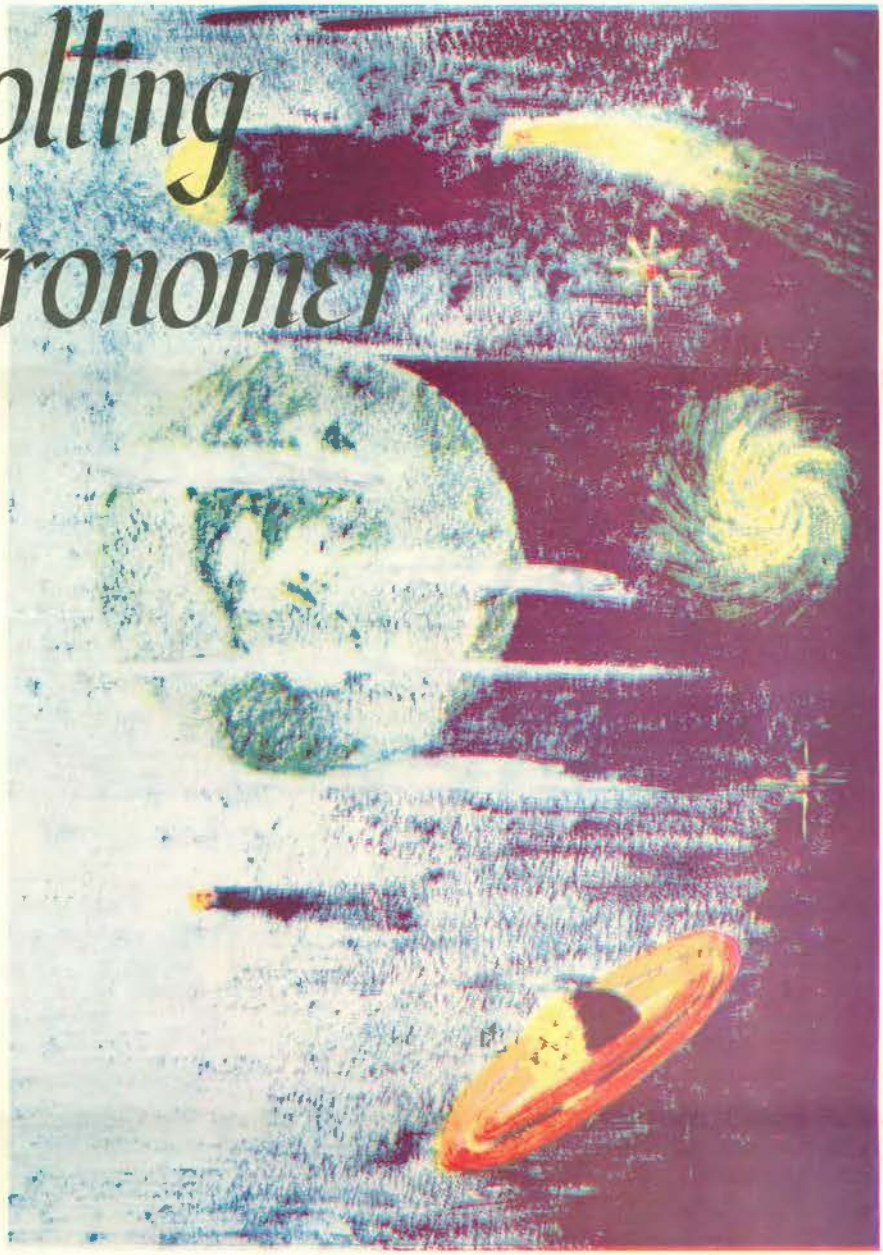
The Strolling Astronomer

Volume 5, Number 8 August 1, 1951

ASSOCIATION OF LUNAR AND PLANETARY OBSERVERS



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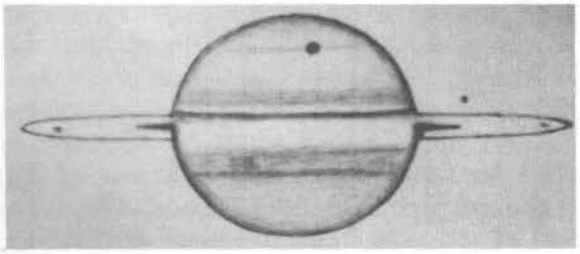


Fig. 1. Saturn
L. T. Johnson.
April 7, 1951.
4^h 12^m, U.T.
10-inch refl. 221X,300X.
(Black spot is Titan
in transit).

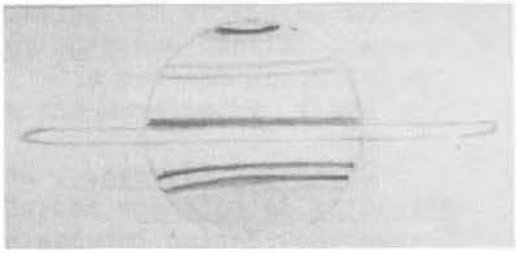


Fig. 2. Saturn.
O. C. Ranck.
June 6, 1951.
1^h 15^m, U.T.
4-inch refr. 240X.

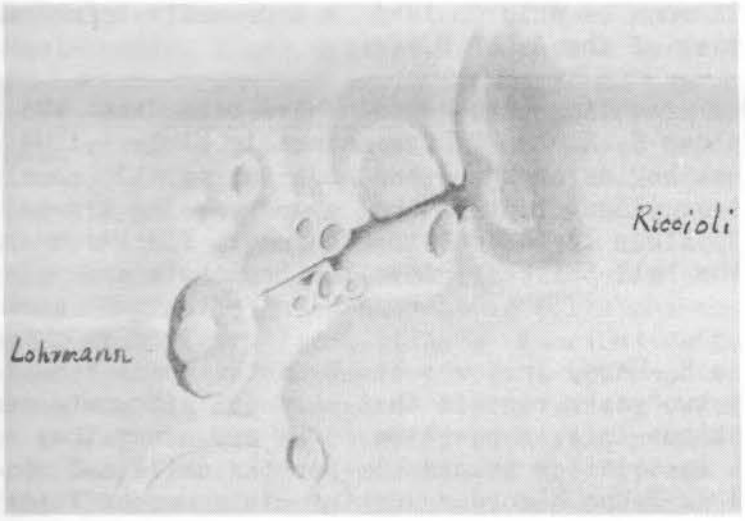


Fig. 3. Lohrmann-Riccioli Region.
T. Saheki.
8-inch refl. 66X to 500X.
February 20, 1951. 9^h 30^m, U.T.
Colong. = 76°.9.

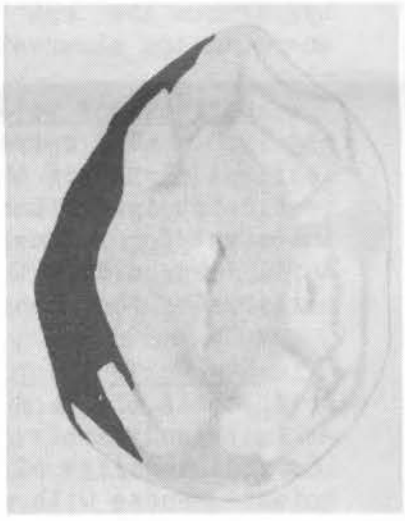


Fig. 4. Aristarchus
E. E. Hare
12-inch refl. 300X.
Sept. 24, 1950.
4^h 45^m, U.T.
Colong. = 61°.4.

