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Second A.L.P.O. Convention Issue

Drawing of Region of Aristarchus, Herodotus, and Schroeter's Valley by Elmer J. Reese with a 6-inch reflector at 320X on February 12, 1957, 1 hr. to 3hrs., Universal Time. Colongitude = 57°.9 at 2 hrs., U.T. Clear sky and fairly good seeing.

THE STROLLING ASTRONOMER

1203 North Alameda Street

Las Cruces, New Mexico

In This Issue

ANNOUNCEMENTS

Possible Blank Pages in Previous Issue. We have accidentally discovered a number of copies of the January-June, 1957 issue (Tenth Anniversary Issue) with some blank pages. We have no way of knowing how many such defective copies may have been unintentionally mailed to subscribers. We are anxious to replace all such defective copies with good copies, at once and without charge. Therefore, please let us know if there were any blank pages in your January-June, 1957 issue; and if so, please accept our apologies for this blunder.

Personal Thanks. We want to express our thanks to the many good friends who have had such very kind things to say about our Tenth Anniversary Issue. We have failed to answer all of the letters and cards individually, but we do wish here to say that your encouragement is most appreciated. We hope that our "monster" issue does reflect something of the present stature and future potentialities of the A. L. P. O. We confess to being proud of this issue - but please don't expect a repeat performance soon!

1956 A. L. P. O. Mars Map. A map of Mars based on observations of the Association of Lunar and Planetary Observers during the 1956 apparition has been prepared by Mr. Frank Vaughn, the Mars Recorder, and was mailed out as an insert in the Tenth Anniversary Issue. Considerable praise must go to Mr. Vaughn for his remarkable work in compiling this map; it will mean much to anyone who has ever attempted such a project to say that the map rests in part upon approximately 1200 drawings. The map itself impresses us as worthy of great praise. There is less of delicate detail than on the 1954 A. L. P. O. Mars map, presumably largely or wholly because of the obscured state of the atmosphere of Mars near the 1956 opposition - a circumstance about which many observers complained. The map will certainly be useful to anyone planning serious work during the coming 1958-9 apparition, and it is not too early to start preparing for Mars again.

The 1956 map is on sale by the Editor for the sum of one dollar.

Two New Staff Members. We now have as the Assistant Mars Recorder:

Leonard B. Abbey, Jr.
822 S. Mc Donough Street,
Decatur, Georgia.

In his new position Mr. Abbey will assist Mr. Vaughn in various ways, as needed. Much remains to be done in working up 1956 data, and we expect to publish a series of papers on Mars in 1956 as this study progresses. Mr. Abbey will continue as Uranus-Neptune Recorder. Some wag has suggested that he should add Pluto to his domain, for as yet no adequate map of the surface of Pluto exists!

We have also selected as our first Comet Recorder:

David Meisel,
800 8th Street,
Fairmont, West Virginia.

We were for a time hesitant to start a Section neither lunar nor planetary; but comets offer a legitimate and rather neglected field to the amateur observer, and we know of no other Comet Section in the United States. The A. L. P. O. naturally does not wish to establish new Sections that would duplicate, or interfere with, work already being done. Mr. Meisel's youthful enthusiasm and the appearance of two bright comets in 1957 (so far) persuaded us to make the venture. We shall appreciate it very much if our members will give the Comet Section and its Recorder their support. They might begin by sending in observations, drawings, and photographs of Comets Arend-Roland and Mrkos.

Mr. Meisel is a student at the University of West Virginia and is personally known to those of our members who have been at certain recent amateur astronomical conventions. He asks all A. L. P. O. members interested in the work of the Comet

Section to drop him a postcard. The Comet Recorder and the Editor discussed some rather ambitious plans for the new Section at Kansas City during the League Convention. We hope to be able eventually to furnish detailed instructions for observing comets and even our own ephemerides of newly discovered comets.

Request for Observations of Lunar "Saucers". Mr. Alika Herring, one of our Lunar Recorders, requests observations of these curious objects; he writes as follows: "This type of object is a very shallow depression, usually circular, which is found in some of the walled plains. Perhaps the best known examples occur in Ptolemaeus, where there are several; others are known to exist in Archimedes and in Plato. These 'saucers' may very possibly furnish an important clue as to the origin or formation of these craters. For this reason it is felt that a list of these objects might prove to be a worthwhile contribution to lunar study.

"As far as I know at the present time, none of these objects occurs outside of the three craters named. The fact that all three are walled plains seems significant, and it appears certain that more 'saucers' will be found in other craters of this type. Of course, they may also be found in other type craters or on the maria themselves.

"It should be pointed out that these 'saucers' will be visible only under extremely low lighting. Observers are asked to search both the morning and evening terminators carefully for these objects, and if any are noted, to send the information to me, describing the position as accurately as possible as well as including the date and time in U.T. so that colongitude can be computed."

Mr. Herring's address is 3273 Liberty Blvd., South Gate, Calif.

Notes on the International Lunar Society. Bulletin No 3, dated May, 1957, informs us that there are now nearly 300 Fellows in this truly international lunar organization. The President is Dr. H. P. Wilkins, the famous English selenographer. Among studies in progress are a critical survey of the floor of Plato in Great Britain and an examination of 9 photographs by Dr. Lugo in Venezuela of the total lunar eclipse on November 18, 1956. There are two kinds of membership in the I.L.S., Fellows and Senior Fellows. The latter kind requires the execution of original work of a high standard.

By now all American members should have received the first Journal of the I.L.S. Any who may not have done so should write to Walter H. Haas, 1203 N. Alameda, Las Cruces, New Mexico.

Injury to Albert Ingalls. The many friends of this veritable father of modern American amateur telescope-making may have heard by now of the serious automobile accident in which he was involved on August 2, 1957. "Red" Stong of the Scientific American staff wrote us on September 4 that our old friend "Unk" is making a remarkable recovery, and the physicians think that it may eventually be complete. "The gang" is invited to write to "Unk" during his convalescence, preferably short letters of a page or less, personal bits or scraps, maybe a good gag, and not too much shop talk on telescope-making. His address is 7 Holly Street, Cranford, New Jersey. Our sincere wishes go to him for a full and speedy recovery.

ASTRONOMICAL CONCLAVE AT KANSAS CITY

by Walter H. Haas

The Astronomical League held its eleventh annual National Convention at Kansas City, Missouri on August 31 - September 2, 1957. Most of the final day was given over to an event of special interest to the readers of this periodical, the second Convention of the Association of Lunar and Planetary Observers. The site of the meeting was the singularly lovely campus of the University of Kansas City, a sylvan setting in the midst of a large metropolis. The host society was the Astronomy Club of Kansas City, and much credit must go to Convention Committee General Chairman Gene Tandy and his many co-workers for the care and thoroughness with which they prepared a most enjoyable meeting for all of us.

Many delegates began to arrive already on Friday, August 30 by auto, train, bus, and plane and found their way to the registration desks in the lobby of the University Playhouse, where later all Convention Sessions were held. The Exhibits Hall was beside the Playhouse, and a word of praise is certainly in order for the many and varied astronomical exhibits. It was especially good to see some really impressive displays from many foreign countries. We must particularly thank various persons who contributed to the A.L.P.O. Exhibit, among others Frank Vaughn (original of 1956 Mars map), Steve Sinotte and Mike Kaiser (drawings), David Meisel (comet drawings and photographs), Lyle Johnson (lunar and planetary photographs), Carl Richards (early volumes of The Strolling Astronomer), and Alika Herring (drawings). Delegates stayed at a new dormitory just a few minutes walking distance from the Playhouse. The University Cafeteria was also nearby so that physical facilities were certainly most convenient. They were so convenient, in fact, that some attendees, including the writer, saw nothing of Kansas City itself throughout the Convention after some initial unplanned wandering through Kansas City, Kansas in search of the University of Kansas City.

One of the most pleasant aspects of amateur astronomical gatherings is certainly the renewal of old friendships and the making of new ones. The writer feels that he has been very fortunate in this respect. These contacts are cherished more with the passing years. It is only to be regretted that there is usually very little time to talk with any one particular person - Conventions are rather busy affairs.

The League Council began a series of meetings on the afternoon of August 30 with retiring President Grace Scholz as Chairman. Since the Council only meets during League National Conventions, there is much business to be discussed then. Council members, 'tis said, seldom complain of an excess of sleep.

The formal part of the Convention opened on the morning of August 31. A roll call of member societies of the Astronomical League was followed by reports from Officers, Section Chairmen, and the Nominating Committee. Fully 269 persons registered for the Convention. If that is not a record for an amateur astronomical gathering, it must be very close to one. The neighboring Mid-Western societies obviously supported this meeting very well.

The afternoon of Saturday, August 31 began with a Moonwatch Session, the Chairman being fittingly Dr. Armand Spitz. Contributors included Dr. J. Allen Hynek of the Smithsonian Astrophysical Observatory, Col. Owen Clarke of the U. S. Air Force, Jack Wagener of Edmund Scientific Corporation, and Steadman Thompson of the Moonwatch National Advisory Committee. Mr. Thompson's remarks on "Who Knows What About the Russian Satellites?" apparently caught the attention of the local press, more so than any other Convention event. In retrospect it would seem that singularly little was known or knowable about the approaching launching of a Russian artificial satellite on October 4, 1957. There was considerable discussion of simulated satellites, lights on aircraft to be flown across Moonwatch stations for training the teams of observers. Mr. Wagener described a device developed by Edmund Scientific Corporation; brightness and angular speed of a pseudo-satellite can be varied at will, the application to the training and testing of Moonwatch observers being evident.

After a short break, the Convention continued with a lecture by Dr. Joseph Shipman, the Director of the Linda Hall Library of the University of Kansas City, on "Rare Astronomical and Related Sciences Books." There followed a tour of this remarkable technical library of several hundred thousand books, which began as a private institution.

The evening of August 31 brought a scheduled star party but, alas, also brought clouds. In place of the intended training-session for amateur observers there was a discussion of observing - a very lively discussion, Dale Cruikshank and Steadman Thompson tell us. Others stayed outside and were rewarded by seeing the skies clear enough to reveal some stars. There were high hopes of observing the occultation of Saturn by the moon near 10:24 P.M., C.S.T. - but a cloud bank low in the west blotted out the spectacle.

On the morning of Sunday, September 1 Mr. Edwin Friton conducted a Session on Meteoritic Astronomy. Special mention should go to a paper contributed by Dr. Lincoln

La Paz of the University of New Mexico, a translation of Russian papers about the gigantic fall in eastern Siberia in 1947. Much of this information was certainly new to the audience, and one must be impressed by the care and thoroughness with which the Russian scientists investigated this fall. The afternoon featured the ever-enjoyable Junior Session, the Business Meeting, and a paper on "Mathematics and Astronomy" by Dr. William C. Doyle.

The Convention Banquet assembled at 6:30 P.M. in Massman Hall of Rockhurst College. Numerous astronomical door prizes were given away by commercial exhibitors - books, telescopes, etc. A few unlucky people seemed to hold several numbers merely very close to the winning ones. Dr. Richard N. Thomas of the National Bureau of Standards at Boulder, Colorado gave a brief but instructive talk about "Some Current Problems of the Solar Chromosphere". It was good to see Dr. Thomas' very keen interest in the training of young persons in astronomy and other sciences, a matter of much importance in our time. His clear exposition of current solar studies gave much insight into the astronomer's tools and he pointed out many places where more investigating is needed to choose between different hypotheses.

The sleepy delegates returned to the University Playhouse on the morning of Monday, September 2 for a Mid-States Regional Meeting and then a Panel of Experts, entertainingly moderated by Mr. Charles Federer of Sky and Telescope. Dr. Thomas was the willing target of not a few of the questions. His answers showed how rapidly some fields of modern astronomy are changing.

Then came our own A.L.P.O. program. These papers were presented:

1. The Enigma of Meteoritic Impact Flares on the Moon, by Robert M. Adams.

2. A. L. P. O. Observations of Jupiter During the 1956-57 Apparition. Slides prepared by Henry P. Squyres.

3. The Diophantus-Delisle Domes, by Leonard B. Abbey,Jr.

4. The Limb of the Moon, by Patrick Moore. Read by Phillip Budine.

5. Comet Arend-Roland, by David Meisel.

6. The First Ten Years of the A.L.P.O., by Walter H. Haas.

7. The Canals of Mars, from Schiaparelli to 1956, by Frank R. Vaughn.

8. Some Recent Observations of Plato, by Alika Herring. Read by Walter H. Haas.

9. The Night Side of the Moon, by H. Percy Wilkins. Read by John Reed.

10. The Vendelinus Cleft, by John D. Bestwick. Read by Alan McClure.

11. Amateur Astronomy, by A. C. Larrieu. Read by Steadman Thompson.

12. Can Southern Amateurs Make a Greater Contribution to Planetary Observations?, by Frank M. Bateson. Read By Grace Scholz.

13. Unpublished Mercury Observations from October, 1956 to Date, by Owen C. Ranck. Read by Steadman Thompson.

14. Historical Sketch of the Principal Pre-1900 Observations of the Apparent Nocturnal Luminescence of the Planet Venus, by Richard M. Baum. Read by title only because of lack of time.

It is intended to publish most of these papers in The Strolling Astronomer, many of them in this issue. We hope that you will enjoy reading them, even if you were with us at Kansas City. We must thank our friends in England for contributing fully four of the papers on the program.

Two Award Pins were presented to A.L.P.O. members for their outstanding services to the Association. It is sometimes dangerous to give awards, for to say that John is the best implies that James is not the best. There are certainly many members to whom we have been indebted during the ten years of our existence for many things, for meritorous observations, for Strolling Astronomer articles, for help as Section Recorders, for ideas in correspondence, etc. We think that we can all agree, though, that the two awardees chosen do deserve special praise. They are David P. Barcroft, for his constant, selfless, and generous assistance as our Secretary in many different ways and Frank R. Vaughn, for his remarkable (and very time-consuming) job of directing the work of the Mars Section in 1956.

We have no ready way of telling which A.L.P.O. members were with us at Kansas City, and there may be errors in the following list. If we may trust memory, attendees were: Leonard B. Abbey, Jr., W. I. Abbott, Robert M. Adams, Harry E. Brown, Ralph N. Buckstaff, Phillip W. Budine, Dale P. Cruikshank, Robert Curnutt, L. A. Doyle, Roy Ensign, Charles A. Federer, Walter H. Haas, Robert A. James, Craig L. Johnson, C. S. Johnson, Lyle T. Johnson, Mike Kaiser, Alan McClure, Russell C. Maag, Charles Martens, David Meisel, John W. Reed, Carl P. Richards, Donald A. Rosenfield, Grace C. Scholz, Stephen Sinotte, Steadman Thompson and Frank R. Vaughn - a total of 28 persons.

The Astronomical League will hold its 1958 Convention at Ithaca, New York on the campus of Cornell University.

No definite plans have yet been made for a 1958 A. L. P. O. Convention. We shall be glad to have the opinions of our readers on this subject. Future announcements will be made in this periodical.

Meantime, tentative plans are under way for a really large amateur gathering at Denver, Colorado in late August or early September, 1959. It is to embrace the Astronomical League, the Western Amateur Astronomers, and the A.L.P.O.

I wish here to thank all who contributed in any way to the Second A.L.P.O. Convention and especially to thank the Astronomical League and the Astronomy Club of Kansas City, whose goodwill and fine coöperation made an A.L.P.O. gathering possible.

Figure 1
Dr. Armand Spitz at Astronomical League National Convention at Kansas City.

Figure 2
A.L.P.O. Staff Members at Kansas City Convention. Left to right: Frank R. Vaughn, Walter H. Haas, Leonard B. Abbey, Jr., David Meisel, and Robert M. Adams.

Figure 3
Part of Banquet Tables at Astronomical League National Convention.

Figure 4
Robert M. Adams Speaking to Second A.L.P.O. Convention at Kansas City, Sept. 2, 1957.

HISTORICAL SKETCH OF THE PRINCIPAL PRE-1900 OBSERVATIONS OF THE APPARENT NOCTURNAL LUMINESCENCE OF THE PLANET VENUS

by Richard M. Baum

(Paper Contributed to the Second A. L. P. O. Convention, September 2, 1957.)

Next to the Moon and, if we accredit them important in our considerations, several energetic fragments of the asteroid belt, Venus is our nearest neighbor. In its journey round the sun, it approaches closer than Mars at its most favorable opposition, coming within 26 million miles of us; and at this epoch subtending an angular diameter of 60 seconds of arc, that is to say one-thirtieth the diameter of the Moon seen with the unaided vision, so that with an optical magnification of 30 Venus shows of an equivalent size to the Moon as seen with the naked eye. Under slightly less conducive conditions of proximity Mars has yielded a most remarkable amount of data respecting its physical properties and conditions at the surface. Yet with Venus, the circumstances produced by its situation relative to Earth are such that we are totally unable to facilitate ourselves of this singular propitiousness of dimension and propinquity. For Venus, it need hardly be said, revolves about the common source of planetary animation in an orbit interior to that of the terrestrial, and consequently exhibits phases analogous to those of the Moon; thus as Venus vacates the region of its orbit on the farther side of the Sun from ourselves, and imperceptibly swings towards us, not only does its apparent size increase, but the amount of visible sunlit area suffers a decrease. By the time Venus attains its maximum diameter, then, it lies in inferior conjunction and to our disadvantage has its nocturnal hemisphere turned fully in our direction; theoretically therefore Venus should not be visible -it is hardly surprising that close approaches of Venus are never awaited with the same anticipation and enthusiasm accorded to those of Mars, or even Jupiter. Yet Venus is not entirely the inane object that theory demands at this epoch.

As if in contradiction to the natural laws of light, the night hemisphere is observed, as has been recognized since 1643, to actually shine with a pale greyish, sometimes copper, luminescence; indeed the phenomenon is not confined solely to manifesting itself at inferior conjunction, exceptionally acute observers have produced well authenticated evidence affirming its visibility from soon after Venus traverses eastern quadrature through to the opposite corresponding phase in the west. We note here, a remarkable semblance to what can only be described as a visually identical apparition shown by the Moon, the ashen light or earthshine or lumière cendrée, so various is its calling. Superficially this curious affinity extends to a most peculiar exactitude of trivial detail in that in the planetary apparition we see clearly a distinct miniature of that shown by the Moon. Visual coincidence terminates the analogy however: whereas sunlight reflected off Earth is intercepted by the Moon causing its night side to be weakly lit with a light sufficient to render the features of that side easily discernible, thus giving rise to earthshine, with Venus specular reflection from an adjoining source such as a satellite cannot be presumed the instigator; so far as is known Venus is not attended by a satellite companion of a size necessary to produce the observed astronomical phenomenon (1). The impression gained upon perceiving the appearance, in fact, is that of sunlight reflected by the upper rarefied layers of the cytherean atmosphere suffusing the Stygian gloom of the night hemisphere with a vague, glowing twilight.

Concluding an invaluable historical contribution to the study of aphroditography, Camille Flammarion declared:

> "In conclusion, we believe that nothing can be affirmed regarding the rotation of Venus, inasmuch as the absorption of its immense atmosphere certainly prevents any details on its surface from being perceived. The most careful discussion of all the observations leads us to think that the grey spots now and again seen upon Venus are the effects of contrast due to solar illumination, and that the less definite shadings are of an atmospheric nature, incapable of furnishing us with any serious data as to the rotation of the planet. We may state here more than ever that every man sees after his own fashion. We are sure of nothing.

The globe of Venus may turn underneath its opaque envelope without any motion manifesting itself to our eyes - unless by some transitory and uncertain effects. No one has ever seen on the surface of Venus any clearly characteristic spot analogous to those shown on the discs of Mars and the moon. The maps of Venus made up to the present time are illusions." (2).

