Volume 4, Number 1 January 1, 1950

THE STROLLING ASTRONOMER

Mailing Address

The Strolling Astronomer
Institute of Meteoritics
University of New Mexico
Albuquerque, New Mexico

SUBSCRIPTION RATES

1 Year........................$3.00
6 Months...................... 1.50
1 Issue....................... .25

STAFF

-Editor-

Walter H. Haas, Instructor in Mathematics
Astronomer, Institute of Meteoritics
University of New Mexico
Albuquerque, New Mexico

-Counsellor-

Dr. Lincoln LaPaz, Head of Mathematics Department
Director, Institute of Meteoritics
University of New Mexico
Albuquerque, New Mexico

-Acting Venus Recorder-

Thomas R. Cave, Jr.
265 Roswell Avenue
Long Beach 3, California

-Acting Jupiter Recorder-

Elmer J. Reese
241 South Mount Vernon Avenue
Uniontown, Pennsylvania

-Acting Mercury Recorder-

C. B. Stephenson
Yerkes Observatory
Williams Bay, Wisconsin

SECOND CONFERENCE OF WESTERN AMATEUR ASTRONOMERS

All who attended agree that the First Conference of Western Amateur Astronomers, held at Los Angeles last August, was a highly enjoyable and successful affair. At the close of this Conference there was appointed a Committee to discuss plans for a 1950 Conference. The Chairman of the Committee is Mr. David P. Barcroft, Franchi Building, Madera, California. We have recently received the good news from Mr. Barcroft that the Peninsula Astronomical Society has kindly invited the Second Conference to meet at Palo Alto, California, in August, 1950. This invitation has been accepted. The Chairman of the Convention Committee of the Peninsula Astronomical Society is Mr. H. A. Wallace, 2139 Buchanan St., San Francisco 15, California. Both Mr. Wallace and Mr. Barcroft belong to the Association of Lunar and Planetary Observers, and we very much hope that all of our members who can do so will plan to be at Palo Alto next August. Meet your astronomical friends there, and exchange ideas. We shall furnish more details about the Conference as they become available.

Errata in December Issue. On pg. 9 of our last issue the Universal Time date of M. Williams' observation of "a thin hair-like bright line" was November 3, not November 2.

On the same page we erred in the interpretation of E. J. Reese's drawing of Aristarchus on November 3. In reality, Reese drew only the floor of the crater. It follows that the east inner wall was of normal brilliance for early morning lighting and that the two tiny crater-pits at the foot of this wall were observed. Each of the two most prominent dusky bands on the east inner wall has one of the crater-pits at its foot and continues westward beyond the pit for perhaps three or four miles on the floor itself, as shown on the drawing. In addition, Reese perceived two shallow craterlets several miles in diameter near the southeast rim of Aristarchus. Each held shadow. These craterlets are doubtless difficult objects; they are certainly seldom seen as craterlets.

PRINCIPLES OF ENVIRONMENTAL ADAPTATION AS APPLIED TO POSSIBLE LUNAR PLANTS

by James C. Bartlett, Jr.
(continued from December issue)

So far as I can see, there is only one entirely reliable index which we may safely use as a guide, namely, response to the sun. The sun is fundamental to the planetary life of every member of the system whence it follows that regardless of the metabolism or morphology of extraterrestrial vegetation we may reasonably expect to find a common response wherever such vegetation may exist. Such a response, reduced to essentials, would be by way of growth, development, and decline. In relation to the moon, if it can be shown that given lunar areas undergo a seasonal change of color which can be related to the movement of the sun in the lunar sky, we may reasonably ascribe the phenomenon to the appearance, development, and decay of vegetative organisms - for it must be remembered that "the four seasons" on the moon would be the four quarters of the long lunar day.

Does the moon offer such evidence? Unless some very gifted observers have been entirely and systematically deluded, the answer must be a positive "Yes".

And what is its specific nature? Certain formations and areas show what has been called a high-sun darkening; i.e., they grow darker as the sun gets higher - a singular fact, by the way, if one adheres to the incident lighting school. But this is not all. (If it were, we might perhaps forget all the rest.) Not only do they grow darker, but they change color. Moreover, the effect appears to be related to the sun's heat rather than its light. One here thinks of Pickering's announcement that the closer a formation is to the lunar equator, the more rapid and lush - if I may use such a term - is its high-sun darkening.

We have no terrestrial examples of inorganic soils or rocks which change color or become darker at the approach of noon; and since the moon is said to be bone dry, this would mean practically the complete absence of chemical reactions. Such reactions with extremely few exceptions take place only in the presence of water or when the reacting substances are in solution.

Moreover, as one school of thought holds the moon to be covered rather deeply with a layer of coarse, granular or pumiceous material, it is even more difficult to understand how such a surface would become darker under noon illumination. As the interstices in this assumed material would then be most directly illuminated with reduction of interstitial shadow to or near zero value, one would suppose that such a surface should become brighter.

What is certainly observed, in the areas showing seasonal change, is precisely the reverse. Nevertheless, many authorities take a dim view of high-sun darkening. It is, they say, an illusion. Nothing darkens - everything brightens; but some surfaces become so much brighter than others that the latter seem to become darker by contrast.