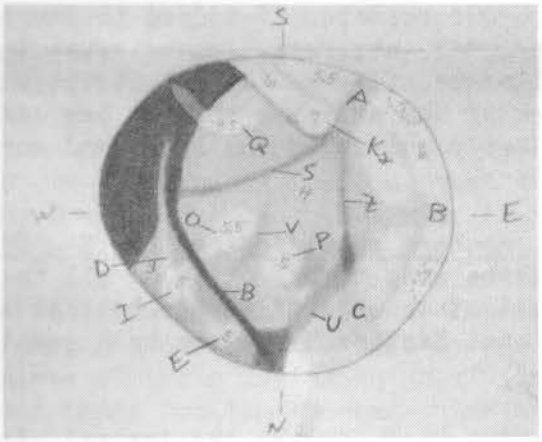


Fig. 5. Conon.
E. J. Reese
6-inch refl. 240X.
Dec. 20, 1950.
1^h 45^m, U.T.
Colong. = 39°.1

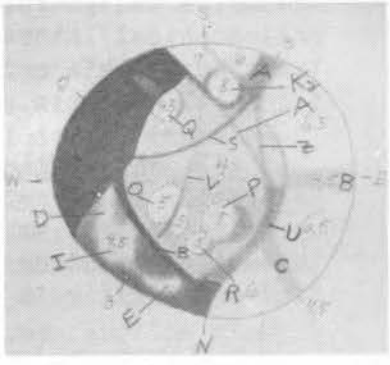


Fig. 6. Conon.
E. J. Reese
6-inch refl. 240X.
Nov. 30, 1949.
1^h U.T. Colong.= 25°.9

ANNOUNCEMENTS

Change of Address. Readers are requested to note that the address of The Strolling Astronomer and of its editor is no longer 133 S. Alameda St., Las Cruces, New Mexico but has now changed to:

1203 N. Alameda St.
Las Cruces, New Mexico

All communications should be directed to this new address, although forwarding arrangements have been made at previous addresses. The editor regrets the recent frequency of these changes of address. There is good reason to think, however, that the present address will be more durable than its two predecessors in Las Cruces and that the editor will soon be able to take a more active personal share in the observational activities of the A.L.P.O.

Corrections to July issue. Our preceding issue should have been dated 1951, not 1950. The correct date of Volume 5, Number 7 was naturally July 1, 1951. The last clause of the sentence that begins on pg. 9 and ends on pg. 10 should read: "it is further obvious that excellent optics and steady seeing are most important for success." On pg. 13, lines 13-14 one should read: "In March and April Johnson drew the rings off the ball brighter than the temperate and polar portions of the planet.

New Venus Recorder. Mr. Thomas R. Cave, Jr., who has been the Venus Recorder of the A.L.P.O. during the last two years, regrets that personal circumstances make it inadvisable for him to continue in this position. We are sorry thus to lose the services of Mr. Cave. The Association thanks him for the skill and conscientiousness with which he acted as Venus Recorder and for his numerous Interim Reports upon this planet.

As a new Venus Recorder we have been fortunate in securing:

Dr. James C. Bartlett, Jr.
300 No. Eutaw Street
Baltimore 1, Maryland

Dr. Bartlett assuredly needs no introduction to our readers, if indeed to American amateur astronomers at all. He is an energetic observer, an entertaining writer, a delightful correspondent, and a colleague of very wide scientific and philosophical interests. Our readers will remember the articles that he has contributed to this periodical; he has also frequently published in larger and more technical journals.

All observations of Venus by A.L.P.O. members should be mailed to Dr. Bartlett. Likewise, all work upon Venus should be sent to Mr. O'Toole; all that upon Jupiter, to Mr. Both. Observations of all other objects should be sent to the editor, pending the appointment of additional Section Recorders both qualified and willing to serve.

French Books upon Mars. We acknowledge with many thanks the arrival of a complimentary copy, sent by the author, of Physique de la Planète Mars. (Introduction á l'Aréophysique), by Mr. Gérard de Vaucouleurs. The author is a Research Associate of the Institut d'Astrophysique at Paris. It is our hope to present

a review of this book in a future issue. It is, briefly, an expansion and a continuation of Mr. de Vaucouleurs' excellent The Planet Mars, the English translation of which was reviewed in our June, 1950 issue. His present book is an excellent compilation of modern astrophysical data in the developing field of "the physics of Mars."

Students of Mars should take advantage of a remarkable value being offered by Librairie Hermann, 6, Rue de la Sorbonne, Paris, France. One may purchase from them E. M. Antoniadi's La Planète Mars, published in 1930 and still of great value as a classic by the leader of one of the chief schools of visual observers. The price is (in this country) one thousand francs, which is less than three dollars at the present rate of exchange! (In buying one should naturally ascertain the exact prevailing value in U. S. currency of 1,000 francs from some local source). Even if you can't read French, you can look at the charts and drawings of Mars, which are alone worth much more than the price. In this book Antoniadi forcefully developes his arguments that large telescopes are essential to the successful study of Mars and that the "canals" are resolved into natural-looking finer details in adequate instrument.

New Author, New Subject. Mr. John E. Felber, Box 636 Federal Square, Newark 1, New Jersey is a new contributor to these pages. His telescope is an 8-inch reflector. His interests include, besides astronomy, radio and aviation. He is employed in the printing and publishing business. The editor was very glad to meet Mr. Felber when the latter recently visited southern New Mexico; it is always pleasant to become personally acquainted with A.L.P.O. members and their friends. Our colleague's subject is certainly one that all of us have some ideas upon! At the very least it is a fascinating psychological phenomenon of our fast-moving times; at the very most - who can say? The editor, alas, is a social outcast who has never seen a "flying saucer", no indeed anything in the sky that he felt unable to identify with some plausibility. Neither have the majority of his friends and associates in New Mexico. However, he is willing to listen to the evidence of those who have, or who think that they have.

INTERPLANET FLYING SAUCERS? - SHEER NONSENSE

by John E. Felber

The flying saucer stories will be back again next spring (when they most seem to appear), especially if there is a war scare about. The latest rumor is that they are interplanet space ships on the Plynt theory of lift and that smaller remote-controlled units are also flying about. It is said that these things are observing us and have been doing so for several years.

As far as I know, no astronomical association has officially disagreed with this view, probably feeling that it would be a waste of time and that it all seems too ridiculous. But yet, if no one cares to differ officially, these reports will grow; and while the scientific mind continues to pass them off as folly, the general public may start believing them. Then in time, when the real truth is finally reasoned out, the public will blame the quiet attitude of the astronomer and will have lost confidence in any of his information services, regardless of their useful nature. I believe that action should be taken now to debunk these foolish rumors. Certainly the science of astronomy and the study of the interplanetary deserves a little more consideration for the long, hard work of others than to allow it to be mocked by publicity-minded people.

I have studied most thoroughly all the "saucer" reports in the last four years. After compiling thousands of bits of data I have come to the conclusion that the saucer is definitely not of interplanetary source. Further, in my opinion, in only one out of twenty incidents which are reported is anything actually seen that could be a possible mystic craft. I believe that the things sighted could be a new type of military craft, such as a guided missile, an experimental jet or rocket, or an advanced type drone using some sort of radar brain and controlled remotely by radio. This is as close as one can come to a flying saucer. One can not disclaim the craft entirely, as it has been tracked on radar screens and has been seen by too many reliable observers, particularly near experimental military bases.