Written in 1897 before the advent of effective planetary photography and searching filter techniques, we observe here a clear exposition of the whole complex of the cytherean problem, not only as it then existed but as it does now, although the authoritative disdain in which visually observed features were held has currently been modified to a dubious acceptance of the possibility first dimly perceived late last century. The attitude towards the ashen light, however, remains irrevocably implacable, the opinions expressed being much the same as in Flammarion's day, a state likely to persist so long as our present confusion reigns unhindered. This pronounced negative aversion on the part of authority has found outlet in several hypotheses. Thus in 1936 E. M. Antoniadi inferred a subjective character, in so doing reversing his early views and observations. Barbier, of the Marseilles Observatory, from a failure to secure sufficient confirmatory observations as the planet traversed inferior conjunction September 8, 1935, similarly concluded. About the same period Danjon considered it explicable on the assumption of a secondary spectrum effect, whilst his colleague Schlumberger attributed it to after-image effects. Earlier, in 1873 and 1876 to be exact, the British observer N. E. Green obtained significant empiric results, in that several test observations appeared to demonstrate that whatever the cause and mechanism of its existence, the ashen light can not be assigned an objective reality. Now these opinions would carry greater weight but for one fact; they rest on the assumption that the visibility of the dark side is solely confined to the period when Venus is but thinly manifest as a crescent. If this were so, then there would be every reason for accepting these proposals, or at least their implication. It is not true, however, for we are in possession of irreproachable evidence, dating as far back as 1715, testifying that the dark side may under suitable conditions be seen when the fraction of illuminated area is about 0.6 the whole.

This single fact immediately invalidates the thesis of illusion, and reveals a field of study not only of interest and importance to an understanding of this sunwards sister world of ours, but perhaps more significantly of relevance in the investigation of terrestrial - solar electromagnetic relationships.

Previously, interest in the appearance has suffered considerably from a deficiency of historical detail, and a complete lack of understanding as to the actual manifestation of the phenomenon. Naturally the situation respecting its acceptance as real has been aggravated, and induced much misunderstanding, and in order to mitigate this state I have attempted to collect all the observations made previous to 1900. No claim to completeness is made, for it is obvious such cannot be achieved. Numerous vague cross references met with in researching amongst the old runs of serial publications - the chief source of information - demonstrate that the published accounts represent only a small fraction of the number of times the phenomenon has been seen.

I

Up to the seventh decade of the nineteenth century it was largely assumed to be of the utmost rarity, and consequently described as such in all the principal textbooks of the day. Humboldt wrote of only three observers having seen it; Mädler of but two, the great Arago of five, whilst the German astronomer Winnecke erroneously announced his detailed daylight studies of 1871 as the first since those of Mayer in 1759!

In 1806 Schröter thought he was the first observer, but unknown to him C. L. Harding, at Göttingen, also espied it about the same date, and Olbers drew Schröter's attention to Mayer's reports of 1759. Arago quoting from a French translation of a curious English publication called Astrotheology, first published in 1714, the date of the French edition being 1729, relates an undated English observation made anterior to the latter date (and seemingly before 1714 according to Marth).

The earliest observation to come before our attention is that discovered by Professor C. V. Zenger in Riccioli's "Almagestum Novum", where it transpires a record that can only be interpreted as of the ashen light is given under date January 9, 1643. In view of the poor optics of the time and the extreme delicacy of the appearance, one can only appreciate two facts, (i) that the phenomenon at that time was particularly strongly displayed, or (ii) that Riccioli must have been possessed of acute powers of sight and of a telescope of above average quality.

Yet for all these early observations, excepting for their publication, and the passing of various opinions as to the nature of the appearance, the cytherean ashen light was locked away in the cabinet of astronomical curiosities, and never really studied as anything other but an oddity. But in the month of July, 1868, one Professor A. Schafarik, of Prague, chanced to perceive the dark side, though somewhat unsteadily. The appearance so interested him, however, that he resolved to gather together all the available reports he could uncover. As he later wrote, "It was known to me for a long time that there were on record far more observations of this phenomenon than is ordinarily supposed." (3). And by 1873 the work was done, and published in The Monthly Notices of the Bohemian Academy of Sciences, July 18, 1873. A few weeks later Winnecke issued a review of the paper. Fortunately for futurity, a larger audience was able to avail itself of this important collection through Schafarik's himself communicating an anglicized version to the British Association for the Advancement of Science for reading at the September 1873 convention to be held at Bradford. The student requiring to consult it directly will find it interred under the title On The Visibility of the Dark Side of Venus in the proceedings of this session. (4). Altogether twenty-two observations are listed and briefly described; in each case the original source is referred to. The earliest is that of circa 1714 and the last 1871. Besides assembling the available accounts, Schafarik included a broad review of the several hypotheses advanced in explanation of the phenomenon, giving in some detail his own view, which is currently of interest in that he proposed Venus to be covered entirely by a phosphorescent ocean, thus anticipating Menzel and Whipple. Historically Schafarik's paper is important (yet it is more so scientifically considered), inasmuch as it marks the first attempt to collect on a comprehensive scale the obscure observations, and also draws notice to the existence of a potentially important field of investigation. It is perhaps opportune here to remark upon a curious error which it contains, that can only be deciphered as a typographical error. Schafarik details a personal observation as having been obtained on August 9, 1870, the fraction of illuminated area to the whole disc being 0.35. In actuality the value of the latter quantity for the given date in 1870 was 0.866. Students conversant with the post-1881 editions of Webb's invaluable handbook are acquainted, though unwittingly, with the correct date, August 9, 1871. The mistake appears to have escaped general notice.

Schafarik was followed by another assiduous Prague observer, Professor C. V. Zenger, to whom we are indebted for resurrecting the earliest report known to us. His first literary contribution to the subject appeared in The Monthly Notices of the Royal Astronomical Society, 1877. This was superseded by a longer work published in the same quarter in 1883.It is in this that Zenger informs us of the 1643 observation of Riccioli; for the most the rest of the paper is given over to broadly reviewing observations by Schröter, Harding, Mayer and others, besides some clearly recorded experiences of the author in January, 1883. Special emphasis is laid on the estimates of color of the luminescence.

Subsequently the student becomes aware of a decided increase in the reported number of observations, a fact easily attributable to the increasing popularity of planetary study following the stirring work of Schiaparelli, and no doubt to the greater number of telescopists: the quality of optics might also be argued as contributory, but when one considers that remarkable observation of Riccioli made only three decades or so after the ingenious Galilean application of the telescope to astronomy, one doubts the validity of this train of thought.

We note a larger number of observations: yet too we become aware of a certain resistance to believe in the objective reality of the phenomenon, and consequently this reluctance is reflected in a most forceful manner by the almost complete absence of any further discussion on the subject. And excepting an interesting review of the more important studies up to 1895 by the British observer Gemmill, issued in that year in The Journal of the British Astronomical Association, no further work of value to contemporary observers may be mentioned. After 1900 the same penchant not to assume the phenomenon as real predominated, as it still does, with the same enigmatic consequences. It is not within the project of this paper to proceed beyond this latter chronological limit, so that at this juncture we may enter upon a review of the observations preceding it.

II

1. - 1643, January 9. The Jesuit astronomer Riccioli writing in his Almagestum Novum states, "Erat planeta Solem versus rubicunda in medio flavescens et in parte a Sole aversa coerūleo-viridis, sed illa varietas a vitro tubi probabiliter fuit .... Semi-annulus lucidus, quo a tergo coronabatur, erat forte a Jove et Saturno illam illustrantibus, utpote orientalioribus." (5). Professor Zenger may be credited the first to draw attention to this description. It is not widely mentioned, and is absent from Webb's handbook. The reddish hue might well have been caused by chromatic aberration, but the greenish blue tint was seen on several occasions by leading nineteenth century observers employing achromatics of good quality.

2. - Circa 1715.⸸ observation by William Derham, Canon of Windsor. Described in Bk. V of his curious physico-theological encyclopaedia, Astro-Theology, sub-titled, A Demonstration of the Being and Attributes of God, from a Survey of the Heavens, London, 1715. In a footnote to the May, 1876 issue of The Astronomical Register, the famous nineteenth century computer Marth writes, "It may, perhaps, be well to reprint here the oldest known statement of the secondary light of Venus having been observed, ... found in the first edition . . . of Derham."

> " . . . . And this sphaericity, or rotundity, is manifest in our Moon, yea, and in Venus too; in whose greatest falcations the dark part of their globes may be perceived, exhibiting themselves under the appearance of a dull and rusty colour."

Of greater interest is this footnote which was added to the third edition of 1719.

> "What I have here affirmed of the Secondary Light of Venus, I have been called to an account for, by an ingenious astronomer of my acquaintance. But I particularly remember, that as I was viewing Venus some years ago, with a good 34 foot glass, when she was in her Perigee, and much horned, that I could see the darkened part of her globe, as we do that of the Moon soon after her change. And imagining that in the last total eclipse of the Sun, the same might be discerned, I desired a very curious observer that was with me and looked through an excellent glass, to take notice of it, who affirmed that he saw it very plainly."

According to Marth the total eclipse alluded to was that of May 2, 1715, at which time Venus was a morning star well beyond greatest elongation.

3. -1721, June 7. Christian Kirch, first astronomer of the Royal Academy of Sciences of Berlin. Mentioned by Schafarik and Zenger. Kirch using moderate optical power in bright twilight saw the faintly shining dark side as belonging to a circle of lesser radius than the illuminated part of the disc; a not unusual effect of earthshine, as may clearly be verified any fine evening when the crescent is slender. (6).

4. -1726, March 8. Another observation by Kirch made under similar conditions to that of 1721. (7).

5. - 1759, October 20. Andreas Mayer, professor of mathematics at Gryphiswald University. A well known and oft quoted report. Mentioned by Schafarik but strangely enough not by Zenger; at least it is not credited by name as such by Zenger, thus;- Zenger writing of the two Kirch reports states, "He (Kirch) mentions that he once saw the entire dark side of Venus on October 20, 1759, at noon, although the southern declination of 21° 50' greatly interfered with good definition." Comparing this to

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⸸ The full details of this were unknown to Schafarik whilst he was compiling his paper; he gives the date of the 1729 French edition as obtained from Arago's writings.

Schafarik's account of Mayer's observation there can be no doubt that Zenger meant Mayer, " . . . . . Andreas Mayer . . . . . who, on October 20, 1759, observed Venus, culminating only 10° from the sun, with an unachromatic transit instrument of only 1½ inches aperture, and saw the whole disk like the crescent moon which reflects the light of the earth." (8). Webb quotes Mayer so,"etsi pars lucida Veneris tenuis admodum erat, nihilominus integer discus apparuit, instar lunæ crescentis, quæ acceptum a terra lumen reflectit." In this case the time is entered as 44 minutes past noon.

6. - Circa 1790, Sir William Herschel, detailed by Schafarik and Webb. The former remarks that several times in 1790 Herschel commented on a certain dim luminosity at the dark limb. Examinations of Herschel's observations of Venus do not seem to substantiate this, however.

7. -1793. During the Spring and Summer months Count Friedrich Hahn saw the phenomenon repeatedly both unusually well and often, in twilight, and in full sunlight as well as at night. No other observer of this period saw it so definitely and so frequently. According to Schafarik he used fine instruments. Detailed accounts of what he observed plus two sketches may be found in Berliner Astronomisches Jahrbuch für 1793 (p. 188). Apparently he noted the dark side as of a greyish hue. Oddly Webb gives us to believe Hahn was an inferior observer, but on this obscure matter we have no material upon which to accept such an assertion.

8. - 1806, February 14. J. H. Schröter using his 20-inch reflector, f.l. 27 ft. in faint twilight perceived the dark side describing the limb as brighter than the central regions; the time of this was 7 hours. An accurate observation. This was the only occasion Schröter ever reported seeing the phenomenon, (9).

9. - 1806, C. L. Harding at the Göttingen Observatory; a most reliable and accurate observer, initially Schröter's assistant. Using a large reflector of f.l. 10 feet he saw the dark side on January 24 (Zenger gives the 28th); the cusps projected over into the unilluminated hemisphere, which itself was totally visible. Again on the 28th February it was similarly perceived, and showed of a reddish-grey hue; the observation is dated as having been made on the 20th of the month, in Zenger's second paper. The time appears to have been 6 hours, 12 minutes. Once more,on March 1st it was again noted, as before clearly and prominently. An interesting point is that Harding, according to Zenger, recorded a greenish-blue tint, thus confirming Riccioli's early study. (10).

10. - 1822, February 10. At 5 p.m., J. W. Pastorff at Buchholz, Prussia, the well known sunspot observer, watched Venus when the crescent width was 0.23, and clearly saw the dark side. He also remarks on having seen this same phenomenon on many occasions, so clearly in fact that he was able to distinguish lighter and darker patches within the greyish light. (11).

11. - 1825, June 8. At 4 a.m., Gruithuisen at Munich observed the phenomenon twenty-one days after inferior conjunction; planet at low altitude. (12).

12. - 1855, September 27 and 28. At 11 a.m., G. A. Jahn, Leipzig, clearly saw the dark side. (13).

13. - 1862, January 14. With a 4-inch Gregorian reflector X160 Berry of Liverpool, under good conditions during the evening noted the dark hemisphere which "shone with a faint light similar in appearance to the lumière cendrée in the crescent moon." The impression of this manifestation was enhanced repeatedly in moments of steadiness when the cusps appeared free of motion, and gleamed out with great clarity. (14).

14. - 1863, September. At Uckfield, during the inferior conjunction that occurred on September 28th, C. Leeson Prince obtained a number of interesting observations. In poor disturbed atmospheric conditions on the 23rd of the month, he "thought ...... the whole disk slightly illuminated." On the 25th he further wrote, "I am satisfied that I not only saw the dark body, but also a phosphorescent flitting of light around the edge of the entire disk." The dark side was once more perceived on the 26th, not as distinctly though. On the 27th, it was missed, although a twilight arc around the dark side flickered strongly. At 2 p.m., on the day of inferior conjunction, "The

dark body faintly seen." It was not visible on the 30th, only the twilight arc.(15).

15. - 1865, April 20. Immediately after sunset W. Engelmann, at the Leipzig Observatory, descried the phenomenon shining strongly; the dark side appeared greenish-grey, tinged with blue, and slightly brighter than the surrounding sky illumination. Engelmann observed the ashen light frequently. (16).

16. - 1868, May 23 and June 9, Th. Petty, Deddington, "I was very much delighted on the 23rd May, 1868, by observing her [Venus] dark body very plainly apparent in the telescope, reminding one of the same appearance in the new moon, as seen by the naked eye." On June 9th, the phenomenon was again seen. Both observations were confirmed according to Petty. The time of day is not given, but Schafarik deduced it to have been twilight. (17).

17. - 1868, July 4. At 1 p.m., Schafarik at Prague under poor conditions dimly perceived what seemed to be the dark side. Insufficent optics and the unsteady conditions prevented him from verifying his impression. (18).

18. - 1870, February 5, R. Langdon, Silverton Station, witnessed quite clearly the whole of the dark side in the company of several other observers. Inferior conjunction February 23rd, 3^h 24^m.

19. - 1870, February 22, Capt. Wm. Noble, Leyton, "I observed her at 2^h 10^m, L.M.T., when she was within 24^h 14^m of such conjunction (inferior) .... Constricting the field of view of a Huyghenian eyepiece magnifying 154 times by means of a card diaphragm pierced with a central needle line, I could see, plainly enough, the dark body of the planet. The sky was somewhat hazy, and I could not trace the dark limb quite round; but its difference of tint from that of the surrounding sky was evident the instant Venus was regarded." (19). It is interesting to remember, besides having a definite relevancy to a complete understanding of the phenomenon, that Noble always observed Venus on a bright sky, and always reported the dark side as darker than the sky.

20. -1870, February and March. J. Browning reported having seen the dark side without any special device to improve perception on at least 20 evenings, always in bright twilight and always as darker than the sky. He employed a fine 12¼-inch reflector.

21. - 1871, September 25. At noon Dr. A. Winnecke, Karlsruhre, observed the planet with a heliometer of 34 lines aperture. The cusps appeared to extend over not more than 180°, but in moments of really steady seeing the whole disk of the planet could be made out. The night side was bathed in a faint, unmistakable greyish light, and the phenomenon was so crisply displayed that there could be no doubting its existence. (20). Inferior conjunction took place on this date. Peculiarly, on the 26th, 1 hour, 37 minutes after this event, Noble, probably for the first time, failed to detect the phenomenon. Again on November 6, at 5 a.m., Winnecke noted it. (21).

22. - 1871, October and November, Vogel and Lohse, at the Bothkamp Observatory, saw the secondary light for about 30° from the terminator towards the dark limb, on seven mornings in bright twilight between October 15 and November 12. The glow was yellowish, faint and brightest nearest the terminator. On five other mornings nothing was seen. (22).

23. - 1871, August 9. Schafarik at 11 a.m. "I was regarding Venus in bright sunshine at 11 a.m., when a lady who was with me at that time immediately perceived the whole disk of the planet . . . . I fancied only at moments that I saw a faint line of light all round the greyish disk." Illumination unusually large (0.35); seeing poor. (23).

24. 1873, March 22, 6^h 40^m. Thomas Gwyn Elger, famous selenographer, writes of having seen the ashen light when the fraction of illuminated disk was 0.37, clearly seen. 4-inch refractor, X90 and X210. (24).

25. - 1873. Wentworth Erck, mentioned by Webb but no details given. The note is

reliable, however; Green informs us that conversation with Erck satisfied him that the latter had under reasonable conditions observed something strange on the unilluminated side of Venus. (25).

26. - 1873, April 19. Inferior conjunction 1873 May 5, $5^h\ 51^m$. Langdon, " ...., but the planet on this occasion was seen through the aurora, ... The whole body of the planet was distinctly seen." (Illumination 0.11). (26).

27. -1876, July. Baron Octave Van Ertborn, Aertselaer (Anvers), Belgium, on the 9th, reported an ashen glow at 2 hrs.; on the 13th, he recorded the dark side to be suffused as with an ashy light; inferior conjunction took place at 22 hrs., on the 13th. At $7^h\ 30^m$ on the 14th, this glow seemed to concentrate about the cusps. On the 17th, between 10 hrs. and $10^h\ 30^m$, Ertborn observed the planet, and noted an appearance to the dark side similar to the 13th. 4¼-inch refractor. (27).

28. -1876. Webb lists three observers; Banks, Grover and Arcimis. No details.

29. - 1876. Professor C. V. Zenger, Prague, relates that between September 30 and October 14, when atmospheric conditions were unusually steady he detected close to the terminator a faint reddish-grey light, the red tint only weakly asserted its presence; the glow rose into prominence at daybreak, and curiously at that time spread itself until it extended over two-thirds of the dark side. Previously the dark side would not be made out despite good seeing. (28).

30. - 1878. Mills mentioned by Webb, who gives no other details.

31. - 1878, January 31. The Rev. T. W. Webb; a very accurate and complete description, reproduced here almost in full. "Though a frequent observer of the planet Venus through a long series of years, I have never till yesterday evening seen the unilluminated side, which presented itself rather unexpectedly, as I had not been thinking particularly about it, and was not making it an object of special examination. The air was frosty and hazy, and definition tremulous, and the planet low; my beautiful 9.3-inch With reflector brought nevertheless the horns to sharp points. I had noticed nothing remarkable with a low Kellner eye-piece, but on changing it for 212 I perceived the phenomenon pretty distinctly at intervals; it was much overpowered by the splendid light of the planet, but came out occasionally rather paler and browner than the background of the twilight sky. It was not visible with Browning's E eyepiece of about 357, ... and was equally perceptible when the planet was hidden by a bar in the field." (29).

32. - 1883, January 8 and 9, between 18^h and 20^h, at morning elongation. Professor C. V. Zenger, Prague, obtained beautiful views of the planet projected against a cloudless dark blue sky. On the 8th, at $18^h\ 45^m$ the whole disk was visible, and a reddish-yellow dawnlike glow appeared to invade the night side from the terminator for a distance equal to one-sixth the size of the disk, shading gradually away to invisibility at the darkened limb. The limb of the planet tinged with a thread of coppery light. Vivid color noticed at the terminator, reddish-yellow, waning to reddish at the centre, the whole surrounded by a reddish-brown fringe of light. (30).

