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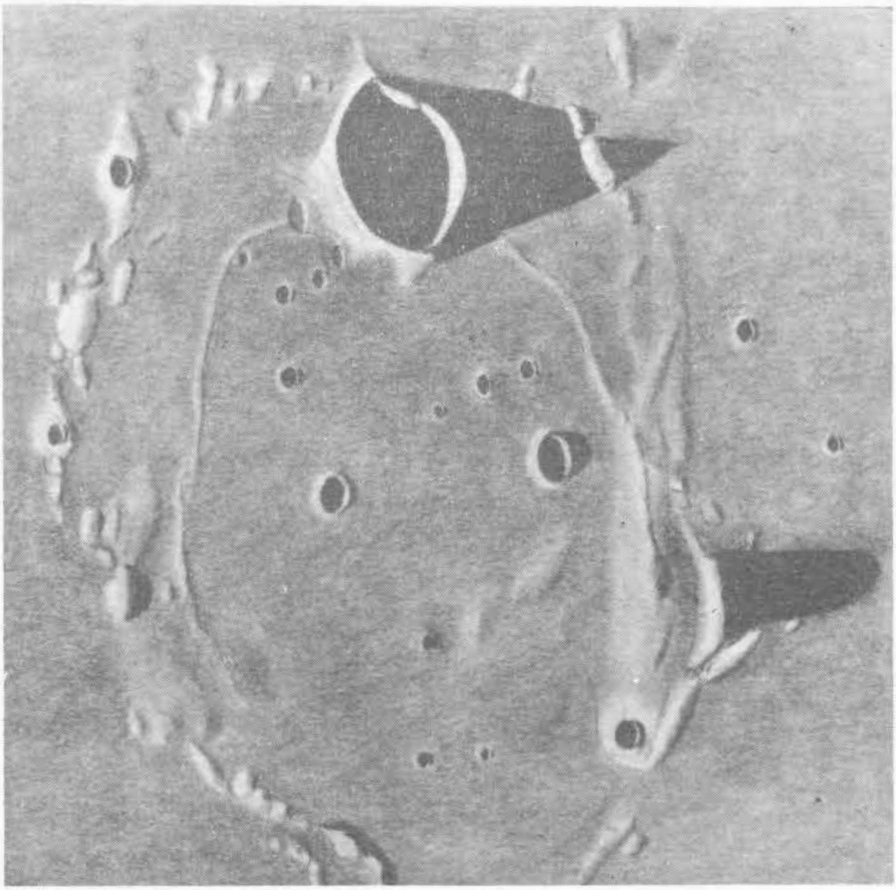

Lunar Crater Flamsteed and vicinity. Drawing by Alika K. Herring on April 16, 1962 at 3 hrs., 15 mins., Universal Time. 12.5-inch reflector, 278X, seeing 4-5, transparency 5. Colongitude 46.3 degrees.

**THE STROLLING ASTRONOMER**

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ASSOCIATION OF LUNAR & PLANETARY OBSERVERS
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# *IN THIS ISSUE*

## THE 1963 APPROACH OF MINOR PLANET 1580 BETULIA

By: Richard G. Hodgson

In a recent issue of *The Strolling Astronomer*[1] George W. Rippen has pointed out the value of observations of the minor planets by amateur astronomers. Members of the A.L.P.O. with moderate apertures will have an unusual opportunity to observe a minor planet making a very close approach to the Earth this year.

*The Handbook of the British Astronomical Association* for 1963 reports that minor planet 1580 Betulia will approach to within only 0.157 astronomical units distance from the Earth, and includes an approximate ephemeris by Professor S. Herrick and Mrs. M.P. Francis of the University of California. The following ephemeris is adapted from *Harvard Announcement Card* 1580:

| Date | Right Ascension (1950.0) | Declination (1950.0) | Distance from Earth |
|---|---|---|---|
| 1963, April 13 | $19^h\ 13^m.2$ | + 41° 20' | 0.589 A.U's. |
| April 21 | 19 13.9 | + 37 48 | 0.482 |
| April 29 | 19 11.3 | + 31 58 | 0.370 |
| May 3 | 19 8.1 | + 27 23 | 0.315 |
| May 7 | 19 3.0 | + 20 47 | 0.262 |
| May 11 | 18 55.1 | + 10 53 | 0.214 |
| May 15 | 18 42.8 | - 4 5 | 0.177 |
| May 19 | 18 23.0 | - 24 49 | 0.157 |
| May 23 | 17 50.0 | - 46 58 | 0.164 |
| May 27 | 16 52.9 | - 63 42 | 0.194 |
| May 31 | 15 19.5 | - 72 52 | 0.238 |
| June 8 | 12 1.7 | - 74 23 | 0.346 |
| June 16 | 10 42.5 | - 72 39 | 0.463 |
| June 24 | 10 16.1 | - 70 39 | 0.580 |
| July 2 | 10 8.1 | - 69 39 | 0.696 |

On May 21 the magnitude of Betulia is not expected to be brighter than 12.5, but its location should afford a challenge to observers equipped with moderate apertures and good star charts. The predicted magnitude is 16.1 on April 21 and June 20. Such a relatively close object will show rapid changes of position, making identification much easier than with most minor planets. Observers should note Betulia's position (photographically if possible) in order to improve our knowledge of its orbit. Many minor planets show irregularities in brightness, indicating irregular shape. From these an enterprising observer might be able to work out the rotation period of the tiny planet.

Reference

1. George W. Rippen, *Str. A.*, *16*, pp. 78-80 (March-April 1962).

*Postscript by Editor.* It will be a challenging project to detect a 13th magnitude asteroid moving 6 degrees per day near the Milky Way. We shall be very glad to hear from readers who succeed in observing Betulia either visually or photographically. As Mr. Hodgson indicates, accurate measurements of brightness by equipped observers will be very welcome.

## ON THE VENUS CUSP EFFECT REPORTED BY BRINTON AND MOORE

By: Dale P. Cruikshank, Tucson, Arizona

In a recent issue of this journal (*Str. A.*, *16*, Nos. 11-12, p. 253), Henry Brinton and Patrick Moore raised the issue of a cusp anomaly observed on Venus. They reported that an imaginary line drawn through the cusps of the crescent of the planet was not always a diameter of the circle including the periphery of the disc. They suggested that micrometer measurements be made in order to help clear up the "mystery".

In the past few years I have made a number of filar micrometer measurements of Venus both in visual light and with color filters. Instruments used include the Yerkes 40-inch refractor (in 1958, 1959, and 1960), and a 12-inch reflector and 4-inch refractor in 1962. These measures were made with quite a different purpose in mind but clearly indicated that the effect reported by Brinton and Moore is due entirely to contrast. When Venus is a crescent the central regions of the planet are brighter than the cusps. Even when they are observed against a daylight sky, one cannot expect the cusps to be as evident as the central regions. As Brinton and Moore point out, underexposed photographs show the same effect. A good example is found in Moore's own book (The Planet Venus, 1st ed., 1958), where in the Palomar blue-light photograph facing page 30, the line of cusps is clearly not a diameter of the circle including the periphery of the planet. Another example of the effect of exposure on the phase of the planet is evident in Sky and Telescope, November, 1962, p. 272, where the Steward Observatory photograph shows the correct phase (crescentric) and an overexposed photo makes the planet look gibbous.

Observers of Mercury encounter the same difficulty with image contrast, though to a lesser degree. The old Schroeter observations of cusp-blunting are probably explained by nothing more complex. Antoniadi noted this effect, too.

My micrometer measurements with color filters have always indicated that the measured diameter of the planet varies not as a function of wavelength, but instead as a function of filter density. That is to say, when I measured the diameter (the line of cusps) with a dense blue or red filter, the value was significantly less than that obtained without a filter. Light yellow and green filters gave intermediate results. Similar observations made as Venus approached inferior conjunction in November, 1962, gave the same results. As the crescent became very thin, in visual light the line of cusps was most often less than a diameter and significantly less than a diameter with the dense blue and red filters. Clearly, if this were an effect due to the Venus atmosphere, one would expect a greater cusp extension in the blue or violet (since Rayleigh scattering on Venus appears to hold in this spectral region according to recent work). Quite the opposite is observed.

The experiences of Brinton and Moore with untrained observers using high and low magnifications would appear immediately to suggest that the cusp anomaly is nothing more than a contrast effect. Naturally, low magnification produces higher contrast - - so high that many experienced observers feel that useful observations of Venus cannot be made with less than about 15 to 30 times magnification per inch of aperture. Low magnification and consequent high contrast are the logical explanation for many "phenomena" of Venus, not the least of which is the often reported region which is darker than the background sky seen just inside the crescent of the planet.

In the editor's note to the Brinton and Moore article, Walter Haas clears the whole matter up in a few sentences. Bad seeing, poor transparency, and low magnification are all factors which wash out faint details on planetary discs and, in the case of Venus, faint cusp-tips.

I might add in passing that I believe that most of the effects of color filters used in observing Venus can be explained on the basis of filter density rather than color. The controversy on this matter reported in recent issues of the Journal of the British Astronomical Association makes it clear that different observers do not get the same results with the same color filters.

## A NOTE ON PHASE ANOMALIES OF VENUS

By: William K. Hartmann, A.L.P.O. Venus Recorder

The evidence strongly indicates that the phase anomaly described by

Brinton and Moore(Str. A., Vol. 16, pg. 253, 1962) is the result of the decrease in surface brightness toward the cusps of the crescent. The very photographs which Brinton and Moore describe as "useless" prove the existence of this decrease. Unskilled observers frequently submit to the Venus Section observations which depict a crescent of the shape described by Brinton and Moore. This accords with their report of confirmation by "totally untrained observers". While I do not question the observations recorded by Brinton and Moore, I think it should be pointed out that the untrained observers commonly lack sensitivity to contrast and are notoriously unreliable when recording phases. Therefore, lest someone should suggest that peculiar phases found in certain drawings confirm the effect, I suggest that most such drawings are not examples of the effect.

We note also that under good conditions the cusps are known to be extended when the crescent is very thin, not reduced as shown by Brinton and Moore. This proves that frequently, if indeed not always, the illuminated atmosphere extends beyond the geometric terminator. Their effect thus probably arises when not all of the illuminated portion of the disk is seen.

Further, it has been reported (cf. Str. A., 16, p. 229) that thin clouds passing in front of the disk can change the apparent phase from gibbous to crescentic. Having observed this effect myself, I believe that it can account for the loss of the faint cusp-tips. The greater prominence of the effect at low powers may result from the greater role of irradiation at such powers.

It should be noted that these comments are similar to those I made recently in discussing the Schroeter effect (cf. the above reference). Both the Schroeter effect and that of Brinton and Moore are probably the result of not seeing the faintest portions of the planet. I conclude again that no real evidence has been presented to show that any of the phase anomalies associated with Venus are due to anything more unusual than contrast effects and the rapid decrease in brightness near the terminator.

## PITON - A LUNAR PROTEUS

By: James C. Bartlett, Jr.

Having digested a recent and excellent issue of Str. A., namely that for January-February, 1961, my eye chanced to fall upon the very last paragraph of text called prophetically Possible Peculiar Aspects of Piton, from which I gathered that some rather off-beat appearances had been reported. Up to this time I had enjoyed no more than a nodding acquaintance with Piton and indeed knew nothing whatever of the mountain, save the barebone statistics one finds here and there in works on the lunar surface spiced now and then by references to peculiar phenomena, generally thought to be lunar atmospheric in origin. Wilkins and Moore,[1] for example, allude to ancient observations by Gruithuisen, and somewhat more recent ones by Pickering, which seemed to indicate unusual appearances; but on the whole material on Piton is rather scanty and also rather inconclusive.

For instance, the delineators of this formation apparently had some difficulty in making up their minds as to whether Piton is one mountain or two; and if two, which way the units are oriented with respect to each other. Elger shows a smaller southern component running N.W. - S.E., and a larger northern component running N.E. and S.W. and diverging one from the other. Goodacre, however, shows two masses of nearly equal size both oriented N.W. - S.E. and parallel to each other; while Wilkins shows a smaller component running N. and S. and a larger to the N. and E. running N.W. to S.E. [ Dr. Bartlett here uses lunar east and west in the older sense, according to which the sun rises in the lunar west and sets in the lunar east. - Editor. ]

Oddly enough, photographic evidence is hardly more enlightening. One of the Lick pictures, taken near sunset, shows an integrated mass with a deep cleft on the east and another on the west, the general appearance conforming more to Wilkins than to either Elger or Goodacre (an excellent reproduction of this plate appeared in The National Geographic Magazine for February, 1958, p. 293, where, however, it is erected). A Yerkes photo, taken with the 40-in. refractor, in early lunar afternoon, shows a trifurcated mass with components running (roughly) N. - S., N.E. - S.W., and N.W. - S.E., while a full moon-view by Lick shows a single, L-shaped object. Finally, the W.H. Pickering Atlas shows a half-moon shaped object (Plate 9A), a solid crescent (Plate 9B), an L-shaped mass (Plate 9C), and two distinct, parallel masses (Plate 9E). In short, the photographic appearance of this mountain is profoundly modified by colongitude, and the appearance at any given time will depend largely upon this factor - but not solely as will shortly become clear. In other words, though the singular changes of appearance affecting Piton are connected with colongitude, they are not all the effects of solar lighting alone. Very definitely something more is involved.

Having thus satisfied myself that Piton was worth more than a casual investigation, I began a series of systematic observations in April, 1961, and concluded them in October of the same year. In all 32 observations were made covering the following colongitudes: 2°.94; 8°.38; 26°.7; 38°.56; 58°.25; 63°.74; 71°.13; 82°.85; 84°.18; 88°.41; 94°.69; 107°.14; 113°.48; 115°.06; 119°.27; 121°.4; 124°.71; 125°.03; 132°.23; 132°.58; 137°.02; 138°.25; 144°.52; 145°.02; 150°.86; 157°.04; 157°.43; 163°.16; 163°.21; 169°.51; 176°.08; and 182°.1. The same telescope - a 4.25-in. reflector - with the same eyepieces and powers, 50X, 120X, and 240X, was used exclusively. It will thus be seen that a fairly comprehensive view of the phenomena was obtained from sunrise to sunset; and if one were to base himself on these observations alone, without considering what they may imply, one would come to the following conclusions all of them demonstrable:

a) That Piton is a solid, crescentic mass; b) that Piton is a solid, irregular mass; c) that Piton is neither, but rather two nodular masses, the southern one much smaller than its northern neighbor; d) that Piton is composed of at least seven distinct masses; e) that the mountain is not a mountain at all but rather the fragments of an ancient ring; f) that Piton is the lunar equivalent of an atoll.

I have recorded some astonishing lunar transformations in my day as functions of colongitude, but nothing to equal this unless it be that perplexing affair in J. J. Cassini which so confounded me a few years back; and I think not even that.[2] Proteus, that whimsical godling of Greek fancy who could take all shapes "from Máh to Máhi", had nothing on Piton.

Manifestly Piton is a solid, integrated mass; the sunrise and sunset aspects make this very clear. Just possibly it might be three masses; but whether the clefts on the east and the west actually divide the mountain into separate units is a question which only the largest apertures can resolve. One of the Mt. Wilson pictures, taken at col. 137°, shows the mountain as a single, irregular mass with a very deep cleft on the east and a much shallower one on the west, neither of which cuts completely through the mountain. E. J. Reese very kindly supplied the writer with a topographical outline sketch, based on Mt. Wilson Photograph H10, a copy of which is given here as Fig. 1. The aspect bears a very close resemblance to the Yerkes photograph mentioned above, and may confidently be accepted as a pretty accurate representation of the true state of affairs.

The question before the house than is simply this: By what process does a single, irregular mass of rock appear to become as many as seven masses each distinctly separate from all the others; or how can a unified mass change its aspect to that of an atoll composed of scattered islets arranged in a definite ring? I think it self-evident that mere change of lighting is quite insufficient to account for transformations of such character, the more so in that the apparent break-up of the mountain into well-defined fragments comes precisely at those colongitudes when shadow is either altogether wanting or its effects at a minimum. It is true that the object in J. J. Cassini may appear alternately as a rectangular block and as a shallow ring plain, and it is equally true that such changes are indubitably related to colongitude in a direct and simple way; but the block is never observed to become seven blocks, and the ring plain is obviously a ring plain and nothing more. Moreover, a factor which contributes heavily to the J. J. Cassini transformations is the obliquity with which objects in such far northern latitudes must always be viewed, a factor which is wanting in the case of Piton which lies a trifle north of the 40th lunar parallel of north latitude. For the same reason the effects of the two principal lunar librations, i.e. in latitude and in longitude, are minimized for Piton.

But before we attempt an explanation for the Piton phenomena, it will be well to become a little more familiar with the mountain itself, and incidentally to remove some common misconceptions in the process.

The commonly accepted height is about 7,000 feet above the surface of the Mare Imbrium; and the mountain is generally described as "bright", but without qualification this is very misleading. It is in fact really bright only at sunrise and again at sunset, a truth which can be readily established by using Pico as a gauge. The brightness of Pico also varies - naturally - with the angle of incidence of sunlight; but the Pico variations are so much less than those of Piton that the former remains conspicuously bright from sunrise to sunset while the latter may - in part - become very dull and grayish and not more than 4° bright. I say "in part" because certain spots on Piton, apparently peaks, may remain from 6° to 7° bright, while the flanks become quite dark. The western face of the mountain appears to be more reflective than the eastern, for at sunrise I have estimated the western flank to be as high as 8° bright while at sunset the eastern flank is estimated at only 5°. [The lunar intensity scale here employed is 0° for black shadows to 10° for the most brilliant marks.-Editor.]