Because of gullible people who will believe anything, stories of 23-inch men, metal that floats in air, and strange, never-before-heard-of languages coming in on the radio have originated in the fantastic imaginations of sensational-minded white liars. Because of this condition it has been extremely difficult to compile any trustworthy information from the 216 reports I have used as a base. The saucers have been called just about everything imaginable. In the non-existent group hallucinations, jesting, lies of publicity-seekers, intoxication, and optical illusions top the list. In the existent (intangible) group we find celestial phenomena, windshield glare, and sunspot reactions.

In the existent definitions (tangible) we find secret military aircraft topping the list, followed by weather balloons, Russian photographers, meteorites, observers from foreign planets, publicity stunt projects, disks from skeet targets, buzz bombs, the work of a dejected inventor, new type of helicopter, cosmic ray testing apparatus, rockets, new type of kite, blimps, and the research of some "big outfit." In the unclassified group we find such nonsense as pennies from heaven, the saucer for the big dipper, reaction to T V commercials, and a witch's jet broom. The size of these "saucers" have been everything from 2 to 2,000 feet; the shape, from a coffee can to a double-sized bath tub, or even a six-room house. In condensing some of the 216 reports screened from some 1,600 incidents, I find that the information points to the fact that if the saucer does exist it would seem to be in various sizes, the most popular being 40 to 60 feet in diameter. It would be round in shape, 10 feet thick, silver-aluminum in color, have exhaust ducts with yellow-orange flames around the bottom edges, and, as optional equipment, have a radar-type pointed screen in the front. The speed is 50 to 1,000 miles per hour, with incredible flexibility and maneuverability.

If these statements are evaluated and studied, they point strongly to a guided missile of some sort rather than to a cosmic ray or a weather balloon, the latest official announcement made to the public by the military groups.

Going over, very briefly, the various popular reports would add little to the findings. The Mantell report (the National Guardsman whose plane disintegrated chasing the object), only showed it to be presumably metallic material. The Smith-Stevens report was simply that there were many objects, all disc-shaped. The original saucer report of Kenneth Arnold was about the same in general terms, along with that of George Gorman, another Army pilot, many months later, and the Adams-Anderson report of two commercial airlines pilots. The Chiles-Whitted report appears to state that the object was shaped like a cigar, unless it could have been viewed from the near side and at the same level. These six reports are the most reliable, and it is obvious that something was sighted. In all cases light, exhaust, speed, and maneuverability (and in one case a "motion felt") were recorded. There were also reports that radar screens actually recorded the flight of the craft. This is not conclusive proof of its actual existence, however, since radar has fooled its operators many times.

A.L.P.O. DRAWINGS OF MAEDLER'S SQUARE AND VICINITY IN 1951.

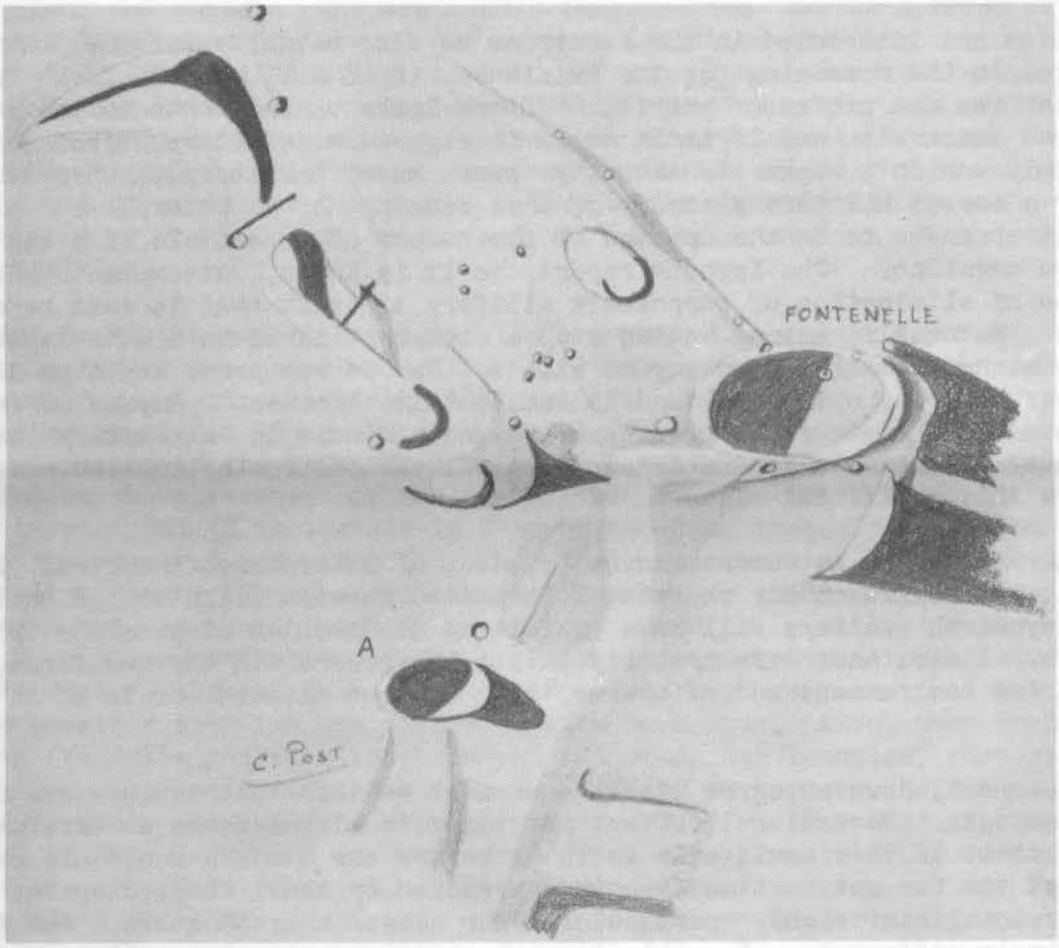


Fig. 1. C. C. Post. 6-inch refl. at 180X. June 14, 1951 3^h 15^m, U.T. Colong. = 24°3.

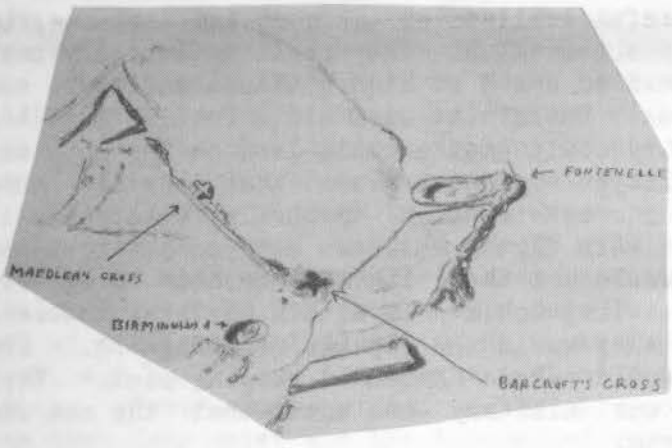


Fig. 2. J. C. Bartlett.
3.5-inch refl. at 100X.
May 16, 1951. 2^h 30^m, U.T.
Colong. = 29°7.