33. - 1884, July 11, Wm. Noble saw dark side as planet lay at inferior conjunction.

34. - 1884, February to October. W. S. Franks commented on the dark side being visible and darker than the sky; Perkins considered it brighter than sky; whilst Saxby failed to detect even a trace of the phenomenon.

35. - 1886, Lohse and Wigglesworth using the latter's fine 15½-inch refractor (Cooke), erected at Scarborough, "On January 2 and February 3, 1886, the dark part of Venus was distinctly seen by Mr. Wigglesworth and Mr. Lohse; it is of a gray colour, except near the terminator, where the secondary spectrum causes it to appear blue, and compared to the bright part of Venus it looks very much smaller."(31).

36. - 1887, October 21 and 26, J. Lamp, Bothkamp Observatory, observed a greyish gleam from the dark side. (32).

37. - 1889, March 16, S. M. B. Gemmill at 8^h, using a 3¼-inch refractor, X45 and X145, "Phosphorescence very plain, seen at the first glance in 45, and well confirmed

independently; complete, slightly within cusps . . . . Ten days afterwards it was detected again, though it was then faint. On the 30th it was not to be seen at all." (33).

38. - <u>1895, July 20, at 4.20 p.m.</u> With a small refractor R. K. Sale under steady conditions, and with inferior conjunction about two months off observed the complete disk, the dark side being as is usual on a daylight sky darker than its background. About a fortnight earlier Sale suspected the visibility of the phenomenon, but it was not quite so certainly displayed as in this instance.

39. - <u>1895, July 25 and August 5,</u> N. S. Aldis, New Zealand, dark side just seen after sunset with a 3-inch refractor. Complete disk visible; as the evening progressed the night side seemed to become less bright, but this may have been due to the planet's descent into more disturbed layers of the terrestrial atmosphere. On the 5th August again observed, but not so strongly.

40. - <u>1895, August 4.</u> At 6^h 20^m C. Roberts with a $6\frac{1}{2}$-inch reflector noted a strong brownish hue to the dark side, but does not mention how the dark side compared in intensity to the surrounding sky.

41. - <u>1895, Summer</u>. E. M. Antoniadi, Constantinople, observed the dark side to be invariably darker than the sky as Venus descended towards inferior conjunction. The phenomenon was distinctly visible.

42. - <u>1895</u>. Leo Brenner at the Manora Observatory first saw the dark side on June 4, 107 days before inferior conjunction. Subsequently hardly a day passed without its being detected. Previous to dichotomy it appeared to belong to a circle of smaller radius than the bright side. Afterwards it corresponded with the geometrical figure of the latter. On each occasion, with the exception of September 2, it was seen darker than the sky. On the 2nd September, however, it appeared brighter than the sky and clearly arrayed to be gleaming with a grayish luminescence. Brenner made all his observations in full sunshine, so that like Noble it is not unexpected that he should always comment on the dark side as darker than the sky.

43. - <u>1895, October 22,</u> H. McEwen, "On Tuesday, 22nd October, after observing Venus from 17.8 hours, the dark side became visible at daybreak, $18\frac{1}{2}$ hours, under a magnifying power (negative eye-piece) of 90 diameters on a 5-inch refractor. It emitted a beautiful golden light, being brightest all round the dark limb in the form of a faint crescent, and gradually getting darker as the middle of the illuminated crescent was approached, thus presenting a most impressive sight . . . . and the circle of the illuminated crescent was observed to be larger than the circle of the dark limb, ... When measuring the diameter with the position micrometer, I placed the comb over the bright crescent, but was still able to see the dark side; . . . . The planet was followed till 20 hours, full daylight, when the dark limb gradually faded out, leaving the usual impression that the interior part of the crescent is somewhat darker than the background of the sky." (34).

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THE CANALS OF MARS, FROM SCHIAPARELLI TO 1956

by Frank R. Vaughn

(Paper read at the Second A.L.P.O. Convention, September 2, 1957.)

I am sure that the audience here is sophisticated enough not to expect in a short talk anything like a complete résumé of the history of the Martian canals as seen by us in the last 80 years. These strange objects do not stand by themselves, unconnected with other features of Mars, and cannot be intelligently so treated. I shall attempt a sort of capsule history, as a refresher to those who may have had a lapse of some years in familiarity with the matter. Those others among you who are familiar with these basic things I shall ask to bear with me, for a moment. We can then go on to other aspects of the canals which I hope you may find of some interest.

Just 80 years ago Giovanni Schiaparelli discovered to be a general phenomenon what had been seen in particular instances many years earlier by Green, Dawes, et al.

Schiaparelli's objects were at first rather broad, dusky shadings crossing the "deserts" or bright areas of Mars, and in general connecting definite points on the surface, i,e. dark projections from the "seas" or dark areas, and isolated dark condensations in the deserts. They also crossed one another freely. In later apparitions of the planet he saw them as much narrower.

It is surely not impossible for us even today to look back and to realize the excitement he must have felt when he finally had these objects well in his grasp. In Percival Lowell's words, "There are celestial sights more dazzling, spectacles that inspire more awe, but to the thoughtful observer who is privileged to see them well, there is nothing in the sky so profoundly impressive as these canals of Mars .... threads to draw one's mind after them across the millions of miles of intervening void."

Two oppositions later, in 1882, Schiaparelli observed the doubling of a number of canals, where the twin members run straight and parallel with each other from beginning to termination.

This remarkable observer continued his researches until about the time when Lowell was reaching great proficiency. Gradually failing eyesight so hampered him that he was forced to quit his studies.

In 1892 William H. Pickering at Arequipa, Peru with a 13-inch refractor observed canals crossing the dark areas (maria), rather pointedly establishing that the latter were solid ground - a point on which not all had been agreed up to that time. Some of these canals run into the regions covered by polar snows, as first reported by Prof. Douglass in 1894. Also in 1892 Pickering discovered a number of tiny oases or "lakes" (descriptively) at the junctions or crossings of canals. These objects were much added to later by Lowell and others.

From 1894 to his death in 1916 Percival Lowell dominated the study of Mars with an intelligence, singleness of purpose, and near-perfect optics in a combination that was unparalleled before, and possibly also since. That his instrument (a 24-inch refractor) was about as perfect as such a device can be, need not be accepted on reputation alone; Bell in his excellent little book "The Telescope" publishes graphical analyses of many of the great refractors, the Lowell instrument being by far the best of any of them in perfection of figure. How delicate Martian researches are may be appreciated from the fact that Lowell found improvement in the image to result from diaphragming the aperture to 12 or 18 inches, except in the very best seeing.

Lowell was fascinated by the thought that Mars might be inhabited by intelligent beings - so much so that at times it almost seems the observations were important only to the extent that they could be made to support such a thesis. If there was, or indeed is, another explanation for the phenomena observed, it is thus questionable whether Lowell could have been the man to develop it. Nonetheless his work was so excellent and penetrating that he must still be regarded today as the foremost student of Mars, from the standpoint of physical observations.

He decided, for esentially the same reasons we give today, that the polar snows were water, and not CO_2, as had been not implausibly suggested. This is on the basis that the dark melt-band about the polar cap grows and dwindles, according as the cap is shrinking or not, and has a bluish color, and that the temperatures on Mars are higher than had been generally supposed. His theoretical work on temperatures is of interest, since his results were of the same order as those given today - perhaps a bit higher.

Lowell observed as a general and repeating phenomenon the "awakening" in the Martian springtime of the winter-bound hemisphere, which begins shortly after the snow-cap commences shrinking; first the development of the melt band around the cap, then the prominence of the polar canals and far southern maria (in the case of southern spring), followed by a wave of darkening toward and beyond the equator, with the canals most prominent in the early Martian summer. He describes the winter canals as "mere skeletons of themselves."

Lowell would never have been guilty of so uneuphonius a word as "pumped", but no other fits so well or comes so readily to mind when through such phrases as "conveyed by intelligence" he gives us to understand that water is moved by artificial means. The oases or lakes at canal-crossings he always saw as round, not failing to point out that this shape was of advantage in conservation of water, though one may wonder if this is actually relevant.

Lowell then, leaves us with the picture of an inhabited planet, run in an orderly way on a planet-wide basis. It is a dry place, the canals and oases being evidence that conservation on a vast scale is required for existence of life. Moreover, it is pictured as a "dying" planet - an omen for the future of the earth.

Let us leave Lowell, bearing in mind the following points, for they are important in what follows: the typical canal for Lowell is an absolutely straight line (arc of a great circle); it is extremely narrow - "spider lines", "gossamer filaments", etc.; the canals darken along their length at a uniform rate (in a given measured instance) of 51 miles per day when the cap begins its melting; what we see are not canals as such, but vegetation along their sides.

From almost the day of their discovery by Schiaparelli, various observers have seen the canals much differently (or not at all!), from the narrow threads of Lowell to the broad patchworks of Antoiniadi, with all manner of variations. The question of the actual appearance of the canals is one of great interest, and is probably, but not necessarily, important in determining what function they serve, if any. Specifically, if we accept beforehand Lowell's idea of the canals as seen by us, as consisting of vegetation along the sides, then it is probably not very important whether they are narrow or wide, smooth or patchy - any combination would suit the idea well. But we would do poorly to go about the problem in such a way. We should assume nothing, and try to get at the correct physical data first, before wandering off into the tempting fields of speculation.

E. M. Antoniadi, a real giant in planetary astronomy for very many years, had a simple but telling criticism of Lowell and others, who saw narrow canals - they did not see the planet well enough to resolve the markings into their parts. Roughly, a typical Antoinadi canal consists of a series of small dark patches more or less linearly arranged. His arguments were convincing to many: he had the use of a 33-inch refractor in what he claimed was often good air. He did not need to diaphragm it to 12 or 18 inches to see what it would show. He was, moreover, a master draftsman, and his depictions of the planets are even now among the finest of their kind.

The red flag had been waved, of course, and a long and often bitter controversy followed. This division of opinion is as strong today as ever, though it is now perhaps more calmly discussed. I think it may be worth our time to examine some of the arguments, to see roughly where we stand today.

Many experiments have been done in the past in an attempt to show what the eye sees under difficult conditions. It is well established that the observer will tend to see as lines a series of dots linearly arranged, or broken lines,or aligned groups of patches,if they are seen under poor enough conditions. This of course is perfectly true. What is wrong is that many have jumped from this fact to the conclusion that the Martian canals are illusions. Perhaps it is too easy to overlook the fact that solid lines also look solid when poorly seen! One other point which experimenters often overlook is that the actual nature of the canals bears no relationship to what we are doing down here - the canals do not know that we are drawing artificial disks!

A Mr. William Carragan, in Sky-Telescope, May, 1956, reported on the results of artificial disk-drawings by a number of persons. They have in common with most of all such experiments the failing that they are artificial, to a very high degree. Some of his results are interesting, however. The drawings by the "observers" indicate that poorly seen lines tend to be carried much further than is the actual case. His other conclusions are typical of such experiments, in showing that one can be fooled by suitable constructions. There have been other such experiments, of course. It has been pointed out that an observer may tend to connect discrete points or projections by a line; however, some photographs show canals simply running off the edge of the disk.

One of our colleagues has suggested that perhaps the best procedure in such tests would be to secure excellent photographs of Mars, and have persons draw them under conditions which simulate telescopic observation. I think this good idea could be carried a step further by having the photographs "doctored" in various ways, so that one can test ideas about illusions on a disk closely resembling the object itself. Perhaps the subject is too complex, but I believe it is made hopelessly so by substituting blank disks with lines and splotches on them for the actual, infinitely varied tones and shadings of the planet itself, as seen against a night sky.

In Harvard Annals, Vol. XXXII, part II, W. H. Pickering vigorously attacked the double canals as being illusions, and with telling evidence based on his examination of observations made at Juvisy with a $9\frac{1}{2}$-inch telescope, and at Lowell Observatory with a 24-inch instrument, in 1896-7. Briefly, in both cases the doubles observed all showed nearly the same separation with the same instrument, but the separations were in each case inversely proportional to the aperture of the telescope! He pointed to the fact that Schiaparelli had also found the same thing true, i.e. as he used larger telescopes the width of the doubles decreased. Another point of interest is that he found Schiaparelli's doubles to run indifferently in all directions, while the much greater number seen by Lowell and the Juvisy observers with one exception lay within 45° of the meridian. He associates this with the fact that Schiaparelli's maps were constructed from long series of observations made while the planet was in different parts of the sky, while the observations at Juvisy and Flagstaff were made from work done when the planet was in "about the same part of the sky." Presumably he meant that since the doubles were a subjective phenomenon the orientation of the head while observing had something to do with it. Pickering believed, of course, that the doubles resulted from physiological effects, caused by a slight mis-focussing of the image of the planet on the retina, and gave experiments which verify that lines can be seen as double when slightly out of focus. To the objection that the telescope would not always be out of focus he answers that, once seen, the observer will refocus until the appearance is verified, it being associated in his mind with excellence of seeing and instrument.

Pickering, of course, did not get off so easily, for his conclusions do not explain why one canal should be seen as double and another equally strong one not. Lowell pointed out that not all the double canals are particularly delicate or hard to see, e.g. Phison and Hiddekel. If the basic question is a difficult one, the problem of the double canals is compounded, and we had better leave it for the time.

Robert Trumpler's opinion, in reporting the 1924 apparition, is that the canals have an appreciable width, and that they are real surface features, with the possible exception of the faintest lines, which may be due to contrast effects. He found the canals to be darker in red photos than in yellow, and suggested a blue-green color for them. He believed, however, that they are compatible with the natural-formation idea.

Jumping ahead a bit, because of its relevance to Trumpler's work, from P.A.S.P., 1947, Edison Pettit, after a period of "breaking in" with 6-inch and 20-inch telescopes, and during which time no canals were seen, gradually became able to see several canals, one after the other, at moments of the finest definition; and more and more were noted in the positions of those in Schiaparelli's map, although Pettit's acquaintance with the canals of Mars was confined to the information that "they were supposed to exist." He made drawings which showed them as straight and narrow, and he states that at the best moments he could even see that they were green in color.

On August 18, 1956 Tom Cave, an experienced observer of some skill, wrote me: "In views like this one, when the image of Mars is stationary for seconds at a time, I see the canal structure as exceedingly narrow fine lines ....." He had a 12½-inch telescope of good quality to judge by the detail shown in his drawings.

On another date he writes "Exceedingly fine seeing at times. With 450X mare detail appears to break up into fine minute disconnected detail; however, rectilinear detail does not appear in any way resolved."

Other observers see the canals as being in the range of very narrow to very broad, and perhaps the most significant feature is that the character of the canals as seen by various people bears no clear relationship to aperture, seeing quality, or even experience. To illustrate: from observations made in 1956 from July 1 to Sept. 1, the drawings of Tom Cave, Walter Haas, and Alika Herring, all with 12½-inch telescopes, showing the canal Oxus well on several occasions, each with variable seeing conditions (0 poorest to 10 perfect), were compared. Herring showed it always very narrow in seeing ranging from 3 to 8; Haas as somewhat broader, but about the same width in seeing from 3 to 5, and Cave always as quite broad in seeing from 3 to 8. Some other canals were thus noted for other observers less formally, with similar results. I think this illustrates what some have perhaps suspected; and that is that whether Antoniadi with broad diffuse canals, or Lowell with exceedingly fine lines, is right is possibly a meaningless question. Regardless of what the truth is - whether narrow or broad, or somewhere in between, we are unlikely to arrive at an answer through visual observations as undertaken up to now. I think the subjective effects are so great and variable between observers that the first task is to evaluate these in some sort of standard way under rigidly specified conditions. I do not know whether this is possible, but I do feel it an important task for someone to undertake - it would have great value, if successful, on many other objects, and perhaps also for other purposes.

One possible clue to the nature of the canals is that some observers skilled enough to show good internal consistency in their work, and who draw most canals as quite narrow, will yet show some others as quite broad, suggesting large real differences in the widths of canals; on the other hand those who generally draw broad canals show much less difference in widths seen, e.g. the Thoth-Nepenthes when full-blown may be shown as hardly wider than any other canal. Of course, it is impossible for the eye to tell, when a mark is <u>much</u> narrower than the resolving power of the instrument, whether an increasing visibility is due to increased width, increased darkness, or both.

One of our members who enjoys stirring things up, or "rocking the boat" has concocted the following argument: "It is reasonable, and is even well known that roughly aligned blotches or shadings may be seen as lines under common observing conditions, but it is less reasonable that true lines can be seen as roughly aligned blotches or shadings." One can almost hear the approval of Antoniadi!

Lest we begin to think that a dead-end has been reached on the question of the canals it will be well to remember what was said at first - that the canals do not stand alone - they are but a part of the entire planet, and can be approached less directly from other sides, and perhaps even better so.

That the canals are somehow bound up with seasonal changes on Mars is now beyond question. Just what this connection is, however, is a matter for careful study.

Gerard de Vaucouleurs, in his magnificent "Physics of the Planet Mars" examines this problem in a most searching way. To quote Dr. de Vaucouleurs " .... these seasonal variations are strongly correlated with the latitude of the spots (dark areas), and in such a way that the moment when the spots begin to undergo their seasonal darkening is delayed in direct proportion to the distance of the spot from the polar zone. This immediately suggests that these seasonal variations are connected with and governed by some physical agent proceeding from the polar cap and expanding outward in all directions."

The above rather obvious statement derives from 600 observations, using visual intensity estimates at the Peridier Observatory in 1939 between planetocentric longitudes 179° - 269°, or from the beginning of the Martian springtime in the southern hemisphere to the summer solstice for that hemisphere. De Vaucouleurs constructed cartouches of intensity similar to those done by Lowell 40 years earlier, graphed them in order of increasing latitude from the south polar regions, and drew a line through the points of maximum slope (or points of maximum rate of increase of intensity). The corresponding rate of movement of the darkening wave northward was 28 miles per day, or considerably slower than the rate found by Lowell. De Vaucouleurs emphasizes that the observations were too scattered to permit precision in conclusions.

The really significant result he found, however, was that if the observations are plotted as a function of distance from the pole, the deviations are smaller than if plotted against distance in terms of what have been assumed to be waterways. His final conclusion is, therefore, that the darkening agent is airborne, rather than carried by surface arteries. I should like to object vigorously to both the methods used and to the interpretation of the results. In the first place the eye is a very poor judge of the intensity of markings not immediately adjacent to one another. Even so, its chief value is qualitative rather than quantitative in such work. This statement is capable of full documentation, though there is no time for it here. In the graphs themselves it is obvious that fully half the cartouches do not intersect the chosen line even near the point of maximum intensity gain, and the indication is thus that we may very well be dealing with a phenomenon which is by its nature not a linear one, as de Vaucouleurs has assumed. For example, temperature, which must increase as one gains distance from the pole (and which certainly must affect any organic growth) is not allowed for. The scatter of observations is large enough that it is difficult to see how one idea, which depends on waterways, could be almost summarily dismissed in favor of another which I think presents even greater difficulties <u>on the basis of the evidence given</u>. Let us accept for the moment that de Vaucouleurs' evidence seems good, and that hence his conclusion of airborne water is reasonable. I believe it is also reasonable to examine the consequences. The rather constant rate of darkening demands an almost uniform process of diffusion through the atmosphere of the water vapor; yet we know that Mars as well as the earth has its own atmospheric currents, which may be quite strong to judge from the movements of clouds. There is even good evidence that the air circulation pattern of Mars bears strong resemblances to those on the earth, and certainly such evidence would be incompatible with any sort of diffusion process which produces the regularity of darkening as interpreted by de Vaucouleurs. We would in fact expect that artificial or natural progression through waterways would much more closely conform to the cartouches as observed by Lowell or de Vaucouleurs, than would atmospheric diffusion. I speak for neither one idea nor the other in all this, feeling that we are yet a long way from such easy solutions. One or the other (or neither) may be correct, but we do need better controlled studies in order to speculate intelligently Perhaps visual or photographic photometry will provide the necessary data; the former is slow and tedious, and the latter will require consistently better photographs than are yet generally attainable fairly consistently over considerable periods of time. Nevertheless such goals are certainly in sight.