In general the observed variations in brightness run as follows: At colongitude 2°.94, brightness 8°; by col. 26°.7, brightness has fallen to 6°; between col. 63°.7 and 88°.4 average is 7°; but at col. 121° it is down to from 5° to 6°. Between col. 150°.8 and 169° the mountain is very dull, averaging only 4° and looking distinctly grayish but with bright spots, notably one in the north and the other in the south, which vary from 5° to 6° bright. Between col. 176° and 182° (sunset) the brightness is about 5°.

These variations are quite constant and do not, to my mind, suggest the influences of either volcanic or meteorological agencies. As to the cause of them, we shall consider this in due course; but let it be sufficient now to note that the mountain is very bright at sunrise, that this brightness begins to fall off with a secondary brightening with approach to noon, and that in late afternoon the mountain becomes very dull with another brightening just before sunset. It is also necessary to note that the dull phase comes at a time when the mountain appears to be divided into several segments, including its atoll-like ring aspect, and that these segments may vary in relative brightness, some being as high as 6° bright while others may be no more than 4°. Occasional exceptions are also noticed, as at col. 145° when the northern segment appeared 4°.5 bright and the southern 5°, August 1, 1961; while at col. 144°, September 29, 1961, the southern segment was a dull 4° and the northern segment 4° at the crest with a 5° base. Such variations from the norm are very slight and are not significant.

There is considerable evidence to show that the mountain is quite rough in surface texture and that it varies in composition, as for instance a terrestrial mountain would if composed of, say, schists injected by granites. It must be clearly understood, however, that the comparison does not imply a similarity of materials in the case of Piton, only that Piton reflects light in such a way as to suggest reflection from different types of surfaces.

This result is also suggested by examination in light of differing wavelengths, for which purpose Wratten Filters were used by the writer during almost every observation. The overall picture thus obtained is that of an object which is brighter in red and yellow light than in green and blue, and is consistently dullest in blue light. The following table will give the relations found, in which a scale of 1, 2, 3, and 4 was adopted with 1 being brightest and 4 dullest. The initial N stands for "neutral" meaning that no appreciable brightening or dulling was observed in the color used.

Table of Filter Reactions

| Colongitude | Blue | Green | Red | Yellow | Colongitude | Blue | Green | Red | Yellow |
|---|---|---|---|---|---|---|---|---|---|
| 2°.94 | 2 | 1 | 4 | 3 | 124°.71 | 4 | 3 | N | N |
| 8.38 | 3 | 2 | N | 1 | 125.13 | 4 | N | N | 2 |
| 26.70 | 4 | N | N | N | 132.58 | N | N | 1 | 2 |
| 58.25 | N | N | N | N | 137.02 | 4 | 3 | N | N |
| 63.74 | 4 | N | N | N | 138.25 | 4 | 3 | 2 | 1 |
| 71.13 | N | N | N | N | 144.52 | N | N | N | N |
| 82.85 | 4 | 3 | 1 | N | 145.02 | 4 | N | N | 2 |
| 84.18 | 3 | N | 2 | 1 | 150.86 | 4 | 4 | N | 2 |
| 88.41 | 4 | 3 | N | N | 157.04 | 4 | N | 2 | 1 |
| 94.69 | 2 | N | N | 1 | 157.43 | 4 | N | N | 1 |
| 107.14 | 4 | 3 | 2 | 1 | 163.16 | N | N | N | 1 |
| 115.06 | 4 | 3 | 2 | 1 | 163.21 | 4 | 3 | 1 | 2 |
| 119.27 | N | N | 1 | N | 169.51 | 4 | N | 2 | 1 |
| 121.40 | 4 | N | N | N | 176.08 | N | N | N | 1 |

It will be observed that in 28 observations Piton was found dullest in blue light on no less than 17 occasions, while yellow light gave a value of 1 for 11 observations and red for only 4, though the mountain was consistently brighter in red light than in blue or green. Only once - at col. 2°.94 - was the usual order of things reversed, when Piton was definitely brightest in green and blue and dullest in yellow and red. It will also be observed that the results are not consistent with those at the same or at closely comparable colongitudes; and unless the differences are to be referred to simple errors of estimation, there would appear to be no apparent connection between them and varying states of the transparency, which, in most cases, varied no more than a point and in only two cases by as much as 2 points (on a scale of 0 to 5, with 5 best).

However, at no time did the writer observe anything which suggested local lunar mists or other obscurations, though on one occasion, April 25, 1961, at $3^h\ 37^m\ 30^s$ U.T., I could not define the morning shadow sharply. It so happens, however, that the morning shadow falls athwart a naturally dark area to the east of the mountain so that optical confusion is quite possible. Again, on April 28, 1961, at $4^h\ 35^m$ U.T., I found a local haziness at the western and sunlit base of the mountain; but this was very indefinite, and, as Prof. Haas suggested, may well have been due to the inability of a small aperture to define the points of contact of the mountain with the mare.

And now, before we consider the amazing transformations of Piton with the advance and decline of the lunar day, we must take speial notice of the very dark, semi-circular area of surface immediately east of the mountain; for it is virtually certain, in my opinion, that this area is to the transformations as cause is to effect.

To begin, there are actually two very dark areas east of Piton, that which is immediately adjacent to the mountain and another still farther east which is separated from the former by a very thin division. The eastermost area is not quite so dark as the other and is of very different extent and shape, being a well-defined triangle with the vertex pointing eastward. It ends on the south in a curious prolongation at the end of which is a much smaller triangle, also pointing eastward. This last

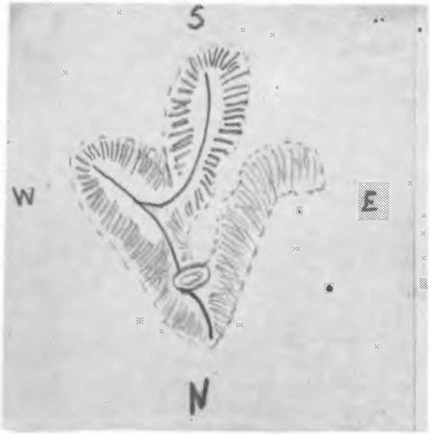


FIGURE 1. Lunar Mountain Piton. Copy from tracing by E.J.Reese of Mt. Wilson Photograph H 10. Col. 137°.

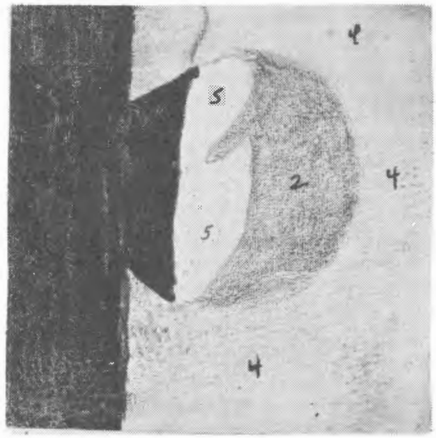

FIGURE 2. Piton. Oct. 2,1961. $6^h\ 34^m$ U.T. 4¼-inch. refl. at 240X. S = 1 - 3. T = 1. Col. 182°.1. James C.Bartlett,Jr.

FIGURE 3. Piton. Aug. 19,1961. $1^h\ 49^m$ U.T. 4¼-inch refl. at 240X. S = 6. T = 4.Col. 2°.94. James C. Bartlett, Jr.

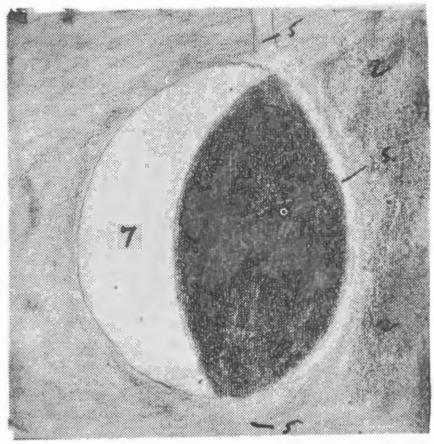

FIGURE 4. Piton. April 28, 1961. $4^h\ 35^m$ U.T. 4¼-inch refl. at 240X. S = 5. T = 4 to T = 2. Col. 63°.74. James C. Bartlett, Jr.

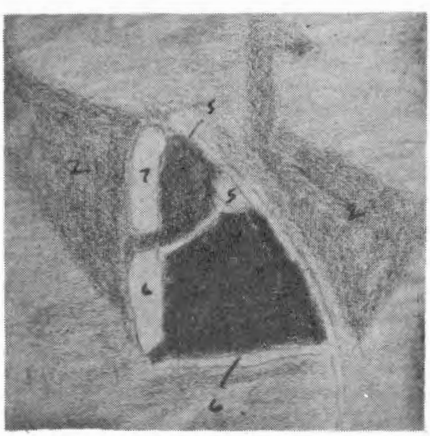

FIGURE 5. Piton. Sept. 23,1961. $3^h\ 48^m$ U.T. 4¼-inch refl. at 240X. S = 6. T = 5. Col. 71°.13. James C. Bartlett, Jr.

feature somewhat resembles a flag flying from a flagpole and gives a very distinctive appearance to the region. Though this triangular dark area, together with its "flag", darkens under a high sun and breaks up into faint patches and streaks with approach to sunset, we need not consider it here. It plays no part in the phenomena of Piton. But the semi-circular dark area is the key to an understanding of the whole affair.

Its earliest history is unknown because, unfortunately, at sunrise the shadow of Piton covers the area; but with approach to noon this semi-circle becomes increasingly darker, and I have estimated it as only 1° bright as early as col. 63°, i.e., very nearly as black as shadow. It is very evident on photographs. Its behavior in the afternoon is somewhat variable. I have sometimes estimated intensity as 1° as late as col. 124°.71, but 2° at col. 121°.4 at other times. A definite fading begins in the late afternoon, and at col. 169°.51 I have found the area nearly 4° bright. Just before sunset, however, it again darkens rapidly and again assumes a well-defined semi-circular shape (Fig. 2). I have so seen it when the intensity was estimated as only 2°. This last phenomenon is most important, for it shows pretty conclusively that this semi-circular area of surface is in reality a very shallow depression. It is possible that with sufficient aperture a slight shadow might be found under the eastern rim at sunset, but this is naturally invisible to a small instrument.

Though the structure of this area, assumed to be a shallow depression, would well explain its rapid darkening with approach to sunset, one should notice that its high-sun darkening cannot be so explained; and I hardly think that anyone would seriously argue for mere contrast as the explanation. It is certainly a little hard to believe that the adjacent <u>mare</u> surface becomes so very much brighter as to convert a local area of light gray surface to one nearly as black as shadow.

So much for this particular area immediately adjacent to Piton on the east. We shall return to it a little later. In the meantime we are ready to consider the remarkable phenomena of the mountain itself.

At sunrise the more or less monolithic structure of Piton is clearly revealed (Fig. 3) together with the western cleft or valley. At sunset we see the same thing (Fig. 2) but with a deeper cleft on the east. Both as to observed structure and shadow, therefore, the mountain appears as a solid mass.

For some days after sunrise Piton retains the appearance of a solid mass, resembling a crescent (Fig. 4) forming one lune of a circle of which the other is the high-sun dark surface on the east; but it may also appear as an L-shaped structure of unusual thinness (Fig. 5). As might be expected these differences are related to the libration in longitude, the crescentic phase being prominent when the center of the disc is displaced toward the eastern limb and the thin, L-shaped phase when it is displaced toward the western limb. This does not mean that the eastern side of the mountain is necessarily much steeper than the western side. The reason would appear to be something quite different.

With approach to noon we get the first hint of those apparent alterations which convert Piton from one mountain into several and ultimately into a ring-shaped formation resembling a coral atoll. Thus in Fig. 5 we see the beginning of definite divisions in the solid mass and also the appearance of an apparently isolated bright mass on the very edge of the high-sun dark area. Note also the thin white line forming the boundary of this area which here, incidentally, appears more triangular than circular as if squeezed out of shape; but this aspect is merely an effect of the libration in latitude plus the development of high-sun bright spots on the northern edge of the area.

By col. 107° the whole appearance has radically altered, and we now see what appears to be a complete ring of isolated masses, islets as it were enclosing a 1°.5 dark lagoon (Fig. 6). This phase is observed through


FIGURE 6. Piton. Sept. 26, 1961. $2^h$ $52^m$ U.T. 4¼-inch refl. at 240X. S = 5. T = 4. Col. 107°.14. James C.Bartlett, Jr.


FIGURE 7. Piton. Sept. 27,1961. $2^h$ $47^m$ U.T. 4¼-inch. refl. at 240X. S = 3. T = 5. Col. 119°.27. James C. Bartlett,Jr.

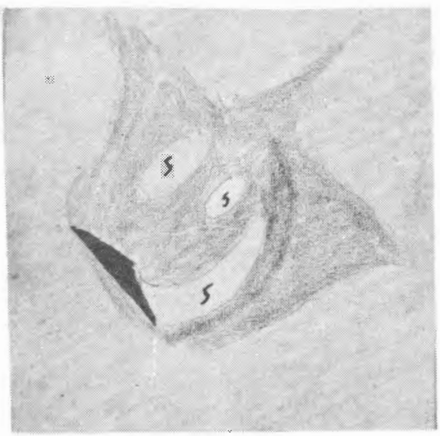


FIGURE 8. Piton. July 31, 1961. $5^h$ $41^m$ U.T. 4¼-inch refl. at 240X. S = 6. T = 3. Col. 132°.58. James C. Bartlett,Jr.

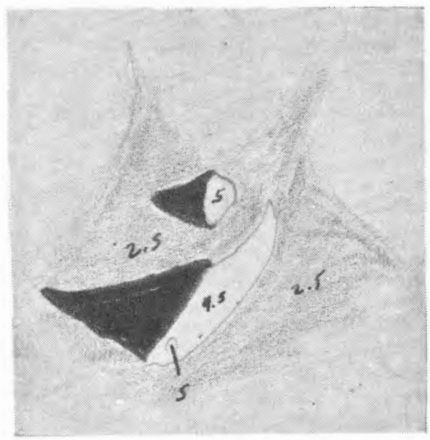


FIGURE 9. Piton. Aug. 1,1961. $6^h$ $05^m$ U.T. 4¼-inch refl. at 240X. S = 5 to 6. T= 3. Col. 145°.02. James C. Bartlett, Jr.

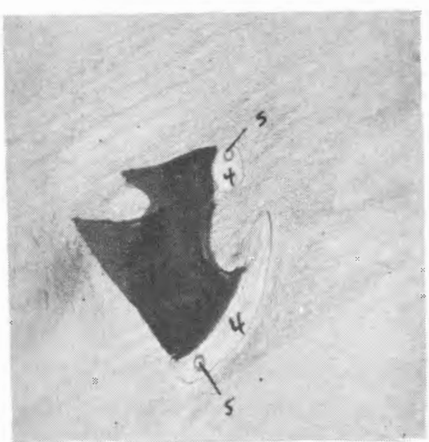


FIGURE 10. Piton. Aug. 2, 1961. $6^h$ $38^m$ U.T. 4¼-inch refl. at 240X. S = 5. T = 3. Col.157°.43. James C. Bartlett, Jr.

col. 115°; but by col. 119° further extensive changes have taken place, and the atoll has given way to three elongated masses oriented N.W. - S.E. The "lagoon", meanwhile, has begun to brighten and is perhaps 2° intensity (Fig. 7). This trifid appearance is maintained through col. 132°, at which time a well-marked shadow has developed westward from the base of the easternmost mass which now appears to have curved around west through north (Fig. 8).

Twenty-four hours later the mountain has been reduced to two masses (Fig. 9), the southern component being much smaller than the northern. It is important to notice that each throws its own shadow, as if physically separated. A day later still the two shadows have merged into one with two peaks, and here an important fact must be noticed. In Fig. 10 we have the appearance at col. 157°.43, August 2, 1961, and Piton still appears to be composed of two masses; but observe that part of the shadow of the major component, i.e. that portion between the components, appears to arise from nothing!

At col. 157°.04, September 30, 1961 (Fig. 11), we find the explanation. Here the mountain is seen as a single mass from which the shadow arises normally, and on the east we see the well-known cleft. Comparing this view to Fig. 10, it is clear that part of the apparent separation of the two masses was really composed of this cleft. The other part, therefore, must have been composed of the mountain itself; but because the crest at this point (and at this particular time) was of the same color and intensity as the floor of the cleft the eye could not distinguish between them, and so there appeared to be merely open space - open space which threw a shadow! A much larger aperture doubtless would have dispelled the illusion; but in any event the fact is of prime importance to an understanding of the whole series of phenomena. It may be objected that the existence of the two individual shadows is not thereby explained, as indeed it is not; yet there is no great mystery in the matter. We have only to suppose that the main mass of the mountain dips down to the level of the mare, in which case a very slight shadow would doubtless escape detection with a small glass, to spring up again in a peak at the southern end, which would throw a considerable shadow. Hence one would have the reality of two distinct shadows and the illusion of a discontinuity.