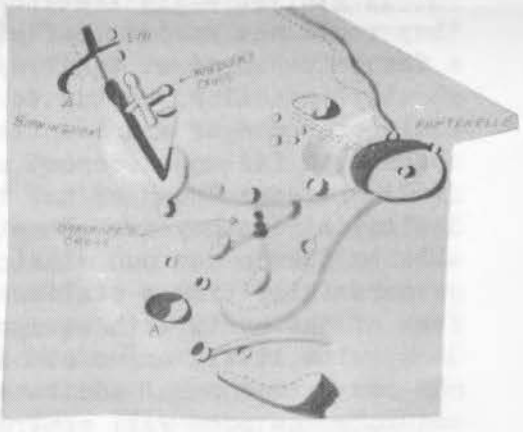


Fig. 3. E. J. Reese.
6-inch refl. 240X.
June 15, 1951. 2^h30^m, U.T.
Colong. = 36°2.

What little is known seems to point to some sort of military object. A national news magazine claimed as much; but its information was far from being decisive and detailed, just as was the brief Army statement maintaining the object to be a plastic balloon carrying cosmic ray weather-testing apparatus.

I am not interested in the saucer as to its actual existence; but I am interested in the reasoning for its existence, if it does exist. If it is not an illusion (as one professor put it, "If one looks up and wants to see saucers, he will see saucers") and if it is not a foreign-made military object (since they certainly wouldn't risk its identity over enemy territory), then it must be either a secret military aircraft of this country or an interplanet space ship. The latter seems to be the opinion of the author of an article in a men's truth-fiction magazine. The Keyhole report, as it is known, attempts to show by the process of elimination of supposedly military aircraft that it must be of foreign origin. Naturally, anyone making such a statement is given a wide latitude; for his opinions are as good as anyone else's. No one can prove anything different, and therefore the opinions stand as substantial theories. Anyone of importance or reputation must face embarrassment and ridicule in attempting to reason otherwise. For this reason no astronomical association or institution has come forth with its official views.

As an amateur astronomer and a student of interplanet theories, I am extremely interested in any reports of suspected foreign flights. I believe that some day earth dwellers will have to take up the problem of possible interplanet feelers. I feel that life probably exists everywhere, in various forms, depending on its environment and, of course, that unknown element (or is it an energy?) time.

I cannot, however, agree with the present beliefs that saucers are of interplanet origin. Regarding life that may exist in higher forms on external galaxies, I doubt if they could ever reach us before our own sun would die out. They are lost too far out in time ever to be reached by man. Regarding our own galaxy, or local star cloud, particularly our closer charted stars, the situation is entirely different. Possible life on those planets is really a very important issue and one of great interest, for it has been under consideration for many years. The planets, of our own solar system of course, are our best source for any possible visitors or messages from space. The star Alpha Centauri, 4½ light years away, is another source; and as the previously mentioned article suggested, so is Wolf 239. We have yet to prove that planets even exist with these stars, but it is assumed quite probable.

I feel strongly that if "things of intelligence" existed and were nearby they would not resort to flying saucer observation. They would undoubtedly have a far better plan at their disposal than to speed at high altitude and to be constantly in motion, hoping to observe us. Doesn't it seem a bit foolish that they would risk danger and identity when they could just as well land on the moon and observe in far more secrecy and stability? One must assume that they are hundreds of years ahead of us in telescopic construction (probably electronic in design) since they are so apt in space ship flying while we have as yet been unable to pierce our own stratosphere. Would not these "things" be able to observe us more fully from a stationary body? They could come within several thousand feet of the earth with equipment that they would be capable of designing. The moon, with little or no atmosphere, would make a splendid viewing post. Even our own astronomers admit this, and our military men admit that the one who controls the moon will control the earth.

Assuming that Centaurians or Wolfians have no military ambitions against us, they would certainly have a wonderful spot for snooping. They could land on the opposite side of the moon and use this as their base, moving over to the visible side in secrecy. They could use the moon as a supply base for parts, equipment, food and fuels. One can hardly imagine them taking a trip of some 28 trillion miles, spinning around the earth a few times and then taking off again for another 28 trillion miles jaunt to get back home. Surely the spacious, stable moon is far better than cramped, unstable speeding craft. In addition to a viewing post, a listening post would also seem a very important item. We know that our radio signals should reach them on the moon, assuming that they have equipment equal to or better than ours. They would probably set up radio beams for other incoming and outgoing trips and also a master station for signals to their original base.

The author of the report mentioned before seemed to think that our radio signals would be far too complex for them to understand. It must be admitted that they would sound strange to an non-earthian. But over a period of years, and the author mentioned 10 to 300 or more, "they" would have ample time to learn our ways. In view of the fact that television travels in a straight line and does not follow the curvature of the earth like radio, its broadcast distance is rather short. But if it travels in a straight line it should reach the moon, similar to the way that our radar did. If their receivers are of advanced quality and design, the viewers should be able to see us along with all we have. Just what a space man would think of wrestling, comedians, roller derbies, soap operas, U.N. meetings, and what not may seem amusing and most confusing at first. It can't be denied, though, that they would certainly see more that way than they could possibly see from the air, even with hawk eyes. Also, they could receive radio facsimile and wireless teletype and study our language, photography, and journalism. Recording of these data and sending it back to their "home" for further study would certainly be a requirement. They could gather an immeasurable amount of data regarding our customs, traits, and general living conditions. My convictions are quite positive. The moon would be the place to spy from as long as we can't get there, and any unknown people would certainly know that after viewing our present aeronautical equipment.

I feel that these logical reasonings are the basis for my assumptions and beliefs that flying saucers are sheer nonsense. Of course, I am ready to listen and to welcome any comments or views to the contrary. I believe that we must be ready some day to await an "arrival" of a nearby neighbor. This does not necessarily mean that a foreign interplanet rocket ship is going to request landing instructions in the Martian language from the control tower at La Guardia Airport. The first "arrival", I believe, will be in the form of a radio signal of some sort. There was a rash of Martian radio signals about twenty-five years ago. Undoubtedly there will be more, but we must always be ready for the real thing. The second hint or indication of life elsewhere will be in the form of a shell or missile with inscriptions or hieroglyphics. I think that one type of sign language proposed by the late famed astronomer, Percival Lowell, was that of colored beads similar to those used by the early Indians of North America. The last stage (these stages probably will be hundreds of years apart) would be a guided or controlled small rocket containing samples of their civilization similar to what we put in our time capsules. There would be such items as samples of crafts, manufactured goods, newspapers, currency, costumes, wearing apparel, and books; and they might enclose specimens of their lower life and vegetation. Never can we expect spatial visitors before we even get some sort of hint that they exist and are interested in contacting us. Naturally, centuries

may go by before we ever receive any hint at all that there is "higher life" elsewhere. But since we are progressing along these lines, this always improves the possibility of learning something. In other words, we are going forward, not backward, even though we are moving at a slow, uncertain rate. And may I point out that because of the last war and the fear of another, vast work is being done in the rocket-jet atomic energy field that might otherwise have been delayed for many years.