Monsieur de Vaucouleurs dismisses underground propagation of the life-giving water on the grounds that we need better evidence of "Martians" to accept it - a most curious statement considering both what we have to work with on Mars, and the generally very high quality of de Vaucouleurs' work and writings. He supplies elsewhere, incidentally, a difficulty to his own thesis of uniform diffusion by pointing out that the darkening wave in the Hellespontus was in 1939 less than one-half as rapid as the "general wave."

There is much more to be said that is bound up with the nature of the canals of Mars, probably most of it having to do with the entire phenomenon of Mars itself. Any honest man will admit that on this, as with most problems, there are more questions than answers. With the predominantly visual methods we must of necessity use it as likely that as a prelude to any real progress, we must "know ourselves", to paraphrase a much underrated man. Subjective phenomena do not disappear when we recognize their presence. Is it variability alone in the structure of the eyes of different people that causes one to see narrow lines where another sees broad ones under similar conditions? If the matter goes deeper than this, possibly all that can be done is to try to arrive at some working method of allowing for such discrepancies through intelligently controlled experiments.

Perhaps some will be disappointed that I have failed to come up with an original thesis on the canals of Mars. I hope not, for such things come cheap for those with more imagination than knowledge. My aim here has been only to implant a little doubt in the minds of some who may have decided without evidence that this or that theory is "true". If these few minutes have caused a thought or two to spring up in the minds of some, I shall not feel too badly about speaking on a subject on which doubtless more than one person here has more knowledge than myself.

THE LIMB OF THE MOON

by Patrick Moore

(Paper read at the Second A.L.P.O. Convention, September 2, 1957.)

I felt greatly honored at being asked to present a paper at the first A. L. P. O. Congress; I feel even more honored at being asked to submit a second paper now. My only regret is that I cannot present it in person. One of my ambitions is to attend one of your Conventions; I hope to realize it one day. Meanwhile, I send my greetings to you all.

If I can say anything that will interest you, it must, I think, be in connection with the Moon. It is true to say that much of our knowledge of the surface topography is due to the work of amateurs; we are always hearing of impending "maps to end all maps" under way by professional observers, but somehow these maps always fall by the wayside, and the only well-known "official" map, that of the I. A. U., is(frankly) not satisfactory. It is particularly defective near the limb, and this is one reason why I would like to discuss the libration zones; the other reason being that I have myself paid much attention to these areas, and although I lay no claim to skill either as an observer or draughtsman I do at least know some of the difficulties of studying them.

Formations in the libratory regions are foreshortened and hard to observe, particularly as they have to be caught when well placed with regard to both libration and illumination. The first difficulty is that of nomenclature. Some time ago, I and other members of the Lunar Section of the British Astronomical Association undertook a study of the region of Hausen, in the Dörfel Mountains area, not far from Bailly on the south-east limb. Consulting five different maps, I found five different Hausens. Which was the "true" one? Schröter, for whose work I have the greatest respect, allotted the name to a large crater beyond Bailly, and this was the Hausen we adopted; but in general, confusion reigned supreme. The "Hausen" of the I.A.U. map, for example, might have been anything. Neither is it safe to put blind faith in the official "measured points", since near the limb, at any rate, these measures are not nearly so accurate as many people think. It is therefore best to select some definite feature which cannot be mistaken, and then work from that. In the area which we were studying at that time, the only safe anchorage proved to be Bailly itself.

It follows from this that the mapping of the libration areas is still very far from complete, particularly as nearly all lunar maps are drawn to mean libration - and thus do not show the critical zones properly. Wilkins' map is the exception, as it includes special charts of the libratory regions, but even this is not exhaustive, as Dr. Wilkins himself is the first to stress. I maintain that if an observer using a 3-inch refractor carries out a full study of any particular libration zone, over a period of several months, he will be able to record a considerable amount of detail which is not yet officially charted. There is work here for all.

On the other hand, it is unwise to lay claim to the discovery of new formations until every reference has been checked. I once "found" two large craters not far from Newton, and blithely assumed them to be new. Subsequently I found that Schröter had charted them both as long ago as 1793, even though they had been dropped from the modern maps.

One of the fascinations of lunar limb study is, however, that there is always the chance of breaking new ground. It may take years for confirmation to be obtained. In 1939, I was using a 3-inch telescope to study the region beyond Otto Struve, near the north-east limb, and detected a large, imposing crater with a central crater and much inner detail. Not for ten years did I manage to see it again, and I was not really satisfied until E. A. Whitaker, at Greenwich, managed to photograph it. The reason is, of course, that it cannot be seen at all unless libration and lighting are exactly right. The nearest named formarion on Wilkins' map is "Caramuel"; but I am still not entirely satisfied that his Caramuel is the same as the formation to which I am referring, and all the other maps of the area show nothing at all.

Of course, certain specialized papers in astronomical journals, particularly the <u>Journal</u> and <u>Memoirs</u> of the British Astronomical Association, and <u>The Strolling Astronomer</u>, are most helpful, and indeed provide most of our information. I must here pay particular tribute to the work of two outstanding British workers, R. M. Baum and K. W. Abineri. But although part of the limb has been effectively covered in this way, it is a very small part - less than five per cent., I imagine. During the past few years I have been paying attention to the north-east limb in the region of Pythagoras and Xenophanes; there are many important and as yet uncharted craters and other features, but it will be years yet before I am ready to produce a map of the area, even though I observe it with my 12½-inch reflector as often as I can. There are other problems, too. Why do none of the <u>maria</u> extend right over the limb? Small "plains" have been found which lie entirely in the libratory zones; such is the Mare Incognito, discovered by Wilkins. But the basic problem remains. Neither must we forget the bright rays which extend from the averted hemisphere, thus enabling us to make a start in plotting the positions of ray-craters which we can never see. This work was begun by Shaler and continued by E. F. Emley and Wilkins; several probable ray-craters have been located, but I have a feeling that there are many more of them, awaiting discovery by any energetic observer who is willing to spend a great deal of time in searching for them.

It may be asked: Why is it so important to map the lunar limb areas? The answer is, I think, twofold. First, various other lines of investigation require an accurate knowledge of the limb profile - a knowledge which we certainly do not have at the moment, though I gather that this particular problem is being energetically tackled by American photographic workers. Secondly, selenographers want to know as much as they can about not only the accessible features, but also the lesser-known ones. Until then, we cannot pretend that our knowledge of the Moon's surface is at all complete.

To study any particular limb area, one must adopt a long-term policy; taking advantage of favorable opportunities, when lighting and libration are at their best, the observer must make drawings as often as possible, until eventually he knows the area well enough to be able to produce a proper chart of it. I wish that some scheme could be brought into operation whereby groups of observers would combine; one group tackling the north-east limb, another the south-west limb, and so on. Is it possible that some such program could be worked out? At any rate, the opportunities are unlimited; the work is most interesting, really important, and well worth doing. A large telescope is not necessary (though always helpful, of course!) and the main requirements are enthusiasm and plenty of patience.

I hope you do not feel that I have wasted your time in saying all this; and again, my very best wishes to the Convention and to all of you personally.

1961: A SUGGESTION

by Patrick Moore

On February 15, 1961, there will be a total solar eclipse visible from parts of Europe; areas of South France and North Italy will lie in the zone of totality. This will certainly mean that astronomers from all over the world will assemble there. Something of the sort occurred in 1954 in Scandinavia, as many of us remember.

Amateur as well as professional astronomers will certainly go to France or Italy in 1961, and this seems to me to provide us with a great opportunity. Europeans such as myself have heard many reports of the amateur astronomers' congresses held periodically in America, when observers from all parts of the United States have the chance to meet and exchange views; but few similar meetings are held on the European side of the Atlantic, partly because of language problems and partly, I expect, because of the greater difficulties and restrictions of travel. Nobody can doubt that such congresses are extremely valuable, as well as being pleasant; more information can be given in half an hour's talk than in dozens of letters, and in any case one always works better with a colleague whom one knows personally. Professional workers are able to meet at I.A.U. congresses and elsewhere, but for amateurs such meetings are much rarer.

My suggestion is, therefore, that some effort be made to organize an Amateur Astronomers' Congress in 1961, to be held near the zone of totality just before or just after the eclipse. Many people will be in the area, and it should be relatively easy to arrange a "get-together".

A good deal of organization would be involved. A meeting center and a program would have to be arranged, and it would be necessary to have the co-operation of the leading amateur societies both in Europe and America, but I feel that it can be done; it would be the first Amateur Astronomers' International Congress, and everybody would benefit by it.

If the scheme were adopted, it is not too early to begin and to appoint a committee including representatives from the various national societies. This would take time, since most of the work would have to be done through the medium of the post, and it is my personal opinion that the most effective initial impetus could be given by the amateurs in the United States.

Let me stress, very clearly, that I have not investigated any of the difficulties involved. I am fairly certain that many national societies would be interested and would co-operate, because I am in touch with people in various parts of the world, but I have naturally made no previous official suggestion - nor am I the person to do so. I merely want to bring the idea to the notice of members of the A.L.P.O. If it is considered practicable, I feel that it may do a great deal to help international co-operation in amateur astronomy; if not, I apologize for wasting your space.

Postscript by Editor. Mr. Moore is not the first to make the proposal described above, but I cannot remember that it has previously been published elswhere. We are anxious, of course, to learn the reactions of A.L.P.O. members to the suggested meeting, both those in the United States and those in other countries.

Mr. Russell Maag, the new President of the Astronomical League, has made a beginning by appointing a committee to study the proposed international amateur meeting. The chairman is Mr. Steadman Thompson, 1174 Broadview Ave., Columbus 12, Ohio. The other members of the League Committee are Dr. James Gant of Washington, D.C. and the Editor. Remember, your ideas are most welcome!

SOME RECENT OBSERVATIONS OF PLATO

by Alika K. Herring

(Paper read at the Second A.L.P.O. Convention, September 2, 1957.)

This prominent walled plain is positioned at approximately 10 degrees east longitude and 50 degrees north latitude. Sunrise therefore occurs at approximately one day after First Quarter and sunset about one day after Last Quarter. Plato is very conspicuous during the intervening two weeks, partly because of its comparative isolation but also because of the blackness of the floor, which is one of the darkest areas on the moon. For this reason it is probably one of the most familiar objects on the lunar surface to the casual observer.

Plato, like most lunar craters, is approximately circular in shape but is greatly foreshortened, due to its high latitude, into an ellipse which is compressed very roughly in the ratio of 2 to 1. Variations in the shape due to libration are, however, quite noticeable over a lunation. The walls, which span approximately 60 miles from crest to crest, are extremely rough and jagged, particularly on the west rim, where peaks with elevations are as great as 7 to 8 thousand feet may be found. As is common for this type of lunar formation, the inner walls are much terraced in addition to the large landslip on the east side of the ring.

The floor of Plato has been described by various observers as very level and smooth. Unfortunately, such is not the case. The writer, under certain conditions of illumination, has seen the floor as quite convex, certainly as much or even more so than might be expected from the general contour of the lunar orb. Neither is the floor of Plato as smooth as it is usually depicted to be; under a very low angle of illumination the floor has been seen to contain a number of extremely low ridges and swells, besides the crater cones and pits which dot the floor. The terrain can only be described as rolling. In addition, the floor contains at least one of the large shallow depressions which are popularly known as "saucers".

The texture of the floor itself, with the proper illumination, and when seeing conditions are sufficiently good to permit the use of comparatively high powers, has a definite appearance of roughness and gives the observer the impression of endeavoring to see small scale irregularities which lie just beyond the limits of resolution. It is almost as if the observer were suspended high above one of the lava flows or malpais which occur here and there in our western states. The floor of Plato undoubtedly has a similar texture, but since it is situated on a world of low gravity and no erosion, must be extremely rough in comparison with our terrestrial examples.

Plato has been an object of great interest to serious students of the moon for more than three quarters of a century, and has probably received more intensive study than any other lunar formation. This is because of the many puzzles and mysteries it has presented from time to time. We have had more reports of supposed mists and obscurations on the floor of Plato than from any other area; the details visible on the floor itself will often vary in visibility in a strange and unpredictable manner.

Spots which have been easily seen for lengthy periods will often disappear, sometimes for years and then suddenly reappear without warning. Craterlets which may be easily visible at a certain illumination will not be seen at a later time under approximately the same conditions, but other craterlets will appear which were previously invisible. A comparison of the charts produced by several of the leading selenographers of this and the last century will reveal the interesting fact that only a fairly small percentage of the total detail shown will be common to all the charts. These discrepancies can not all be attributed to the personal equation, instrumentation or observing conditions of the various participants.

The following is a short summary of observations made by the writer from October, 1953 to March, 1957. During this interval a total of 38 drawings were made, under a wide variety of conditions. Although it was impossible to obtain a consistent series of sketches due to the vagaries of weather and seeing conditions, the sketches may still be considered to be fairly representative of most of the various degrees of illumination occurring over a single lunation.

The instruments used were two Newtonian reflectors of 8 and 12½ inches aperture. While of home manufacture, both of these telescopes received the benefit of some years of my experience as a professional maker of high precision paraboloidal mirrors. The result in both cases was an extremely high optical quality. This high precision of optical surfaces was necessary to give the maximum in contrast of faint detail as well as the most critical resolution of the finer lunar detail, some of which is certainly as difficult in terms of resolution and contrast as the most difficult detail found on some of the other planets. It might as well be stated in passing that only the highest precision in the optical elements is permissible in a telescope intended for planetary use.

Estimates of seeing were rated on the standard scale of 0 for hopeless to 10 for perfect. However, as is well known, this method of rating atmospheric steadiness is a very arbitrary one and is largely dependent upon the individual judgement of the observer. The writer does not know how his estimates of seeing conform with the norm, but it should be noted that in any case they will be consistent within the framework of his own observations. An estimate of 3 would be about the lowest at which any useful work can be done, and a rating as high as 7 or 8 would indicate very good seeing indeed.

It was at first intended to tabulate the data accumulated into graph form, but because of the previously mentioned inability to obtain consistent observations this was found to be impractical. It was instead decided that the data could be more lucidly presented if incorporated into a table of statistical visibilities. This table, along with the chart of detail (Figure 5) which accompanies this paper, should be self explanatory.

It should be noted that all of the craterlets in Plato, with the exception of numbers 8, 9, and 33, have appeared as spots from time to time. This is more apt to be the case under higher illuminations, and in this respect the spots may be considered to be analogous to the many other similar spots which may be found on the lunar surface. This is not, however, an invariable rule. Spots may occasionally appear as craterlets under a high light and craterlets as spots under lower illuminations. An explanation is not easy to find. While it might be conceded that differences in seeing conditions have an important bearing on the problem, this is certainly not the complete answer.

The craterlets in Plato appear to fall into two general classes, crater cones and crater pits. Of the crater cones, numbers 1, 2, 3, 4, 8, and 33 have been distinctly seen to possess a cone which is elevated above the surrounding area. Craterlets numbered 5, 6, 7, 11, 19, 20, 21, 22, 24, 26, and 34 appear to be pits or depressions in the floor itself and have not as yet been seen to possess any sort of raised rim. It is of course possible, and perhaps probable, that under more favorable conditions some of these crater pits may yet be translated into crater cones, at some future time.

All of the indicated crater cones and a large number of the crater pits have been seen to hold a shadow from time to time. However, the crater cones tend to

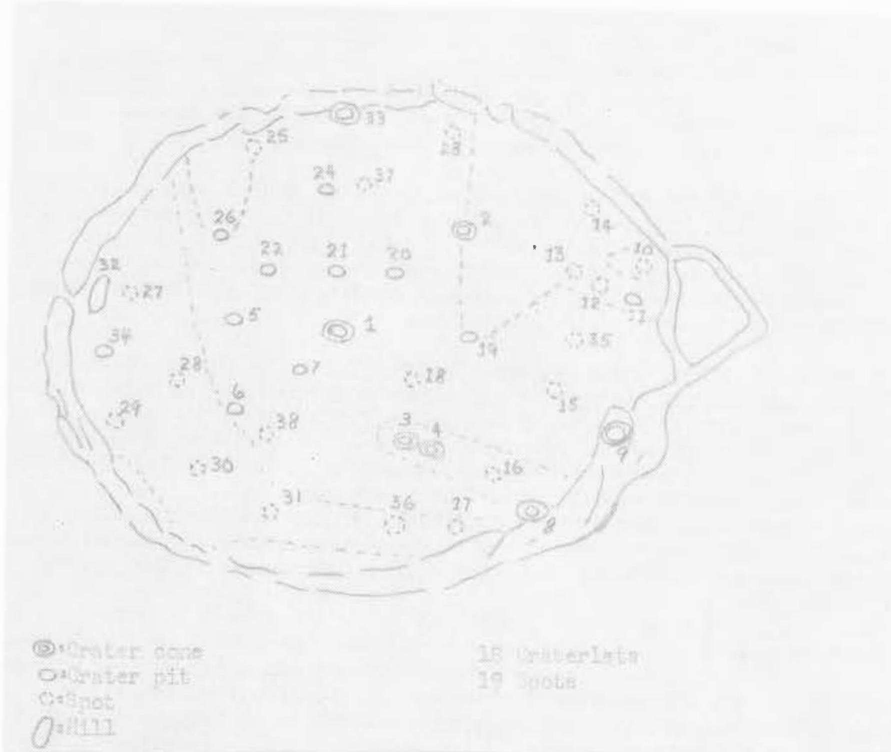

Figure 5. Chart of Lunar Walled Plain Plato in 1953-57 by Alika K. Herring. Based on 38 Drawings with 8-inch and 12.5-inch Reflectors. Compiled to March 14, 1957.

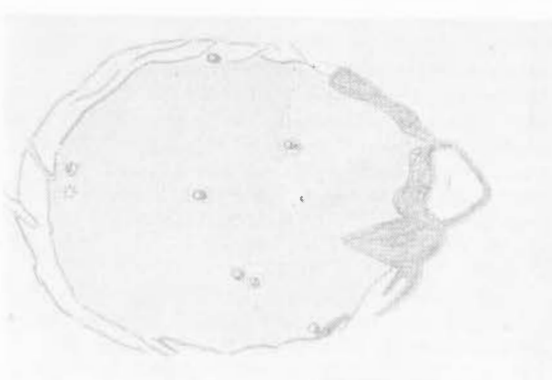

Figure 6. Plato.
Alika K. Herring.
12.5-Inch Refl. 264X.
June 30, 1956. 12^h5^m, U.T.
Seeing 3. Transparency 5.
Colongitude = 176°.7.

**Figure 7. Plato.
Alika K. Herring.
8-Inch Refl. 177X-309X.
December 31, 1955, 7^h40^m, U.T.**
Seeing 6-8. Transparency 5.
Colongitude = 116°.0.

VISIBILITY OF DETAIL IN PLATO-1953-57. Based on 38 Drawings with 8 Inch and 12.5 Inch Reflectors. Compiled to March 14, 1957. Data by Alika K. Herring.

No.	No. Times Seen as Craterlet	No. Times Seen as Spot		Colongitude when seen as craterlet:
1.	33	3		Normally always seen so.
2.	27	5		" " " ".
3.	24	9		" " " ".
4.	23	4		" " " ".
5.	1	6		129°13
6.	3	3		26°42; 38°75; 20°47
7.	1	4		38°75
8.	3	-		129°13; 176°67; 158°26
9.	10	-		13°64; 26°42; 38°75; 164°07; 20°47; 34°27; 145°81; 158°26; 37°41; 39°87
10.	-	1		
11.	2	7		158°26; 39°87
12.	-	8		
13.	-	3		
14.	-	1		
15.	-	6		
16.	-	11		
17.	-	8		
18.	-	3		
19.	2	22		19°76; 20°47
20.	1	14		39°87
21.	2	5		158°26; 39°87
22.	2	7		38°75; 51°73
23.	-	1		
24.	2	4		26°42; 49°26
25.	-	5		
26.	1	14		20°47
27.	-	7		
28.	-	1		
29.	-	2		
30.	-	4		
31.	-	3		
32.	-	-	(Hill)	
33.	5	-		19°36; 176°67; 20°47; 34°27; 158°26
34.	3	1		158°26; 39°87; 64°39
35.	-	2		
36.	-	1		
37.	-	2		
38.	-	1		

The numbers used refer to the chart of Plato, Figure 5.