The astonishing metamorphoses of Piton are repeated, colongitude for colongitude, with a high degree of regularity and to this extent might be thought to result wholly from changes in the solar lighting; but occasionally such major differences are observed that it seems clear that we must look further for the explanation. For instance, on August 29, 1961, at col. 125°.13, Piton appeared to be composed of four isolated fragments which may be taken as the normal appearance at this time; yet on May 3, 1961, at col. 124°.71, the mountain appeared in its solid phase as a broad, crescentic mass!

Such are the protean transformations of Piton. How are we to explain them?

Let us first return to that semi-circular dark area immediately to the east of the mountain. The facts that it appears to be a very shallow depression, that it is more or less circular, and that Piton forms the western arc of this circle rather strongly suggest that Piton may not be a mountain at all in the proper sense of the term but rather the remains of a rampart which at one time encircled an ordinary crater, the walls of which have everywhere disappeared save on the west. It seems possible, therefore, that Piton may represent an extreme example of the partially submerged crater. That this high-sun dark depression is in fact the remains of an ancient ring, of which Piton forms a still surviving fragment, is also strongly suggested by the delicate, 5° white boundary previously mentioned, which probably represents the surface outline of the original walls.

We are now ready to see how all of this fits into the picture of

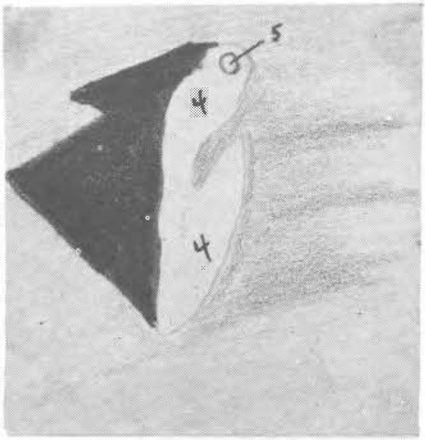


FIGURE 11. Piton. Sept. 30, 1961. $5^h\ 14^m$ U.T. $4\frac{1}{2}$-inch refl. at 240X. S = 1 to 3. T = 3. Col. 157°.04. James C. Bartlett,Jr.

Piton's astonishing changes of aspect, culminating in the illusion of an atoll. We start with Fig.4, in which the mountain itself has the appearance of a solid, crescentic mass. Notice the thin, white boundary of the semi-circular dark area. If we measure the width of the true mountain in relation to the width of the dark area it becomes very clear that the "islets" forming the eastern arc of the atoll (Fig.6) cannot be a part of Piton itself, which is represented rather by the western arc of spots. The eastern objects, therefore, including those on the north which form the base of the L-shaped structure (Fig. 5), are really high-sun bright areas which under a low sun are quite invisible. It is very probable that they mark the course of the vanished rampart enclosing the whole high-sun dark circle.

Before writing this paper I discussed the Piton phenomena with E. J. Reese, who expressed the opinion that the atoll aspect and the break-up of the mountain into fragments was simply the result of the development of high-sun bright areas. Mr. Reese also kindly supplied a tracing from Lick Photograph M4a illustrating these bright areas, the whole fairly representing the appearance at col. 129°. With this view I would agree completely in regard to the eastern chain of spots; but in regard to the western - which represents Piton itself - I do not see that the explanation is tenable.

Returning to Fig. 6, we observe that the western components are separated by clear spaces which have the same intensity as the interior of the lagoon, or high-sun dark area. Now these apparent clear spaces are actually part of the mountain itself; therefore, if we are to suppose that they arise merely as contrast effects, then we must also suppose that local areas on the mountain have become so brilliant as to make adjacent areas look at least 2° dark. Yet it is precisely at this time that the bright parts of the mountain are really at their dullest, falling in some instances to 4° intensity and looking quite gray rather than white. Moreover, comparison to Pico (which was made by the writer at every observation) will show that no such super-brightening has taken place, and that in fact the brightest parts of Piton are obviously inferior to any part of Pico. Nor can it be merely coincidental that the development of the apparent dark spaces (which convert the solid mass of Piton into discrete fragments) are part and parcel of the high-sun darkening of the lagoon itself. Rather the one is certainly an extension of the other. What then is the nature of this high-sun darkening? I will not use the naughty word "vegetation"; rather I will say that <u>something</u> develops in the semi-circular area immediately east of Piton, which something becomes extremely dark with approach to noon.

Whatever merit there is in the argument contra that high-sun darkening is an illusion, caused merely by an increase in brightness of the surrounding surface, I should be happy to see anyone prove the point with respect to Piton. For it happens that Piton lies nowhere near a really bright surface, but rather on the relatively dark <u>mare</u> surface, laced it is true by vague whitish streaks, rays, and splotches but none of them more than 5° bright and many of them much less. Moreover, the camera, as well as the eye, testifies that the high-sun dark area east of the mountain is really <u>dark</u> - period. In fact, it is sensibly black in high-sun photos.

This area, therefore, is composed of a something the color of which is much darker than any part of the adjacent surface, a color which may be any very dark color, e.g., a very dark green or blue, but which at the lunar distance and with small apertures ranges from a very dark gray to almost black. Let us suppose too that this dark something overflows, as it were, the eastern flank of Piton itself, thus causing the very narrow aspect when the libration is toward the west, and let us suppose also that this something sends forth tentacles which flow up the flanks of the mountain, across the crest and down the western side. What would be the effect? Why, so far as the eye is concerned, it would be to convert one mountain into apparently several mountains. But should you ask me why this material creeps up the mountain in bands, rather than advancing in a solid wave, I should have to reply that I do not know - any more than anyone knows why the dark material on the walls of Aristarchus also manifests itself as bands.

The point is that the phenomenon appears to be identical with the phenomenon of a banded crater. I propose therefore that in Piton we may have a unique specimen - a banded mountain. It is well known that the bands in Aristarchus are variable, which is to say that in some lunations certain ones do not develop and others may differ in intensity relative to one another; and once I saw Aristarchus when no bands at all were visible. The same is true of Herschel and of Proclus, and will probably be found true of all banded craters. Thus if the Piton phenomenon is the same as the Aristarchus-Herschel-Proclus phenomenon, we have an explanation for its solid aspect on May 3, 1961, at a colongitude when normally it appears as several masses. The Piton bands had simply failed to develop.

So I read the riddle of Piton. Perhaps in a few years the first astronauts will stand in the shadow of the mountain, and this theory will have become as dead as the Diplodocus; but in the meantime there may be gold in those isolated lunar hills one finds here and there, and so I may have a go at another - say Spitzbergen.

References

1. Wilkins, H.P. and Moore, Patrick; The Moon; Faber and Faber, Ltd; London, 1955; p. 233.
2. Bartlett, J.C.; "Object Beta - A Case of Identity"; Str. A.; May-June, 1955; p. 54.

Note by Editor. We are indebted to Dr. Bartlett for a very well-written and detailed discussion of apparent lunar changes in one well-known feature, using the methods pioneered many years ago by W. H. Pickering.

The interpretation of such changes has been controversial. A most critical point is that of whether real high-sun darkenings occur. The Editor would doubt that visual work can ever objectively establish such darkenings as distinct from contrast-effects. Mr. Reese has commented in these pages on the amazing transformation of Jupiter IV from a bright moon on the sky near its primary to an object looking fully as black as shadow when in transit across Jupiter.

The identification of high-sun lunar features with topographical detail visible under lower lighting is also often most troublesome. The best approach is presumably by means of measures of photographs.

## OPTICAL STUDY OF CLEAR AIR TURBULENCE

By: John K. Newell

Abstract. Observations using both stars and planets as light sources for optical analysis (along the lines of the Foucault test for telescope objectives) indicate that the principal atmospheric distortion of optical

wave fronts usually is attributable to one or more layers of clear air turbulence. These observations suggest that a camera-equipped astronomical telescope can be used to determine accurately the altitude of the clear air turbulence, and that a simple mechanical device at the telescope may determine wind velocity in the vertical vicinity of theturbulent layers. Wind azimuth in the turbulent layers is directly observable at the telescope.

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Of common concern to meteorology and astronomy is the atmosphere's characteristic modification of optical wave fronts passing through it. If the wave front approaching the earth from a celestial object may be thought of as modeled by a very thin disc of metal foil, then its broadside arrival to fill the image plane in an astronomical telescope will find this model of a wave front no longer flat, but badly battered, dented, and wrinkled. The atmosphere as a whole is not guilty of this assault and battery upon the optical wave front. This crime against astronomical seeing usually seems attributable to a single, thin layer of turbulent air - although sometimes two or three such layers contribute to the wave front distortion.

These atmospheric optical effects have long been of concern to astronomers. Such effects have been studied in detail[1] and many of the studies have been published, particularly in <u>The Journal of the Optical Society of America</u>. Lunar and planetary observers are keenly aware of such atmospheric effects on light because of the frustration of trying to confirm, photographically, glimpses of fine detail observed visually in fleeting moments of unusually tranquil seeing. However, most of the studies of seeing have had to do with the scintillation (brightness changes) of stars. Attempts to correlate scintillation and poor seeing with atmospheric layers or conditions have been considerably fewer.

Dr. Elmar R. Reiter of Colorado State University says[2]: "Indications of atmospheric turbulence are detectable through the scintillation of stars, which is caused by slight inhomogeneities of density.[3] The amplitude of scintillations may be obtained photographically. There seems to be a correlation between wind velocity and wind shear at the jet stream level on the one hand, and the scintillation on the other hand. The high-frequency oscillations (150 cps) of the star image seem to have their origin at the average tropopause level, while the low-frequency oscillations (25 cps) have no marked correlation to wind velocities below 20 kms."[4]

Evidence points to two contributors to atmospheric distortion of optical wave fronts: 1. wind shear, and 2. temperature inversion in which temperature rises directly with altitude.

Dr. W. M. Protheroe of the University of Pennsylvania reports that "it does appear safe to conclude that stellar scintillation is an excellent indicator of the winds at the maximum-vector shear layer."[5]

Dr. Enrique Gaviola, of the National Astronomical Observatory at Cordoba, Argentina, says that the wave front distortion is "produced by the relative displacements of a warmer air mass above a colder one. The comparative 'warmer' and 'colder' refer to the temperature of the adiabatic lapse-rate curve extrapolated from the neighbouring layer. The temperature of the 'warmer' layer may actually be less than the one of the 'colder' ones, provided it is above the one necessary for convective adiabatic equilibrium."[6] "Often", he continues, "at one or more levels in the atmosphere, a warmer layer of air lies above a colder layer. Motion of either layer relative to the other can cause a system of waves or ripples ... a few centimeters or inches long."

Both Gaviola and Protheroe have analyzed the optical Fourier spectra of these waves of atmospheric inhomogeneity - Gaviola, photographically with the Bosque Alegre reflector, and Protheroe, with a specially designed optical Fourier analyzer using a transmission grating.[7]

However, some study of these atmospheric wave patterns can be made visually, using a telescope without camera or grating. Jean Texereau of the optical laboratory of Paris Observatory advises[8] that if the telescope is pointed at a bright star and the eyepiece removed, "we see streaming rapidly across the mirror the 'flying shadows' of the perturbed wave: more or less periodic striae, fairly wide, spaced 2 to 4 inches apart, and of weak contrast except when the turbulence is particularly severe. Often two systems of striae are visible, superimposed one upon the other, and moving in different directions."

This "flying shadow" pattern is produced in one or more layers of air which may be at the same time associated with conditions of shear and temperature inversion. Differences in refractive indices of small portions of air in these layers cause disruption of the wave front arriving at the telescope from a star or planet. The effect is observed when the telescope objective is used as in the Foucault test, with a star as the light source. This test usually is employed as a laboratory check on the surface and figure quality of a telescope mirror. Sensitivity is high - linear magnification of mirror surface variations is on the order of 100,000 times. If this Foucault test is conducted with the light passing through a disturbed refractive medium (as in Schlieren photography of the effect of compression waves in a medium flowing past a model), then the test provides an accurate visual image of the pattern of disturbed refractivity. Hence, the "ripples" in a clear atmosphere may be imaged directly upon the objective of a telescope, where they may be studied visually or photographed.

My own observations with a reflecting telescope of 12½ inches aperture indicate that a bright planet is a much better light source for this kind of observation than is a star. The planetary light source renders observation of the "flying shadows" much easier and offers more image contrast than does stellar light. Undoubtedly, stars have been used in most past studies of scintillation for the simple reason that (to the naked eye) stars scintillate and planets shine with a steady light. This is true because the column of light through the atmosphere to the pupil of the eye is essentially a cylinder of light about 7 mms. in diameter, the approximate measure of the dark-adapted normal pupil. This slender column of light is easily disrupted by atmospheric ripples, which usually are on the order of 8 centimeters, more or less, in wave length. These waves, of length considerably longer than the pupil diameter, cause modulations in the star's apparent brightness and position. The light path from a bright planet to the eye is, however, far from cylindrical. Passing through the atmosphere is a cone, truncated by the 7 mm. eye pupil at one end and spreading to subtend the entire planet at the other, with there an angular diameter of possibly more than 45 seconds of arc. A horizontal cross-section from this cone in the stratosphere would be some meters in diameter, and thus would contain several atmospheric ripples a few centimeters in wave length.

Using planets (usually Jupiter, sometimes Saturn or other planets) as light sources in twilight and in early darkness, I made 27 observations of atmospherically caused "flying shadows". These observations were carried out from September, 1961, to January, 1962.[11] Attempts were made in these 27 observations to correlate work at the telescope in San Antonio, Tex., with meteorological information obtained from the U.S. Weather Bureau in San Antonio. Inquiries were made by telephone. The azimuth of the shadow pattern flow was noted, and inquiry was made as to at what altitude (or altitudes) wind azimuth corresponded to the direction of shadow pattern flow as observed in the telescope. With one (and only one, in observations allowed as valid) coincidence in wind and shadow pattern azimuth established, then information was sought from the Weather Bureau on wind shear and temperature conditions at that particular altitude.

It should be noted that my backyard observatory is some 10 miles from the Weather Bureau's balloon release point, and that my observations were made, in extreme cases, as long as two hours after balloon release

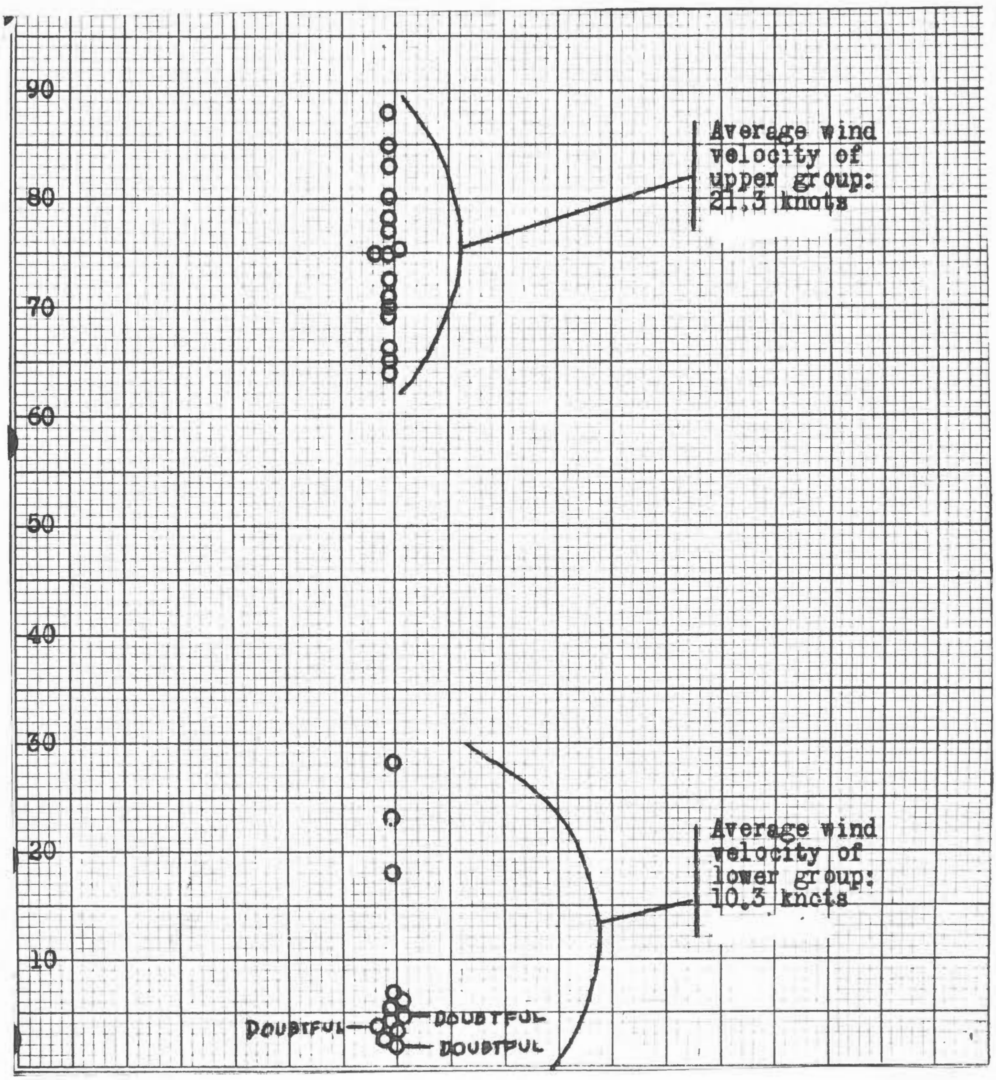


FIGURE 12. Graphical presentation of observed altitudes of layers of optical turbulence as determined by John K. Newell. Each circle represents a single such observation. The scale on the left gives altitude above mean sea level in thousands of feet. See also text of Mr. Newell's article in this issue.

time. But if these adverse conditions and the relatively small number of observations may be admitted as evidence, then there is a good indication that the atmospheric layer causing anomalies in optical wave front behavior is usually, if not always, characterized by both wind shear and temperature inversion.