Rumors are wild and many; but it is certain that a moon rocket will be attempted (at least, a one-way radio-controlled project), along with a remote-controlled drifting rocket orbit around the earth (for weather pressure and atmospheric data) within the next 50 or 75 years. The fact that an atomic-powered submarine is being built is an indication that we are not too far behind in the development of atomic engines. The fact that we did not bother to experiment with more easily built larger type engines such as power generators, ocean liners, and other projects where the design problems would not conflict with space, size, weight, etc., surely shows that the job is not so difficult as it would seem. The major problems seem to be what to do with the used contaminated radioactive material and how to develop lubricants that can stand tremendous heats and metals that are not weakened by heat. Rumors have it that secret plans are about one-third of the way toward completing the basic engine. It is interesting to note that a submarine and a potential moon rocket are of about the same size. Of course, our present V-2 type rockets (not using atomic power) are indeed small. But valuable information is being compiled that will be of great importance at a later date. So far V-2 rockets have reached altitudes of about 300 miles, a far cry from the 25,000 miles predicted as necessary to break away from the earth's gravitational force.*

I would like to guess, however, that if an atomic-powered rocket reached a height far less than that, we could expect surprising results. Man knows so little about outer space that he must be ready to expect "strange things" despite proven theories of physics. We are definitely approaching the era of space ship flying, and the moon will be the first stop (about 20 or 25 hours, based on speeds of 10,000 miles per hour), which is quite obvious. In the event we don't get there before some one from "elsewhere" does, it is certain that "they" can make good use of it as far as spying is concerned.

All in all (and I must say that my opinions are worth no more than anyone else's), as an amateur astronomer and student of interplanetary theories, I can't believe any report, even with truckloads of salt, that flying saucers (if they exist) are of interplanet, interstellar, or inter-galaxy origin.

THE CAPABILITIES AND LIMITATIONS OF SMALL TELESCOPES

(Foreword by Editor. The following article is taken from a discussion reported on pp. 139-141 of The Journal of the British Astronomical Association for October, 1946, and is reproduced here with the kind permission of the Council of the British Astronomical Association. The information given about the resolving-power and the light-grasp of small instruments should surely interest all users of telescopes. In particular, the basic concepts involved in discussing limit of resolution deserve careful study. We should not be too prone to quote the

* A two-stage rocket, a V-2 and a "bumper", attained an altitude of about 250 miles. No single V-2 has gone much above 100 miles. The earth's gravitational force does not vanish at 25,000 miles from the earth or at any other distance; it is inversely proportional to the square of the distance. - Editor.

familiar 4".56/A as a magic cure-all for every telescopic problem; and it is the purest nonsense to contend, as some have done, that planetary features cannot be seen unless their diameter or width equals or surpasses 4".56/A.)

THE PRESIDENT.--I will ask you to return your thanks to Mr. Sellers for his interesting talk.

Dr. Steavenson is now going to open a discussion on "The Capabilities and Limitations of Small Instruments". I will ask him to speak to you.

DR. W. H. STEAVENSON.--I shall confine my remarks to some points relating to the extreme limits of telescopic performance, in the resolution of planetary detail and in the power to reveal faint stars.

It is of course well known that the resolution of a close double star depends on the sizes of the spurious disks of the components, and the formula proposed by Dawes (4".56 divided by aperture in inches) is in close agreement with the experience of most observers. But very few textbooks point out that the formula is not applicable to planetary detail, or give the reason for this. Cassini's division in Saturn's ring and the shadows of Jupiter's satellites, both visible with apertures between 2 and 3 inches, are well-known examples of the relatively superior resolving power of a telescope when applied to planets. Experiments have often been made on terrestrial objects also, and give similar results. Recently I have myself found that a black dot on a white ground is visible when subtending an angle about one-third of that corresponding to the Dawes formula, and I find that Sir William Herschel obtained a similar result in 1804. For a dark line visibility is attained at an angle of something like one-fifth of the Dawes limit. The reason for the apparent discrepancy, is, in the main, the low intensity of the illumination of planetary surfaces, each element of which has consequently a relatively small spurious disk. Of course, the disks are really of their full theoretical size, but, as with a faint star, their central parts are alone bright enough to be perceptible and there is not enough light towards the periphery of each to obliterate the fine detail observed, though it may reduce its contrast.

In the matter of light-grasp the tables printed in most books are quite misleading, as they are based on the incorrect assumption that a one-inch aperture will only just show a ninth-magnitude star. Actually for telescopes of small or moderate aperture, say of 6 inches, or less, the limit is something like two magnitudes below what is given in most tables. Curiously enough the incorrect curve gives correct values at its extreme ends, marked by the naked eye and the Yerkes 40-inch. This is due in the one case to the uncorrected aberrations of the expanded pupil, and in the other to greater unsteadiness of image and loss of light by absorption in the glass.

THE PRESIDENT.--Dr. Steavenson has given us a very instructive account. The subject is now open for discussion.

MR. A. HEATH.--Is the limit of light-grasp considered to be the faintest star glimpsed with averted vision?

DR. STEAVENSON.--That is the limit which I myself adopt.

DR. M. W. OVENDEN.--Surely when we ask if a given close double can be resolved or not, we are once again on physiological ground. The resolution or otherwise depends upon the smallest difference in light intensity in the overlapping diffraction disks that can be detected, and this, I think almost certainly, will vary to some extent from eye to eye.

I have one question I would like to ask: I gather that the Dawes formula for resolution is empirical; in the physics laboratory, the criterion of resolution of, say, two close spectral lines is taken as Rayleigh's law, where, assuming the same intensity distribution in the two lines, they are said to be resolved if the intensity of each at the point of overlap of the two intensity surves if 0.405 of the maximum intensity of each. Can anyone say whether the Dawes formula gives much about the same limits of resolution as the Rayleigh rule, or does it differ widely?

CAPTAIN M. A. AINSLIE said that some of those present might remember that in his Presidential Address to the Association in 1929 he referred to the ideas on resolving (or dividing) power due to Professor A. W. Porter, F. R. S., who considered that resolution in the case of two equal stars was effective when, on passing from the centre of one star image to that of the other, the drop in light at the half-way point was just, and only just, perceptible. At this half-way point the curve obtained by adding the ordinates of the two intensity curves would be (for a short distance) a straight line: the point of intersection of the two curves being the point of inflexion on them. On either side of the point of intersection one curve rises as fast as the other one falls. Professor Porter's limit of resolution from this point of view works out at approximately $\frac{4".0}{A}$. When the centre of one star disk falls on the dark ring of the other, we have the two star images $\frac{5".3}{A}$ apart and the drop in the intensity curve at the halfway point is very marked, and the stars would be separately visible even if they were considerably closer: though this condition (centre of one star on dark ring of other) is often given in textbooks as the "limit of resolving power".

With the Dawes empirical limit, we have the diminution of light at the halfway point quite appreciable, though not great: a normal eye would probably easily recognize the darkening. Professor Porter, however, points out (in a paper read to the Royal Microscopical Society in 1920) that "the question of resolving power is not an exact branch of science: the human element enters; and in consequence no exact statement can be made". The formulation of an exact law giving the resolving power for a pair of stars of unequal brightness would be a rather complicated problem (see Journal, 40, I, pp. 12-14, 1929 October).

