pages. ...$3.75

Reviewed by Charles A. Haas

The author has treated the subject of education in ancient times in a very systematic manner. He has clearly done much research. He discusses various ancient cultures and shows how in their development they gained from the progress of other nations. At the end of each chapter are references to source material for the subject under discussion. The book is written in an interesting manner, and each chapter can be read separately. The author discusses the ancient peoples of the earth from the dawn of history to the time of the Hebrews. He shows what each culture gained from the nations with which it came into contact by commerce or by war. He traces what each nation produced in the fields of education and of religion. Trade made records essential, and thus writing became necessary. Literature was also produced as Man progressed and as his moral and religious life developed. Being primarily concerned with education, this book relegates military events to secondary importance.

The person interested in education in ancient times will find this book of great value. Students and teachers will benefit from reading it. It shows how commerce, morality, religion, and government progressed side by side in ancient times - as they still do in modern times.

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Flight Handbook, Fifth Edition. Edited by Maurice A. Smith. Philosophical Library, New York, 1955. 282 pages. 217 photographs and drawings in text. 12 illustrations inset. ...$6.00.

Reviewed by Robert McIntyre

Flight Handbook is an outstanding descriptive treatment of the environment, engineering principles, and real and potential developments associated with all types of aircraft. Subject matter includes the atmosphere, classic and supersonic aerodynamics, aircraft structures, reciprocating and gas-turbine power plants, aircraft instruments and armament, navigation systems, and a particularly detailed discussion of the most recent propulsion advances.

Although the reader is assumed to have some degree of technical training, the book is written in semi-technical style with an abundance of explanatory illustrations. This is so well done that Flight Handbook has been made extremely informative for persons not having a scientific or aviation background without detracting from the interest or value to the highly trained.

It is obvious that Flight Handbook was written to meet a specific requirement - the requirement for an up-to-date, general aviation reference. It is equally obvious that as much effort was expended in the selection of topics as was probably expended in the actual writing. Invariably, the general descriptions are amplified with additional details at just the times that the reader wants them.

Flight Handbook is highly recommended for all persons with even a passing interest in aviation and should be considered essential to the personal libraries of all aviators. Its value to the aircraft engineer and professional aviator, in lieu of numerous specialized texts with their often unwanted detail, cannot be over-emphasized.

VISUAL OBSERVATIONS OF COMET AREND-ROLAND 1956h AND OTHERS

by David Meisel

Bright comets are a rare occurrence in the life of the amateur astronomer. This means that when such an object is discovered an effort should be made to carry out reliable observations of it. Most comets remain very faint objects of little interest to the planetary astronomer, except of course when the astronomer discovers them himself. This is due to the popular misconception that only professional astronomers with large telescopes can make useful observations of comets and the physical changes that occur in them. This is not necessarily true as our British colleagues have been very active in this respect! It is the author's hope to instill some of the British enthusiasm in American amateurs.

The visual observation of all comets is a desirable and necessary supplement to photography since minute details and rapid changes usually cannot be recorded with satisfaction on a photographic plate. Observations of rapid changes should be reported (if the observer is quite sure that the change is objective and not just a change in observing conditions) to other observers or to the nearest observatory. In the United States, however, it is the seemingly general practice of most observatories to ignore an amateur's report of changes in a comet due to a doubt as to the amateur's reliability. It remains, therefore, for amateurs in this country to combine their efforts both photographically and visually in order to prevent wasted efforts and lost time.