Average height of the optically disturbing layer was held to be 45,000 feet. But graphic distribution of the reports on altitude leaves a distinct gap at the 45,000 feet level, with groupings of readings at higher and lower levels (Figure 12).

The relatively large number of observations indicating that the disturbing layers were at from 60,000 feet to 90,000 feet possibly has something to do with the temperature inversion common at these altitudes. All observations in this group indicate some wind shear, with the velocities ranging from 8 to 29 knots and averaging 21.3 knots.

In each of the 12 lower-altitude observations (below 30,000 feet) there is some special condition which might have bearing. When three of these readings were taken, the Weather Bureau could supply no information for altitudes above 75,000 feet (in two cases) and 80,000 feet (in one case.) There were no temperature inversions reported for these three instances, and the three readings may not be valid. But for the other nine observations in the low altitude group, definite wind shear and temperature inversion were noted.

These observations would seem to indicate that the principal optical disturbance in the atmosphere may be produced at that altitude at which the combination of wind shear and temperature inversion is strongest.

Protheroe reports two sets of data[12] , 46 observations from Bedford, Mass., and 67 from the University of Pennsylvania, in which the mean height of the disturbing layer of air was 9,087 meters and 8,690 meters respectively. This compares with an altitude average above 13,000 meters at San Antonio. Protheroe (in a private communication, June 1962) suggests that this difference may be due to the higher average tropopause level at the latitude of San Antonio.

He reported that good wind direction correlation was obtained by using a slit in his scintillation measuring instrument. Nevertheless, this correlation was on slit angle increased or decreased by 180 degrees, since the sense of motion was not determined by the pattern. The San Antonio observations, using planets, leave no doubt as to direction - the azimuth of motion of the "flying shadow" patterns can be observed directly.

It is suggested here that by using both stellar and planetary light, there need be no dependence upon balloons or any means other than optical for determining altitude, velocity, and azimuth of the principal optically-disturbing layers of clear air turbulence.

The ripple patterns produced by stellar light passing through the atmosphere are easily photographed, and Gaviola has done so.[13] Since patterns produced by planetary light sources are stronger in contrast, there is little doubt that these, too, can be recorded on film.

The light path from a star to a telescope objective is, for all practical purposes, a cylinder of the diameter of the objective. The light path from a planet to a telescope objective is a truncated cone, the sides of which have a known angle of convergence. If a planet and a star appear as neighbors in the celestial sphere, then a layer of disturbing atmosphere cuts a horizontal slice across cone and cylinder at the same time. This slice can be photographed as stellar cylinder and planetary cone, recording ripple marks of measurable wave lengths in each. Simple trigonometry can then be used to calculate the slant range to the layer of disturbance, and the altitude of the layer (when the angle of elevation of the telescope is considered) can be derived.

It is further suggested here that the rate of angular motion of the planet-illuminated shadow pattern across the field of view of the telescope can be measured. If a moving reticle could cause an artificial ripple pattern to move across part of the telescope's field of view, and the rate of the reticle motion could be varied at will, then reticle motion could be made to correspond accurately with shadow pattern motion and then measured. (That rates of motion of striped patterns can be matched accurately has been demonstrated in precision military instruments - the fuse-setting mechanism on the 90mm. anti-aircraft gun, for example.)

Protheroe says that the rate of motion of the scintillation patterns correlates well (although not necessarily in a 1 to 1 relationship) with wind velocity in the disturbing layer. Thus, if the rate of motion of the "flying shadows" is known, then wind velocity can be derived. Use of a relatively short-focus astronomical mirror should be of help in making these measurements. The aperture should be at least 10 inches.

Although optical study of clear air turbulence might, at first glance, seem absurd, more study along these lines would appear to be warranted.

Summary. Visual observations made with a reflecting telescope $12\frac{1}{2}$ inches in aperture pointed at bright planets produced data on atmospheric distortion of optical wave fronts. These data were correlated with U. S. Weather Bureau information on wind direction, wind velocity, wind shear, and temperature over the San Antonio, Tex., area. Principal atmospheric disturbance of optical wave fronts usually is confined to three or less layers of the atmosphere, and these layers of clear air turbulence often occur in the vicinity of both wind shear and temperature inversion. It is suggested that a suitably-equipped telescope may be used in night observations to obtain direct and accurate information upon 1. altitude of principal layers of clear air turbulence, 2. azimuth of winds in such layers, 3. velocity of winds, and 4. wave length of the ripples of optical disturbance.

References and Notes

1. Lord Rayleigh (Scientific Papers, Vol. III, p. 102, Cambridge University Press, 1920) expresses the amount of wave front deformation $\delta$ produced by a body of air with a path length of $l$ differing in temperature from the surrounding air by $t$ degrees Centigrade:

$$\delta = 1.1\ lt \times 10^{-6}$$

2. Meteorologie der Strahlstroeme, Springer-Verlag, Vienna, 1961, p. 188.
3. Boutet, 1950, Annales de Geophysique 6 (4): 322-330; Gifford and Mikesell, Weather 8 (7): 195-197; Keller, 1952: Ohio State University Research Foundation Contract AF 19 (604)-41, Technical Report No. 1; Protheroe: Journal of the Optical Society of New York 45 (10): 851-855; Royal Meteorological Society 1954: Quarterly Journal, 80 (344): 241-260.
4. Gifford, 1955, Bulletin American Meteorological Society, 36 (1): 35-36; Gardiner et al., 1956: Lowell Observatory, Contract AF 19 (604)- 953, final report, 1956.
5. Science, 17 Nov., 1961, Vol. 134, p. 1596.
6. Astronomical Journal, June, 1949, Vol. 54, p. 155.
7. Op. Cit., Notes 5 and 6.
8. How to Make a Telescope, Interscience, New York, 1957.
9. These studies would never have been attempted but for encouragement given by three scientists whom I met as a newsman attending a symposium at Colorado State University, Sept. 1961: Dr. Wallace Brode, president of the Optical Society of America; Dr. Norman Hillberry, Director of Argonne National Laboratories; and Dr. Herbert Riehl, Director of the Dept. of Atmospheric Science, Colorado State University. Told of my amateur astronomer's interest in the "flying shadows", each of the three said, in effect: "amateur or not, it's your project -- investigate it."
10. Hoffeit, Sky and Telescope, Jan., 1950, p. 58.
11. At least one bright planet is available at some time during most nights of most years. Observations reported here, however, were interrupted by the progress of all major planets toward conjunction with the sun in Feb., 1962.
12. Science, 17 Nov., 1961, Vol. 134, p. 1595.
13. Astronomical Journal, June, 1949, Vol. 54, p. 155.
14. Loc. Cit., note 5.

Remarks by Editor. Mr. Newell will welcome constructive criticism of his paper by qualified readers. His address is 234 Metz Ave., San Antonio 23, Texas. The apparent correlations found with wind shear and temperature inversions will stand on far firmer ground if they are confirmed by other studies with a larger number of observations, at different stations, and at many different times of the night and of the year. Should enough interest develop, the study could even be undertaken as a special A.L.P.O. project. Much attention might need to be given to the determination of suitable observational techniques.

It is symptomatic of the wide interest in the general problem discussed above that one of the future projects at the White Sands Missile Range, where the Editor works, is a study of "natural atmospheric turbulence". The experiment is called Aerospace Photographic Reconnaissance Experiment (APRE), and the principal contractor is Minneapolis Honeywell. The method of securing data will be to photograph a resolution pattern on the upper wing surfaces of an aircraft from a camera borne in the gondola of a balloon. The aircraft will fly at selected altitude differences below the balloon, thus changing the atmospheric layers in the optical path.

## KLEIN'S "NEW" CRATER - ANOTHER LUNAR PUZZLE

By: Francis J. Manasek

(Abstract of paper presented at the 1962 A.L.P.O. Convention at Montreal.)

The development of selenography during the 19th century was discussed as a contributing element to the widespread search for lunar surface changes. The role of such influential lunar observers as Birt and Klein was also examined, and the "discovery" in 1877 by Klein of a "new" crater in the Mare Vaporum near Hyginus was examined in the light of the above circumstances. The crater was described by Klein as being about 4000 meters in diameter, darkly colored and without a wall. Edmund Neison stated that his observations indicated that the crater was not present (i.e., not observed by him) between 1870 and 1875.

Following Klein's announcement, the crater was extensively observed, and many descriptions of it seem to indicate that it was a structure whose visibility was very sharply limited by lighting conditions. It was frequently recorded as a hill, and the surrounding region often appeared dark with no traces of the crater visible.

Modern observations substantiate the existence of the object, although its diameter is much less than Klein's estimate of 4000 meters. It is, of course, impossible to say whether Klein's crater really was a "new" object or not.

At the conclusion of the presentation of this paper, Mr. Ernst Both made several comments. He noted that Klein had made several such "discoveries" and was almost obsessed with the notion of finding lunar changes. He also mentioned that the data given by Klein in his announcement of the "discovery" were not clear enough to allow for positive identification, and much confusion resulted when other observers tried to locate the object.

## LUNAR METEOR SEARCH

By: Madame Jean Pierre Jean, Le Centre Francais,Montreal

(Paper read at the 1962 A.L.P.O. Convention at Montreal.)

Prior to July, 1961, no observation had been undertaken although I had become interested on learning that some studies were being made to ascertain whether meteorites reach the surface of the Moon, and if so, whether impacts can be observed. The amount of meteoritic matter reaching the Earth suggests that proportional amounts must also fall on the Moon; and since the Moon has little or no atmosphere, there would be some definite probability that meteorites of great enough size may produce detectable flashes on impact with the lunar surface.

In July, 1961, with one other observer, I set up a station at my home in Montreal using my 3½-inch refractor, and this station has been active ever since.. I interested a few others who joined the observing group at my house from time to time; and observations continued with changes in the numbers, and in those attending, and so the existence of the station became known to the Centre Francais. An announcement was made in their Bulletin, which created some further interest in these observations. The group became larger by means of the addition of interested persons unconnected with the astronomical society so that my home became the central point for these observations. Several of the members brought their own telescopes, and so with several instruments in operation the Moon was kept under constant observation during the whole of the periods required by the A.L.P.O. Lunar Meteor Search on the three assigned nights per month when weather conditions permitted the schedule to be followed.

Interest continued and observing experience was gained to the point where it became desirable to set up another station. With more than one station in operàtion, confirmation of impacts may be achieved; and continuity of observation is assured should local conditions interfere, as with the Moon in certain positions, city lights, buildings, or other obstructing objects. The first station to be set up, other than at my home, was at Ste. Genevieve de Pierrefonds, using a 2½-inch refractor; and later another group was established in Montreal at a North End location, where an 8-inch reflector was available. A fourth station is being organized at Ste. Dorothee, on Isle Jesu, which will also use an 8-inch reflector.

During observations a varying intensity has been noted in the faint illumination of the dark part of the Moon's disc, at times some features being discernible; and a cone-shaped form has been seen extending from the terminator to the limb of the Moon in the region of the equator. In winter observations distinct violet color was seen along the terminator, which although not reported by other observers in Canada or in the U.S.A. was noted by many observers in England.

## THEORETICAL ASPECTS OF THE LUNAR METEOR

By: Kenneth Chalk, A.L.P.O. Lunar Meteor Search Recorder

(Paper read at the Tenth A.L.P.O. Convention at Montreal.)

For a number of years now the A.L.P.O. has sponsored a systematic program, the aim of which is to discover whether it is possible to observe a meteor on the moon. The method is simple: participants observe the unlit part of the moon simultaneously for three scheduled days each month, when the moon is still a crescent. Any flashes or trails seen against the moon's disc are reported to the Recorder. When these are compared later, it becomes obvious whether these flashes are terrestrial or lunar in origin. A terrestrial meteor at the center of the disc for one observer would be completely missed by another station more than 0.22 miles distant. A lunar meteor would be visible to all observers with large enough telescopes.

Telescopes ranging in size from 3 to 16 inches in aperture have been employed in this program; but although emphasis is placed on the importance of larger instruments, most of those currently in use in the search are only 6 - 8 inches in aperture. Observers scan the entire unlit part of the moon for faint, slow-moving points; and the fast, bright streaks that are obviously terrestrial are usually ignored.

The indirect aim of the Lunar Meteor Search is to discover the approximate extent of the very tenuous lunar atmosphere. This question is still in a very unsettled state, with the results of different observers varying by several orders of magnitude. A positive result of the Lunar Meteor Search might at least set a lower limit to the possible surface density of the lunar atmosphere.

## The Lunar Atmosphere

Jeans has computed that if the r.m.s. speed of the gas molecules in an atmosphere is 1/5 the velocity of escape, then it will take several hundred million years for half the gas to escape. If the ratio is as much as 1/4, then half will escape in several thousand years; and if it is less than 1/6, then the gas will remain indefinitely.

Selected Gases Showing r.m.s. Speeds and Value of $\sqrt{\bar{V}^2}/V_E$ for the Moon

| Gas | $\bar{V}$ (0°C.) | $\bar{V}$ (100°C.) | $\sqrt{\bar{V}^2}/V_E$ |
|---|---|---|---|
| Nitrogen | 0.49 kms./sec. | 0.57kms./sec | 1/4 |
| Oxygen | 0.46 " " | 0.54 " " | 1/5 |
| Argon | 0.41 " " | 0.49 " " | 1/5 |
| Carbon Dioxide | 0.39 " " | 0.46 " " | 1/5 |
| Krypton | 0.29 " " | 0.34 " " | 1/6 |
| Xenon | 0.21 " " | 0.25 " " | 1/9 |

We see that nitrogen, oxygen, and indeed all lighter gases will be absent from the lunar atmosphere, while its most probable constituents are krypton, xenon, and traces of argon and carbon dioxide.

Having established that some gases might be retained in an atmosphere around the moon, it is necessary to consider its possible density at ground level and in the regions where a meteor might be expected to become luminous. The results of different investigators lead to widely divergent results, and it is not the purpose of this paper to evaluate the reliability of these different values. A few experimental results show the range of possible conditions:

Pickering, by measuring the separations of double stars during lunar occultations, observed relative displacements of 0".2 to 0".4. These indicated a ground density of 1/8000, or 1.25 x $10^{-4}$, that of our own atmosphere.

Fessenkoff, in 1943, claimed to have found no lunar atmosphere greater than $10^{-6}$ our own in ground density. He relied on polarization to identify light scattered by the lunar atmosphere.

Lipski, in 1949, claimed to have discovered an atmosphere with a ground density of $10^{-4}$ our own.

Dollfus, investigated possible lunar twilight with the 20-cm. coronagraph at Pic du Midi, and claims he would have detected the presence of any lunar atmosphere with a ground density greater than $10^{-9}$ our own.

It is necessary now to consider the densities or pressures likely to be encountered where lunar meteors would be expected to become luminous. In our own atmosphere, this layer occurs mainly between altitudes of 45 and 100 kms.

There is given below a relation which expresses the approximate variation of atmospheric pressure with altitude. By substituting the values shown for Earth and Moon, we obtain the graphs of log P vs. h shown in Figure 13.

## Calculation of Graph: Log P vs. h

The following approximation was used to relate height in the atmosphere (h) with atmospheric pressure (P):

$$h = -\frac{R\,T}{M_0 g}\log\frac{P}{P_0}\log e.$$

where h = height in kms.
R = 8.31 joule $mole^{-1}$ $deg.^{-1}$.
T = average Absolute temp.
$M_0$ = average molecular wt., gms.
g = acceleration of gravity.
$P_0$ = ground density of atmosphere.

Into this expression the following values were inserted. The graphs of Figure 13 were worked out from the relation below:

| Earth E | Moon M |
| --- | --- |
| T = 288° K | T = 288° K |
| $M_0$ = 29 gms. | $M_0$ = 45 gms. |
| g = 981 cms./sec.$^2$ | g = 163 cms./sec.$^2$ |
| $P_0$ = 1 atm. | $P_0$ = various. |

$$\text{Log } P - \text{Log } P_0 = \begin{matrix} E & -5.129\ h. \\ M & -1.323\ h. \end{matrix}$$

Here all ground densities considered, with the exception of the lowest, indicate that a meteor on the moon might flare into brilliance within altitudes similar to those observed on Earth. It is only for values in the neighborhood of those obtained by Dollfus that there is no part of the lunar atmosphere dense enough to cause a sufficiently bright fireball.

## Brightness of a Lunar Meteor

Because of the great distance to the moon, a meteor occurring there would have to be much brighter than one in our own atmosphere in order to appear the same apparent magnitude. There follow the calculations to correct for the effect of distance. It is assumed here that an observer is unlikely to see a meteor trail fainter than apparent magnitude 4 in his telescope. Using a 6-inch, this means that a meteor fainter than about magnitude 11 is likely to be missed. This assumption allows a safety margin in the calculations.