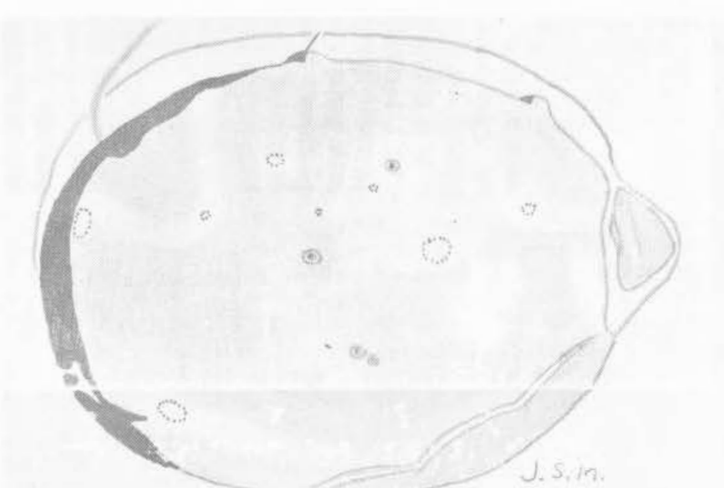

Figure 8. Plato. Joe S. Miller. 12.4-inch refl. 175X, 310X. June 8, 1957. 5^h 30^m, U.T. Colongitude = 34°3.

hold a shadow more often than the crater pits, which would seem to indicate that the latter are comparatively shallow depressions.

Craterlet number 9 perhaps deserves special mention. It lies on the lower end of a ridge which juts out onto the floor from the northeast wall. The odd thing about this craterlet is that the east and west walls of its rim are considerably higher than the north and south walls, so that under some illuminations it appears to be more of a division in the ridge than a craterlet proper. Under other lightings its true nature is obvious. It might also be noted that the diameter of this craterlet is about two miles, which makes it the largest object of all the detail listed. It would be a rather conspicuous object were it not situated in the foothills of the northeast wall.

As for the possibility of apparent changes in the sizes of the craterlets, most of them appear to be quite consistent in this respect. The one possible exception is the well known "twins", numbers 3 and 4, which do appear to vary in size with respect to each other. Usually number 3, the western member of the pair, has appeared to the writer to be the larger and more distinct, but on occasion number 4 has appeared to be of equal size or even somewhat larger than its neighbor. The situation appears to be very similar to the classical examples presented by the well known pair of Messier and W.H. Pickering as well as Beer and Feuillé. There is little reason to believe that these apparent changes in size are caused by anything other than changes in illumination.

Some mention must be made of the actual dimensions of the floor detail seen in Plato. As was previously pointed out, craterlet number 9 is the largest object with a diameter of about two miles. This is an estimate only as the writer has no means of making accurate measurements. The diameter of number 1 was, however, measured by W.H. Pickering and a determination of 7000 feet was the result. He further obtained diameters of 3 to 4 thousand feet for the next four in size. Some of the finer detail seen by the writer is obviously very much smaller than this and must range downward, particularly in the case of some of the spots, to as small as several hundred yards.

Note must also be made of the obvious tendency of the detail to arrange itself linearly. Several distinct rows of spots and craterlets may be noticed, as for instance, the distinct line marked out by numbers 12, 35, 15, 16, and 17. Another distinct row might be drawn through 11, 12, 13, 2, 37, and 25. Lesser chains might be indicated by 17, 36, 31, and 21, 20, and 22.

This linearity of detail is certainly more than accidental. While the writer makes no claim to being a geologist, it should be apparent from a study of the many terraces and landslips on the inner walls of Plato that the interior was at some time in the past liquid and very much higher than its present level. As it cooled and solidified, it also subsided, the strains of subsidence eventually proving to be too much for the crust. The end result would find these stresses relieving themselves as fractures in the crust. Pent up pressures of the still heated interior would tend to relieve themselves at various points along these lines of weakness, resulting in spatter cones and vents surrounded by ejecta and other volcanic debris.

There are no doubt very many similar terrestrial cases. The writer calls to mind the row of crater cones which are so conspicuous a few miles west of Albuquerque, New Mexico. These are very obviously the end result of a similar relief of internal pressures along a fault in the crust of the earth. There must certainly be many more analogous instances.

The writer has long been of the opinion that all of the spots on the floor of Plato contain a volcanic vent of some kind. And since, during the time he has had this formation under intensive study, he has been able to translate an increasing number of spots into craterlets this theory would appear to be at least partially substantiated.

The writer sincerely hopes that the foregoing remarks will serve in some measure to create an interest in this intriguing formation which will entice other observers into the study of Plato. Plato has much to offer the tyro as well as the more seasoned observer. For those beginning selenographers who are anxious to acquire skill in the mysterious art of lunar sketching, it may be stated that Plato is probably the easiest of all the major lunar formations to draw. And there is always the possibility that the observer, be he tyro or otherwise, who faithfully and systematically studies Plato may someday find himself in the position of being able to make a material contribution to our knowledge of the crater and even perhaps discover a solution to some of themysteries which lie therein.

THE NIGHT SIDE OF THE MOON

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

(Paper read at the Second A.L.P.O. Convention, September 2, 1957).

The night on the moon is very long, for it lasts 14 of our terrestrial 24 hour periods. It seems probable that something may happen during the darkness but little appears to have been done to find out the condition of the surface. The matter is of considerable interest and what follows is the result of the author's own investigation over a period of 30 years.

The researches of Pettit and Nicholson in 1930 at Mt. Wilson indicate that with the sun at an altitude of 10 degrees the temperature is -58° Fahr. and it falls to -243° Fahr. immediately after sunset. Even if these results are in error it remains demonstrated that there is a sharp drop in temperature at sunset and this is maintained throughout the lunar night. The greatest variation takes place in the equatorial region but a temperature around -243° Fahr. is experienced over the entire disc which is in shadow.

Although direct sunlight is withdrawn from the night side, the hemisphere turned towards the earth is nevertheless strongly illuminated, reaching a maximum at New Moon. As seen from the lunar surface the earth then appears fully illuminated and it has been estimated that our planet reflects the sunlight to such a degree that the lunar landscape receives sixty times as much light as the Full Moon reflects on to the earth. It is a matter of common observation that when the moon is at the crescent phase the darkened portion of the disc is easily discernible with the naked-eye. Although the earthlit portion normally disappears to visual observation at the First Quarter it can be detected photographically until 3 days before the Full Moon and reappears 3 days after this phase.

A remarkable series of photographs of the earthlit portion was obtained some 25 years ago by Quenisset at the Observatoire Flammarion, Juvisy, France, some of which show a considerable amount of detail. But a critical investigation requires visual observation using moderate powers on telescopes of considerable aperture. The only previous investigation appears to have been that of the late Professor Shaler, of Havard College Observatory, who used the 15-inch refractor for the purpose. But since then 80 years have passed and a new investigation was desirable.

Professor Shaler stated that his researches indicated that whatever feature was bright at the Full Moon was also bright when illuminated by earthlight. The author has found that while this statement is in general true it requires modification in detail.

The author's researches may be summarised as follows:

On the earthlit portion by far the brightest object is the crater of Aristarchus (54), together with the mountain mass immediately to the north and a bright ray-like spur from the east wall of the crater. The bright ray systems associated with Tycho (83), Copernicus (35), Kepler (56), which are the chief and most striking systems on the disc; Proclus (5), Anaxagoras (44), and other centers which are so conspicuous at Full Moon, can be traced but are not particularly prominent. The rays around Copernicus, Kepler and Anaxagoras are then comparatively feebly marked while only some of the Tycho rays can be described as bright. The brightest portions are individual spots on the course of the rays. Over the entire darkened disc isolated spots attract attention by their brilliancy and are indicated on the accompanying

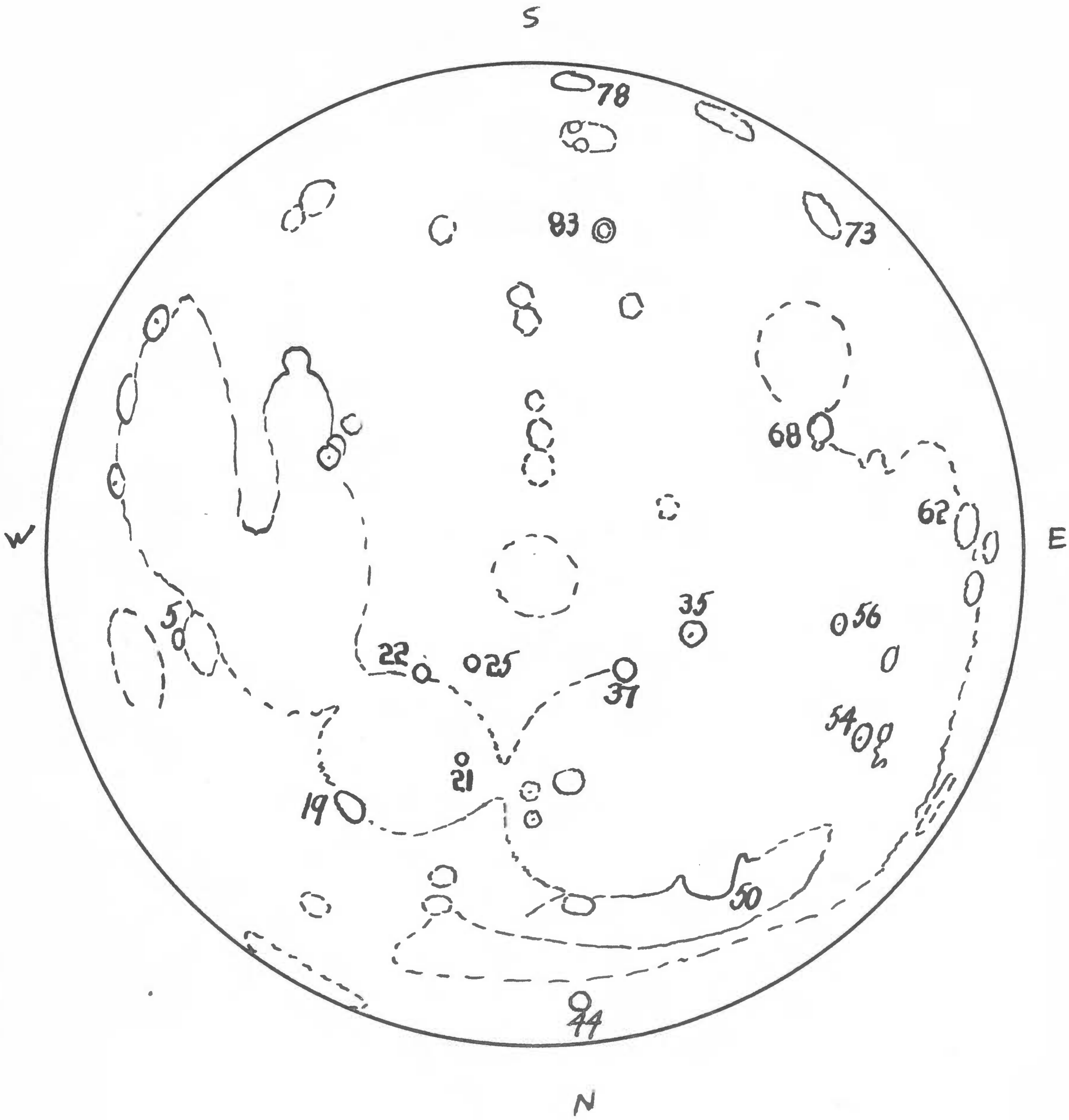

Figure 9. Key Chart of the Moon. The Numbers are Lunar Features Mentioned in the Article by H. Percy Wilkins.

diagram. Such spots and areas are especially evident near the eastern limb and in particular to the south and northeast of the walled plain Grimaldi (62). A portion of the western rampart of this, in itself, dark formation, is also very brilliant.

The craters of Manilius (25), Menelaus (22), and Conon shine brightly; the Jura Mountains (50), bordering the Sinus Iridum, are also quite conspicuous, but the mountains Pico and Piton, which are bright in full sunshine, are but feebly so in the earthlight, while the spot Linné (21), is very difficult to detect. On the day side and under high illuminated Linne appears as a very bright spot; illuninated by earthlight it is a feeble nebulous mass, but a bright spot can be seen to the south which is not visible in sunshine.

Turning from the bright to the dark areas, the interior of the great formation Schickard (73), exhibits the dark areas almost as well by night as by day. Perhaps the crater of Eratosthenes (37) is the most interesting of all lunar formations to study under night conditions. Near sunrise or sunset Eratosthenes appears as a well marked crater, with lofty walls which cast strong shadows. Towards Full Moon this appearance is entirely lost and Eratosthenes then becomes an assembly of dark spots on a lighter background in which it is difficult to trace either the outline or details. The peculiar motion of some of the dark spots during the lunar day was attributed to some kind of vegetation by Prof. W. H. Pickering. In the earthshine Eratosthenes presents the same patchy appearance as it does at Full Moon which suggests, even if it does not prove, that the dark spots brought out by high illumination are not due to vegetation but to the nature of the surface there.

Some regions which appear bright at Full Moon are not so in the earthshine; among these are the interiors of Gassendi (68) and Posidonius (19).

Since the earth appears almost fixed in the lunar sky, the direction of the earthlight is nearly constant and may be regarded as approximating to that of the solar radiation at Full Moon. By comparison with sunlight that light reflected by the earth is comparatively feeble and the outlines of the various lunar features are revealed solely on account of the differences in their light reflecting capacity. The intensity of the earthlight depends upon the phase of the earth and the distribution of seas and land on the sunlit portion of the terrestrial disc. The earth is fully illuminated at New Moon, in Quarter phase when the moon is also at a Quarter, and in crescent phase between the Quarters and Full Moon. The intensity of earthlight is increased when the great land mass of Europe, Asia and Africa faces the moon and is decreased when the vast expanse of the Pacific Ocean is thus disposed. The extent to which the Polar Regions are exposed and the amount of cloud in the terrestrial atmosphere are also factors.

Hence the intensity of the earthshine and the visibility of detail is subject to considerable variation. Lack of appreciable detail, when the lunar surface appears of a uniform tint, is not frequent but occasionally happens. No detail whatever, not even Aristarchus, could be detected on the earthlit portion of Jan. 7, 1919, although the sky was very clear and the moon favorably placed.

Remarkable variations in brilliancy may be seen in connection with the light area immediately to the northeast of Grimaldi (62). This area extends from the crater of Riccioli northwards for at least 200 miles and, probably, as much from east to west. Although always visible, when any detail can be detected, it is sometimes the most conspicuous area on the darkened portion and while not so bright as Aristarchus attracts the eye on account of its great area. Another smaller and often more brilliant area is that to the south of Grimaldi (62), which is also variable in intensity. Both were exceptionally bright during 1935.

It is well known that the bright rays associated with Tycho (83), do not start at the wall of the crater, which is surrounded by a dark zone. This dark ring is very prominent in the earthlight. One of the rays runs southwards to the east of Newton (78) and this ray appears to be displaced towards the west when compared with its location on the day side. At Full Moon a very dark, almost black, spot appears to the southwest of Copernicus (35), but this cannot easily be detected in the earthshine. On the other hand the crater of Tobias Mayer, which lies to the northeast of Copernicus, appears relatively brighter in the earthshine than it does in sunshine.

Temporary bright spots in the earthshine are rare but were recorded by Schroeter in 1788 and Grover in 1865. Such flares, lasting up to 15 minutes, may have been due to meteoritic impact. The sudden glows, only lasting a few seconds, witnessed by some observers are difficult of explanation but may be caused by electronic bombardment. The writer has seen such transient glows within Aristarchus and also Copernicus.

As already stated this aspect of selenographical research has been neglected in the past and in this paper the writer merely presents the results of his personal observations. Despite the difference in temperature the majority of the details which can be distinguished in the earthlight appear unchanged when compared with their aspect in sunlight. Shadows due to the earthlight can sometimes be detected. As in sunlight so in earthlight the mountainous regions appear brighter than the low lying plains. While observation shows this clearly certain regions appear relatively brighter in earthlight than in sunlight. Seen under the best conditions the disc exhibits a remarkable and striking appearance. On the darkened background with the faintly marked plains, craters and mountains are a number of bright spots, some almost star-like in appearance, and resembling lights. Variations occur in both the intensity and number of these spots and in this connection it is important to note that under the most favorable conditions some of the then bright areas, more especially that to the northeast of Grimaldi, can be resolved into a number of separate bright points. The use of the word "lights" does not, of course, imply that they are of an artificial nature. When men land on the moon artificial lighting would hardly be needed in the night time because of the intensity of the earthlight. It is possible, although certainly not probable, that some of the temporary bright spots seen in the earthlight may actually be lights due to the extrusion of incandescent matter by the operation of internal (i.e. volcanic) forces.

CAN SOUTHERN AMATEURS MAKE A GREATER CONTRIBUTION TO PLANETARY OBSERVATIONS?

by

Frank M. Bateson

(Paper read at the Second A.L.P.O. Convention, September 2, 1957).

One of the most arresting facts about amateurs in the Southern Hemisphere is the volume and quality of their Variable Star work. Amongst their observers they have the world's two ace observers—Reginald de Kock in South Africa of the A.A.V.S.O. with a yearly total of around 6,000 observations, and Albert Jones, of the New Zealand Variable Star Section with an annual total of 10,000 observations. A conservative estimate places the variable star observations from the Southern Hemisphere around 30,000 each year.

It has always been that southern observers have figured prominently in this work and well known names stud the pages of their records. It almost appears that variables exercise some peculiar fascination on observers in lonely places.

Equally arresting is the comparative paucity of serious planetary work from the south, outside of that done during favorable Martian oppositions. By serious planetary observing I mean regular recording of the brighter planets apparition after apparition. There have been, of course, brilliant planetary observers in the southern lands but they have been few and far between.

It has been suggested to me that perhaps I can account for this state of affairs, and, if it is correct then perhaps suggest some remedies. Recently ("THE SCIENTIFIC MONTHLY" 1957 March) Dr. John B. Irwin called attention to the complete unbalance in telescopic facilities between the Northern and Southern Hemispheres. He was, of course, discussing professional observatories. In these the unbalance is not only in the large instruments, but extends down to small instruments and observatories. Professionally he points out that the obvious solution would be the establishment of a number of southern observatories, financed and operated by the various Northern Hemisphere nations and universities.

That solution would also partially solve what is the basic drawback in the south--isolation. Isolation from meetings such as this; isolation from the stimulating contact with professionals; often even isolation from other amateurs. It is not surprising, for after all the countries concerned are comparatively newly settled. A hundred years ago the States did not have many astronomical societies and few amateurs. These often worked alone. Australia and New Zealand in particular are still in much that stage.

A good Observatory always tends to create astronomical interest and societies in its vicinity. From these too spring the serious observer. But since we don't wish to wait till the justified vision of more Observatories in the South becomes reality, perhaps we can reverse the process. Can sufficient interest be created by local Societies to bring about both the establishment of professional Observatories and then with that stimulus the serious amateur observer?

I think the answer is a very definite yes -- provided we can count on the support, guidance and interest of northern societies. Auckland, the largest city in New Zealand, also has one of the Dominion's most active astronomical societies. This society has for some time now advocated the establishment of an Observatory in or near that city together with a planetarium. So well has it done that a donor has been found who will provide funds for a reflector--Newtonian Cassegrain of about 20 inches. The site has yet to be located.

But normally this is the exception rather than the rule, for communities being generally small, such donations are not to be expected. Suppose we examine the instruments of amateurs in New Zealand to see whether the unbalance in the professional observatories extends to amateurs.