To locate a comet one must first have an ephemeris. However, because the mathematics involved in the computation of a comet's orbit from initial observations and then the conversion of the elements of the orbit to Right Ascension and Declination are beyond the abilities of many amateur observers, they will be omitted from this article. Those interested in orbit computation should refer to <u>Observational Astronomy for Amateurs</u> by Sidgwick and <u>Determination of Orbits of Comets and Asteroids</u> by R. T. Crawford. Search ephemerides for periodic comets due to return in 1957 may be found in the <u>British Astronomical Association Handbook for 1957</u>. Ephemerides for new comets are given in the BAA <u>Circulars</u>, which are issued from time to time containing information on new discoveries. The <u>Harvard Announcement Cards</u> contain, though less frequently, an ephemeris for a new or rediscovered comet. The positions from the <u>Circulars</u> may be directly used by observers possessing 'scopes equipped with setting circles without plotting on a chart. Those whose 'scopes do not have circles have to plot the path on an atlas. Two atlases are usually needed, one like the Skalnate Pleso with magnitudes down to 7.5, and one like Webb's Atlas, that goes down to 9th magnitude. With a little practice, using nebulae instead of comets, objects may be located with relative ease.

After the comet has been located record the name of the comet, both with the proper name and serial number, as "Comet Mrkos 1956b". Then record the date in Universal Time and decimals of a day. When the preliminary data have been recorded the observer is ready to begin his observations. Accurate timing is required for measures of position and records of sudden or rapid changes. These may be obtained from WWV time signals or Western Union. An accuracy of one-tenth of a minute is usually sufficient.

The next step is to determine the comet's position. This may be done by making a field sketch showing its position relative to the surrounding stars or by reading the position from circles. This much is sufficient in most cases of well-observed comets since the accuracy of the drawing is not superior to the position obtained from photographs. If the amateur desires accurate positions for orbit calculation of a new comet, the time difference between the transit of the comet and that of a star of known position may be used to obtain the Right Ascension of the comet, and by using the field diameter of the eyepiece the Declination may be derived. The field sketch may later be compared to an atlas and then determination of the position may be done without loss of observing time. (Be sure to include scale and orientation as well as location of each field sketch.)

Next record the observing conditions noting such things as image steadiness, limiting stellar magnitude at zenith, altitude of comet above horizon, atmospheric transparency, moonlight, and anything else necessary for correct interpretation of the included observational data.

After the positions are determined the observer is ready to make photometric observations, that is, estimations of the total stellar magnitude of the comet. Up to the present time the magnitude of comets is not well defined in terms of uniform scientific laws or standards. This same difficulty arises in the study of nebulae and clusters. This is because the images of these objects are extended, not points as are the stars, and their boundaries are ill-defined. The easiest method is to compare the image of the comet to that of a star out of focus. The observer should put on a low power eyepiece and should locate several stars near the approximate stellar magnitude of the comet in the field. Assign an arbitrary magnitude to the star that is the brightest. Then, using this star, estimate the magnitude of the faintest star. Lastly, use these two stars as the extremes and estimate the magnitude of the third star or of any additional stars which the observer may want to use. Indicate these magnitudes in the sketch and state whether they are true or arbitrary magnitudes. Then rack out the eyepiece until the stars' disks are the same size as the comet's focused image. Then choose the star nearest the brightness of the focused comet, and estimate the brightness of the comet to one-tenth of a magnitude. The star's true magnitude may be worked out at a later time and corrections applied when necessary. As in the observation of variable stars, this process requires much practice and patience.

The accuracy of the derived magnitude of a comet depends heavily on many factors, such as conditions of the sky, nearness of comparison stars, angular expanse of comet and color of comparison stars. Thus it may be seen that the accuracy of a given observation at best will usually be within one-third magnitude. The factors affecting the consistency of the resulting magnitudes such as magnification employed, aperture of instrument and type used, and the type of sky background should be noted in the report.

The most interesting aspect of comet observation is the study of a comet's structure. This may be done even with modest equipment, as the limiting magnitude for comets, under ideal conditions, is about two magnitudes fainter than the stellar limit. At least three drawings of a comet should be made each evening or each observing period. A drawing at low power should show the fainter aspects and details such as tails, halos, and envelopes. A drawing at medium power should show general details of the coma jets and central condensation. A drawing at high power should show minute details in the coma, central condensation, and nucleus if one is present. All sketches should include the Universal Time, orientation of the field with respect to north or south, and the scale in order to determine the size of the images.

In the report of a comet the nomenclature given below should be used and a written description of the features given.