Does this make sense? I think not. In the case of Plato and Grimaldi, convenient comparisons can be made between the two dark formations and adjacent dark areas, e.g., the Mare Imbrium and the Oceanus Procellarum. I hardly think that any experienced observer would care to contend that either of these maria becomes so much brighter at noon as to cause a nearby spot which seemed gray at sunrise to become black by noon. Moreover, examination of Plato and Grimaldi with color filters, comparing them by the same means to the two maria, shows them to be not only relatively but absolutely darker. A red filter, used at about noon, will make Grimaldi as black as sin. In the same light the Oceanus also becomes darker - but it is far from equalling Grimaldi. In green light, on the other hand, the two are nearly comparable.

Another consideration will show the fallacy of the darkening-by-contrast theory. If the increasing altitude of the sun brightens surface A so much more than surface B that the latter appears to become darker in relation to the former, then it follows absolutely that with the decline of the sun B should show a reverse change of relation to A since B depends for its value upon the changing value of A. At any rate, if B darkens because A brightens, it is obvious that while A is growing dimmer B can scarcely be darkening even more. Yet this is what is often observed. The maximum darkening of Plato is said to come well after noon, i.e., after the adjacent surfaces are certainly becoming dimmer. The same is sensibly true of Grimaldi, though the phenomena of this formation are much more irregular. It is noteworthy, in relation to Grimaldi, that certain patches on the floor, especially and characteristically on the southeast floor, remain very dark long after noon and sometimes almost up to sunset though all comparison surfaces west of the formation have by then become very sensibly dimmed.

Well, then, what is the evidence from color? Perhaps I may digress for the nonce to remark that while some lunar colors can be seen with the unassisted eye at the telescope, they are by no means obvious and at noon are hopelessly lost in the overpowering glare which reduces everything to terms of light and dark, i.e., of white and black. Consequently, lunar colors are most accurately "seen" by filters, which paradoxically do not show the color at all but rather its absorption index. The latter may be used to establish the true color.

When Grimaldi, for example, comes out from the terminator under a low morning sun, to the unaided eye its surface commonly appears grayish though at times a faint brown tint is observable. If then examined in yellow light, green light, and red light successively, very little difference is to be noticed in the tone of the surface in the three colors, though it will be somewhat darker in green - because of the brownish color which appears to be the basic soil color - than in yellow.

A day after sunrise, however, very marked relative differences are established in the three colors. When compared to the nearby <u>Oceanus</u>, Grimaldi appears to be about the same darkness as seen in green light which, moreover, gives a more or less uniform tone over the whole floor of the great walled plain. In red light, however, it is seen to be very much darker than the <u>Oceanus</u>, while certain areas on the floor of the formation itself now turn <u>black</u> and are sharply differentiated so that the pattern of floor detail is radically different for red than for green light. At sunset the floor is once again nearly uniform in darkness.

We have, therefore, good evidence for a change of color no less than for an absolute darkening; and since the color changes can thus be readily demonstrated by filter work we must suppose that they are real. They do not correspond to anything we know about minerals, but they do act precisely as we have experience of developing vegetation which shows many changes of color throughout its life-to-death cycle.

So much for the evidence. It remains to be seen whether we can accommodate the concept of a lunar vegetation to the known-or theoretical-conditions of the lunar surface. There are two classical approaches to the problem of extraterrestrial life, neither of which has much to commend it. In the one, attempts are made to compare the environmental relations of terrestrial organisms (ecology) to the known or fancied environment of another planet. In the other, environment is totally ignored; and reliance is placed upon the ability of an Infinite Nature to adapt to <u>any</u> type of environment.

In practice they work out as follows: Comparison of terrestrial and extraterrestrial environments leads to a kind of elimination process, which generally ends in eliminating everything save perhaps a few insignificant forms such as bacteria, protozoa, algae, etc. The fallacy of this process should be apparent, for there is no valid reason to suppose that extra-terrene organisms would *necessarily* correspond metabolically or morphologically to any familiar to us. Hence, the fact that higher terrestrial forms could not exist in a given extraterrestrial environment is no guarantee that higher forms *per se* could not so exist.

The second approach is philosophically sound enough, but it immediately closes the door to scientific investigation of the evidence. Indeed, when this approach is carried to its logical perfection, no evidence is necessary. Everything becomes possible and even probable so that we may populate Jupiter no less than Mars, an asteroid no less than a large planet. But science must deal with ponderables and must work with measurable quantities. Hence, from the strictly scientific viewpoint this approach is inadmissible.

It seems to this writer that a third approach offers better possibilities. Instead of wasting futile energy in attempts to decide whether a worm or a man, as such, could exist in a given environment, it might better pay us to determine whether *principles* of nature exist whereby living matter (protoplasm) could be adapted to the environment. If we find that adaptation is possible, we need not trouble ourselves over the mode or the necessary morphology. There are adaptations of common principles familiar to us here which are quite as radical and as remarkable as any we might expect to find on the moon. To cite but one example, by a simple adaptation of the principle of equalized pressure (lattice-work skeleton) one of the most delicate sponges ever heard of, the so-called Venus Flower Basket, is enabled to live under a water-pressure which would stove in the sides of a battleship. Let us see, therefore, whether principles of nature exist whereby protoplasm might be brought into favorable adaptation to what we believe to be the lunar environment.