DR. R. d'E. Atkinson.—I think it is most important to stress, as Dr. Steavenson has done, the fact that the usually quoted formula is by no means applicable for this purpose. It may also be of interest to point out that this would still be true even if the intensity were uniform throughout the diffraction disk. If we consider a piece of Saturn's ring, with the Cassini division, and let every point of the ring be the centre of a uniform disk, these disks will overlap to produce uniform illumination as long as we are well away from the division; but as soon as we come within one disk-radius of the edge of the division we come to a region where some of the disks which should have overlapped are missing. From this point on, to the middle of the division, the intensity will fall off gradually, and beyond that it will rise again. There will thus be a region where the intensity is less than that of the ring, and it becomes, as has been said, a physiological question how small a drop in intensity one can perceive; certainly this drop need not nearly be down to zero at the centre. The pattern of the light which is missing is the exact complement of that which would be present if we had no ring at all, but a luminous strip as wide as the division; in both cases we could see that something was there, but its apparent width is determined by diffraction and not by its true size, just as in the case of a single star. The argument, of course, is strengthened by the fact that the intensity actually is greatest at the centre of the diffraction disk, as Dr. Steavenson has said.

MR. R. REYNOLDS.--May I ask Dr. Steavenson what would happen if you extended the curve (relating to aperture and visible magnitude) to include the 100- and 200-inch telescopes?

DR. STEAVENSON.--I imagine that the results would fall short of the limits commonly given, but only on account of increased unsteadiness of the image, other things being equal.

MR. C. A. PADGHAM.--Speaking as a variable star observer, I would like to ask Dr. Steavenson where he thinks the curve of faintest useful comparison star against telescope aperture would be with reference to the limiting magnitude curve which is usually given? It is obviously impossible to make a reliable estimate if one can see the star for only a few seconds every minute.

DR. STEAVENSON.--I think I have most confidence in estimates made of stars about two magnitudes brighter than the absolute limit on the occasion of the observation.

MR. P. M. RYVES.--I can confirm that comparisons of brightness are easiest to make and most reliable when there is neither too much nor too little light. With regard to the limit of visibility of telescopes of different sizes, Dr. Steavenson's curve, based on actual observation, differs from what he calls the theoretical curve in giving much fainter limits for the smaller apertures, but the curves gradually approach one another and coincide on reaching a 40-inch refractor. I should prefer to put the case rather the other way round because it is obvious that the theoretical curve, or I would rather call is the textbook curve, suffers from a large zero error, it being based upon the assumption that a ninth magnitude is the faintest star that can be seen with a 1-inch telescope, whereas the limit is nearly two magnitudes fainter. If the textbook curve is moved down so as to coincide with the other curve, at the start it will be in fair agreement for the smaller apertures but will gradually depart, showing a limit about two magnitudes too faint for a 40-inch. This is because no account has been taken of the loss of light through absorption by the glass, which becomes increasingly great with the larger and thicker object-glasses, nor of the greater unsteadiness of the image mentioned by Dr. Steavenson.

In my own case I have found that the faintest star I can see with a $1\frac{1}{4}$-inch is 11th magnitude, with a 2-inch 12th magnitude, a 3-inch 13th magnitude, and a 5-inch 14th magnitude, which is in agreement with theory from the calculated areas of these apertures. After about 5 inches the loss of light through absorption begins to cause an appreciable falling off in effectiveness, and according to Dr. Steavenson's evidence this amounts to about two magnitudes for a 40-inch refractor.

DR. STEAVENSON.--When I said that the two curves coincided at their lower ends I was referring to the naked eye. Of course I agree with Mr. Ryves that they do not do so for a 1-inch telescope.

MR. F. J. SELLERS.--Leaving for a moment theoretical considerations as to resolving power, here are some instances of experience. Some years ago two sunspots appeared, about equal in size and each just visible to the naked eye. They were of the "regular" or round type and their separation when not far from the centre of the solar disk was almost exactly the same as the angular separation of the two main stars (pairs) in Epsilon Lyrae. Using a dark glass I could

quite definitely separate the sunspots with the naked eye, but I have never been able to separate Epsilon Lyrae. Some of the best solar photographs I have seen have been taken with an aperture of 1-inch. A 6-inch O.G. stopped down to 1-inch would result in a very fine, small aperture lens, no doubt, but the detail in sunspots and the granulation of the solar surface was very remarkable, in large scale photographs of 2 to 3 feet solar disk diameter, and considerably beyond what one would expect from theoretical considerations.

DR. STEAVENSON.--The contracted daylight pupil gives greater acuity than is obtainable at night, when the peripheral aberrations of the eye have full play. This accounts, I think, for the apparent discrepancy noted by Mr. Sellers.

MR. J. V. THOMSON.--It is very encouraging to members of the Variable Star Section possessing small telescopes to find they can see stars of 1½ mangitudes more than the textbooks indicate.

OBSERVATIONS AND COMMENTS

On pg. 5 we present three more drawings of Maedler's Square; these should be of interest in connection with the articles upon this lunar region by J. C. Bartlett, Jr., in our December, 1950, issue and by P. A. Moore in our July, 1951, issue. These drawings invite comparison with Figures 2, 3, and 4 on pg. 5 of our July issue. In March, 1951 D. P. Barcroft "discovered" a black cross-shaped feature near the north corner of the Square of Neison and Maedler. Reese and Haas have found this "Black Cross" to consist of separated shadows of ridges and valley-like enclosures; the resemblance to a cross grows less noticeable as the quality of the view improves. Although Barcroft thinks that this feature is the curious "cross" observed by Maedler and others (refer to the two articles mentioned above), Reese and Bartlett have remarked another object much more in accord with the description in Der Mond. Both Barcroft's new Cross and Maedler's historical Cross, as thus identified, are marked on Figures 2 and 3 on pg. 5. These drawings of Maedler's Square by different observers may serve to interested readers as the basis of a comparative study of how different observers draw the same lunar object. Maedler's Square is close to the north limb of the moon so that the libration in latitude has relatively great effects upon its appearance. The southeast wall of the Maedler-Neison Square, whose apparent disappearance stimulated Dr. Bartlett's study of the region, is absent from Figures 1 and 2 on pg. 5 but is probably shown as a narrow and diffuse bright streak on Figure 3.

We next shall conclude from our July issue the discussion of A.L.P.O. observations of Saturn in March-May, 1951. Figures 1 and 2 on pg. 1 will help illustrate this discussion.

During April Bartlett found the South Equatorial Belt to grow considerably darker and to develop an increasingly wavy south edge. Clearly less conspicuous than the North Temperate Belt on April 2, it equalled this belt in intensity on April 11 and surpassed it on subsequent dates, except that the two belts were equally dark from May 17 to 22. The work of other observers does not appear to confirm this darkening found by Bartlett, though it is only proper to mention that he observed Saturn far more frequently than anyone else.

The Equatorial Zone - North Tropical Zone, which lay between the two main belts, was usually by far the brightest part of the ball. Bartlett thought that its brightness definitely varied and once suspected it to be composed of large

brilliant ovals. In mid-April Moore suspected a very thin and faint belt just south of the shadow of the rings, which may well be the Equatorial Band of recent years; probably it is this belt which Bartlett often recorded in May as a "gray band" near this position, a feature otherwise very puzzling. Johnson, Osawa, and Haas found the N. Tr. Z. (north of projected rings) to be definitely brighter than the rather dull E. Z. (south of projected rings). The E.Z.-N. Tr. Z. was white to Bartlett, creamy white to Moore, and yellowish to Osawa.