In 1956 the Royal N.Z. Astronomical Society took a census of telescopes owned by its members. Whilst good planetary studies can be made with small instruments they are best done with refractors of five inches aperture or more, and reflectors over six inches. Of refractors the census showed 5 instruments, from 5" to 9". There were 20 reflectors over 6". The questionnaire on which the census was based went to 206 members, of whom 100 completed the forms. The majority of those who did not are known not to possess instruments. Allowing for instruments belonging to members of local societies not affilated to the Royal N.Z. Astronomical Society, there are in New Zealand some 35 suitable instruments in amateur hands ranging up to 14" reflectors. But the number of serious, regular planetary observers is never more than 6.

New Zealand has one professional Observatory--the Carter Observatory in Wellington, doing excellent work with a very small staff specialising mainly on solar and auroral work. Whilst in Australia, and even more so in South Africa, the professional position is better, the position of the amateurs as far as equipment is concerned is very much the same in New Zealand. For instance, in the February, 1957 Issue of the <u>Monthly Notes of the Astronomical Society of South Africa</u> are assembled the records from its members during the 1956 apparition of Mars. Six observers are represented with reflectors of 6" to 12" apart from the use of the Johannesburg Union Observatory's 9" and $26\frac{1}{2}$" refractors.

It does appear that even allowing for the smaller populations there is some unbalance between instruments available to southern amateurs compared to their northern counterparts. Judging from publications it would appear that in the States one of the most active phases of astronomy is telescope making. How many of these instruments, many of them by all accounts excellent, are put to worthwhile use? How many after the first flush of excitement are used at all? It should be possible through the A.L.P.O. to pool some of these instruments for use in the South as cases of good observers, without adequate instrumental equipment, are found from time to time.

Too often does the southern amateur get left with the feeling that he is being lectured to. We are all familiar with the northern member who publicly bewails the lack of planetary data coming from the south. After drawing attention to the position no more than a ripple is left; certainly it does not encourage observations since this is no way to encourage them. An observer, when starting out, is attracted by the accessibility of information on the subject he wishes to observe. Take any astronomical book--and they are nearly all published in the northern hemisphere--and recall any that pay the slightest attention to even simple facts such as the different aspect of the planets as viewed south of the line. That is a position which, with even today's high cost of printing, can easily be remedied.

Perhaps the most vexed question concerns the publication of results. I don't think that anybody present will deny that to adequately cover an apparition, of say Juipter, it is desirable to have the observers spread as widely as possible in longitude and latitude. Nor will you deny that the best use can only be made of the observations by coöperation of the observers in pooling their records at some central organisation.

Assume for a minute that you are a member of the Jupiter Section of the N.Z. Astronomical Society. You would receive from myself as its Director guidance and instructions. You duly make a few records and mail them to me. Then you—being human—eagerly wait to see them written up in our local <u>Journal</u>. You find in due course a brief annual report in which your name is mentioned, and possibly, if the observations have been numerous enough a longer résumé of what has been seen. But you are also told that to make the best use of the results they, along with those from other observers, have been sent to the B.A.A. Jupiter Section. After possibly quite a long time you do find mention of your records in their publications. But by that time you have grown cold on observing a planet. Whilst the older and more regular observers are content, as I am, to make their observations and send them away to where they can be used without thought of personal mention, it must be remembered that the beginner receives his greatest encouragement both from publication and from seeing his name in print and being able to discuss his results with other observers. One can't blame him, for it's a natural human trait.

Now the Director of a southern planetary section is faced with a dilemma. He can attempt from the records of his few observers to present a full report in the Journal of his local Society, knowing full well that he often has too few records to really make a worthwhile contribution. Or he can simply collect the records and transmit them to the B.A.A. or A.L.P.O. Usually he adopts a compromise. He publishes, in his local Journal, a report or superficial article giving the main features recorded without any attempt being made to determine rotation periods, etc. He hopes in this manner to keep the interest alive in his Section. And he leaves it to the B.A.A. to use the records to better advantage as they undoubtedly do.

It is obvious that such scattered articles, spread amongst the various astronomical Bulletins and Journals of the southern societies, have no permanent value. Nor can they be regarded in any way as a serious contribution to our knowledge of the planet concerned <u>taken on their own</u>. It is much the same as if all the various planetary observers in the States were to communicate their results to their nearest local Society. These societies in turn would publish the results. The value of the records would then be far less than they are now when handled by the A.L.P.O. It is absolutely essential that results on the planets be pooled and the larger the pool the better our understanding of the planet will be.

It is obvious that, even with so few and so scattered observers, the southern Societies can collectively make an important contribution to this pool. Perhaps ways and means can be found by which the A.L.P.O. through its various Recorders, or even by the appointment of a special Southern Recorder can promote and foster this interest. One needs only to take a leaf out of the Variable Star Observers' books. The A.A.V.S.O. has always done much to stimulate and encourage their southern observers. It should be possible to do the same for planetary observers.

I personally am quite satisfied that the A.L.P.O. can play a big part in this, by fostering and extending planetary work in the south through coöperation and encouragement and by remembering southern observers in their instructions and predictions. Later this year I shall have the opportunity of visiting your country. During that visit I sincerely hope that I shall have the opportunity of meeting your Officers and Recorders. Perhaps some of you will be able to ponder on the brief remarks I have made and then together we may be able to do something to remedy the present position.

<u>AMATEUR ASTRONOMY</u>

by A. C. Larrieu

(Paper read at the Second A.L.P.O. Convention, September 2, 1957).

Let us consider the role of the amateur astronomer in science. Situated far from the official world, he contacts it by the work which he organizes with full independence and by a free choice; he is outside the human battles which sometimes divide the professional astronomers as well as other mortals. What services can amateur astronomy render? What services? -only the greatest ignorance can keep one from knowing them. It is sufficient to look over the history of science to perceive quickly the influence of these isolated observers, an influence arising from wise and patient amateurs devoted to various studies and carrying on their investigations in the several branches of astronomy according to their aptitudes and their previous knowledge, whether practical or theoretical.

Copernicus, to whom we owe a knowledge of the true system of the world, was an amateur astronomer, and his ideas were for a long time laughed at by the public before being definitively accepted. Another amateur, as we know well, the musician William Herschel, the discoverer of Uranus, was the first to gauge the heavens, with the aid of his giant telescopes constructed with his own hands and to take a giant step forward in astronomy that no professional astronomer has been able to equal. And Le Verrier, director of a tobacco factory, who became interested in astronomical calculation and, giving himself up to study of planetary perturbations achieved the discovery of Neptune. And Lord Rosse who constructed an immense telescope of 72-inch aperture by the aid of which he discovered distant nebulae.

Carrington, born at Chelsen, Massachusetts, in 1826, had two observatories for the study of the sun built at his own expense and published a catalogue of "3735 Circumpolar Stars Observed at Redhill" -Waren de la Rue, born in 1815, member of the Royal Society, was the initiator of photographic methods. It was as an amateur also, that Goldschmidt, a painter with a studio in Paris, discovered with a small refractor 14 asteriods. W. Lassell, born in 1799, brewer of beer but interested in science - occupied simultaneously with commercial affairs and scientific studies - put his theoretical knowledge into practice by constructing for himself - as did William Herschel - the instruments with which he searched the skies. In 1847, he discovered a satellite of the planet Neptune and, later, at Malta, found two more satellites and 500 new nebulae.

Finally, to talk a little about my town of Marseille, I must cite Jean L. Pons, who, along with Messier, discovered the largest number of coments. It was as porter at the observatory of Marseille that Pons began his practical work and his study of the sky. The most remarkable of his observations was, in 1810, that of the return of Encke's Comet. Pons left the amateur ranks in 1813, becoming an official astronomer at the observatory of Marseille and, at the solicitation of the Baron de Lach Pons was called by the Duchess of Lucca to be head of the observatory in that town - he died at Florence in 1837.

And now, to end these incomplete notes, let us come down to recent history, for which the oldest among us will have only to search their memories to discover easily the very important and decisive part that your great country has taken in the advance of astronomical science and the parts played in it by the amateurs and the professionals.

I am happy to state that our association - the A.L.P.O. - has had for its cradle an intimate gathering of devoted men interested in the science of astronomy and I send you a wish: it is that our scientific association, which is an assembly of faithful adepts of the cult of Urania, receives even further encouragement - from wherever it may come - to give it the hope of a future even more brilliant and more fruitful.

<u>Note by Editor</u>. Mr. Steadman Thompson translated Mr. Larrieu's paper from French into English.

<u>ON THE EXISTENCE OF CATTERMOLE'S DIOPHANTUS-DELISLE DOMES</u>

by Leonard B. Abbey, Jr.

(Paper read at the Second A.L.P.O. Convention, September 2, 1957).

Of all the many and varied lunar surface features, the domes are, to many observers, the most interesting. As their name suggests, these inconspicuous objects are

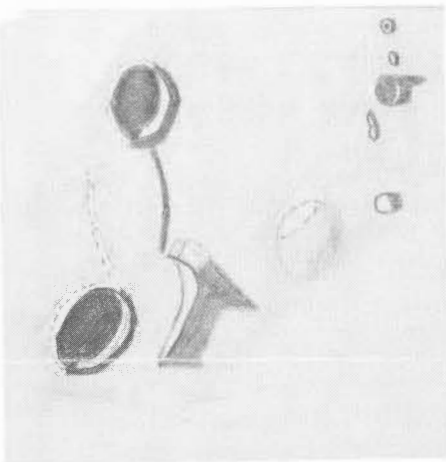

Figure 10. Diophantus-Delisle Region. Leonard B. Abbey, Jr. 8-inch refl. 225X. March 14, 1957. $1^h\ 35^m$, U.T. Colongitude = 62°9.

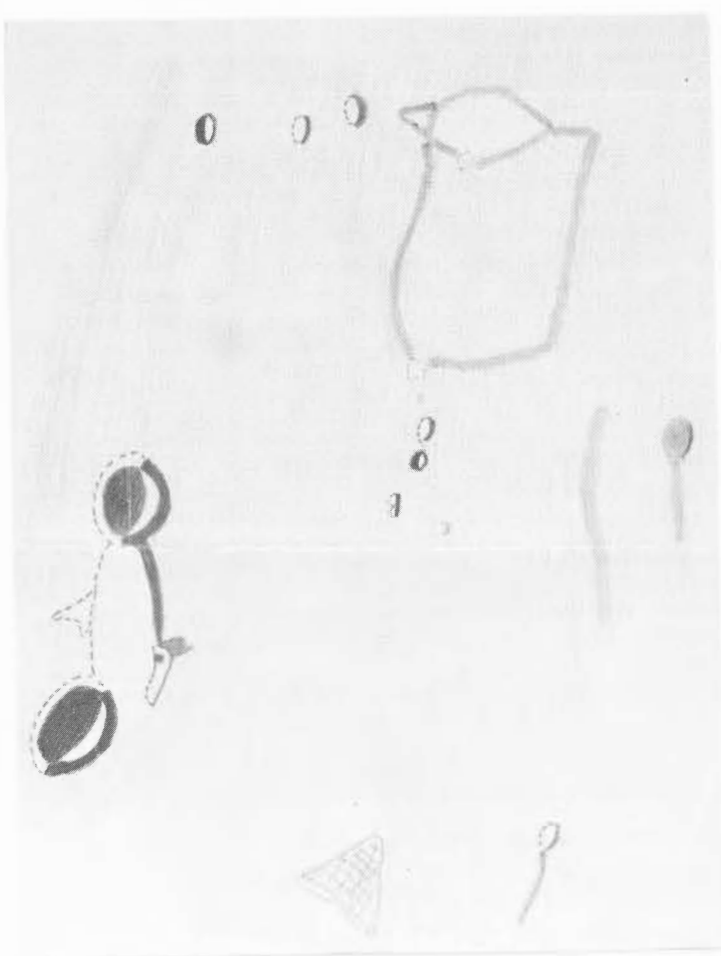

Figure 11. Diophantus-Delisle Region. Leonard B. Abbey, Jr. 8-inch refl. 225X. August 7, 1957. $2^h\ 0^m$, U.T. Colongitude = 45°8.

usually seen in the form of circular or slightly elliptical "bubbles" or "swellings". They give the appearance of having been formed by some internal force pushing up the surface material without being able to penetrate it. Moore[1] states that all domes have been observed to be quite dark objects at lunar "sunrise", and to change to a much lighter shade at higher solar altitudes. Cooke[2] suggests that this may be due to their surfaces containing many minute fissures, which hold shadow under conditions of low solar lighting.

Searching through lunar literature, one finds little, if any, mention of objects that may be associated with the domes with which most modern day selenographers are familiar. In fact, active observation of these objects dates from 1932, when Robert Barker drew attention to them by his observations of the very large dome inside the great walled plain Darwin. Barker's description of this object as a "huge cinder-heap, a lunarian dust-heap which bristles with roughness—like a selenite slag-heap"[3], seems to fit Cooke's explanation of the variable-darkness phenomenon. In the years since Barker's observation, the number of known domes has been increased from under 10 to several dozen. The locations of newly found domes are reported regularly in selenographic journals, particularly those published in Europe.

One puzzling thing is that none of the old observers reported the domes, even though regions containing domes must have come under close scrutiny for other purposes. Goodacre does not give special attention to them, even though his map[4] shows several of the more prominent ones. The maps of Mädler, Lohrmann, Schmidt, Elger, and Neison are, of course, not detailed enough to show such inconspicuous objects. This well illustrates how elusive the domes may be. While I was searching for Larrieu's domes near Hortensius, they were overlooked several times. It was only on the third attempt that sucess was achieved. Once located, they were not difficult to find on later occasions.

One of the more interesting problems posed by the domes is that of the "central pit". About 50% of all known domes have been observed to have these tiny pits, invariably located in the exact center of the dome. Perhaps these pits have some relation to the "summit craters"; small pits apparently identical with those found on the domes, and concentrically located on the summits of lunar mountains. The "summit craters" located on the central peaks of Theophilus, Gassendi, Herschel, and Capella are perhaps the easiest to see in amateur instruments. Like the "central pits", the major feature of interest in the "summit craters" is that they are <u>always</u> located in the exact center of the peak. This fact may prove to be of great importance in connection with speculations as to the origin of the lunar features. According to Baldwin[5] the total number of such pits caused by chance meteor hits as called for by the Meteoric Impact Theory should be about 15. Wilkins and Moore [6], using the great 33" refractor of the Meudon Observatory, and the 25" refractor of the Cambridge Observatory, have raised this number to over 60. Moore's argument against the Meteoric Theory is further strenghtened by the fact that there are no "near misses". Pits are never located on the sides of the domes, <u>always</u> exactly in the center. When a peak or dome has two pits, they are symmetrically located in respect to the center.

In recent years the domes have been studied by Wilkins, Moore, Cattermole, and Cooke. Observers interested in studying the lunar domes are refered to Part I of the catalogue of Moore and Cattermole[7], the first such catalogue to be published.

One of Cattermole's most interesting observations is that made on May 20, 1956[8]. The observation was originally made in an attempt to verify a cleft, shown by Goodacre between Diophantus and Delisle. It is not, however the cleft that interests us. The most amazing feature of Cattermole's drawing is the multitude of domes shown. Over 30 new domes are depicted in an area of only a little over 1500 square miles. Such a density of domes has never before been suspected or recorded. The existence of these domes, if proven, could play an important role in the study of the origin of the lunar features.

On the night of March 14, 1957, an observation of the area under consideration was made by the writer in an attempt to verify Cattermole's objects (Figure 10). Of Cattermole's 31 domes, none were seen, with the possible exception of one area which may have been Cattermole's dome #1. A later observation (August 6, 1957) under better seeing conditions, was more successful: Three domes which may be identified with

Cattermole's domes No. 11 and No. 12 and one dome-like object that Cattermole has not labeled as such were seen. Cattermole's domes 25, 26, 27, 28, 29, 30, and 31 were definitely identified with some of the low-lying "ridges" that are seen on the floor of the Mare Imbrium (Figure 11).

Cattermole's dome #21, a large mass occupying the area between Diophantus and Delisle, was not seen as such; however, the ridge connecting the north edge of Diophantus and the mountain mass east of Delisle (Wilkins' "Beta") may be the edge of a large, but extremely low-lying dome. It seems that if present, such a large, low object could not correctly be called a dome.

The above statements raise the question as to just which objects fall under the definitions of "dome", "ridge" and other accepted formation types. It seems that objects that do not exibit the rounded sides, symmetrical or semi-symmetrical shape, and the (apparently) smooth surface of our "prototype" domes should not be classified as such. The "ridges" in question are very low-lying, and give the appearance of being "ripples" in the surface of the Mare. It is possible that under conditions of extremely low lighting they may be mistaken for chains of domes, however under higher lighting there is no resemblance, while the true domes stand out quite plainly. Carrying Cattermole's method a little further, one might call the lunar plateau Wargentin a dome!

Having examined the question of whether or not Cattermole's objects may be classified as domes, we come to the relative prominence he has given them in his drawing. Cattermole's observation was made with Moore's 12½" reflector; both of mine with an 8" reflector. We both used comparable powers (i.e. 360-90). Seeing conditions were also comparable. Most of the objects in Cattermole's drawing should be visible in a 6", and certainly all of them should be seen in an 8". In addition to this, a recent study by Whitaker of the best available photographs of the region failed to reveal the domes; even though the Hortensius-T. Mayer domes were easily visible. One is obviously led to the conclusion that the Diophantus-Delisle domes must be much less prominent than suspected by Cattermole.

The question of whether or not the Cattermole objects are domes is still largely unanswered. It has been shown that seven of them are, in reality, low-lying ridges. One is dismissed on the grounds that it is too large, low, and irregular to be included in the definition of the dome. Three of them have been confirmed. This leaves 23 domes unconfirmed. It has been shown of what importance the confirmation of these domes may be to lunar feature formation theories. It is hoped that more amateurs will see fit to examine this region in the near future. Speculations, observations, and other comments are invited. Interested observers are referred to the publication of the Lunar Section of the British Astronomical Association, "The Moon", for reports of further findings.

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8. Cattermole, P.J. "The Region Around Delisle and Diophantus." The Moon. 5:1, September, 1956.

9. Ibid.

MERCURY OBSERVATIONS FROM OCTOBER, 1956 TO MAY, 1957

by Owen C. Ranck

(Paper read at the Second A.L.P.O. Convention, September 2, 1957).

This paper is an effort to bring the Mercury Section of the A.L.P.O. up to date. We have received for the month of October, 1956 eight drawings; for February, 1957 one drawing; for April, 1957 five drawings; and for May, 1957 one drawing, plus two fine detailed reports from Dr. Sandner in Germany.

I shall first take the apparition of October, 1956. Of the eight drawings received there were six by Cragg and two by Ranck. Cragg secured the most excellent views of this difficult object that I have seen for some time, in fact since I had received an excellent group from Haas earlier in 1956. Mercury passed perihelion on October 9, 1956; and correspondingly Cragg shows the Argyritis bright spot close to the east limb. This spot was placed nearer and nearer to the center of the disc from October 14 to October 27, mostly because of the change in phase but partly because of changing libration as Mercury approached its aphelion of November 22. Cragg also suggests a strong southern libration since detail appears to be placed south of the positions given on the Antoniadi map. He further comments that Argyritis appeared much larger to him, and also brighter, than usually represented on maps of the planet. At this time the cusps were generally recorded as brighter than the rest of the disc (except Argyritis). Cragg shows a prominent dark streak extending in from the terminator a bit south of the "apparent equator", a streak which he prefers to identify as Horarum Vallis. This feature too is shown farther south than on the map.

For the morning apparition of February, 1957 I was overwhelmed to receive the grand total of one observation, by Chester J. Smith with his fine 9.5-inch refractor. Smith saved the day. His drawing shows very prominently Solitudo Persephones, with Solitudo Atlantis more or less combined with it. He drew Criophori very wide, but not very dark, as seen in the past by Haas, Cragg, Roth, your writer, and many others. Smith shows Maiae near the terminator and one dark streak, which he has not named, extending from the terminator. The latter may be an extension of Criophori. Argyritis is not shown. On this date Mercury was just past the half way mark between its perihelion and aphelion points. Smith observed the planet twice within a 24-hour period, and he adds that he could find no shift of the detail in that time.