(To be continued)

MERCURY IN AUGUST AND SEPTEMBER, 1949

by C. B. Stephenson

All dates and times here are by Universal Time. Antoniadi's nomenclature will be used for the surface markings (his map is on pg. 193 of F. L. Whipple's *Earth, Moon, and Planets*).

Mercury was observed during its August-September eastern elongation by Donald O'Toole (6" reflector), Vallejo, California; E. K. White (7".5 reflector), Kimberley, B.C., Canada; and C. B. Stephenson (6" refractor), Chicago, Ill. This apparition was not a favorable one for evening observations, owing to the southerly position of the planet. It was, however, a favorable apparition for daylight observations, since at greatest elongation the separation between sun and planet was only about 1° less than the maximum possible. White and O'Toole displayed remarkable energy in observing on a total of 27 occasions between August 27 and September 19. White obtained, in addition, a valuable series of measures of the breadth of phase (distance between centers of limb and terminator) and disc diameter on nearly all dates on which he observed, using a filar micrometer of his own construction; the present writer feels that Mr. White should be praised, not only for his industry in constructing a micrometer for himself but for his perserverance in making a large number of measures of a difficult object. Measures of the disc diameter and breadth of phase near dichotomy constitute a very worthwhile program of observation, and this work is recommended to all micrometer-equipped readers who are interested in Mercury.

Stephenson observed on August 3, 9, 10, 20, 21, and 22 at values of i ranging from 22° to 59° and of k ranging from .96 to .76; it was not possible for him to observe near dichotomy. The quantity i is the phase angle (sun-Mercury-earth), and k is the theoretically illuminated fraction of the entire disc. Except for one mid-day observation, these views were obtained immediately before or immediately after sunset, in generally poor transparency and seeing. On all occasions a light shading of the terminator was thought to be present, and markings were suspected. The only markings considered to be surely visible, however, were Criophori on August 10, Aphrodites on August 21, and Criophori again on August 22. On August 10 Stephenson suspected that Criophori consisted of two components, the southern one, if present at all, being much the fainter and narrower of the two. This contradicts his past observations of the marking as well as Antoniadi's opinion of its form but is in partial agreement with its usual delineation by G. V. Schiaparelli and W. H. Haas. Haas, however, usually makes the components narrower and more widely separated than Stephenson's impression of August 10. Neither cusp was suspected of being brighter than the general surface at any time, with the exception of the north cusp on August 10. A cusp-cap was not suspected on this date, however, until the planet was so low in the sky that the disc was developing a colored fringe; and the observation is unreliable. O'Toole in a good view on September 10 drew a dusky terminator band and what probably corresponds to Atlantis or Criophori and also drew S. Persephones as an extensive area darkening the entire region of the south cusp, as on Antoniadi's map. No cusp-caps were drawn by O'Toole.

White evidently saw no dark markings but made the following observations of the appearance of the cusp-caps: August 27, the north cap was large and brilliant, the south smaller and less brilliant; August 28, both visible but less conspicuous than on August 27; September 1, no caps seen; September 2, no caps seen, in a rather hazy sky; September 6, no caps seen at $0^h 15^m$, but at $21^h 00^m$ a north cap was conspicuous; September 7, north cap very bright, with a less bright south cap now visible; September 8, appearance similar to September 7; September 9, north cap bright, no south cap seen; September 12, small bright north cap suspected, no south cap seen. The north cusp was thus found to be consistently more brilliant than the south cusp. There seems to be some correlation between the seeing and transparency on the various dates and the conspicuousness of the cusp-caps seen on the same dates-good seeing and transparency producing conspicuous caps, poor conditions producing none. The correlation is not exact, however, and in any case could not affect the relative brightness of the two cusps. Reducing the observations to "no atmosphere", then, seems to leave at least a brightening of the north cusp relative to the southern one on September 6 and 9, separated by a period of "normalcy". This opinion rests upon a study of White's notes about the conditions of observation. An inspection of the table of phase-observation data reproduced below shows that O'Toole's observations were not made at times very suitable for confirming or contradicting White when the latter thought that a bright and conspicuous cap was present. Probably the best comparison is provided by O'Toole's observation of September 10 at $2^h 30^m$ and White's of five hours earlier.

O'Toole here confirms White on the difference between the cusps, as well as on the absence of a south cap; but he saw no north cap. There is, of course, abundant confirmation from other observers, past and present, that the south cusp is usually the less bright of the two.

We now consider observations of the phase near dichotomy, probably one of the most worthwhile programs of observation of Mercury that can be executed with a small telescope. White and O'Toole each made a fine series of numerous observations of the phase before and after dichotomy. They found that dichotomy occurred considerably earlier at the September elongation than it should have occurred theoretically, i.e., that it occurred before i = 90°. Their observations are listed below where i is given to the nearest whole degree. White's measures of the disc diameter and breadth of phase were each averages of four readings; only the ratios, breadth of phase to diameter, are given here.