The Head:

(a) Coma (1) (The main part of the comet, usually oval or round, from which the tail is formed): size and shape (variations in these from each observation to the next or hour to hour variation); (2) hoods or halos (if any): diameters, development, position angle (from north to east as $0^{\circ} - 90^{\circ}$) of the major axis (if ellipical), whether the halos contain dark interspaces; (3) jets or appendages (if any): length, p.a. (of tangent to the jet at the nucleus, if curved), and/or distance and p.a. of several points on a curved jet.

(b) Central condensation of the coma (if any): size and shape; if elliptical, p.a. of the major axis.

(c) Nucleus (not to be confused with the central condensation; the nucleus is typically either a small - few seconds arc diameter - uniform disk with a sharp boundary, or a star-like point): single or multiple; if multiple, the relative positions of the components within the coma; disk or stellar; size of disk; sharp or diffuse.

Tails:

(a) Anomalous tails (i.e. directed towards the Sun): length, p.a. or co-ordinates, curvature.

(b) Main Tail. Length and position (p.a. and distance, or co-ordinates, of points along its axis); any distortions or divisions; condensations; position measures or estimation in relation to the coma as often as possible; any fluctuations of light (points or areas) moving along the tail - probably physiological.

The magnitudes and texture of these features should also be noted if the experience of the observer allows. The magnitudes of the parts may be compared to the brightness of nebulae under similar conditions. A better method is to divide the coma and tail into zones of equal intensity and estimate relative intensity, then convert to stellar magnitudes by adopting an intensity standard of one for the brightest portion of the coma and the faintest portion as zero, and all other zones as fractions between these two numbers. Then, the magnitude of the faintest portion must be estimated by the extra-focal method. When the two magnitudes are known, the intensity is multiplied by the difference between the magnitude values. The product of this is added to the value of the brightest portion in magnitude to obtain the true magnitude of the portion to which the particular intensity value was assigned.

A convenient form for routine comet observations has been developed by the British Astronomical Association Comet Section. The following data being required, a separate sheet (4 x 6 cards) in ink is devoted to the observations of each comet:

(a) Date and time (UT), or date and first decimal.
(b) Place of observation.
(c) Approximate R.A. and Dec.
(d) Estimated stellar magnitude.
(e) Diameter of coma (minutes of arc) and other descriptive data, including degree of condensation of coma:
    0-No condensation
    9-Sharply condensed at center.
(f) Aperture and magnification.
(g) Observational conditions: sky, moon, etc. Scale adopted for describing atmospheric transparency:
    0 Hopeless (mist, fog, etc.),
    5 Average,
    6 Milky Way pretty clear,
    9 Perfect.
(h) If field sketch is added: location, orientation, scale (on the reverse of the card).

An opportunity to employ the above methods of observation may be had during the coming months when a new and bright comet 1956h passes perihelion and also comes within approximately sixty million miles of the earth. (See Figure 4.)

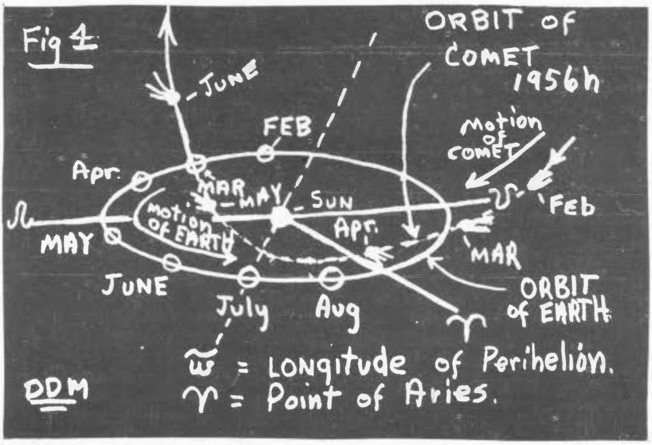

Figure 4
Sketch of Orbit of Comet 1956h.