## Brightness Required for a Lunar Meteor to be Visible from the Earth

A six-inch telescope is taken as an example.

It is assumed that the average distance (d) of a terrestrial meteor is 50 miles. The distance (D) of a lunar meteor would be about 250,000 miles.

Then $\frac{D}{d} = 5 \times 10^3$, and $\left(\frac{D}{d}\right)^2 = 2.5 \times 10^7$.

The brightness of the lunar meteor must thus be $2.5 \times 10^7$ times that of a terrestrial one of the same apparent brightness.
The 6-inch telescope increases the apparent brightness by about $10^2$ (5 magnitudes), and the required brightness factor is reduced to $2.5 \times 10^5$.

To express this factor as a number of magnitudes, n, we solve:

$$2.5 \times 10^5 = 2.5^n.$$

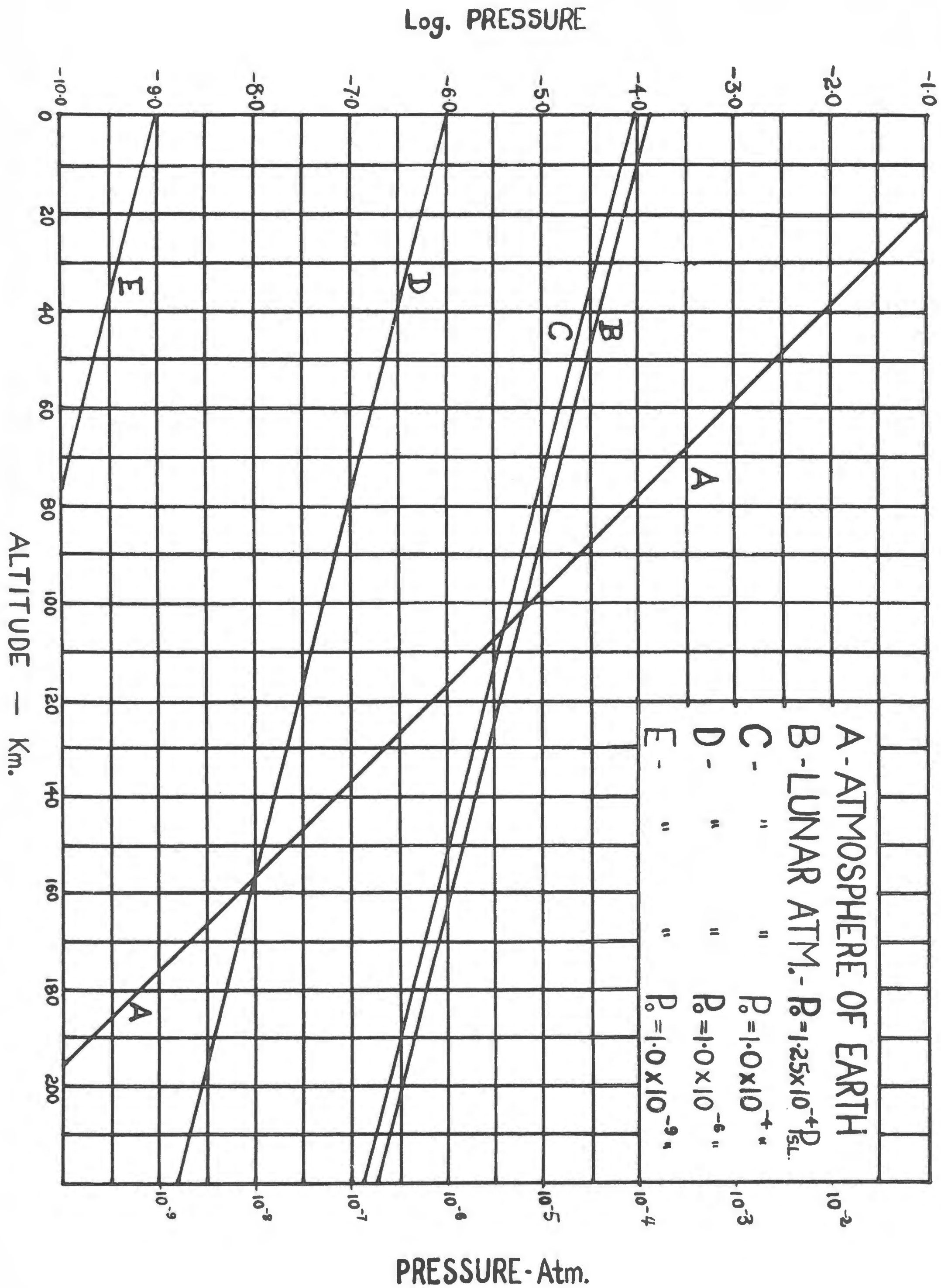


FIGURE 13. Graph of log pressure vs. altitude in kms. in the earth's atmosphere (A) and in the moon's atmosphere for various assumed lunar surface pressures (B,C,D, and E). The unit of pressure is the sea level pressure in the earth's atmosphere. Contributed by Kenneth Chalk. See page 21 for Mr. Chalk's development of mathematical relation between log pressure and altitude.

This becomes:

$$\log 2.5 + 5 = n \log 2.5. \text{ (Common logs.)}$$

Solving for n,

$$n = 13.6 \text{ magnitudes.}$$

Hence a meteor near the moon must be nearly 14 magnitudes brighter than a 4th mag. terrestrial one if it is to be reported, or it would have a magnitude of about - 9.6 at a distance of 50 miles.

It is not known exactly how often meteors of this brightness occur on the earth, but it has been estimated by Millman that nearly 27,000 meteorites reach the earth each year. Since in his tracking program observers are asked to report anything brighter than magnitude -2, it may be inferred that a meteor need not be mag. -10 before the meteorite can reach the ground, and so these probably form a small fraction even of those striking the earth's surface.

Now on the same basis, but considering the smaller diameter of the moon, and the fact that observations cannot at any one time be made of more than about 3/8 of the lunar surface, the number of annual lunar meteoritic impacts is only about 525. Observers in Montreal watch no more than 2 hours per month, on the average, or 24 hours each year. Hence the number of impacts during this time is only 1.4 per year on the average, and the number of sufficiently bright fireballs is much smaller.

In the four years from October, 1957, to September, 1961, members of the Montreal Centre of the Royal Astronomical Society of Canada observed on a total of 82 scheduled nights, during which time they reported 12 flashes. All these are unconfirmed. Owing to weather conditions, this does not represent 82 hours of observation, but perhaps 3/4 as much. Obviously, many of these flashes were of terrestrial origin. The lack of the lunar meteors which would be expected may be explained in a number of ways. First, the small number of -10 fireballs. Second, some of these flashes might have been confirmed had other large telescopes been in use at the time. A 6" might detect a flash which would be entirely missed by a 3". Often there was only one telescope on duty, so no confirmation was possible.

Figure 14 shows a selection of the trails that have been reported by observers during the past four years. The first is an example of the expected appearance of a lunar meteor trail as described by Öpik in Moore's Guide to the Moon. Other reports have shown phenomena including points of light, trails across the moon, and everything between. It is not impossible that some of the shorter trails may have been lunar meteors which were missed by other observers.

At any rate, the results to date of the A.L.P.O. Lunar Meteor Search in no way contradict the assumptions which make the sighting of one of these objects possible. The question of the lunar atmosphere and of the lunar meteor is still open.

Observers are cordially invited to write the Lunar Meteor Search Recorder and to participate in this program. Useful coordination of results requires adherence to a preset schedule. During the next few months this schedule is as follows: 1963, April 27, 28, and 29, 8:30 - 9:30 P.M.; May 26, 27, and 28, 9:00 - 10:00 P.M.; and June 25, 26, and 27, 9:00 - 10:00 P. M. All times here are Local Standard Times.

## LUNAR-TYPE TERRESTRIAL VULCANOIDS

By: Patrick Moore, F.R.A.S.

(Paper read at the 1962 A.L.P.O. Convention at Montreal.)

The question of the origin of the lunar formations is still a matter

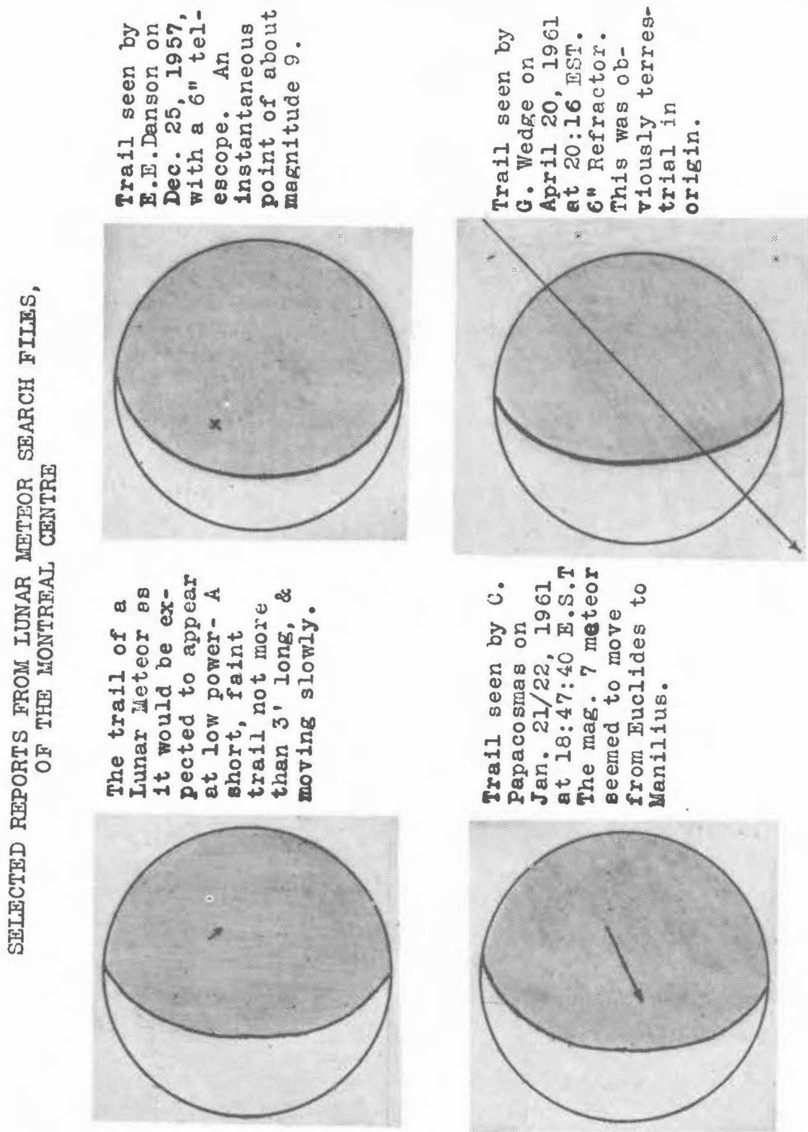


FIGURE 14. Theoretical lunar meteor trail and several sample objects recorded during Lunar Meteor Search observations by members of the Montreal Centre. Contributed by Kenneth Chalk. See also text of his article.

for debate, and the arguments between the supporters of the impact theory and of the igneous theory have aptly been termed the "Hundred Years' War". Of course, it is clear that there must be both impact and volcanic craters, and the main point at issue is whether the really large formations - such as Ptolemaeus - were produced by bombardment or not. I believe not; but I think I am in something of a minority these days.

More than a decade ago an important and most interesting book was written by R. B. Baldwin. Its title was The Face of the Moon; no doubt it is familiar to nearly all members of the A.L.P.O. In this book, Baldwin dealt with terrestrial meteorite craters, and maintained that the depth-diameter ratios of meteorite craters, bomb craters, and lunar craters are closely linked, so that when they are plotted a smooth graph is the result. The conclusion drawn was that this result is an important piece of evidence in favor of an impact origin. Behrmann has since questioned some of the figures, and my own measures of crater depths indicate that the correlations are not so perfect as might be thought - but it is certainly true that the discrepancies are not very great, and that it is possible to produce artificial craters which bear an outward similarity to lunar forms. I maintain, however, that this proves nothing at all, if only because there are certain volcanic formations on Earth which fit even better into the graphs.

Several significant features have been described in the Adrar Mauritanien region of French West Africa, in the neighborhood of Richat. They take two well-defined forms: (1) single-walled, craterlike despressions, with diameters ranging from 500 to 1500 meters; (2) large, circular-shaped areas, up to 50 kms. in diameter, in which a concentric disposition of alternating ridges and valleys is evident. Of the five main features, Temimichat-Ghallaman, Ténoumer, and Richat itself are aligned, with the Dôme des Semisyât near Richat, and Guelb-Aouelloul not far off. They are described by T. L. Monod in the Bulletin de la Direction des Mines, 1952. They are undoubtedly volcanic, and the almost circular shapes suggest that the arching of the originally horizontal strata was caused by a laccolithic intrusion of molten igneous material - in the way that I have suggested for lunar forms. Judging from the photographs, the Richat craters look much more "lunar" than, say, the Arizona Meteorite Crater; and the depth-diameter ratios fit in excellently.

I have not seen these features myself, but I have been to an equally interesting group - near Reykjahlið in the Lake Mývatn area of Iceland. I went there in the summer of 1960, and spent some time in examining the fascinating and spectacular formations Hverfjall and Lúdent. My measures may not be precise, but I do not think that they are far out, and they agree with the values given by the eminent Icelandic geologist Sigurður Thorarinsson.

Hverfjall is a striking feature. It is virtually circular; the distance across the wall-crest is 0.78 miles according to my rather rough measures, while one wall attains 200 feet, and the other 300 feet. The spectacular central peak is about 80 feet high. Both the interior and exterior slopes are gentle, so that the climb is an easy one - in fact it amounts to nothing more than hard walking. Our "expedition" consisted of two, myself and my cousin Brian Gulley (who is not an astronomer in any sense of the word, but who helped me greatly with the measures); and it took us only a few hours to reach the crest, starting from Reykjahlið. The photograph given here (Figure 15) was taken from the crest of the northern wall.

Lúdent, which is visible from the wall of Hverfjall, is smaller, and has no central peak; but is interesting because of the various associated craters. It has walls about 150 feet high and a diameter of about half a mile; the shape is regular except for the intrusion of a large crater, Lúdent A, which has a similar depth-diameter ratio. Also similar is a third feature, Hraunbunga.

Of course it would be wrong to suggest that these Icelandic volcanoes are entirely similar to lunar forms; in particular the floors are not depressed below the outer level (at least, not to any appreciable extent). Yet the obvious resemblance is there, and nobody who has been to the region can fail to be struck by it. When we go back to Iceland, sometime during 1962, I hope to make some more exact studies.

It seems, then, that the graphs linking impact craters, bomb craters,

FIGURE 15. Photograph of the volcanic crater Hverfjall in Iceland. Mr. Brian Gulley in foreground. Photograph taken by Patrick Moore in summer of 1960. See also text of Mr. Moore's paper in this issue.

and lunar craters are in no way conclusive with regard to the origin of the Moon's surface features; and I have no doubt that it would be possible to make artificial "igneous" craters which are just as lunar-looking as the impact ones. Meanwhile, it appears that some of the lunar craters are definitely not of meteoritic origin; these are the small chains of the Hyginus type. On the other hand it is only logical to suppose that impact pits are quite common, so that both processes of formation have applied. There are features on the Moon which look remarkably like true terrestrial-type volcanoes, but these also are small, and the large craters are clearly different. My own view is that the forms and the distribution indicate an origin which was neither cataclysmic nor violently explosive, and that a milder uplift-and-subsidence process is much more likely; but a final solution will probably not be obtained until samples of the lunar crust become available for analysis in the laboratory.

## DIMENSIONS OF THE LINNÉ CRATERLET

By: Joseph Ashbrook

1. The observations reported in this note refer to the craterlet (about 1.2 kms. in diameter) that is located within the Linné white patch, in selenographic longitude $+11^\circ.80$, latitude $+27^\circ.73$. In this discussion, we use the terms <u>east</u> and <u>west</u> in the ordinary selenographic sense (i.e., Linné is east of Bessel and west of Archimedes).

2. Just after sunrise on Linné, there is an interval of an hour or two when the east rim casts a prominent exterior shadow. There is a corresponding brief interval just before sunset when the west rim throws a prominent shadow. The long black spire of sunrise shadow can be seen in a telescope as small as a 4¼-inch refractor, according to R. M. Baum[1]. In the older literature, the eminence causing the shadow is sometimes called a hill, mound, or cone; but it is actually the raised rim of the craterlet.

With a 10-inch reflector at 200X, I have observed the east wall exterior shadow on February 4, 1960, and on May 11, 1962, and that of the west wall on July 23, 1962. On each occasion, I made repeated estimates of the shadow length, in order to find wall heights by the method I explained at the 1962 ALPO Convention in Montreal.[2]

3. The details of these observations may be seen in Table 1. There *f* is the estimated shadow length, expressed as a fraction of the

unforeshortened diameter of Bessel (assumed to be 16.1 kilometers); A is the solar altitude above the horizon of Linné; and H is the exterior height of the craterlet rim, in meters.