Date	Time	i	Observer	Phase	B.P./D. (White)
1949, Aug. 31	0^h45^m	70°	White	Terminator slightly convex	0.545
Aug. 31	2^h50^m	70°	O'Toole	Phase gibbous	
Sept. 1	1^h00^m	72°	White	T. least bit convex	0.520
Sept. 1	2^h45^m	72°	O'Toole	P. gibbous	
Sept. 2	1^h00^m	73°	White	T. "ogee" or almost straight	0.51 (not reliable)
Sept. 2	2^h40^m	73°	O'Toole	P. gibbous	
Sept. 3	2^h45^m	75°	O'Toole	P. gibbous	
Sept. 4	2^h45^m	77°	O'Toole	P. gibbous; good view	
Sept. 6	0^h15^m	80°	White	T. straight? Cusps project	0.47
Sept. 6	2^h20^m	80°	O'Toole	P. slightly gibbous	
Sept. 6	21^h00^m	81°	White	T. straight or little concave	0.494
Sept. 7	21^h15^m	83°	White	T. exactly straight	0.50 (good image)
Sept. 8	21^h00^m	85°	White	T. concave, often looks straight	0.50
Sept. 9	2^h50^m	85°	O'Toole	P. near half(?)-poor view	
Sept. 9	21^h30^m	87°	White	T. definitely concave	0.486 (very sharp image)
Sept. 10	2^h30^m	88°	O'Toole	Finest view of all. T. slightly concave	
Sept. 12	2^h40^m	92°	O'Toole	T. called concave, in poor view	
Sept. 12	21^h00^m	94°	White	T. very concave	0.375
Sept. 13	2^h35^m	94°	O'Toole	T. concave	

Both observers agree that dichotomy occurred before $i=88°$, and White's observations indicate that it may have occurred with i as small as $81°$. His micrometer measures indicate that the breadth of phase was less than the semi-diameter for some time before theoretical dichotomy; this condition has been both confirmed and contradicted by past observations. It has been argued that, because the terminator is poorly illuminated, one tends to see the phase too small, especially when observing on a daylight sky. One is surely not certain that such an effect was not in operation in the case of the present observations. However, O'Toole had a good view at $i=88°$, when the terminator was seen concave; and White characterized the image as "good" at $i=83°$, when the terminator was seen straight. At any rate, these observations are in accord with the past experience of many observers, who have found that dichotomy occurs early at eastern elongations and late at western elongations. It would be of great interest to make further phase observations at future elongations. It is scarcely possible to overemphasize the importance of exercising care in making these observations, particularly since the phenomenon in question is a theoretically unlikely one. If the terminator appears less convex than predicted by theory, one should take the utmost care to insure that he has not been misled by the faintness of the terminator or by dark surface markings near the terminator, which may be near the limit of visibility and not consciously seen.

Postscript by Editor. Since Mr. Stephenson's manuscript arrived, we have received these additional observations of Mercury from Mr. S.C. Venter in Pretoria, South Africa. He used a 3-inch refractor at 120X.

Date	U.T.	i	Phase	Remarks
1949, Sept. 2	$16^h\ 30^m$	75°	convex	seeing poor
Sept. 5	16 15	79	convex	seeing poor
Sept. 6	16 15	81	slightly concave	
Sept. 8	16 15	85	definitely concave	

Mr. Venter's work would place observed half-phase at $i=80°$ and thus is in good accord with the results of Messrs. White and O'Toole that there was a definite difference from theory last September. Venter and White are in especially good agreement; since the former observed near sunset and the latter both near sunset and in mid-afternoon, one has more confidence that this difference represents more than the invisibility of the dimly lit terminator of Mercury. If not so explicable, it is presumably (as with Venus) an effect of an atmosphere.

METEORITES OUTSIDE THE EARTH'S ATMOSPHERE

On pp. 505-506 of Popular Astronomy for December, 1949, Dr. C. P. Olivier discusses an observation by Mr. John J. O'Neill of a dark object seen against the moon on October 2, 1949 (E.S.T. date). Mr. O'Neill has kindly sent us a very detailed report on this object, and we hope to say more of it in a future issue. Dr. Olivier considers the possibility that the object was a giant meteorite (or tiny asteroid?) about 13,000 miles away. There is, of course, no reason why a meteorite in space should not occasionally reveal itself as a dark object against the moon or some other bright background; but the sizes required of the meteorite are great. The distance could hardly be less than 120 miles, for the meteorite would luminesce at lower altitudes (Dr. Olivier's remark). Extensive studies by W. H. Pickering with a 12-inch refractor of an artificial disc showed that occasionally a dark dot only 0".16 in diameter could be seen clearly (Harvard Annals, Volume 32, Part 2, pp. 144-145, 1900). A meteorite about six inches in diameter would subtend an angle of 0".16 at a distance of 120 miles. However, it would probably have to be several times that large to call attention to itself as a rapidly moving dark object; it would also need to be larger to be detected at distances greater than 120 miles. A body of these angular dimensions ought, incidentally, to cause occasional brief occultations of stars.