The North Temperate Belt, the chief belt in the northern hemisphere, was wide, dark, and double. According to Bartlett, it further remsembled the South Equatorial Belt in having a wavy south edge and a flat north edge. Observers often glimpsed a complex structure in the N.T.B. but could not draw this delicate structure properly. Bartlett found the N.T.B. to grow considerably less dark and more narrow during April, while at the same time its south edge became less wavy. Again, however, other observers fail to confirm this trend. Haas suspected a bumpy south edge on April 17. Bartlett found the N.T.B. either brown or gray, but the gray was chiefly observed when the belt was not dark enough to show colors well. The components of the doubled N.T.B. were easier to divide than were those of the S.E.B. Osawa on March 30 found the space between the N.T.B. components bright, an appearance regularly drawn by Ranck; but some other observers show this space dusky (Figure 1 on pg. 1).

The ball of Saturn was rather dull north of the N.T.B., and a dusky North Polar Region capped the north limb. A North North Temperate Belt about midway between the north edge of the N.T.B. and the north limb was usually incomspicuous, even delicate, although Bartlett sometimes found this belt as dark as the S.E.B. even if much narrower. Bartlett observed the N.N.T.B. to grow much darker near the end of April; and its color to him was gray, or rarely brownish. Bartlett gave close attention to the color and intensity of the North Temperate Zone (between N.T.B. and N.N.T.B.) and found both aspects rapidly variable. Its hue was often some tone of brown, sometimes gray, and occasionally bluish gray or greenish. We might here quote a general remark by Bartlett: "The manifestation of greenish shades on the ball I find not uncommon in Saturn, though not of frequent occurrence or prolonged duration." The N. Te. Z., according to Bartlett, was distinctly more dusky in April than in March; from April 6 to 29 it was always darker than the South Tropical Zone - South Temperate Zone, reaching a maximum of darkness near April 16 and another near April 27. It may here be confirmatory that Johnson, Moore, and Haas in April occasionally described the ball as more dusky in middle and high northern latitudes than in middle and high southern latitudes. In May the N. Te. Z. averaged lighter to Bartlett than in April; it was brighter than the two southern zones mentioned above on May 2, May 10 and 11, and May 22 to 30 but darker than them on intervening dates. This observer caught glimpses of a very complex structure in the N. Te. Z.; sometimes there appeared to be a lacework of dark festoons across the zone (see also Figure 4 on pg. 1 of June issue), and once he observed splotchy and whitish cloud-like masses against a dusky background. Bartlett found the North North Temperate Zone (between N.N.T.B. and N.P.R.) to be bright throughout April; it was usually yellow in color, though white on April 4. The appearance of the shaded North Polar Region was greatly variable to Bartlett. Sometimes there was a small, black north polar cap; more often there was a larger, light gray shading; sometimes the shading was absent; and occasionally a white spot capped the north limb. There was no obvious or consistent periodicity in these variations.

A few of the observers recorded a very small number of central meridian transits. However, the only rotation-period that may be derivable from them is

for a small, very dark spot on the south edge of the North Temperate Belt observed by Bartlett with difficulty on April 2 at $1^h\ 24^m$, U.T., and on April 11 at $2^h\ 32^m$. The measured position of the spot on a drawing placed it 49°0 east of the central meridian on each occasion - a very curious coincidence. We can then compute the following periods from the interval of 217 hours, 8 minutes between the observations:

Assumed No. of Rotations	Period of Rotation
20	$10^h\ 51^m.4$
21	10 20.4
22	9 52.2

Though far more evidence is needed to fix a reliable period, the one of 10 hrs., 20.4 mins. accords best with past work.

During the last month we have received observations of the brightness of Uranus from S.C. Venter of Pretoria, South Africa and from K. B. Cockhill, F. J. De Kinder, W. E. Leeson, H. J. MacCordick, A. R. MacLennan, and (Miss) I. K. Williamson of the Montreal Centre at Montreal, Quebec, Canada. Our Canadian colleagues have thus done approximately as much on this project as everyone else combined! These estimates of the brightness of Uranus require only the simplest equipment; binoculars are quite sufficient. Let's all work a little harder on this project when the planet is again in the evening sky next year. Full instructions appeared in our February, 1951, issue.

Mr. Tsuneo Saheki of Osaka, Japan, directs attention to a lunar "valley" connecting the northwest wall of Riccioli to the southeast wall of Lohrmann. Being so near the east limb of the moon, this region is best examined for topographical detail about a day before full moon. A drawing by Mr. Saheki on February 20, 1951, at colongitude 76°9 is reproduced as Figure 3 on pg. 1. He then found the east half of this valley to be broad, and it cut through the northwest wall of Riccioli. The west half, however, was very narrow and cleft-like; and the valley terminated at a small bright pit on the southeast outer wall of Lohrmann. This valley appears to be shown as a cleft on Section XIX of the Wilkins map of the moon. However, Saheki's drawing differs enough from the Wilkins map that further study of the region appears very desirable. Saheki reports that this valley was discovered by Mr. Sakuzo Miyamori on April 5, 1936 with a 4-inch refractor and that it was observed in 1946 by A. Kitami with a 3-inch refractor, by E. Date with a 10-inch reflector, and by S. Murayama.

Figure 4 on pg. 1 is a drawing of Aristarchus by E. E. Hare, already described on pp. 11-12 of our March, 1951, issue. It may be compared with Figure 4 on pg. 1 of our March issue, a drawing of Aristarchus by E. J. Reese under very similar solar illumination. Readers should note how the superior resolving power of Hare's 12-inch telescope has enabled him to see much delicate detail on the walls of Aristarchus and to break up one of the dark bands on the wall into finer structures.

Figures 5 and 6 on pg. 1 are samples from E. J. Reese's large, growing, and excellent set of drawings of the lunar crater Conon. They are typical of his views of this object. We have added to these two drawings - quite without improving the overall artistic effect, we are sure - lettering intended to identify some of the streaks, wall bands, mounds, etc. This nomenclature is Mr. Reese's

own and is not in general usage; however, it is employed in discussions of Conon in The Strolling Astronomer. It is our intention that the lettering on Figures 5 and 6 should help you, our readers, to follow the descriptions in this periodical of work on Conon. Better still, why not turn your telescope upon Conon soon after first quarter and make a drawing showing your seeings in this lunar region?

Two drawings of Conon by E. E. Hare with his 12-inch reflector at 525X in rather poor to fairly good seeing (variable) are of topographical interest. The one was obtained on December 17, 1950, at colongitude 139°; the other was made 2 hrs. and 20 mins. later on December 18, 1950, at 14°9. "Streak" Z was seen as the shadow of a ridge, its bright west side being especially notable at the foot of wall band A. "Cleft" V was seen as a shadow-filled valley. The east inner wall of Conon was drawn much broader than it usually is under higher lighting - perhaps a fairly common effect (error?) near sunrise and sunset.