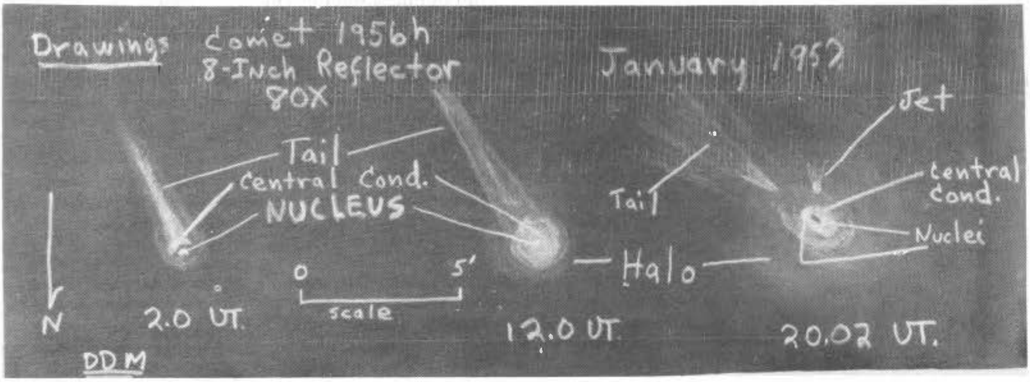

Figure 5

Drawings of Comet 1956h by David Meisel on January 2, 12, and 20, 1957.

Comet 1956h was discovered on 1956, Nov. 8.9 U.T. by Dr. S. Arend. At this time the comet was in Triangulum with a magnitude of about 10. Since that date the comet has moved into Pisces increasing in magnitude (though unsteadily) to 8.5 on 1957, Jan. 25. The comet is now (Jan. 25) about 2' of arc in diameter with a tail 5' or 6' of arc long in p.a. 50°.

Personal observations (see Figure 5) indicate much change in the form and magnitude of this comet. The observations were made with modest equipment (8-inch reflector) and the observing site is not as ideal as that of some other astronomers. However, it may be seen that useful as well as interesting observations may be made by amateurs. The magnitude of this comet does not follow exactly the "inverse fourth law", which is generally used as a rough estimate for comet magnitude when the object is moving in a parabolic orbit. The "inverse fourth law" states that the magnitude of a comet varies inversely as the product of the square of the distance from the earth, in astronomical units and the square of the distance of the comet from the sun, in astronomical units. Further variations from the derived formula given below should be noted as early observations indicate deviation by as much as two magnitudes.

Daily changes in coma appearance have also been noted. Accurate observations of these changes would contribute to knowledge of the physical nature of comets in general. Photographs are also valuable if the observer is equipped to make them. (See Reference 1, page 236).

Total magnitude $= 4.5 + 10 \log r + 5 \log p$,
where 4.5 is a constant whose value varies with each comet, r is the radius vector or heliocentric distance, and p or Δ is the geocentric distance. (Both distances are given in astronomical units.)

The writer's equipment is an eight-inch reflector mounted equatorially with no circles or drive. Orthoscopic and Erfle eyepieces giving magnifications of 40X, 80X, and 210X are used with the 'scope. The observing site is in the city limits on the side of a hill. Two street lights are located just 300 feet from the site. As the reader can see, the site is far from ideal, thus making photography impossible. So the writer has to be content with making visual observations comparable in accuracy to photography . The methods employed are the ones found most suitable to the above conditions and equipment. It would seem, therefore, that observers with much better equipment and conditions could make extremely valuable observations of 1956h and other comets.

Below are the elements of the orbit of Comet Arend-Roland 1956h:

(a) The time of perihelion: T = 1957, April 8.4281 U.T.
(b) The longitude of perihelion measured in the plane of the comet's orbit from the ascending node: $\omega = 308^{\circ}.5680$.

(c) The longitude of the ascending node from the first point of Aries: $\Omega = 215^{\circ}.0743$.
(d) The inclination of the comet's orbit to the plane of the ecliptic (if greater than 90°, the motion is retrogade): $i = 120^{\circ}.2745$.
(e) Distance of perihelion: $q = 0.3192153$ A.U.