The following ideas about the visibility of extraterrestrial meteoritical phenomena are extracted from a paper read by W. H. Haas at the Twelfth Meeting of the Meteoritical Society in Los Angeles in September, 1949. One may first ask about meteors luminous in planetary atmospheres. A meteor as brilliant as the full moon, or of stellar magnitude -12, at a distance of 100 miles would be of magnitude 5 at the distance of the moon, of magnitude 16 on Mars at its closest, and of magnitude 17 on Venus at greatest elongation. Except for the moon, such meteors are beyond the grasp of any but giant telescopes. However, those rare terrestrial fireballs reported to rival the sun in brilliance (stellar magnitude -27) would be easily visible in telescopes of ordinary size; and some lucky amateur might just happen to see one sometime. Meteorites striking the solid surface of an atmosphereless body at their cosmic velocities will produce impact-flares. Dr. Lincoln La Paz has computed (Popular Astronomy, Vol. 46, pg. 277, 1938) that a meteorite having a mass of 176 pounds and a velocity of 31 miles per second will cause a flare of magnitude -1.5 as seen from the earth when striking the moon (if that body totally lacks an atmosphere). Mercury is often said to lack an atmosphere. Using the result above and doubling the velocity because Mercury is closer to the sun, it follows that a 176-pound meteorite striking an atmosphereless Mercury will cause a flare of magnitude 10, a 22-pound meteorite a flare of magnitude 12. Perhaps a 15-inch telescope could reveal such flares against the dark hemisphere of Mercury on a twilight sky. Finely divided meteoritical material left suspended in the atmosphere of Mars after a meteorite's passage might account for some of the obscurations of detail reported by students of that planet. Meteoritical impact-craters have little or no chance of being recognized as new objects on the moon (the mapping of such small detail is too incomplete) and are quite undetectable elsewhere.

Many of the papers read at the Twelfth Meeting of the Meteoritical Society have now been published in the Contributions in Popular Astronomy or are available elsewhere to most of our readers; therefore, the summary begun in the October, 1949, issue will not be continued.

MARS AND SATURN

On January 15 the angular diameter of Mars will be 8".7, large enough that ordinary-sized instruments of good quality should begin to show a fair amount of detail. The north pole will be tipped toward the earth by

22 degrees. Quantity ☉, the heliocentric longitude of Mars so measured as to be 0° at the vernal equinox of the northern hemisphere, will be 64°. All observers are again requested to contribute each month's observations no later than the tenth of the following month. Only in this way can the data be studied promptly and efficiently.

During the last month observations of Mars have been contributed by T.R. Cave (8-inch refl.), T. Cragg (6-inch refl.), W. H. Haas (6-inch refl.), E.E. Hare (7-inch refl.), L. T. Johnson (10-inch refl.), S. Murayama (6-inch refl.; 8-inch refr.), and D.O'Toole (6-inch refl.). In November and December the north polar cap was brilliant and conspicuous. Hare made a visual estimate at the telescope of its angular diameter on December 1 and obtained 39 degrees. Haas obtained 40 degrees as the average of measures of his four drawings from November 25 to December 16 and 30 degrees as the average of two visual estimates on December 12 and 14. (All dates here are by U.T. and in 1949). It is puzzling that there is still very little evidence of the spring decrease in size of the north cap, though ☉ had reached 51° on December 16. The bordering north polar band is prominent; it is extremely broad if only moderately dark. Perhaps it was less dark in November and December than in September and October. Haas alone regularly sees a very narrow, much darker north edge to the north polar band. The doubtless atmospheric south cap is still diffuse and rapidly variable in size, shape, and brightness; it is often invisible, and it is always smaller and dimmer than the north cap. In December the south cap was sometimes rather plain to Haas so that it is perhaps becoming more pronounced with the advance of the southern autumn. Casius, Acidalium, and the Propontii have been seen well as large, wedge-shaped dark markings in high northern latitudes, where Ceraunius also perhaps presents a similar aspect. On November 24 O'Toole had a clear view of Nepenthes canal, which Haas drew on November 25. Several observers have seen a number of white areas, sometimes of large extent, on the limb and the (sunset) terminator; these are presumably clouds. Often the equatorial and southern features look vague and ill-defined.

On November 25 E. Pfannenschmidt wrote in part as follows: "Mars here has recently disclosed the awaited large north polar cap with rather prominent polar cap band. We do much work with visual Schott and General filters of excellent quality; and the north cap shows considerable increase in size in blue and violet, indicating cloud and mist masses above it. Very frequent observations of huge meteorological phenomena over the continental disc portions near the Martian equator have been reported with yellow light filters (cloud type II). Cloud types of class I have been observed in blue and violet light near the north cap but are principally centered over the southern disc portion. It is interesting to note that the majority of the clouds of both types I and II are found on the sunrise terminator [strictly speaking, on the limb]." We congratulate our German colleagues on these excellent results with color filters on the small disc and urge American observers to make more use of filters of known transmissivities.