Table 1. Outer Wall Heights of Linné

| U.T. | | f | A | H |
|---|---|---|---|---|
| 1960, Feb. | 3.996 | 0.6 | 0°.75 | 100 meters |
| | 4.002 | .5 | 0.83 | 101 |
| | 4.010 | .3 | 0.91 | 70 |
| | 4.044 | .3: | 1.19 | 94 |
| 1962, May | 11.009 | .7 | 0.79 | 119 |
| | 11.010 | .6 | 0.80 | 108 |
| | 11.012 | .6 | 0.82 | 112 |
| | 11.015 | .5 | 0.85 | 110 |
| | 11.017 | .4 | 0.87 | 86 |
| | 11.022 | .4 | 0.93 | 93 |
| | 11.024 | .5 | 0.95 | 115 |
| | 11.025 | .4 | 0.96 | 96 |
| | 11.026 | .3 | 0.97 | 75 |
| | 11.031 | .3 | 1.02 | 80 |
| | 11.033 | .35 | 1.06 | 95 |
| | 11.037 | .3 | 1.09 | 85 |
| | 11.049 | .28 | 1.23 | 85 |
| 1962,July | 23.357 | .5* | 1.94 | 65 |
| | 23.359 | .7* | 1.92 | 89 |
| | 23.362 | .7* | 1.89 | 88 |
| | 23.363 | .8* | 1.87 | 99 |
| | 23.365 | .9* | 1.86 | 107 |

In several cases, marked by * in Table 1, the unit of estimated shadow length was the diameter of Aratus C, which according to Arthur[3] is 0.0022 times the lunar radius.

Taking averages and mean errors for each night of observation, we have:

1960, Feb. 4, East wall: $H = 91 \pm 7$ meters (4 observations).
1962, May 11, East wall: $H = 96 \pm 4$ meters (13 observations).
1962,July 23, West wall: $H = 90 \pm 7$ meters (5 observations).

4. From my observations, it appears that the rim of the Linné craterlet rises about 94 meters above the surrounding plain, and that there is little difference between the east and west rims. There are three other determinations, none of a very satisfactory character, that may be compared with my result:

a. J.F.J. Schmidt's crude estimates of 1866-68 average about 90 meters for the east rim height.[4]

b. W. R. Birt[5] measured a few early sketches by Huggins, Noble, and Tacchini, deducing 81 meters for the east rim, 150 for the west. This result deserves little weight, because of the large uncertainty of shadow lengths in drawings not made especially for the purpose of height evaluation.

c. W. H. Pickering estimated the extent of the exterior shadow with the Harvard 15-inch refractor on two nights in 1897, when the sun had risen on Linné about seven hours and four hours before respectively. Pickering's result[6] of "rather over 40 meters" is probably too small, because he observed too late after sunrise, and so saw a very short shadow that presumably ended on the lower slopes rather than on the surrounding plain.

5. We next consider the more difficult problem of the interior depth

of the Linné craterlet. On May 13, 1962, with the 10-inch reflector at 210X, I managed to make six estimates of the east-west length of the interior shadow cast by the west wall. The comparison craters were Linné A and Linné E, whose diameters Arthur[3] gives as 0.0024 and 0.0033 lunar radii, respectively. The average of these comparisons gave the shadow length as 0.00088 lunar radii at 1962, May 13.016, when the sun's altitude as seen from Linné was 22°.2. The depth of the crater comes out at 602 meters. This value is quite uncertain, because of the difficulty in judging the size of the tiny shadow inside the bright patch. There appears to be no previous published determination.

6. Several types of further observations are suggested by this beginning:

a. More estimates of exterior shadow lengths are desirable, particularly at evening illumination. I was surprised to find my few west wall observations to give practically the same results as for the east wall, as some Linné observers have reported a qualitative impression that the west wall is loftier than the other.

b. Observers with large telescopes are invited to make quantitative estimates of the interior shadow. Statements that on such a time on such a date, the east-west diameter of the craterlet was 0.7 in shadow are of great value.

c. Watch Linné carefully as the terminator crosses it. At sunrise, time when the first speck of light becomes visible as sunlight touches the rim. At sunset, it is easier to determine the moment when the last trace of illuminated craterlet rim blacks out. With larger amateur telescopes, these times should be determinate to perhaps two or three minutes. From such observations, the rim height can be very simply calculated by John Westfall's formulae.[7]

d. There are many formations very similar to Linné that have received far less observational attention. For example, Posidonius Gamma and Lassell D are likewise large bright spots with small, deep, nearly central craterlets. All of the techniques mentioned in this note can be applied to them also.

7. Conclusions. The craterlet inside the Linné bright spot has been found to have walls 94 meters higher than the surrounding plain, and to be (with much uncertainty) 602 meters deep. Its diameter is approximately 1.2 kilometers. Observations of this craterlet and of those in other lunar bright spots are important for extending the empirical diameter-depth and diameter-height relationships to smaller objects.

References


1) R. M. Baum, Strolling Astronomer, 10, 93-94, 1956.
2) J. Ashbrook, Strolling Astronomer, 16, 214-216, 1962.
3) D. W. G. Arthur, Communications of the Lunar and Planetary Laboratory, 1, No. 11, 1962.
4) J. Ashbrook, in a discussion of Schmidt's Linné observations, now being prepared for publication.
5) W. R. Birt, Monthly Notices of the Royal Astronomical Society, 29, 63-68, 1869.
6) W. H. Pickering, Harvard Annals, 32, Pt. 2, 201, 1900.
7) J. E. Westfall, Strolling Astronomer, 10, 127, 1956.


Note by Editor. A.L.P.O. lunar observers are very strongly urged to follow up the important suggestions for useful lunar investigations made in Dr. Ashbrook's paper. As is often true, the possessors of the larger telescopes, say 10 inches in aperture and more, will have the greater opportunities. The observations made should be mailed to Lunar Recorder John E. Westfall, 3104 Varnum St., Mount Rainier, Maryland. The Editor will undertake to forward any observations which are sent to him.

## A NEW SIMULTANEOUS OBSERVATION PROGRAM

By: Charles H. Giffen

Abstract. In 1961, a Simultaneous Observation Program for the A.L.P.O. was organized and carried out under the direction of Clark R. Chapman (1, 2, 3). It was very successful, even for the limited support it received. Many people have expressed the desire for it to be repeated and enlarged. In his conclusions about the first program, Chapman has pointed out the need for this action. Recently, especially since the Tenth A.L.P.O. Convention in Montreal, numerous discussions have taken place concerning a reopening of the Simultaneous Observation Program. A consensus of opinion indicates that this should be done.

Therefore, a new Simultaneous Observation Program (S.O.P.) has been set up through the cooperation of A.L.P.O. officials to run from mid-April, 1963 through the year 1964 and perhaps longer. This article describes that program, its objectives, its observational requirements, observers' participation procedure, and other details. Some attention is given to the various opinions expressed by members of the A.L.P.O. on the subject of the old and a new S.O.P. Finally, a schedule of the "targets" through June, 1963 is given together with the particular types of observations to be made.

Two items must be given special mention concerning the new S.O.P. One is that of extensive participation for each target and project. The second is that of adhering to the prescribed observational procedure for each target and project. These factors are important because the success of such a participation program depends entirely upon the quantity and the quality of the observations contributed.

1. Opinions and Objectives. This section of the paper has been compiled from the comments of many people, but particularly from Chapman, Director W.H. Haas, and the writer. Some of the opinions have been expressed elsewhere, but no attempt has been made to credit any one source since the various opinions have many backers. Not always have people agreed on all points. Responsibility for this interpretation of thought presented here should not be placed upon anyone but the writer.

First, we turn to the need for active participation. Any new S.O.P. needs the intensified support of many participating observers, much more so than in the 1961 program. Fifteen contributors per target date seems to be about minimum for an effective analysis of data in most situations; twice that number might still prove to be deficient for other projects. In 1961, the total number of participating observers was only 36, and the largest contribution for any target date was only 18. For a new S.O.P., total participation from the A.L.P.O.'s membership is urgently needed.

While essentially a program for the study of visual observations, a new S.O.P. needs extensive contributions in the form of photographic observations made in conjunction with the visual studies. Chapman had only one usable photograph (out of three that were submitted) in 1961. It has recently been found that remarkable results can be obtained by means of photographs; the usefulness of these in analyzing visual studies is obvious and should be exploited to every extent in any new S.O.P.

Participation in 1961 was not only limited, but often observers made rather useless observations -- perhaps because of misinterpreting Chapman's instructions, observers' unpreparedness, and (almost certainly) plain carelessness. Much of this type of thing has been pointed out in the work of the different A.L.P.O. Sections and has contributed to the generally deplorable state of affairs which Recorders, readers, and conscientious observers are confronted with. Not only should a standard of excellence be established for observations in a S.O.P., but this should be extended to and maintained in all A.L.P.O. work. Observers are not compared just at times of simultaneous observing projects; they are compared every time a Recorder receives and studies observations received from them and every time their observations appear in print.

There are arguments for making a new S.O.P. a semi-permanent part of the A.L.P.O., all of which are very convincing, provided that the extent of participation given to such a program remains sufficiently great and provided that useful results for the program continue to be obtained. These results must be results about simultaneous observing per se. Such a program must not supplant the regular work of other A.L.P.O. Sections, even though it may often actually help these Sections, and vice versa. Much has been said also about making any semi-permanent S.O.P. a sort of "training" function for new observers; this has been perhaps damaging publicity which might turn more experienced observers away from such a project.

Instead, a new S.O.P. should receive the attention of observers from all levels of experience (as the requests for total participation also indicate). There are several reasons for this, including the fact that observers may be rather skilled in some observations, but no observer is skilled in all observations; we should never stop trying to perfect ourselves, for we are never perfect. The most useful things to be derived from total, intensive participation are those which deal with the correlation and interpretation of observations. Chapman found that many observers showed systematic errors; a new S.O.P. should investigate this aspect extensively as a means of determining the nature and amount of personal errors so that these may be applied to other observations submitted to the A.L.P.O. Sections. Following this line of thought, a new S.O.P. should further investigate the possibilities of other errors in observations, systematic and random. We remark that these considerations indicate that much will be available for the A.L.P.O. Recorders to learn from such a program; it is important that they take fullest advantage of this.

Any new S.O.P. should approach specific problems from many angles. I. e., any effective S.O.P. must be flexible. In particular, a set pattern cannot be used for the various targets and projects; Chapman's case was different, where he was studying essentially the results that could be obtained from planetary drawings. Also, such a program will not consist entirely of large scale target dates, but it will include smaller special projects and local, controlled projects. In this connection, the ingenuity and enthusiasm of every individual observer and Recorder should be exercised. Nevertheless, all such efforts should be contributed directly to the S.O.P., so that they may be compared with other studies. Simulated observations of various types provide much information about visual capabilities and should be attempted (4); such a project is being planned for the Eleventh A.L.P.O. Convention in San Diego in August this year.

While a new S.O.P. is in effect, it should be used for trying out newer observational techniques (in order to evaluate them). It might also be used for experimenting with others which are less well thought out. It can certainly provide a basis for the comparison of different observing methods (5).

This, then, is approximately what considered opinion says any new S.O.P. should be. What it will be is a different story, to be determined by the participation given, the nature of the results obtained, and the person(s) administering the program. Although the direction of the new program is presently under the sole supervision of the writer, it is expected that this will be effectively changed shortly; already, the help of several members of the A.L.P.O. has been elicited in the preparation of the new program. The nature of this change will become apparent in an announcement to be made shortly. The rest will depend upon those for whom this project is intended, the observers, and upon the course of events.

2. Observational Participation. As indicated, participation is sought from every contributor to the A.L.P.O. For the most part, the observational procedures will be almost identical with the "standard" A.L.P.O.

Section procedures prescribed for a planet, where these exist. In some instances, special observations or techniques will be requested, and these must be complied with to ensure success for the project being carried out. Miscellaneous observations will be welcomed for any target or project, but observers should not try to invent their own procedures to supplant those of the S.O.P. Always, written reports should accompany the observations submitted.

Observations should be submitted (if at all possible) within two weeks after each target to the writer at the following address:

Charles H. Giffen
Mathematics Department
Princeton University
Princeton, New Jersey.

Every effort should be made to have each observation reach the writer no later than three or four weeks after it has been made, as these will be forwarded to the appropriate A.L.P.O. Recorders immediately after being analyzed. If it is wished that drawings or other observations submitted be returned, then copies should be sent with the originals together with an instruction for the observations to be returned and a stamped, self-addressed envelope for this purpose. If no extra copies are sent with observations which are to be returned, then they will be returned by the Recorders after they have seen them. Observers may also wish additional information or results about particular targets to which they have contributed observations, and should feel free to ask.

Drawings will usually be requested for most targets and projects. They are of importance because of the extensive usage of them in A.L.P.O. programs. Whenever an observer is drafting a sketch, extreme care must be given to the accurate placement and representation of details seen. Always, observers should comment upon their judgment of the quality and subjectivity of the drawings -- or of any observation. In addition, the the standard observing forms for the various A.L.P.O. Sections should be used for one's drawings, whenever these are available and suitable.

Written reports for each target observation should be made and should supply all data and miscellaneous information not otherwise given in the observations, especially as regards observing conditions, methods, subjectivity, etc.

If nothing else, observers cannot be too wordy or prolix in furnishing information for the new S.O.P. Attention should be given to each detail, however minute and unimportant it may seem. Observers should not, in their efforts to leave no details untouched, overlook standard, important data such as time, date, seeing, transparency, etc.

3. Observational Techniques. This section only summarizes the "standard" types of observations to be made in most situations, and it gives an abbreviation for each which will be used in the S.O.P. target schedule later in this paper. The target schedule will describe other techniques to be employed.

Central meridian transits will be denoted by T. These can be made in a number of well-known ways (with or without micrometers or with the "high-low-center" method of estimating times just before, just after, and just at the central meridian passage of features); care should be taken to indicate how these are obtained and also the relative certainty of each observation.

Intensity estimates will be denoted by I. These, too, can be made in different ways; however, the A.L.P.O. scale should be used, with 10 brightest and 0 darkest. Again care should be taken to explain how the intensity estimates are secured. The best procedure is to determine the intensities of a very bright object and a very dark object and then the intensity of an object intermediate in brightness in terms of the estimates secured for the first two objects; then one proceeds to estimate the brightness of the rest of the objects, always trying to work about mid-way (in intensity) between two objects whose brightnesses have been determined rather early in the process. When using this method, one should indicate the order in which the estimates have been secured (i.e., number them consecutively as they are made), and then after each new estimate is secured, the numbers of the earlier "standards" should be given in parentheses. It is clear that the earlier observations in such a sequence make the best comparison standards for new intensity estimates.

Conspicuous estimates will be denoted by C. These should be made according to the standard A.L.P.O. procedures, but observers should explain in writing the criteria used in arriving at the estimates. A procedure similar to that outlined above for intensity estimates might be imitated.

Drawings will be denoted by D. The standard information about observing conditions, instrument, location, magnification, etc. should be included together with an appraisal of the quality of the drawing.

Phase estimates will be denoted by P. The quantity we wish to measure in k, the fraction of the planetary disk that is illuminated, which is numerically equal to the ratio of the apparent width of the illuminated planetary disk to the apparent cusp-to-cusp diameter. Estimates can be made directly; but they can be made more accurately in the following way, which should be employed whenever at all possible. A circle representing the total (illuminated plus unilluminated) planetary disk is drawn, at least an inch in diameter (usually about two); this should be done without extra marks to indicate north-south points or other opposite points on the circumference. Then, while at the telescope, the observer should sketch in the terminator very carefully so as to obtain a drawn outline exactly representing the shape of the image as seen by that observer. The unilluminated portion of this representation of the planetary disk should be hatched out with parallel lines ( ///// ). The time of this observation should be carefully noted on the representation, and this drawn representation itself is then sent in as the phase estimate.

Satellite phenomena will be denoted by S. The timing of first, second, third, and fourth contacts should be attempted as well as times of estimated mid-phenomena and central meridian transits of satellites and their shadows. Central meridian transits of satellites or shadows should be isolated from other central meridian observations and included with the timings of other satellite phenomena. In the schedule of targets, the Universal Times of satellite phenomena from the A.E.N.A. will follow the abbreviation.

Filter observations will be denoted by F. The notations B, G, Y, or R following F will indicate observations to be made in blue, green, yellow, or red light, respectively. Recommended filters for the various colors are: blue, W 47, W 47B; green, W 57, W 58; yellow, W 15; red, W 25, W 23A. Other approximate equivalents may be used; it is absolutely necessary that the particular filters employed be indicated together with the magnifications employed with each filter during each observation with them. Allowance should be made for the dimming caused by the filters. The type of observation to be made with a filter will be indicated whenever possible by the appropriate abbreviation. Thus, F-B, G, Y-I, C, D would indicate that intensity estimates, conspicuous estimates, and drawings should be made with filters for blue, green, and yellow light. Whenever filters are employed in any observation, they should be indicated.

For observations indicated by the abbreviations, the abbreviations will be given in order of (approximate) decreasing importance. The following types of observations for the various planets will be adopted as standard:

| | |
|---|---|
| Mercury | P; D; I; C; F-G, Y, R-P, D, I, C. |
| Venus | P; D; I; C; F-B, R, G, Y-P, D, I, C. |
| Mars | D; T; I; C; F-B, R, Y, G-D, I, C. |
| Jupiter | T; D; I; C; F-G, Y, R-I, C, D. |
| Saturn | T; I; D; C; F-G, Y-I, C, D. |

The word "standard" in the schedule means that the particular sequence of observations should be made as given above. It is understood that transits (T) are to be made throughout an observing session for Jupiter, Mars, and Saturn. Also, for Mercury, Jupiter and Saturn, filter (F) observations may include those made in blue light (B), whenever a filter is available that does not cause too much dimming; in the red, the same holds true for Saturn.