Let us now look at the evening apparition of April, 1957. The planet was fairly well placed in the evening sky, 16° N. to 18° N. in declination during the period covered. There were five drawings in all, one by Chester J. Smith and four by your writer. I would say that the most conspicuous features were again Argyritis and Horarum Vallis. Most of the drawings agree that the south cusp was the darker and the north one fairly bright, in accord with Antoniadi's map. Two drawings made by the two observers on April 15, almost three hours apart, are in good agreement. Smith makes the comment that Mercury's features must have been greatly displaced. They were, for at this point in Mercury's orbit there would be an eastern libration. Smith recorded the south cusp as very blunted and claimed that the north one was visible past the expected terminator.

One further observation of this apparition was made by Stephen Sinotte. The detail here was described as faint streaks at the limit of visibility. It is the opinion of the writer that these probably are boundaries between areas of different intensity. One line probably is an extension of Horarum Vallis.

I would now like to summarize the work of our German colleagues. They made an effort to compare the predicted and the observed times of dichotomy, finding very little difference between them. They also say that the observations were not too reliable since the seeing conditions were very bad. This bad seeing also prevented

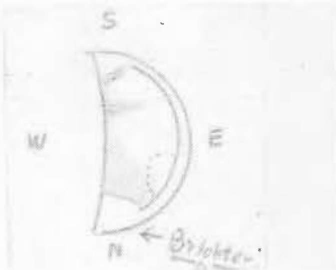

Figure 12. Mercury.
Owen C. Ranck.
4-inch refr. 120X.
October 11, 1956.
$11^h\ 55^m$, U.T.

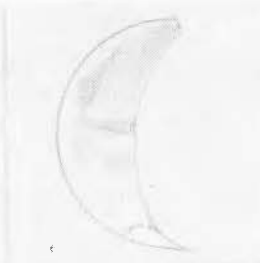

Figure 13. Mercury.
Chester J. Smith.
9.5-inch refr. 180X.
April 15, 1957.
$3^h\ 0^m$, U.T.

Figure 14. Mercury.
Thomas Cragg.
6-inch refr. 180X.
October 25, 1956.
$18^h\ 50^m$, U.T.

Figure 17. Chester J. Smith with his Observatory and 9.5-Inch Refractor. Refer to Article by Mr. Smith in this issue.

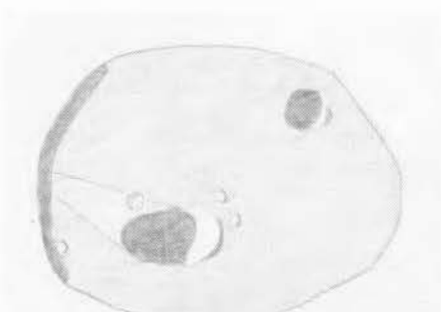

Figure 18. Lunar Crater Cassini. Raymond Oxford. 4-inch refr. 180X, 240X. April 30, 1955. $3^h\ 0^m$, U.T. Colongitude = 5°.3.

Figure 19. Lunar Crater Plinius. Robert M. Adams. 4.3-inch refr. 168X, 201X. April 10, 1954. $1^h\ 50^m$, U.T. Colongitude = 351°.1.

their recording very much detail. Dr. Sandner used a 4-inch F:17 refractor. The German group also attempted to photograph the planet; but there was only five minutes of time on each day suitable for doing so, after Mercury was sufficiently bright in the twilight and before it got so low that the atmosphere became too disturbed for a photograph. They assert that the detail on Mercury is much easier to see than that on Venus, and I think we can all agree with them there. Dr. Sandner made an effort to compare their work with the map of Antoniadi. I notice myself that they do not have any bright spot in the position of Argyritis, according to both Antoniadi and our own A.L.P.O. map by O'Toole. Dr. Sandner comments that very dark streaks were identified as Horarum Vallis, Admenti Vallis, and farther south Solitudo Hermae Trismegisti. Solitudo Criophori was reported to be very dark. Solitudo Aphrodites was very faint. Atlantis was reported as 30 degrees wide. One small band could not be identified on the Antoniadi map; Haas reported this feature and mapped it. Solitudo Phoenicis appeared very dark; and 30 degrees to its west there was noticed a dark spot, thought to be Solitudo Jovis.

All of Dr. Sandner's past observations were lost during the war in the bombing of Nuremberg.

It is said that Copernicus, who died at the age of 70, although he searched for Mercury never saw it. This circumstance may be some consolation to those in the present who have failed to find it.

Some observers of the past have placed an extremely high mountain near the south cusp, claiming it to be about 10.7 miles high and explaining in this way the blunting of this cusp. Schroeter gave the planet a rotation period of 24 hrs., 5 mins., and 48 secs. Schiaparelli disagreed with this period as "far from the truth". As you all know, it is generally accepted that Mercury keeps the same face continually towards the sun.

I hope that during future apparitions more of our members will try their hands at this neglected object.

PROGRESS REPORT OF THE A.L.P.O. LUNAR METEOR SEARCH PROJECT

by Robert M. Adams

Again we wish to report the results of our efforts on behalf of the lunar meteor search project. The second year of observations covers the period from August, 1956 through July, 1957. Along with sunspot maximum we have encountered a very high incidence of cloudy and rainy weather-which has made inroads on the number of reports, particularly during the latter half of this period of observations.

The following people have engaged in the lunar meteor work since August 1, 1956, submitting one or more reports each:

Lee Angle, Fort Worth, Texas. 82-inch McDonald reflector.

James Berg, Dyer, Ind. 4-inch refl.

W. F. Duncan, Galveston, Texas. 6-inch refl.

Lyle T. Johnson, Welcome, Md. 16-inch refl.

G. H. Johnstone, Albuquerque, N. Mex. 6-inch refl.

Jerome Kaltenhauser, Lindstrom, Minn. 6-inch refl.

Mary C. MacKensie, Montreal, Quebec, Canada.

Contributing observers were I. Williamson, 80-mm. refr.; Sidney Sundell, 3.5-inch refl.; T. Noseworthy, 6-inch refl.; W.A. Warren, 6-inch refl.; K. and Z. Zorgo, T.F. Morris, and D. Yane, 6-inch refl. and 5-inch refl.; E. Danson and M. Mendelssohn, 3.5-inch refl. and 6-inch refl.; R. Venor, 12-inch refl.; N. Gauss and B.F. Lafin, 6-inch refl.

Ian C. McLennan, Edmonton, Alberta, Canada.

Several observers during the eclipse of the moon on November 18, 1956.

Robert L. Miles, Woodland, Calif. 8-inch refl.

Frederick Pilcher, Topeka, Kansas. 3.5-inch refl.

L. J. Robinson, Sylmar, Calif. 10-inch refl.

Eugene Spiess, Manchester, Conn. 5-inch refr.

Steadman Thompson, Columbus, Ohio. 6-inch refl.

Fred R. West, Andover, Mass. 8-inch refl.

We are still encountering many difficulties when attempting to obtain overlapping reports. In addition to the usual reasons for not observing such as the weather, trees, other engagements, and freezing weather, we must now contend with increasing demands on observers for other types of observations such as those connected with the I.G.Y. (International Geophysical Year) and particularly with the increasing demands of the Moonwatch program. In spite of all of these vicissitudes we still have a respectable number of reports. I should particularly like to take this opportunity to thank the Montreal team for their persistent observations. I notice an increase of the numbers of reports of faint flashes. One might expect that the first confirmed lunar meteor will be a faint one.

Mr. Berg reports a "lunar meteor" at $6^h\ 7^m$, U.T., during the lunar eclipse of November 18, 1956. It was reported to be near Cleomedes and of one second duration. It was in the nature of a streak rather than a flash.

Mr. Duncan saw a "possible flash" during the lunar eclipse at $6^h\ 33^m$, U.T., northeast of Aristarchus. He states that he saw several flashes on November 27, 1956 between $11^h\ 10^m$ and $12^h\ 5^m$, U.T., all of which were dim: at $11^h\ 26^m$ south of Proclus, at $11^h\ 28^m$ near Gutenberg, at $12^h\ 2^m$ south of Picard, and at $12^h\ 5^m$ inside the west shore of Mare Crisium. He observed on November 28, 1956 from $10^h\ 55^m$ to $11^h\ 45^m$, U.T. but with negative results.

Mr. Angle saw a faint streak of light with a very short path while looking through the 82-inch McDonald reflector at $2^h\ 15^m$, U.T. on August 10, 1956. Mr. Angle is not an experienced observer and reported to Mr. John Farrell. I should perhaps take time out to explain at this point that there are many meteors in our own atmosphere that are very faint and have short paths. I spend much time at my 10-inch ' scope doing variable star observations and make up lists of telescopic meteors for Dr. Olivier as a by-product. The greatest number of streaks of light thus seen are faint and have short paths. It behooves all of us to recognize that many so-called lunar meteors are nothing more than such faint earth meteors.

Mr. Johnson observed on October 10 and 11, 1956, $2^h\ 30^m$ to approximately $3^h\ 9^m$, U.T. with wholly negative results.

Mr. Johnstone reported these observations: September 11, 1956, $2^h\ 15^m$ to $2^h\ 55^m$; March 7, 1957, $3^h\ 0^m$ to $3^h\ 45^m$; April 6, 1957, $3^h\ 10^m$ to $3^h\ 45^m$; and July 4, 1957, $3^h\ 30^m$ to $4^h\ 25^m$ (times by U.T.). The reports were all negative.

Mr. Kaltenhauser observed on August 30, August 31, and September 12, 1956, one hour on each date with negative results. He reports seeing a faint streak at $1^h\ 0^m$, U.T. on February 6, 1957 at the end of an observation period which started at $0^h\ 35^m$ (?). This "streak" was of $\frac{1}{2}$ second duration on the southeast limb between Eichstadt and Bourand. (We are sorry to lose the services of Mr. Kaltenhauser. He plans to enter college in the fall of 1957. He has left a trail of good reports for the past two years, and we hope that he can find time to send us an occasional observation.)

The Montreal observers have built themselves an outstanding series of observations. Most of the observations were made by two or more observers, spaced sufficiently far apart to be able to verify a lunar meteor. It is very interesting to note that there was not a single verification. Their work was particularly careful and was carried out along strictly scientific lines: August 12, 1956, $1^h\ 0^m$ to $1^h\ 24^m$ (U.T., as usual), negative; August 13, 1956, $1^h\ 0^m$ to $1^h\ 30^m$, negative; October 9-10, 1956, $23^h\ 30^m$ to $0^h\ 30^m$ (a period of one hour), two observing groups, negative; November 8, 1956, $23^h\ 0^m$ to $23^h\ 30^m$, negative; November 9, 1956, $23^h\ 0^m$ to $23^h\ 30^m$, two observing groups, negative; November 18, 1956, $5^h\ 45^m$ to $6^h\ 40^m$, bright instantaneous flash over the Doerfel Mountains at $5^h\ 59^m$, second observing group from $6^h\ 0^m$ to $6^h\ 39^m$ with negative results, and third observing group from $5^h\ 45^m$ to $6^h\ 40^m$ with results also negative, suggesting that the flash mentioned was an earthly meteor; December 6, 1956, $23^h\ 0^m$ to $24^h\ 0^m$, negative; December 7, 1956, $23^h\ 0^m$ to $24^h\ 0^m$, negative; January 4, 1957, $23^h\ 20^m$ to $24^h\ 0^m$, a bright flash of one second duration between Pierce and Macrobius at $23^h\ 39^m$, a second group from $22^h\ 30^m$ to $23^h\ 30^m$, negative results; January 5, 1957, $23^h\ 4^m$ to $24^h\ 0^m$, negative, two other groups also observing at the same time with negative results; March 7, 1957, $2^h\ 0^m$ to $3^h\ 0^m$, three groups all indicating negative results; May 6, 1957, $1^h\ 0^m$ to $2^h\ 0^m$ and $2^h\ 0^m$ to $3^h\ 0^m$, negative; May 7, 1957, $1^h\ 0^m$ to $2^h\ 0^m$ and $2^h\ 0$ to $3^h\ 0^m$, negative.

The Edmonton, Canada group report that several of their members observed during the whole of the lunar eclipse of November 18, 1956 with completely negative results.

Mr. Miles reports that he observed on several occasions: August 1, 1956, $11^h\ 30^m$ to $12^h\ 0^m$(?), U.T., negative; August 12, 1956, $4^h\ 0^m$ to $4^h\ 30^m$, negative; September 11, 1956, $3^h 15^m$ to $3^h\ 45^m$, negative; September 12, 1956, $3^h\ 15^m$ to $3^h\ 45^m$; October 28, 1956, $12^h\ 45^m$ to $13^h\ 15^m$ (?), negative; October 29, 1956, $12^h\ 45^m$ to $13^h\ 15^m$ (?), negative; November 9, 1956, $2^h\ 0^m$ to $2^h\ 30^m$, negative; November 18, 1956, eclipse observation negative; June 4, 1957, 5^h to 6^h, negative; June 5, 1957, 5^h to 6^h, very faint flash-exact record not kept.

Mr. Pilcher observed on two occasions with negative results: January 9, 1956, $23^h\ 30^m$ to $24^h\ 0^m$ and April 6, 1957, $3^h\ 0^m$ to $3^h\ 30^m$.

Mr. Robinson writes that he saw a streak suspect near Albategnius across Ptolemaeus at $6^h\ 35^m$, U.T. while observing the total lunar eclipse of November 18, 1956 from $6^h\ 7^m$ to $7^h\ 38^m$.

Mr. Spiess is credited with the longest series of observations made by an individual. These include, all with negative results: August 12, 1956, $1^h\ 0^m$ to $1^h\ 32^m$, U. T.; September 16-17, 1956, $23^h\ 50^m$ to $0^h\ 38^m$ (a period of 48 minutes); October 7, 1956, $21^h\ 55^m$ to $23^h\ 42^m$; October 9-10, 1956, $23^h\ 15^m$ to $0^h\ 40^m$; October 11-12, 1956, $23^h\ 40^m$ to $0^h\ 35^m$; November 9, 1956, $22^h\ 55^m$ to $23^h\ 30^m$; January 5, 1957, $22^h\ 40^m$ to $23^h\ 18^m$,; February 4-5, 1957, $23^h\ 8^m$ to $0^h\ 20^m$; March 4-5, 1957, $23^h\ 50^m$ to $0^h\ 16^m$; March 7, 1957, $1^h\ 55^m$ to $2^h\ 42^m$; May 3, 1957, $1^h\ 20^m$ to $2^h\ 58^m$; May 7, 1957, $1^h\ 15^m$ to $2^h\ 30^m$; May 8, 1957, $1^h\ 30^m$ to $3^h\ 10^m$; June 3, 1957, $1^h\ 57^m$ to $2^h\ 2^m$; July 2, 1957, $0^h\ 58^m$ to $2^h\ 8^m$; July 3, 1957, $1^h\ 50^m$ to $2^h\ 18^m$; July 5, 1957, $2^h\ 33^m$ to $3^h\ 16^m$.

Mr. Thompson observed on four different occasions: August 31, 1956, $10^h\ 1^m$ to $10^h\ 30^m$; September 28, 1956, $10^h\ 28^m$ to $11^h\ 1^m$; October 28, 1956, $10^h\ 44^m$ to $11^h\ 16^m$; February 24, 1957, $10^h\ 28^m$ to $11^h\ 3^m$. All work resulted in negative findings.

Mr. West, on the other hand, reported "finds" all over the place. On only three occasions he reports seeing four "streaks": September 28, 1956, $10^h\ 25^m$ to $11^h\ 3^m$, U.T., at $10^h\ 40^m$ a "flashing streak" over Archimedes; September 29, 1956, $10^h\ 25^m$ to $10^h\ 55^m$, a bright streak at $10^h\ 43^m$ north of Sinus Iridum; October 11, 1956, $0^h\ 25^m$ to $1^h\ 8^m$, a streak at $0^h\ 45^m$ over Mare Nubium followed by another 30 seconds later west of the first one.

An analysis of the above reports certainly goes to show that we have not verified the observable existence of lunar meteors; and judging by the amount of observations to date, if such observable meteors exist, they are not common. It is hoped that as this project continues we can enlist the interest of more groups such as the Montreal group to observe under the leadership of a project coördinator. However, the observations of individuals are most welcome.

BUILDING A 9.5-INCH REFRACTOR

by Chester J. Smith

After having made several small refracting and reflecting telescopes, the last being a very fine 6-inch F:17 refractor, I decided that I would like a refractor of 9 or 10 inches. It would have been much easier to have a larger reflector; but I have always been partial to refractors because of their very sharp definition on close double stars, since there is less scattered light and the background is darker and freer from specks of light surrounding bright stars than with reflectors. I ordered a 9-inch F:17 objective from Mr. John Mellish and started at once to make the wood patterns for the various parts. (I work as a tool and die maker in a large iron foundry and hence have access to the company's machinery for doing this work after working hours.) Mr. Mellish later wrote me that glass blanks 11 inches in diameter were available so that I could have had a 10.5-inch objective, but by that time I had already made the tube for 9.5 inches of clear aperture.

While I was gathering the patterns and component parts, I set up the concrete forms for a massive pier. This pier measures three feet by four feet at floor level and extends four feet into the ground, flaring out to be larger at foundation level. Above the ground the pier tapers down to one foot by two feet at the top, 86 inches above floor level. At the top a 3/4 inch steel plate, to which the polar axis casting is fastened, is anchored in concrete by hook bolts.

The concrete forms were ½ inch Douglas fir plywood. Surrounding these forms were 4x4 firs, drawn snug by 3/4 inch steel tie rods, about 10 inches apart from top to bottom. Inside the wood forms were placed wood cores and an electrical conduit to carry the current for the motor drive, switch panels, and various motors for the controls. Four amateur astronomer friends assisted me in pouring the concrete. At one time when the forms were almost filled with concrete one of the stay bars fell out. Fortunately, the bar only cracked a little; and we did not lose four tons of concrete all over the back lawn. The forms were removed after three days, and the pier was perfect and very smooth on the exterior.

On the top steel plate, which has push-pull screws for azimuth adjustment, was attached the elevating block of cast iron. The elevating block was machined to 37°45', my latitude in Oakland, California. There was mounted on this block the polar axis casting, which carries a 3-inch stainless steel shaft riding in aircraft quality double row ball bearings. There is also a large thrust bearing at the lower end. Elevating push-pull screws on the polar axis casting bear against the elevating block for latitude adjustment.

On the upper end of the polar axis shaft is attached by very close machine fit the declination cross-head, which also carries a 3-inch shaft. The declination casting itself is 6 inches in diameter where the tube saddle connects. The tube saddle is an aluminum casting and is ribbed very heavily.

The tube is 13 feet long and has two sections. The upper 6-feet section is fiber-glass, and the lower section is of tapered steel fabrication. The two are connected by an aluminum bell reducer casting. The steel section tapers off to a diameter of 6 inches where the focusing casting, cast iron off a wood pattern and cored out, is attached. The upper end of the fiber-glass section carries an aluninum cell and tube adapter with push-pull screws for collimating or squaring on of the objective. The objective cell itself can be removed in only a minute by unscrewing the retainer ring.

The declination circle is 14 inches in diameter, and the hour circle is 10 inches. The slip-ring right ascension circle is 9 inches in diameter. These circles are made of cast iron from wood patterns. I graduated and numbered them on a vertical milling machine. The slip-ring circle rides on a spring-loaded torque clutch so that the telescope can be moved very easily to any part of the sky and immediately resumes tracking without any lost motion. The torque clutch contains hardened and ground 6-inch steel plates, which ride against a 14.5-degree stub tooth worm gear of 100 teeth. All the plates and the worm gear are super-finished to give smooth action. The worm itself below the worm gear is carried in pre-loaded ball bearings with adjustment to eliminate play between worm gear and worm.