If the work of different observers can be correlated, we can construct a most interesting history of varying, large-scale atmospheric obscurations in the Syrtis Major region. On November 11 at C.M. 328° Johnson in good seeing apparently drew Syrtis close to the terminator. On November 13 at C.M. 347° Haas depicted the east shore of Syrtis near the terminator. On November 15 Hare saw only "vague grayish areas on central part of disc" and was later startled when he computed the C.M. to be 286° - those areas were the usually prominent Syrtis Major! At C.M. 296° on the same date Johnson drew only the north part of this mare; what was drawn was apparently rather light. On November 17 at C.M. 313° Haas remarked that Syrtis was less conspicuous than Casius or the north polar band. On November 19 at C.M. 257° Cave saw Syrtis very plainly with its usual aspect and actually drew structure inside this mare - no mean accomplishment when the diameter of Mars is 5."8! The obscuring mists apparently gathered during the next 26 hours, for on November 20 at C.M. 282° Haas saw no certain sign of the northern part of this mare. He did not know the C.M. while observing, and conditions were average. Syrtis was prominent to O'Toole in good seeing on November 24 at C.M. 259°; and Haas drew it near the limb on November 25 at C.M. 238°, even though the seeing was poor. The mists then seemingly formed again; for Murayama on November 29 at C.M. 314° saw Syrtis very imperfectly, if at all, with an 8-inch refractor of good quality.

T. R. Hake of York, Penna., may have been the only observer of a daylight occultation of Saturn by the moon last November 15; he used a 5-inch refractor at 60X with fair conditions. Disappearance was near $13^h 30^m$ (8:30 A.M., E.S.T.) He writes: "I thought I saw [a dark limb band across the planet] when the disc was half covered; but the view was fleeting, and it might well have been an illusion. During several brief moments of good seeing I got the impression that a dusky band was adjacent to the limb of the moon; but it was fainter than the one I saw with ease when Jupiter was occulted on April 30, 1944. In spite of short time interval involved, etc., the impression was lasting." Was anyone else watching? The editor wrote briefly about these curious dusky limb bands sometimes seen at occultations of planets in Journal of the Royal Astronomical Society of Canada, Vol. 38, pg. 351, 1944.

Data on Saturn have been received during the last month from F. E. Brinckman (two 6-inch refls.), T. Cragg (6-inch refl.), W. H. Haas (6-inch refl.), E. E. Hare (7-inch refl.), L. T. Johnson (10-inch refl.), S. Murayama (8-inch refr.), D. O'Toole (6-inch refl.), E. J. Reese (6-inch refl.), and E. K. White (7-inch refl.) The general description on pg. 11 of the December issue still applies. From mid-November to mid-December the north component of the wide and double South Equatorial Belt was regularly rather dark to Haas; the south component, faint and diffusely outlined. On December 1 Hare suspected a darker southern edge to the North Temperate Belt. The presence of a thin and difficult Equatorial Band has now been confirmed by Brinckman and Reese. A sixth belt (after S.E.B., E.B., N.T.B., and two Polar Bands), a North North Temperate Belt, roughly midway between the N.T.B. and the N.P.B. has been observed by Cragg, Brinckman, and Haas. This N.N.T.B. is broad but faint and may vary in appearance. In good seeing on November 24 O'Toole recorded a fairly bright zone just south of the S.E.B., and Cragg has drawn one or two inconspicuous belts and zones in high southern latitudes. In November and December Haas occasionally found the Equatorial Zone much duller than usual. The projection of Rings A and B against the ball is very hard to see. In very good seeing on November 11 Johnson suspected the Crape Band. He computes that the width of the Ring C projection was than 0."12. Work in recent years would very strongly indicate that the Crape Band was then not the C projection but rather the shadow of Ring C (e.g., The Strolling Astronomer, Volume 3, Number 1, pp. 5-7, 1949); its width would hence be even less than 0."12. Other observers have not even suspected the Crape Band, and it appears likely that planetary features of these dimensions are beyond the grasp of ordinary telescopes. Our observers still barely perceived Cassini's Division at the ansae and dark sky between the rings and the ball, even in mid-December when the Saturnicentric latitude of the earth was -1°.5. In his splendid view on November 11 Johnson saw Encke's Division as a dark dot at the ansae; two other dots interior to Cassini's may be the Third and Fourth Divisions (Cragg suspected the Fourth on October 31). Johnson's success is amazing, for the Saturnicentric latitude of the earth was -2°.3 on November 11!

A transit of satellite Titan on November 28 was observed by Brinckman and Haas with the latter's 6-inch reflector and fair conditions. Ingress occurred at about $10^h 28^m$ (predicted), but even at $10^h 15^m$ the satellite was hard to see. Titan is evidently much the same brightness as the duller outer portions of its primary; for it remained invisible until about $11^h 0^m$, when it was barely visible as a dark spot. By $12^h 0^m$ it was easily seen as a very dark spot, perhaps darker than any Saturnian surface feature. Brinckman placed the satellite on the central meridian at $13^h 0^m$; Haas obtained $12^h 57^m$. An extrapolation of data on pg. 41 of the 1949 Handbook B.A.A. gives a predicted time of about $12^h 50^m$. Haas observed the next transit of Titan on December 14 in poor seeing and found similar appearances. The predicted time of transit was $11^h 44^m$; the observed, $11^h 32^m$. Future transits of Titan will occur on January 15 and 31.