On June 7, 1951, L. T. Johnson wrote in parts as follows: "On May 8, 1951, at 9:42:10 P.M., E.D.S.T. [on May 9 at $1^h\ 42^m\ 10^s$, Universal Time] I was searching for lunar meteors and saw an object which I think certainly must have been one. It was very faint and appeared a few miles west of Grimaldi moving toward the W.S.W. (Position Angle = 250°) in a straight path with a slow, uniform motion. It was visible from 1 to 1.5 seconds and did not seem to vary in brightness in that time. The projected path had a length of about 25 miles so that the velocity in the plane perpendicular to the line of sight would be from 17 to 25 miles per second. As the meteor probably wasn't travelling perpendicular to the line of sight the velocity would be somewhat higher but would be well within the 10-47 miles per second range of [parabolic] meteors observed in our atmosphere. [The velocity could exceed 47 m.p.s. if the line of motion made a small enough angle with the line of sight.] I was looking directly at the object when it appeared and saw it very well. I don't think it could have been a terrestrial telescopic meteor as I have never seen one that moved so slowly. [The motion would have to be almost directly toward the observer.] (I observed 59 last year, mostly during the Perseid shower, using the R.F.T. eyepiece at the Newtonian focus of the 10-inch reflector.) I had previously reported a number of objects which I thought might be lunar meteors, but I always had some doubts. This time I have no doubts". Mr. Johnson's success should encourage others to search regularly for possible lunar meteors. His telescope is a 10-inch reflector. It is also evident that if only some other observer had seen Mr. Johnson's object at the same position on the moon at the same time, then we would have no doubt at all that it was near the surface of the moon. The need for planned, simultaneous searches for possible lunar meteors still exists!

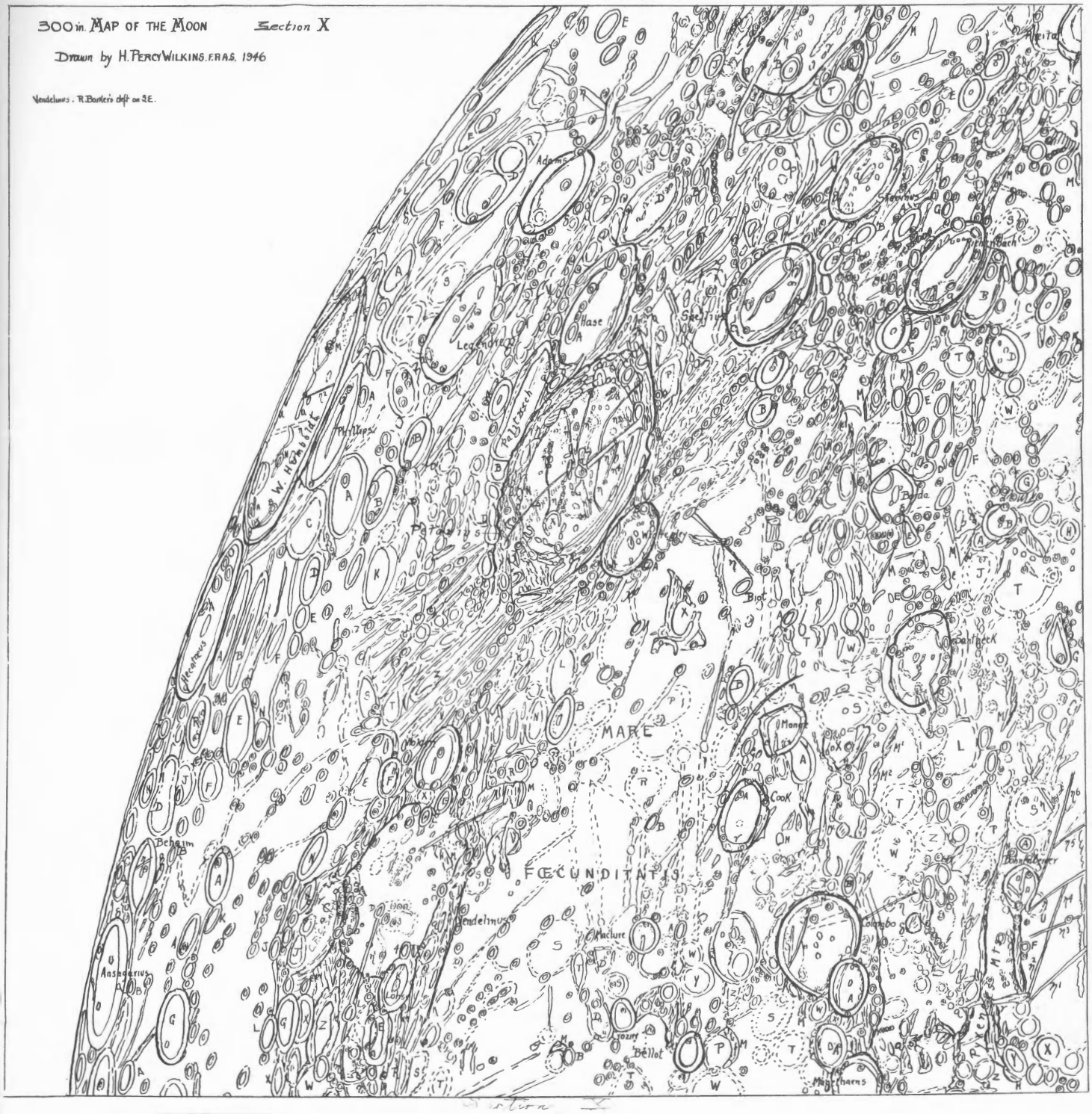
On June 10 M. A. Robins at Chicago, Illinois, spent 30 minutes in searching for lunar meteors. Results were negative.

On April 23, 1951, T. E. Howe made a sketch of the lunar walled plain Plato with a 4-inch refractor at 56X. It was a little past lunar noon on Plato, the colongitude being 109°. The sketch shows the west half of the floor to be darker than the east half and also shows two white spots, presumably floor craterlets. Their positions agree poorly with those of known craterlets in Plato, the low power used perhaps causing a loss of positional accuracy.

In his article, "On the Spanish Meteorites" Mr. Anthony Paluzíe-Borrell described how Mr. José Comas Solá had derived the remarkable mass of 3,200,000 tons for a meteorite causing a fireball observed over Barcelona, Spain, on May 15, 1933. (The Strolling Astronomer, Volume 5, No. 5, pg. 6, 1951). In

correspondence with Mr. Paluzíe the editor argued that the impact of so huge a mass upon the earth's surface would have produced great destruction over a large area and hence that perhaps the meteorite passed out of the earth's atmosphere without falling to the surface. On June 25 Mr. Paluzíe replied as follows: "If a globe 100 yards in diameter falls in the sea a wave must be produced, but in this case it was not seen by men because the event happened at night. The impact effects are thus null.

"I have heard that the Siberian meteorite which fell on February 12, 1947, in the Sihoté Aline Mountains carried before it an air cushion in such a manner that the meteorite, already broken into several parts, rebounded before its actual fall to the ground. A result is that the craters produced were small, the largest is 28 meters (31 yards) in diameter. If this is true it is not impossible that the Canyon Diablo Meteorite Crater must have been made by a very large mass. Besides, a ferrous meteorite does not break up. Surely its diameter much exceeded Comas Solá's one. On the other hand I believe that a meteoritical mass passing through the earth's atmosphere cannot separate from the earth. The brake or stop ('freno' in Spanish) produced by the atmosphere must reduce the meteorite's velocity, even if it is hyperbolic or parabolic; and finally the meteorite falls to the ground. It seems to be that you can accept without reluctance 3,200,000 tons for the mass of the meteorite that crossed the Barcelonian sky."



SECTION X

OF

H.P. WILKINS 300-INCH MAP OF THE MOON