The above elements were computed by Dr. G. Merton, director of the Comet Section of the B.A.A. The equinox used is 1950.

The following ephemeris was computed by Dr. Merton using his own elements:

Epoch 1957.0

	Date 0^h U.T.		R.A.	Dec.	ρ	r	Mag.
1957	March	3	$0^h\ 26^m.3$	$-6°\ 09'$	1.823 A.U.	0.988 A.U.	5.7
		13	0 31.3	−8 25			
		23	0 36.1	−11 02	1.482	0.567	2.9
	April	2	0 40.3	−13 03			
		12	0 54.6	−4 24	0.745	0.337	−0.9
		22	2 01.3	+31 25			
	May	2	4 06.9	+57 18	0.756	0.720	2.5
		12	6 02.9	+63 02			
		22	7 15.0	+62 57	1.360	1.129	5.7
	June	1	8 00.3	+61 48			

The writer is interested in promoting comet observation in the United States and would be interested in receiving any communications or observations of interested persons. Write to this address:
David Meisel, 800 Eighth Street, Fairmont, West Virginia.

An extended ephemeris will be available after the first of April and may be obtained by request from the writer.

Note: In order to see the fainter parts of a comet the observer's eye must have at least 20 minutes of dark adaptation and averted vision must be employed. The writer uses the method of Mr. G.E.D. Alcock as quoted from the B.A.A. Journal, April 1956, page 160:

"When a known comet is observed it is necessary to draw the field for identification of comparison stars in estimating the comet's brightness. Generally when drawing the field I begin by noting stars placed at right angle to each other and then selecting those conveniently placed in the figures of isosceles triangles, and afterwards completing the sketch of the field with fainter stars.

"The comet is always drawn last, never first, since I find it impossible to put stars accurately around a drawing of a comet; and the tails if any are drawn in relation to the stars in the fields.

"This work can be done while the eyes are becoming dark adapted. A faint tail can be detected by moving the field slightly or shaking the telescope a little. Once the tail is seen in this way, it remains a much easier object.

"To assist in getting the eyes dark adapted, I have used dark sun-glasses but I prefer to use a coat over my head and watch a small circle light coming through the gap at my feet. After waiting for several minutes I have a quick look at the comet and then retire again under the coat. The process is repeated until I am satisfied I can see no more than I have drawn already. So an observation of a comet may take well over an hour."

References

1. Observational Astronomy for Amateurs - J. B. Sidgwick, Macmillan and Company. Pages 234 to 256.

2. The BAA Handbook for 1957. (Periodic Comet Ephemerides).

3. The BAA Journal, April 1956.
Pages 156 to 163. Report of General Meeting.

4. The BAA Circulars. Nos. 379, 380, and 381.

5. Webb's Atlas - May be obtained from H. B. Webb, 145 President Street, Lynbrook, Long Island, New York.

6. The British Astronomical Association, Its Nature, Aims and Methods - 1948 Edition. Pages 30 to 33 and page 47.

7. Determination of Orbits of Comets and Asteroids. R. T. Crawford, McGraw-Hill, New York, 1930.

A COMING OCCULTATION OF A STAR BY SATURN AND ITS RINGS

A rare and exciting astronomical event is in store for planetary observers in North America, South America, and Australasia on April 28, 1957; the eighth magnitude star BD-20°4568 will be occulted by Saturn and its rings. In the United States the phenomenon will occur during the morning hours of Sunday, April 28, local civil time date. Pertinent data on the occultation at various stations are given on pg. 123; these are taken from pg. 17 of the 1957 Handbook of the British Astronomical Association. The diagrams on pg. 123 were drawn by Mr. Bradford A. Smith, a member of Mr. Clyde Tombaugh's Satellite Search Group. The star being occulted is of spectral class K5. Since Saturn is in retrograde motion on April 28, the apparent motion of the star is from west to east, from left to right in the diagram.