From November 14 to December 19 Haas made 18 comparisons of the relative brightness of the east and west arms of the rings on 15 different dates. On every single occasion the west arm (left in simply inverted view) was the brighter of the two with Wratten Filter 47 (blue). No difference was ever seen without a filter or with Wratten Filter 58 (green). With Wratten Filter 25 (red) the west arm occasionally looked brighter, but usually there was no difference. While visiting Haas, Brinckman on November 28 independently "discovered" a strikingly greater brightness of the west arm with Filter 47 and confirmed the lack of a difference with Filters 25 and 58 or no filter. He repeated these results on November 30 and December 2. The two observers agree that with Filter 47 the east arm looks dusky and purplish. The relative brightening of the west arm with this filter is probably by no means always equally great, and on November 30 the two observers independently found the difference in brightness less at $12^h 41^m$ than at $9^h 34^m$. No other observations with filters of known transmissivities are available. Certainly additional visual - and photographic-studies with filters are important. Wratten Filter 47 transmits 32 percent of the light in the visual range, the rather flat maximum of transmission being near 4250 angstroms.

What is the cause of this strange appearance? It may have been a very new development on November 14, for Haas saw nothing of it in examinations of Saturn with color filters last autumn as late as November 9 and 13. The same reasoning would indicate that this phenomenon was surely absent in recent past apparitions, and the editor can recall no references to it in astronomical literature. Why should the west arm of the rings reflect more light than the east arm in blue and about the same amount in other colors?

On pg. 12 of the December issue we listed central meridian transits of the terminal ends of a darker section of the North Temperate Belt. These are here continued:

Date	End	Observer	U. T. Central	Conditions
1949, November 25	preceding	Haas	$10^h 25^m$	bad
November 27	preceding	Haas	12 28	fairly good
November 30	preceding	Brinckman	10 12	poor
November 30	preceding	Haas	10 12	poor
November 30	following	Brinckman	11 16	rather poor
November 30	following	Haas	11 11	rather poor
December 2	preceding	Brinckman	11 45	rather poor
December 2	preceding	Haas	11 47	rather poor
December 2	following	Brinckman	13 3	fair
December 2	following	Haas	12 57	fair
December 5	preceding	Haas	9 8	fairly good
December 5	following	Haas	10 13	fairly good
December 7	preceding	Haas	10 23	poor
December 7	following	Haas	11 16	poor
December 14	preceding	Haas	9 28	poor to very bad
December 14	following	Haas	10 14	bad
December 16	following	Haas	11 2	bad
December 19	following	Haas	7 57(?)	bad

The editor interpreted the previously published transits to mean that the rotation-period decreased from about 10 hrs., 14 mins. on November 3 to about 10 hrs., 6 mins. on November 20. He considers that these new data show that the period continued to shorten and reached about 9 hrs., 49 mins. near December 7, after which it was fairly constant. This extremely unusual motion certainly deserves careful study and may be difficult to interpret theoretically. Observations of the darker section, but no transits, were secured by Cragg on November 6, by L. T. Johnson on November 15, and by O'Toole on November 24. A drawing by Cragg on November 6 at $12^h 10^m$ shows the following end somewhat past the C.M. (preceding end on at $11^h 18^m$, according to Haas) and thus constitutes important evidence that the following end shared the remarkable motion of the preceding end from November 3 to 30, when no transits of the following end were observed. On November 30 and December 2 Brinckman and Haas agreed that the preceding end was rather diffuse and indefinite and that the following end was more distinct and easier to "place" for a transit. The preceding end of a brighter section of the North Tropical Zone (between N.T.B. and shadow of rings) was found to be at about the same longitude as the preceding end of the darker section of the belt from November 12 to 25 but could not be seen on November 27 and later. Reese, Brinckman, and Haas have observed transits of detail in the North Temperate Belt besides the darker section here being discussed; but the possible derivation of rotation-periods from them must await more study. (On December 12 and 14 Haas secured a period of 10 hrs., 6.5 mins. for a bend in the belt on the basis of five rotations only.)

VENUS IN NOVEMBER

by T. R. Cave, Jr.

This month's Report will be devoted partly to a discussion of dichotomy and the coming inferior conjunction of Venus with the sun. The Recorder wishes sincerely to thank the many contributors and especially to welcome a new member of the A.L.P.O., Mr. Louis Bellot of Long Beach, Calif., who has submitted some very interesting observations of Venus using his excellent 6-inch reflector of F:13 focal ratio. Three well-known A.L.P.O. members have also sent in their first Venus observations for several months; they are Messrs. Raymond F. Missert of Kenmore, N. Y., E. E. Hare of Owensboro, Ky., and S. C. Venter of Pretoria, South Africa. As usual, all dates and times are by Universal Time.