Photographic and micrometric observations should be made whenever possible, preferably together with visual observations by the same observer, but preference should be given to photographic observations if a choice seems necessary.

4. Schedule of Targets. Table 1 lists the targets through June, 1963. Times for Mercury and Venus are in Local Time; others are in Universal Time. The table includes the times during which observations for the S.O.P. should be made, together with the types of observations to be made (standard or special or other) and the times for particular events (drawings indicated by D, satellite phenomena by S ). In the text below is a description of a special Mercury project being undertaken. In future issues of The Strolling Astronomer, we shall give a schedule for the next three or four months after June, 1963, together with a description of special projects and a short summary of the "standard" procedures and the abbreviations used.

Mercury will be the main target for the month of April and a particularly intense concentration of observations is requested. A similar, smaller scale special project will be carried out in June. A.L.P.O. Mercury Recorder, Geoffrey Gaherty, has kindly helped prepare this project and has given it his full support (recent communication). The only really special features of the program are its intensiveness over a short period of time and the special emphasis being placed upon making accurate phase estimates. If it is possible to observe for an hour or more on any date, several phase estimates may be made; they should be made according to the procedure outlined above. All observations made on the ten dates in April and the six dates in June which are listed in Table 1 should be sent to the writer. All other Mercury observations should be sent to Mr. Gaherty, with the same restrictions as to the necessity of submitting the observations as quickly as possible. Phase estimates made on non-S.O.P. dates may be sent to the writer, particularly if one's observations cannot be submitted to Mr. Gaherty within two weeks after being made. The approximate expected dates for dichotomy at the evening and morning elongations are, respectively, about 21-22 April and 19-20 June.

## References


(1). C. R. Chapman, "A Simultaneous Observation Program", Str. A., Vol. 15 (1961), pp. 90 - 94.

(2) C. R. Chapman, "The 1961 A.L.P.O. Simultaneous Observation Program -- First Report", Str. A., Vol. 16 (1962), pp. 56 - 69.

(3) C. R. Chapman, "The 1961 A.L.P.O. Simultaneous Observation Program --Second Report", Str.A., Vol. 16 (1962), pp. 134 - 140.

(4) C. H. Giffen, "Simulated Visual Dichotomy Observations -- Preliminary Report". Str. A., to appear. Cf. the note on p.6 of the Feb., 1963, Eyepiece.

(5) C. H. Giffen, "Foundations of Visual Planetary Astronomy. I, II," Str. A., to appear soon.

Table I. Simultaneous Observation Program,

Schedule: April 1963 - June 1963.

| Date | Planet | Observing Period | Project and Notes | |
|---|---|---|---|---|
| 5 Apr. | Mars | 02:30 - 03:30 U.T. | Standard. | D at 03:00. |
| 5 Apr. | Mars | 05:30 - 06:30 U.T. | Standard. | D at 06:00. |
| 6 Apr. | Mars | 03:30 - 04:30 U.T. | Standard. | D at 04:00. |
| 6 Apr. | Uranus | 04:45 - 05:30 U.T. | D; I; C. | D at 05:15. |
| 6 Apr. | Mars | 07:30 - 08:30 U.T. | Standard. | D at 08:00. |
| 13 Apr. | Mars | 03:30 - 05:00 U.T. | Standard. | D at 04:30. |
| 17 Apr. | Mercury | P.M. - Sunset L.T. | Special. | Local Times. |
| 19 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 20 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 21 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 23 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 25 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 26 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 27 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 29 Apr. | Mercury | P.M. - Sunset L.T. | Special. | |
| 1 May | Mercury | P.M. - Sunset L.T. | Special. | |
| 19 May | Saturn | 08:00 - 09:00 U.T. | Standard. | D at 08:30. |
| 1 Jun. | Saturn | 07:30 - 08:30 U.T. | Standard. | D at 08:00. |
| 8 Jun. | Saturn | 07:30 - 08:45 U.T. | Standard. | D at 08:15. |
| 8 Jun. | Jupiter | 09:00 - 10:00 U.T. | Standard. | D at 09:30; S at 09:38, 09:45. |
| 15 Jun. | Jupiter | 09:15 - 10:45 U.T. | Standard. | D at 09:30, 10:30; S at 09:20, 09:53, 10:00, 10:37. |
| 15 Jun. | Mercury | Sunrise - A.M. L.T. | Special. | Local Times. |
| 16 Jun. | Mercury | Sunrise - A.M. L.T. | Special. | |
| 18 Jun. | Mercury | Sunrise - A.M. L.T. | Special. | |
| 19 Jun. | Mercury | Sunrise - A.M. L.T. | Special. | |
| 20 Jun. | Mercury | Sunrise - A.M. L.T. | Special. | |
| 22 Jun. | Mercury | Sunrise - A.M. L.T. | Special. | |
| 22 Jun. | Saturn | 08:00 - 09:00 U.T. | Standard. | D at 08:30. |
| 23 Jun. | Saturn | 06:00 - 07:45 U.T. | Standard. | D at 07:00. |
| 23 Jun. | Jupiter | 08:00 - 09:00 U.T. | Standard. | D at 08:30; S at 08:25. |
| 30 Jun. | Saturn | 06:00 - 10:00 U.T. | Standard. | D at 06:30, 08:00, 09:30. |

[Some of the selected target dates, we fear, will already be past when this issue reaches our readers. It is obviously important to begin intensive participation in Mr. Giffen's program as described above as soon as possible. The results may well prove to be extremely informative and rewarding. - Editor.]

## A NOTE ON THE DARWIN DOME AS SEEN IN SMALL AND LARGE APERTURES

By: José Olivarez

The Darwin dome, besides being one of the largest domes on the lunar surface, is one of the most rugged and spectacular domes. The dome's position near the eastern limb of the moon does not deprive it of notice by the casual observer. Yet, it certainly doesn't seem that anybody has bothered to take a close look at the dome lately. Therefore, it is the purpose of the author to give an account of his recent observation of the dome with a 12½" reflector (see Figure 16).

The Darwin dome is transversed by a rill. A second and probably more elusive rill was discovered by H. P. Wilkins in 1924. The second, more elusive rill was not seen by the author on this occasion, possibly due to the blending of the rill with the other profuse detail.

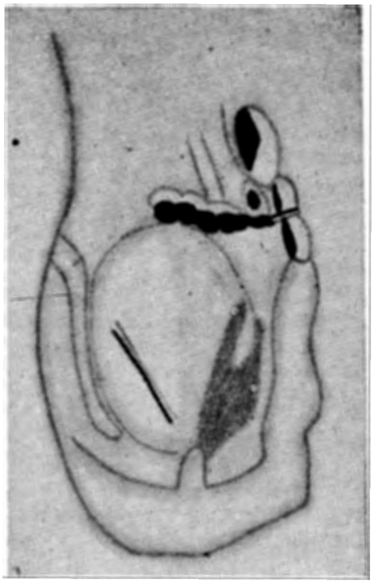

FIGURE 16. Dome in North Part of Lunar Crater Darwin and Environs. Drawing by José Olivarez. 12.5-inch reflector, 235X. June 16, 1962, $3^h$ $10^m$, U.T. Seeing 4. Transparency 5. Colongitude = 71°.1. Lunar south at top, lunar west (in classical selenographic sense) at left.

The dome was certainly not dome-like in the 12½-inch reflector, but instead was a mound of crinkled-like detail. Some of the minute peaks (of which there were many) were seen to project through the lightly shadowed eastern side of the dome. One might say that the dome is a "heap of peaks". Perhaps the best description of the dome was given by R.Barker when observing it with a 12½-inch reflector. He called it a "cindery heap with bristled roughness". This is very true. The dome does not look so rough as it appears through a 12½-inch in a 2.4-or 3-inch refractor. The dome appears rather nicely smooth and of a smaller dimension with small apertures. The dome's dimension appears to increase with larger apertures. This was suspected by the author when he noticed that the dome was of a considerable size in the 12½", while smaller when seen through the same reflector's 3¼" finder-guide refractor. The result is possibly due to the more delicate gently falling and spreading slope of the dome that becomes clearly visible in larger apertures only.

The craterlet chain on the southern side of the dome is certainly most striking. The craterlets get smaller as one goes eastward until the chain becomes a cleft. On the western side of the dome is the rugged wall of Darwin. The rugged peaks from this wall cast beautiful steeple-like shadows over the western edge of the dome.

The observations which the author has just described were made under fair seeing conditions with a good reflector at a very favorable time. The dome was just on the terminator at the time of observations. Interested observers should seek such a favorable condition and observe the dome. They will be well compensated for their trouble (joyous venture?).

Very little has been said in literature generally available to the amateur about the Darwin dome's roughness. It certainly merits observation in both small and large apertures.

## CONCERNING THE USAGE OF EAST AND WEST ON THE MOON

For many years in the past it was universal usage to apply the terms east and west to the moon as the same directions in the earth's sky. Thus with the moon near the meridian in middle northern latitudes, a simply inverting telescope showed west at the left and east at the right. (The diurnal drift in this position of the moon is to the left.) One could also identify the lunar western hemisphere as that of Mare Crisium and the eastern hemisphere as that of Mare Humorum. It did follow, however, that for an observer on the lunar surface, the sun rose in the west and set in the east.

In 1961 the International Astronomical Union passed a resolution to reverse this usage of east and west on the moon. The new system is thus inconsistent with almost all of the large body of lunar literature. It is followed on the new A.C.I.C. Lunar Charts, as many A.L.P.O. members already know. The sun will now, of course, cross the lunar sky in the familiar way from east to west for future astronauts.

On pg. 259 of the November-December, 1962 Str. A. the Editor invited comments on the question of how we should use east and west on the moon in the A.L.P.O. Three colleagues have replied at some length.

On February 11, 1963, Joseph Ashbrook wrote:

"Our main concern is to avoid confusion. Hence I propose that we avoid the astronautical usage as far as possible; any merits it may have are entirely outweighed by the ambiguity it has for the practical telescopic observer.

"For clarity, authors of ALPO papers about the moon should specify in each article whether they are using the familiar selenographic convention (Oceanus Procellarum east of the central meridian, Mare Crisium west of it).

"A convenient way of making this clear might be to specify as 'selenographic' the first place in an article where the term east or west is used. For example: 'I observed the rilles west (selenographic) of Triesnecker . . . .' Or the author might prefer to say explicitly, 'The terms east and west are used in their selenographic rather than astronautical sense in this paper.' The main thing is for no ambiguity to come in.

"My personal feeling is that the IAU adoption of the astronautic convention was very ill-advised, and that practical observers should not follow it."

Dr. Ashbrook remarks that Mr. Charles Federer, the Editor of Sky and Telescope, and Mr. Leif Robinson are in agreement with the point of view described above. He stresses the certainty of confusion and needless misinterpretation of the immense body of descriptive selenographic literature which a change must cause.

David P. Barcroft, the A.L.P.O. Secretary, is also opposed to the new usage. He emphasizes: "We are an association of observers; and we are not astronauts." He wonders how stable the new usage yet is, pointing out that in some modern lunar research north and south are also being reversed. He feels that the confusion which a change would produce far outweighs any advantages for earthbound observers primarily concerned with aspects in our telescopes.

However, the Reverend Kenneth J. Delano of Taunton, Massachusetts is strongly in favor of following the I.A.U. resolution. He feels that we would do very well to adopt an A.L.P.O. resolution in 1963, endorsing the new usage. He points out that ambiguity can be avoided by employing the terms preceding and following, where following is the direction of increasing longitude as determined by the rotation of the moon. (Thus following would be east in the old sense.) The Editor has long favored this use of preceding and following on the rapidly rotating planets Mars, Jupiter, and Saturn. However, is the ordinary lunar observer sufficiently conscious of the moon's slow rotation to permit successful application to our satellite?

We would welcome further constructive comments from A.L.P.O. readers, for we intend to make a decision soon. Once the choice is made, all our members will be expected to adhere to the system chosen. It will also certainly be necessary for some time to make clear in all published uses of lunar east and west in Str. A. just how the writer is employing these directions. Changes always create some confusion, and one must evaluate the advantages and the drawbacks of a new usage before adopting it.

## BOOK REVIEWS

CONSOLIDATED CATALOG OF SELENOGRAPHIC POSITIONS, by D.W.G. Arthur. (Communications of the Lunar and Planetary Laboratory, Vol. 1, No. 11). The University of Arizona Press, Tucson, Arizona. Pages 47-49 + 238 double columns. $5.00.

Reviewed by John E. Westfall

This catalog can be called epoch-making in the sense that, in the future, any serious lunar work in lunar cartography must refer to it. The work is a consolidation of lunar positions from three sources - the measures of Franz, Saunder, and Arthur. A total of 4510 positions are given in terms of direction-cosines $\xi, \eta, \zeta$ to .0001 lunar radius. Also listed, when applicable, are diameters of formations in units of .001 radius (1.08 miles or 1.74 kms.).

As an example of the care used in preparing the catalog, some 323 additional points were rejected and are not included, due to either, (i) excessive differences between observers,(ii) poorly defined features, (iii) inability to find the feature cited, and (iv) uncertainty as to what feature was meant.

Although the main catalog is listed in order of catalog number, most users will refer to Appendix VI, which cross-indexes catalog numbers with the International Astronomical Union name-and-letter system.

The catalog has certain limitations, however, which are not the fault of the author, but rather of the rather primitive "state-of-the-art" of present selenography. For example, although the 4510 points listed represent an impressive amount of effort, they average (for the visible disc) only one point to 1600 square miles (somewhat larger than the state of Rhode Island). Thus, charts of small areas (single formations,for example) will often lack dense enough control for reliable mapping. Secondly, all positions are referred to the ficticious "lunar sphere", with no correction for relief displacement. In conventional, small-scale, orthographic maps, the errors thus entailed will be serious only near the limb. For large-scale, conformal or equal-area maps, however, errors will be noticeable in any area outside the central portion of the disc.

In spite of the above reservations - which are really unavoidable at the present time - I feel that this work is invaluable for the serious lunar student.

TABLE OF B.C. LUNATIONS, Lt. Colonel G.E.B. Stephenson, 124 Duchy Road, Harrogate, Yorkshire, England, 1963. No price given. 14 mimeographed pages.

Reviewed by J. Russell Smith

It is difficult to determine the Western calendar equivalent date of a recorded event in Chinese history. Mr. Stephenson has given us a chronological table of Chinese months with the equivalent date of the start of each month which may be used greatly to simplify date reckoning. The table covers a period of 5,000 years, and the dates have been checked by addition of a convenient multiple of saroses as well as with many solar eclipses mentioned in Chinese records. All dates are in Old Style (Julian Calendar) but may be converted to New Style (Gregorian proleptic calendar) by reference to a conversion table.

TABLE OF A.D. LUNATIONS AND CHINESE INTERCALARY MONTHS, Lt. Colonel G.E.B. Stephenson, 124 Duchy Road, Harrogate, Yorkshire, England, 1963. No price given. 12 mimeographed pages.

Reviewed by J. Russell Smith

This paper is in two parts, and it is a continuation of the author's

Table of B. C. Lunations. The first part is a table of A. D. Lunations. Data for calculating purposes are found on page 2. The table has been compiled from details in Chinese works. Dates earlier than A.D. 1582 are Old Style (Julian Calendar) while those later are New Style (Gregorian Calendar). The second part of the paper is a table of Chinese Intercalary Months starting from B.C. 102 which is B.C. Lunation 1255. In 19 years there are seven intercalary months in order to keep the Chinese lunar calendar linked to the solar year. It is interesting to note from the table how methods of intercalation have changed through the ages.

Reviews of contributions of the American Museum - Hayden Planetarium, New York, New York.

Reviewed by J. Russell Smith

Series 1, Publication 6
Careers in Astronomy, Kenneth L. Franklin, reprinted from Sky and Telescope, September, 1962.

With the beginning of the space age, one finds an increasing interest among students in the field of astronomy. This science, the most comprehensive of all the sciences, attracts a number of students in their pre-college years. However, not enough of these students continue the study of astronomy in college. Along with the space age, we need more and more scientists in astronomy, astronautics, and the related sciences. Mr. Franklin points out that the B.A. degree rarely entitles a person to do anything professionally in astronomy except graduate courses. If one expects to become a scientist of note today, he must continue his schooling until he receives a Ph.D. degree. After this goal is attained, there are many and varied jobs waiting for his talents. Besides teaching and research in a university, we find that industry has opened a new class of positions. The author states that astronomers are now needed to teach engineers the celestial mechanics of satellites and space probes, and to help understand the interplanetary environment and the peculiarities of the stellar background.

The demand in the space field is certain to become greater, and it is up to us to meet a challenge. There are only 1,000 professional astronomers in the U.S. today, and this fact alone makes the field of astronomy quite attractive.

Series 1, Publication 7
The Analemma, Kenneth L. Franklin, reprinted from "Natural History Magazine", October, 1962.