The polar axis is driven by a 150 H.P.. Bodine synchronous motor. Also in the drive is a differentiation driven by another motor for push button control in right ascension. The main drive is 0.14 seconds slow per hour and keeps well placed on an equatorial star. Slow motion in declination is accomplished by a clamping arm on the declination axis housing and is similar to the Ross 20-inch Astrographic at the Lick Observatory. After being clamped securely, this arm is the fixed point for the spring loaded plunger which moves against the telescope tube itself. The plunger screw is turned by another slow motion motor. The wiring is connected to the same punch block as the right ascension slow motion. Guiding is easily accomplished without leaving the eye end of the telescope.

On the west side of the concrete pier is another set of circles, actuated by Selsyn motors on the polar and declination axes. This arrangement permits easy reading while standing on floor level. The worm gear on the polar axis has a take-off for driving the right ascension circle on the pier at the sidereal rate. This right ascension circle is also a slip-ring circle; and once it is set, objects can be located without any more attention.

All the circles have small lights run from a 6-volt transformer.

The Observatory for the protection of the telescope is 15 feet by 19 feet. It has 2 x 4 uprights or studs covered with 5-inch shiplap. The floor is concrete, separated from the pier by insulating material so that vibrations are not transmitted to the telescope. The building has windows on each end and a side entrance. The roof is covered with corrugated aluminum. The roof is separated in the center, and each half rolls out on an outrigger at an end of the Observatory. The roof rollers are aircraft pulleys made of Micaita and have Torrington needle bearings. The rollers ride a $\frac{1}{4}$" by 2" steel rail on end inside the Observatory walls. The roof overlaps in the center to keep out the rain, and so far I have had no leaks.

The telescope tube and the Observatory walls are painted light gray. The polar axis, the declination axis, and the circles are gray wrinkle finish. Other parts and trim are in dark maroon. The pier itself is colored a light green.

I did all the machine work at home on my 11-inch lathe and 16-inch drill press except that I machined the polar axis and declination axis castings on a Giddings and Lewis horizontal boring mill.

I am very pleased with the optical performance of the telescope, as well as with the ease of manipulation. On nights of good seeing I have resolved the close double star 7 Tauri, separation only 0".38, with ease at 600X. During the 1956 apparition of Mars 11 canals were seen at one time in the Thaumasia area. With 400X one can distinguish the true shape of the twin craterlets on the floor of Plato. Watching Oberon and Titania revolve around Uranus over a period of several days is very fascinating. After much observing, I have come to the conclusion that my 9.5-inch refractor will reach the same stellar magnitude as a 12.5-inch reflector. My views on this matter agree with those expressed by Mr. D. W. Rosebrugh in a past article in this periodical.

At present I am building a portable 5.5-inch F:18 refractor to take to the mountains. It will have a weight drive, not an electric one.

Since constructing the 9.5-inch refractor, I have learned that a large American firm charges approximately $12,000 for a similar instrument. I built the telescope and Observatory for just a fraction of this amount, the objective being the costliest item.

Equipment for the telescope includes 15 eyepieces, two of them large Erfles, a star diagonal, a solar wedge, and an astrocamera.

I shall be glad to furnish information on construction and procurement of materials to anyone wishing to build a similar instrument. My address is 9775 Burgos Ave., Oakland 5, California.

BOOK REVIEWS

The Amateur Astronomer, by Patrick Moore. W.W. Norton and Company, Inc., New York, 1957. 337 pages. $4.50.

Reviewed by J. Russell Smith

Patrick Moore, Director of the Mercury and Venus Section of the British Astronomical Association, has given the amateur and the general reader a book to fulfill the needs of the beginner who knows nothing about astronomy. The book is well suited to the amateur who has a small telescope and desires to do something of value in the lunar and planetary field.

This well organized volume contains eighteen chapters which give the beginner a wide coverage in the field of astronomy. One item of particular interest to the amateur is that all the photographs and drawings in the book, with the exceptions of Plates 13, 15, and 16, were made by amateurs. There are sixteen plates, sixty-two figures, and sixteen star maps to help make the subject more easily understood by the beginner. The twenty-nine appendices give a wealth of information needed by the serious astronomical student, and a complete index makes the work an extremely useful reference.

We all know that it is very difficult to keep errors from creeping into the finest book. On page 235, the parenthesis indicating the longitude (System I) on Jupiter is incorrectly drawn. It should be from the center of the South Equatorial Belt to the center of the North Equatorial Belt instead of from the center of the South Equatorial Belt to the south edge of the North Equatorial Belt.

I believe the limiting magnitudes for various apertures on page 275 are generally too low for most of the amateur instruments in this country. Furthermore, it seems the book could have been made more useful had the author given the markings for the pronounciation of the constellations in Appendix XVI or elsewhere.

I heartily recommend the book for the shelf of anyone interested in astronomy.

Fundamentals of Optics

Francis A. Jenkins and Harvey E. White. McGraw-Hill Book Company Inc., New York, 3rd Edition, 1957. Pages 637 vii. Price $8.50.

Reviewed by E. L. Cleveland

Written primarily to be used in an advanced undergraduate college course in optics, this book gives a rather thorough treatment of geometrical, physical and quantum optics. The fact that it is a "third edition" indicates both that it has been successful in its primary purpose and that it is a product of development. The first edition (1937) treated classical physical optics only, the second edition (1950) added treatments of geometrical and quantum optics, and this third edition is a relatively minor revision of the second.

The teacher will recognize many valuable features among which are: an abundance of problems (approximately 500), many references (approximately 100) to books and to original papers in the scientific journals, and short biographies (about 50) of men who have made significant contributions in the field of optics. Those more interested in the theory will be pleased that the development of the theory is generally quite adequate. Those primarily interested in applied optics will be pleased to find that descriptions of many optical devices have been included. Answers to the even-numbered problems are given. The large number of wrong answers is disappointing although it is perhaps explained by the authors' statement that an entirely new set of problems is included. The utility of the book would have been improved had certain reference material (for example, wave lengths of selected spectral lines) been included perhaps in an appendix section.

This reviewer is currently using this book in an advanced under-graduate course in optics and recommends it for such a purpose.

The Conquest of the Antarctic

By Norman Kemp. Philosophical Library, New York, 1957. 152 pages.

Reviewed by David P. Barcroft

Concerning the various Antarctic expeditions in progress during this International

Geophysical Year. Good sketches of the leading members of these expeditions based on personal interviews had with them by the New Zealand author. Interesting and well illustrated.

Mysteries of Science

By John Rowland. Philosophical Library, New York. 1957. 214 pages. Price $6.00.

Reviewed by Charles A. Haas

The author thinks that not all mysteries can be solved by the scientific method of weighing, counting, and measuring. He is sincere and frank in his discussion of various philosophical problems. He has given many years of study to the subject under discussion. The reading is not technical.

The book is divided into five sections - physics, biology, psychology, sociology, and the unclassifiable. The discussions on the Darwin and Marxist theories are valuable to any student in these fields of thought.

The author stresses that in the scientific world man is able to measure his objects, but in the social and religious worlds he cannot do so. How can one measure goodness? How is the mind of Hitler to be compared to the mind of Luther? The author does not agree that science can solve the mysteries; we can measure the electron but cannot say from where its power comes.

In his "Epilogue" the last paragraph is worthy of remembrance: "In the heart of the atom, in the heart of the star, in the heart of man there is mystery. We do not destroy that mystery by denying its existence. But we do, when we deny the mystery, destroy something - perhaps something irreplaceable - in our own hearts".

OBSERVATIONS AND COMMENTS

The Occultation of Saturn by the Moon on September 1, 1957. At Edinburg, Texas Mr. Paul R. Engle and others observed and timed this phenomenon with Mr. Engle's 17-inch modified Cassegrain reflector at 331X with a 28 mm. orthoscopic eyepiece at the Pan American College Observatory. Their results were as follows:

First Contact with Ring A	4^h	27^m	$52^s.8$, U.T.
First Contact with Ball	4	28	14.2
Immersion of Ball	4	28	43.9
Immersion of Ring A	4	29	11.9

The observatory is at latitude 26° 18'N., longitude 98° 11' W., elevation above sea level 94 feet. The timing was accomplished with a tape recorder and W.W.V. signals and is thought to be accurate to within 0.2 seconds.

Mr. Thomas Cragg observed the occultation in fair seeing and reports: "A bright streak along the planet adjacent to the limb of the moon was quite easy at times. It appeared exactly like the Terby White Spot except that it could not be seen on the ball except in the Equatorial Zone area. Without question this streak was a contrast effect showing again why one gets the Terby White Spot, but I was a little surprised not to see the streak all the way across the ball. Since it couldn't be seen against most of the ball it certainly could not be ascribed to lunar atmosphere." The Terby White Spot is a bright spot on the rings beside the shadow of the ball on the rings. It is surely a contrast-caused illusion; but its appearance, and even its visibility, has varied greatly with different observers and telescopes.

More on a Possible Anomalous Occultation On pages 69 and 70 of our January-June, 1957 issue we mentioned some observations by Paul Nemecek and Robert Adams of an apparently anomalous occultation of a seventh (?) magnitude star by the moon during the total lunar eclipse on November 18, 1956. Both observers thought that the star's light dimmed gradually before it vanished.

Mr. Craig L. Johnson of Wichita, Kansas writes of watching a star of magnitude six or seven approaching occultation during the eclipse at about $7^h 15^m$, U.T. "I watched it get closer, and when it was within 5" of the disk, it slowed down slightly and twinkled more....But even so, it snapped out of sight very suddenly." Mr. Johnson was observing with a 4-inch reflector at 167X in fairly good seeing and a very clear sky. Unfortunately, exact times are lacking.

Mr. L. J. Robinson at Sylmar, Calif. writes that he and Mr. Bell, simultaneously and with two different telescopes, observed the same abnormal occultation as did Messrs. Adams and Nemecek. They did not record a time, but Mr. Robinson says that agreement in position with Mr. Tombaugh's photograph (pg. 63 of the January - June, 1957 issue) is very good.

It is unfortunate that none of the four observers of this possible anomalous occultation has supplied a fully satisfactory report as regards time of disappearance, position angle at disappearance, identity of star, etc. As far as the reports go, four different stars may be involved!

Lunar Limb Brightening. Mr. Craig L. Johnson writes that on July 31, 1957 at $2^h 24^m$, U.T., using a 4-inch reflector at 91X, he observed a slight ring of light reaching around the north limb of the moon. The ring was just barely brighter than the earthshine and was about 1,000 miles long. Mr. Johnson expresses confidence that the appearance was not an illusion and that it was not wholly due to contrast. The moon's age was 3.9 days, and the seeing was perfect (10 on a scale of 10, while moisture was literally running down the telescope tube). This lunar limb light is nothing new but has been recorded as long ago as Schroeter's time. It has been variously imputed to a lunar atmosphere, to optical effects, and to mountains on the limb. Perhaps some readers would enjoy investigating this facet of lunar affairs.

Total Lunar Eclipse of May 13-14, 1957. Mr. Patrick Moore has reported observations of this eclipse made by himself and Mr. Peter J. Cattermole with Mr. Moore's 12.5-inch reflector, 6.5-inch reflector, and 3-inch refractor at East Grinstead, Sussex, England. The circumstances of this eclipse in Universal Time were: moon enters penumbra, $19^h 42^m$; moon enters umbra, $20^h 45^m$; total eclipse begins, $21^h 52^m$; middle of eclipse, $22^h 31^m$; total eclipse ends, $23^h 10^m$; moon leaves umbra, $0^h 17^m$; moon leaves penumbra, $1^h 20^m$. The eclipse thus began on May 13 and ended on May 14. Because of trees the 12.5-inch reflector could not be used until after $23^h 0^m$.

Mr. Moore writes: "A constant watch for possible lunar meteor effects was maintained from $21^h 52^m$ (start of totality) to $23^h 4^m$ (six minutes before end of totality, when clouds interrupted temporarily). Conditions were satisfactory; and using the 6.5-inch, one or the other of us was observing the whole disk throughout this time. As had been fully expected, no effects were seen. The eclipse was somewhat 'dark', and the lovely colors seen at previous eclipses were, in general, absent.

"No pre-eclipse observations could be made, due to cloud, and this ruined the program of observing features such as dark-floored craters and bright points. Menelaus was conspicuous all through totality, and was the brightest object on the eclipsed disk until Aristarchus became very prominent in the later stages. Plato was rather inconspicuous, but Grimaldi and Riccioli were prominent and very dark. The ray systems of Tycho, Copernicus, and Kepler were not conspicuous at first, but became very evident towards the end of tatality.

"For the post-eclipse observations the 12.5-inch could be used. Much the most interesting feature was Atlas, as there were two very dark patches on the floor; one southwest of the center of the crater, almost black- as dark as Grimaldi, and a second, fairly dark, to the northeast of the center, while to the east and southeast of the center the floor was gray. This aspect was noted at $0^h 20^m$, U.T., under fairly good conditions. At $1^h 25^m$, after a break due to cloud, we both felt fairly sure that the patches were less dark. Unfortunately, definition failed soon after. We do, however, feel certain enough of this to record it definitely.

"Other observations were not significant. Hercules appeared with its floor gray at $0^h\ 20^m$; Linné was very sharp and well defined as it emerged from the shadow; in Mare Crisium, the Quadrangle reappeared at the end of totality, but the spot Picard 7 (cf. Moore's chart, Journal B.A.A.) was not seen either between $0^h\ 20^m$ and $0^h\ 30^m$ or between $1^h\ 25^m$ and $1^h\ 40^m$. However, it is unlikely that any of these observations are indicative of unusual features. Other formations, such as Alphonsus, Grimaldi, Julius Caesar, Censorinus, and Euclides were examined, and appeared normal. The dark area at the edge of the Mare Serenitatis in the Littrow-Luther area, giving the appearance of an 'outer ring', was well seen, but we would certainly not commit ourselves to saying that it was more prominent than usual.

"As was only to be expected, the results of our observations were mainly negative, the only positive feature of interest being the unusual and possibly temporary darkness of the patches inside Atlas."

An A.L.P.O. Library. We are very glad to announce that an A.L.P.O. Library is being started as a new service to our members. The Librarian will be Mr. E. Downey Funck, 256 N.E. 11th St., Delray Beach, Florida. It is planned to give more details in the next issue, including a list of the few dozen books we shall have as a starter. Meantime, we shall be very glad for ideas and suggestions about the Library, for we are anxious to make it as helpful to A.L.P.O. members as possible.

On Radio Communication. Mr. Robert Leasure, 2380 Gardner Road, R.F.D. No. 1, Galloway, Ohio expresses his keen interest in doing whatever he can to promote radio communication among A.L.P.O. members. He is a licensed "ham" operator, and his call letters are K 8 A G X. He operates on the 80, 75, 40, and 15 meter bands with 180 watts continuous wave radio telegraph or radio telephone. Mr. Leasure will be glad to make any schedule with another station. We would urge our active observers either to contact him or to have their "ham" friends do so - now.

Aristarchus - Herodotus Region. We invite attention to the front cover drawing of this popular lunar region by Mr. Elmer J. Reese - actually, his drawing will repay some study. Mr. Reese says: "Aristarchus appears normal. No bright spot seen near center of floor of Herodotus. Note shadow of a well-marked ridge at south edge of floor.

"F may be shadow of a ridge rather than a cleft. G seen as a very narrow dark line-probably a cleft. C and D are not clefts but smooth, dark, wide valleys surrounded by whiter, rougher regions. M is extremely brilliant. H is a hill with a tiny pit at bottom of west slope. J is a hill with a tiny brilliant spot on its summit-apparently a pit. B is a prominent cleft-like valley. K and L are distinct craterlets and have been seen several times in the past.

"Since no prepared form was used in making the sketch, its positional accuracy is probably poor." (Mr. Reese is surely a modest man. -Editor.)

Figure 15 on pg. 120 is a detailed structure drawing of one of the famous dark bands on the east inner wall of Aristarchus. It was made by Mr. Frank Vaughn under conditions almost perfect at times. Mr. Vaughn remarks: "Although both principal dark bands are similar in structure generally, the south band best shows the details. Both bands evidently represent portions of the east inner wall which have retained the original structure of terraces concentric with the floor. In brief moments of what may be seeing 10, one sees on the south and east walls what is apparently the rubble of former terraces. Cliffs C and D carry on to the south and west as narrower cliffs which disappear after a distance.

"I think the progressive darkening of the bands through the lunation is consistent with the structure here seen."

Mr. Vaughn is of the opinion that many, and maybe all, apparent lunar surface changes can be related to detailed lunar topography. Surely we shall do well to study such topography closely in all cases, and this study will in large measure demand low solar lighting and excellent seeing.

Crater W. H. Pickering. Figure 16 on pg. 120 is a view of the eastern member of the famous pair of lunar craterlets, Messier and W. H. Pickering. The considerable

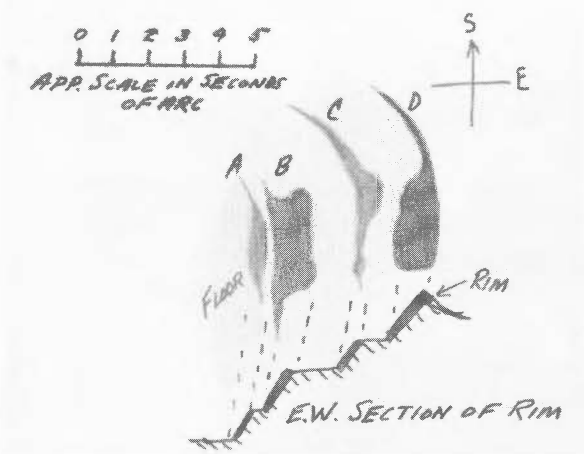

Figure 15. South Dark Wall Band on East Inner Wall of Aristarchus. Frank R. Vaughn. August 19, 1957. 9^h 55^m, U.T. 10-inch refl. 470X. Seeing 4-9 (10 is perfect). Transparency 5. Colongitude = 196°.1.

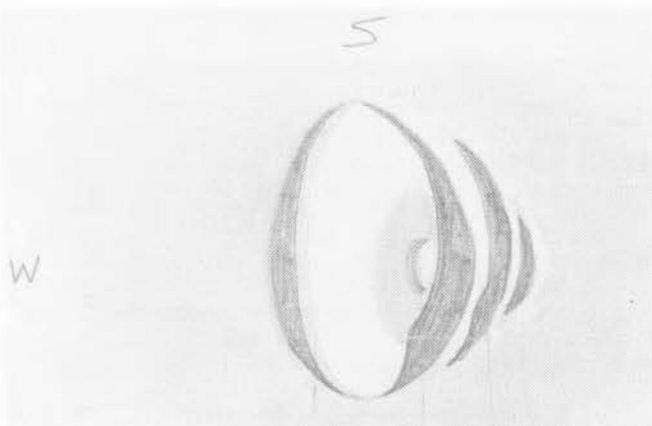

Figure 16. Lunar Crater W. H. Pickering. Frank R. Vaughn. July 14, 1957. 7^h 35^m, U.T. 10-inch refl. 450X. Seeing 3-7. Transparency low (fog). Colongitude = 115°.2.

changes in apparent size and shape of these objects, changes repeated each lunation in at least their general pattern, have been recorded by many different observers. In this splendid view with Pickering near the sunset terminator, Mr. Vaughn urges, we may be able to see an explanation of some of these apparent changes. He directs attention to the triple east rim, the elevation on the floor, and the flattened west rim. The triple east rim in particular can surely account for great seeming changes as the lighting varies.

Concerning the Russian Artificial Satellites. We have admittedly devoted none of this issue to the major astronomical subject of conversation of recent months. We do intend to give much of our next issue to the general subject of artificial satellites and possible amateur studies of them. We have been rather surprised to hear of several observations of these speeding bodies with ordinary astronomical telescopes in which the satellites were actually tracked for a minute or longer. Mr. William E. Kunkel, 2600 Ridge Road, Berkeley 9, Calif. made radio observations of Sputnik I in early October, 1957 and found evidence of a variation in signal level suggesting a spinning of the satellite. Other radio observers might like to discuss this matter.

The Strolling Astronomer

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