This rare passage of a star through the rings gives us the opportunity to attempt several investigations:

1. The amount by which the light of the star is dimmed by the rings may give us information about the density of the rings and even about their total mass. The star may well be dimmed much more by Ring B than by Ring A, since B is brighter than A, and may be dimmed by different amounts in different parts of each of these rings.

2. As the star crosses various divisions in Rings A and B, it may be expected to regain at least part of its normal brightness, all of it if the division is a true space-gap but only some of its ordinary light if the division is merely a place where the ring-particles are less dense. Exact times of such brightenings should give exact information about the positions of the divisions causing them, perhaps more precise than direct micrometrical measures can supply. Careful observations may also check on such matters as the doubling of Encke's Division occasionally recorded by Lowell, Tombaugh, and Capen, among others.

3. Since the star must pass through Ring D, the suspected outer Crape Ring, both before and after its occultation by the bright rings, we may look for a dimming by Ring D as an independent test of the existence of this ring.

This occultation is probably best studied by a combination of methods, visual, photographic, and photoelectric. Thus the search for Ring D will presumably demand the most precise photoelectric photometry. On the other hand, an attentive visual observer may have the best chance of detecting narrow divisions in the rings because the corresponding brightenings of the star will be very brief. Extremely close attention is here necessary, and an accurate clock-drive is a must; otherwise, there is great risk that some divisions will be missed because of breaks in the view. From the point of view of photographic and photoelectric photometry, it is rather unfortunate that the star is so red - K5. We urge each of our readers to think about how his equipment may best be adapted to the occultation - after all, we are not dealing with an everyday event!

Figure 7 on pg. 123 shows the configuration of five of the satellites at 8^h, U.T., on April 28. Titan and Rhea will be almost stationary throughout the whole occultation; but the three other satellites shown will move over appreciable arcs in their orbits. The motion will be counterclockwise around Saturn. It is our hope that

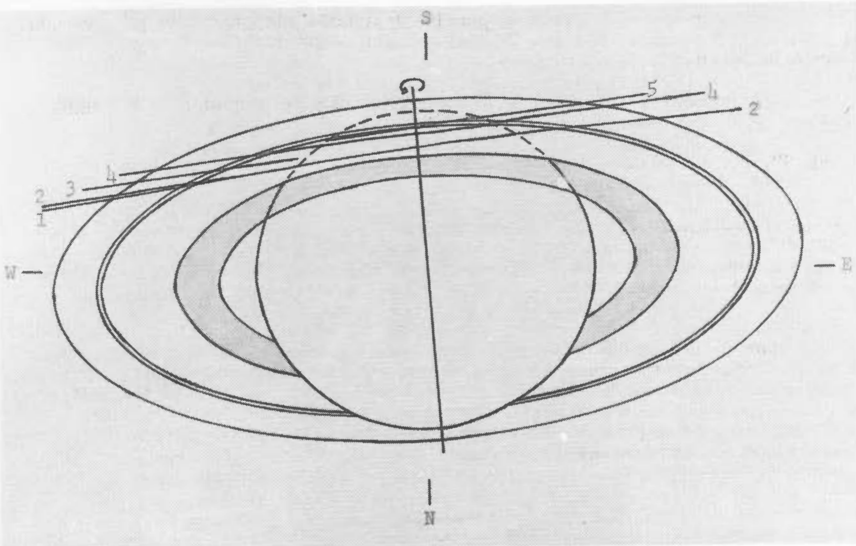

Figure 6
Occultation of Star BD-20° 4568 ($8^m_.0$) by Saturn and Its Rings on April 28, 1957.
Drawn by Bradford A. Smith.
(The numbers in Figure 6 refer to the Stations listed in the following Table).

Table of Data from 1957 Handbook of the B. A. A.