If you have ever wondered about the odd-shaped figure just west of the Galápagos Islands in the Pacific on many maps and globes, then this is the article for you. It is one of the best explanations this reviewer has ever seen. The figure is called the analemma, from the Greek word meaning sundial. Dr. Franklin states that the reasons for the shape of the analemma and its asymmetries involve the geometry of the earth and sun in space - mathematics - and the reactions of the earth to the large mass of the sun-dynamics. In five pages the author gives a lucid explanation of the analemma, and he uses excellent diagrams to aid in the explanation. This reprint is a valuable aid in teaching the causes of the difference between clock time and sundial time.

Series 1, Publication 8
Glossary of Terms Frequently Used in Radio Astronomy, compiled by Kenneth L. Franklin. Distributed by Public Relations Dept., American Institute of Physics, 335 East 45th Street, New York 17, New York. 27 pages, paper, $1.00.

This is a publication that has been needed for some time. It's a guide to understanding ideas and concepts in the recent literature of radio astronomy which has grown so rapidly in the last few years. Dr.

Franklin has given us an explanation of about 135 terms from "altazimuth mounting" to "yagi". Following page 27, one finds a page listing several books on radio astronomy as well as a number of journals which contain occasional articles on radio astronomy. This booklet is highly recommended to anyone interested in radio astronomy and its allied subjects.

## ANNOUNCEMENTS

A.L.P.O. Meeting at San Diego. The Eleventh Convention of the A.L.P.O. will be held at San Diego, California in conjunction with the annual Convention of the Western Amateur Astronomers on August 22-24, 1963. These dates fall on Thursday, Friday, and Saturday. The General Convention Chairman is our own member, Mr. Martin A. Sloan, Route 3, Box 840, Escondido, Calif. The meeting will be in the U.S. Grant Hotel in the heart of downtown San Diego. The hotel was recently redecorated, and the extremely attractive Royal Palm Room will be the setting of the paper sessions. A highlight of the three-day meeting will be a visit to Palomar Mountain on Friday afternoon and evening. The A.L.P.O. papers will be integrated into the overall program in the same way as was done with great success at the Long Beach Convention in 1961. More details will be given in future issues as plans develop.

All A.L.P.O. members who can come are most heartily invited to attend this San Diego Convention. Qualified colleagues are invited to submit papers soon to assist in our planning. These should not require more than 15 or 20 minutes to present, including a brief period for questions from the floor. Members with suitable material are also requested to contribute to the A.L.P.O. Exhibit, more details about which will appear in the next issue.

Astronomical League Convention. The next National Convention of the Astronomical League will be held at the University of Maine in Orono, Maine on July 18-20, 1963. The highlight will be the total solar eclipse on the afternoon of Saturday, July 20. The eclipse will be total at Orono with a duration of 50 seconds, but many may prefer sites on the nearby central line of totality. The host society is the Amateur Telescope Makers of Boston. Summers in Maine offer much to the vacationer. Rooms and meals will be available on the University of Maine campus at reasonable rates. Room reservations should be requested from the Solar Eclipse Convention Committee, University of Maine, Orono, Maine. Registration fees ($2.50 for persons more than 16 years old if sent before May 1) and information requests should be sent to the Amateur Telescope Makers of Boston, Hayden Planetarium, Science Park, Boston 14, Mass.

Mr. Ralph K. Dakin, the President of the Astronomical League, very graciously invited the A.L.P.O. to participate in this Convention. We felt it necessary to decline with regret; we had already accepted the invitation to San Diego, and experience has cast doubt on the wisdom of two meetings in one year. Surely, however, many of our members will want to attend as individuals. We wish the League and them a fine meeting and a very good view of the eclipse.

For the Comet Searcher. Mr. David Meisel, our Comets Recorder, calls attention to the article "Analysis of Comet Discoveries", Journal of the British Astronomical Association, Vol. 72, No. 8, pg. 384, 1962, by M. J. Hendrie. Mr. Meisel thinks that any comet seeker should read this article before going to the telescope.

Congratulations, Gary Wegner. It has been a pleasure to learn that Mr. Gary Wegner, an A.L.P.O. member since 1957, was one of the 40 winners in the 22nd Westinghouse annual Science Talent Search. His subject was "Clues to the Origins of the Planets' Markings". We congratulate Gary on this achievement and wish him every success in his future studies.

New Address for Elmer Reese. Our Assistant Jupiter Recorder has left Uniontown and is at present employed at Temple, Texas. He requests correspondents to use as his temporary address:

Elmer J. Reese
c/o Philip R. Glaser
200 Albert St.
Waukesha, Wisconsin.

Error in November-December, 1962 Issue. Mr. H. M. Hurlburt directs attention to an error in his article in our last issue. On pg. 258, under "Satellites of Saturn", i = 9 (Phoebe), the percentage deviation in the last but one column to the right should be 0.6%, not 6.2%. The error occurred in the manuscript submitted for publication.

Addendum to "Lunar Crater Terracing - A Preliminary Report." Mr. Francis J. Manasek in a letter dated February 17, 1963 requests that the following addition be made to his paper on pp. 245-246 of the November-December, 1962 Strolling Astronomer: "Recent observations (F. J. Manasek, 'Polygonal Crater Distribution', J.B.A.A., in print) of a large number of craters with diameters less than 20 kms. indicate that both polygonal and round (including oval and elliptical) craters are found in all diameter groups but that the percentage of polygonal craters in a given diameter group is proportional to the diameter. In other words, craters of small sizes are less likely to be polygonal than are those of larger diameter. All craters with diameters greater than 20 kms. which were observed had polygonal outlines."

A.L.P.O. Lunar Section Mapping Program. We have received the following invitation to a lunar mapping study from Mr. John E. Westfall, an A.L.P.O. Lunar Recorder:

"The A.L.P.O. Lunar Section currently has available mimeographed outline charts of the Aristarchus-Herodotus region. Interested observers may obtain them through Patrick S. McIntosh, Sacramento Peak Observatory, Sunspot, New Mexico.

"These charts are intended for use at the telescope. Details should be drawn in their correct positions relative to the outlines printed on the map, but observers should not hesitate to correct these outlines if they feel them to be incorrect. The completed sketches, with pertinent information added, should be returned to Mr. McIntosh for evaluation and analysis, in connection with the lunar mapping program outlined in 'Prospects for the A.L.P.O. Lunar Section' in the November-December, 1962 issue of The Strolling Astronomer."

Figure 17 shows the outline chart to be employed in the project just described. The original, but not this published reproduction, is on a scale of 1:500,000. Mr. Westfall transferred detail from the Kuiper Photographic Lunar Atlas to acetate overlays on the scale chosen. He used for this purpose an "Areotopograph", an instrument which normally employs a beam splitter to transfer oblique aerial photographs onto map sheets. Figure 17 is on an orthographic projection.

The Editor strongly urges all interested members to work on this project under the supervision of Mr. McIntosh.

## OBSERVATIONS AND COMMENTS

An Open Letter from the Editor. We receive frequent complaints from members about the failure of Recorders to acknowledge quickly observations and correspondence. It would assuredly be ideal if all staff members, and the Editor most of all, were such model correspondents. It may be, however, that the modus operandi of the A.L.P.O. is not suffi-

A.L.P.O. - LUNAR SECTION -
ORTHOGRAPHIC OUTLINE CHART

ARISTARCHUS-HERODOTUS, 1:500,000

OBSERVER........................ DATE ......... TIME........(U.T.)
INSTRUMENT............ MAG.......... S...(0 to 10) T...(0 to 5) COLONG.........

NOTES (Continue on Back if Necessary):

FIGURE 17. Chart of Aristarchus-Herodotus Region on Moon for A.L.P.O. Mapping Project. See also text on pg. 40.

ciently known to some of our members.

Ours is a volunteer Association. The Recorders are not paid. They give of their own time and, with very few exceptions, of their own money in performing the services which they perform. The Recorders pay for their own memberships like everyone else, and few or none of them would be willing to have it otherwise. Surely, however, a Recorder under these conditions can be forgiven for not rushing to spend postage on letters asking questions answered in elementary books, in acknowledging routine observations sometimes of mediocre quality and sometimes not free from careless mistakes, and in responding to requests for "everything free about astronomy". The political and philosophical climate of the times perhaps encourages us to ask for everything and to offer nothing in return. There is none-the-less much merit in the old-fashioned courtesy of a stamped, self-addressed envelope when one is asking a Recorder for assistance or information. The financial resources of the A.L.P.O. have always been very modest. The recent increases in postal rates, the expense of annual Conventions, and generally rising costs now multiply the budget problems.

It is the efforts of the Recorders that have enabled the A.L.P.O. to accomplish whatever it has to date achieved. Without their help *The Strolling Astronomer* would be very much smaller in number of pages and very much poorer in content. These men are very worthy of your full support. They do and will appreciate your patience and your help in enabling them to do a good job with your observations and other contributions.

**Flamsteed, Milichius, Jansen, Arago Domes, and Longomontanus.** The lunar drawings on the front cover and on pages 43 and 44 are here chiefly presented without editorial comment. The lunar observers among our readers will see much of interest in them, and some will wish to look for details shown at the telescope and also on high-quality photographs.

Mr. Harry Jamieson's observation of the two domes near Arago (Figure 20) is here reported briefly. The estimated diameter of Arago 1 (east of Arago in selenographic sense) was similar to that of Manners, thus 16.5 - 17.0 kms.; Arago 2 (north of Arago) was larger, about 21.5 - 22.0 kms. in diameter. The shadows of the two domes were about the same size. No central pit could be seen in either dome. Mr. Jamieson remarked a valley or depression running from the center to the southeast edge of Arago 2 and an unidentified object on the southwest corner of Arago 1.

**A Professional Request for Amateur Lunar Observations.** The rise of the Space Age has necessarily obliged us to reëxamine the traditional methods of amateur lunar and planetary astronomy. Since the moon is the presumed first target of Man's direct exploration of space, a tremendous amount of current professional scientific research is being done on a body long neglected by professional astronomers. It is thus not strange that some writers in this magazine should feel some doubts about the usefulness of further amateur lunar studies. In this setting the following letter from Mr. William D. Cannell, Chief of the Lunar Observation Section, Lowell Observatory, Flagstaff, Arizona is worth our very careful attention and thought. The letter was written to the Editor on January 11, 1963:

> "1. We have been very pleased to read the many complimentary remarks by ALPO writers in the *Strolling Astronomer* concerning the USAF lunar charts published by the Aeronautical Chart and Information Center in St. Louis, Missouri. However, we are concerned about a tone of discouragement expressed by some of the writers because they feel they are competing with professionals using very large instruments.
>
> "2. The amateur may find it difficult to compete, but surely not difficult to contribute, as he has so well done in the past. The high quality of the USAF lunar charts is due in part to work of the amateur astronomers. The ACIC observers at Lowell Observatory continuously consult the writings of the amateurs to insure that features previously

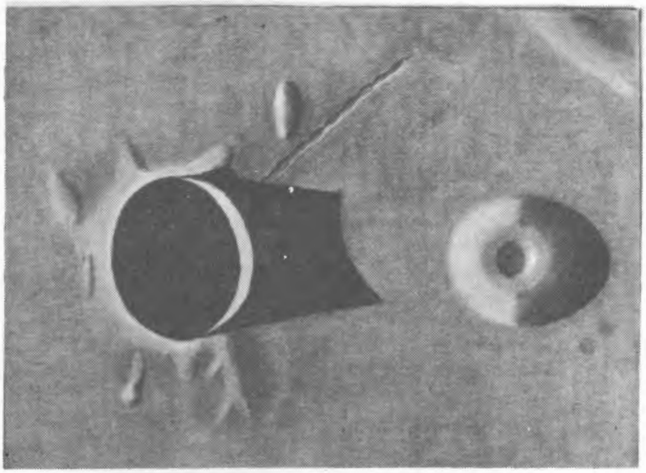

FIGURE 18. Lunar Crater Milichius and Dome to east (selenographic sense). Drawing by Alika K. Herring. April 15,1962, $3^h$ $15^m$, U.T. 12.5-inch refl., 278X. S=4-5. T=4. Colong. = $34^{\circ}.1$.

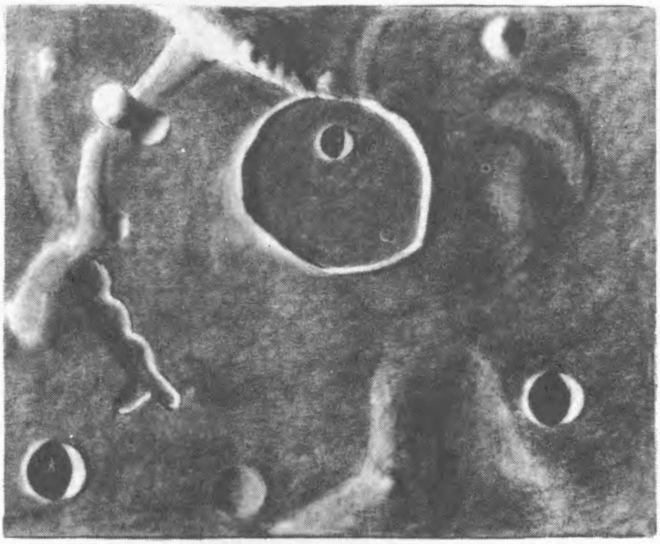

FIGURE 19. Lunar Crater Jansen and vicinity. Drawing by Alika K. Herring. April 11, 1962. $4^h$ $30^m$, U.T. 12.5-inch refl., 278X. S=3-4. T=5. Colong. = $346^{\circ}.0$.

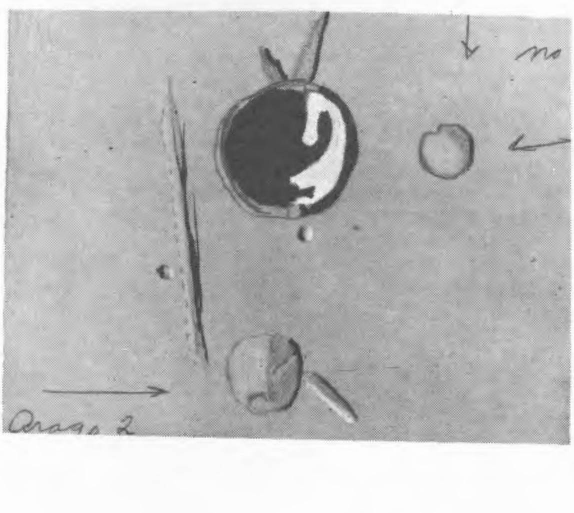


FIGURE 20. Lunar Crater Arago and Domes to its east (selenographic sense) and north. Drawing by Harry Jamieson. May 21, 1961. $3^h$, U.T. 10-inch refl. 222X. S= 6-4 (Tombaugh-Smith Scale). T=5. Colong. = $343^{\circ}.8$. The observer gave chief attention to the two domes.

observed by them are not overlooked. This is an important contribution by the amateurs, since it causes the ACIC observer to observe more thoroughly than he might if he were 'starting from scratch'.

"3. We would like to see more lunar observations by the amateurs published; and, in fact, it would be interesting if they would keep up with us in our program or work ahead so that a genuine contribution could be realized. Perhaps it would be possible for some amateurs to check the published charts and find additional features or corrections, which we would appreciate hearing about. The charts do carry a note to the user which encourages him to make corrections and additions and send them to the attention of ACIC in St. Louis, Missouri. A replacement copy will be returned to those who send in such corrections or additions marked on a sheet.

"4. Eventually, mapping of the moon will surely be done from close-up photography obtained by lunar orbiters. That time will mark the end of our present method of mapping through use of the telescope. Still, there are several years remaining for us to continue our present method to map the visible disk; and it is our hope that amateur astronomers will continue their very helpful observations as they have in the past."

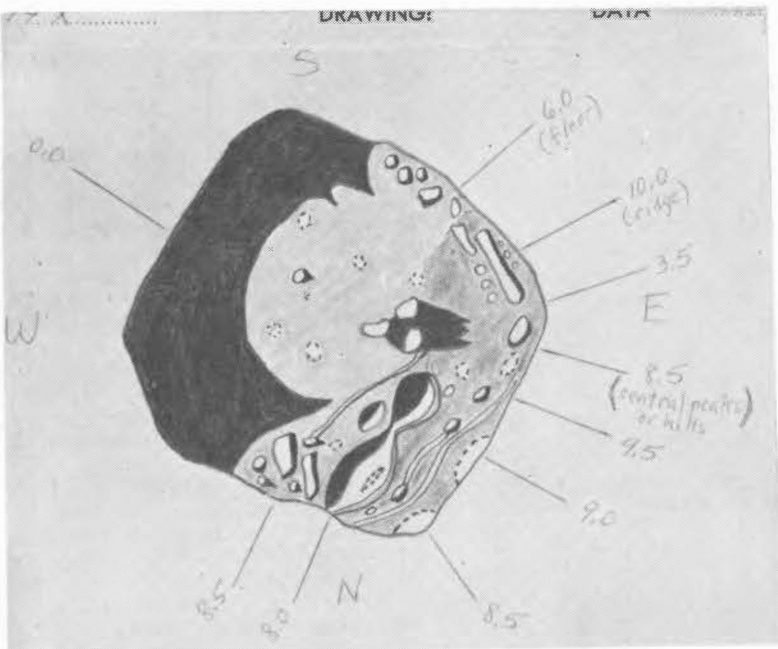


FIGURE 21. Lunar Crater Longomontanus. Drawing by Phillip W. Budine. December 10, 1959. $1^h\ 50^m$, U.T. 4-inch refr. 214X. S = 6. T = 3. Colong. = $28^\circ.6$

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