Dark Hemisphere. A communication received on December 13 from W. H. Haas may well be of considerable importance. He said in part: "A peculiar appearance of the dark hemisphere of Venus was suspected between $23^h\ 10^m$ on December 11 and $0^h\ 40^m$ on December 12....Color filters may be of importance here". Mr. L. Bellot observed the planet at $1^h\ 10^m$ on December 15 and was immediately struck by the unquestionable illumination or phosphorescence of the dark side of the planet. F. E. Brinckman and the Recorder were immediately notified. Cave observed from $2^h\ 35^m$ to $3^h\ 20^m$ on December 15 and was extremely impressed by the very unusual appearance, using 160X on his 8-inch reflector. He confirmed another appearance also first noted by Bellot. Stretching out from near either cusp and arching well into the unilluminated hemisphere was a considerably brighter area resembling a double convex lens in appearance. Cave estimated visually that this "lens" area was at least twice as prominent as the phosphorescence of the remainder of the dark hemisphere. Brinckman easily confirmed these aspects at $3^h\ 25^m$ on the same date. He and Cave found them more easily visible with a blue filter. Readers may recall that Dr. J. C. Bartlett found the dark hemisphere to be visible on October 9, 1949 (pp. 6-7 of December issue). At $23^h\ 14^m$ on November 14 Dr. Bartlett again observed a luminescence of the dark side, on this occasion brighter than the (dark) sky and dark copper brown in color.

[The Long Beach observers have done an excellent job of confirming the remarkable appearances of the dark hemisphere on December 15. Coordinated and confirmed observations of this kind are worth dozens of isolated examples. It will be noted above that on December 15 the "lens" looked brighter than the rest of the dark hemisphere in blue light. Three days earlier, on December 11-12, Haas suspected this same "lens" to be darker than the sky in red light only; it was otherwise invisible. The color of the "lens" was hence presumably blue. Venus was so far from the sun in the sky in October-December (dichotomy in November) that it appears difficult to account for the visibility of the dark hemisphere by projection against the Zodiacal Light or the outer solar corona, nor could this cause explain readily inequalities in the tone of the dark hemisphere. - Editor]

Dichotomy. A very large number of estimates have been received, and for three additional observers it has been possible to determine reasonably well the date of observed dichotomy from drawings submitted. These three cases are indicated by asterisks in the table below. "Observed dichotomy" here means the time at which the terminator was exactly straight. Quantity i is the phase-angle, sun - Venus - earth.

Observer	Observed Dichotomy	i
J. C. Bartlett	1949, November 14.9*	86°9
L. Bellot	November 16.1	87.5
P. D. Bevis	November 14.1	86.4
F. E. Brinckman	November 14.0	86.4
T. R. Cave, Jr.	November 16.2	87.6
P. O. Chorley	November 14.0*	86.4
T. A. Cragg	November 17.0*	88.0
W. H. Haas	November 10.0	84.3
E. E. Hare	November 15.8	87.4
L. T. Johnson	November 10.9	84.7
H. Le Vaux	November 13.0	85.8
R. Missert	November 10.0	84.3
D. O'Toole	November 14.0	86.4
E. J. Reese	November 14.0	86.4
S. C. Venter	November 12.8	85.7

The average of the 15 determinations is November 13.8, and the corresponding value of i is 86°3. Geometric dichotomy, when i equalled 90°, came at November 20.4. The difference is attributed to the atmosphere of Venus. The present observed result agrees pleasingly well with 1937-42 studies, which gave an average value to i of 85°9 when the terminator was straight (J.R.A.S.C., Vol. 37, pp. 146-152 and 193-204, 1943), and with some more recent results reported in past issues of The Strolling Astronomer.

It will be noticed that there is considerable variation in the results of the different observers, fully seven days separating Missert and Haas at one extreme and Cragg at the other. Some possible causes of these discrepancies are: 1-Varying atmospheric conditions, including bad seeing. 2-Local civil time of the observations. Some of our colleagues watched Venus on a daylight or twilight sky; others, on a darker sky. Irradiation must enter in on a dark sky. 3-Differences in aperture and usual magnification. 4-Perhaps most important of all, "personal equation".

The Coming Inferior Conjunction. The planet will be at inferior conjunction at 7^h on January 31. It will then be seven degrees to the north of the sun, and this circumstance will considerably help its effective study at this phase. W. H. Haas requests that observers of Venus then give close attention to these interesting and useful special programs:

1. Record the angular perimeter, which will become much more than 180°. Micrometer measures are the best method, but measures of drawings and even direct visual estimates may also be used.

2. Carefully and regularly compare the color and intensity of the dark hemisphere to those of the adjacent sky. If possible, use color filters of known transmissivities to supplement direct views.

3. Be alert to any peculiar or unusual appearances, such as irregularities in the width and brightness of the prolonged cusps.

In searching for Venus near the sun, be most careful not to bring the sun into the field of the eyepiece. Loss of vision might result.