No.	Station	Disappearance						Reappearance					
		Outer Ring			W.Limb			E. Limb			Outer Ring		
		U.T.		P	U.T.		P	U.T.		P	U.T.		P
		h	m	o	h	m	o	h	m	o	h	m	o
1	Montreal	07	58	257	09	17	232		Low			Sun	
1	Washington	07	58	257	09	18	232	10	46	145		Sun	
2	Lick	08	07	256	09	22	232	10	50	146	11	44	122
4	Rarotonga	08	24	250	09	34	220	10	44	154	11	29	132
5	Wellington		Low		09	40	217	10	37	162	11	21	136
5	Sydney		Low			Low		10	38	162	11	19	136
3	Rio de Janeiro	08	07	253	–	–		–	–			Sun	

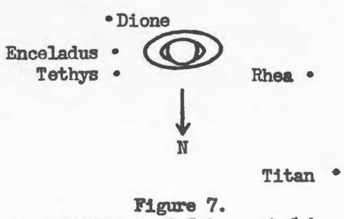

Figure 7.
Positions of Five of the Satellites of Saturn at 8 hrs., U.T., on April 28,1957.
Drawn by Bradford A. Smith.

these satellites may be of some help in determining how much the light of the star is attenuated by passing through the rings, though it is realized that very large systematic errors will attend comparisons of the star seen through the rings with the satellites. It may be more hopeful to try to use the satellites to check on the amount of brightenings of the star as it crosses various divisions in the rings. The mean opposition stellar magnitudes of the five satellites shown in Figure 7 are: Titan 8.3, Rhea 10.0, Tethys 10.5, Dione 10.7, and Enceladus 11.6.

Several things of interest may be noted in Figure 6. The star will not pass through Ring C, the Crape Ring, at all, and in fact will never be seen much inside the middle of Ring B. It will probably be difficult indeed to observe the occultation disappearance and the later reappearance at the limb of Saturn itself. At Rarotonga, however, if we may trust our figure this far, the star will apparently emerge from occultation in Cassini's Division and hence probably with its full brightness.

We quote below part of a communication received from our Saturn Recorder just as we go to press. All observations obtained should be reported promptly after the event to Mr. Thomas Cragg, 246 W. Beach Ave., Inglewood 3, California. Good seeing!

"Such an event was observed and reported in 1917 by Capt. Ainslie and Mr. Knight of the B.A.A.[1] The star observed by Capt. Ainslie and Mr. Knight was about 7th magnitude and was much farther out in the ring system than the coming one. Capt. Ainslie observed a short lived brightening of the star when it was seen through Encke's Division and 'one exterior to same', though the star didn't regain full brightness. However, it was apparently close to full brightness when seen through Cassini's Division. Mr. Knight had considerable difficulty seeing the star through Ring A with a 5-inch refractor. From the above, it is rather obvious that observers wishing to participate in observing this phenomenon should employ the largest telescope available as some doubt exists whether an 8th magnitude star can be seen through the outer part of Ring B.

"It will also be of great interest for observers to indicate where on the ring system the star was first seen in contact with the ring and where it was last seen in contact with the ring as well as the times of these two events. From these data one has a check into the position of Saturn. It should be pointed out that errors in the position of Saturn in A.E.N.A. could be as much as two seconds of arc.

"Capt. Ainslie made his observations with a 9-inch reflector under rather poor conditions during most of the time. With this in mind, members of the A.L.P.O. should avail themselves of this opportunity by trying to follow this event through its entirety. People on the Pacific Coast will have the best view with regard to position, as Saturn transits the meridian around 10:15 (U.T.) for them."

Reference

1. Monthly Notices of the Royal Astronomical Society, 77, 456, 1917.

Postscript. The Editor has found a second observation of an occultation of a star by the rings of Saturn mentioned on page 369 of Volume I of Hutchinson's *Splendour of the Heavens*. On March 14, 1920 three observers watching together "perceived the star to remain shining with comparatively little diminution of lustre, not only through ring A but through the densest part of ring B." Unfortunately, the aperture employed and the magnitude of the star are not stated.

We might note in passing that the rings will be opened to almost the maximum possible extent on April 28, 1957; and the light of the occulted star will not need to pass through so much material of the rings as at smaller inclination-angles.

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