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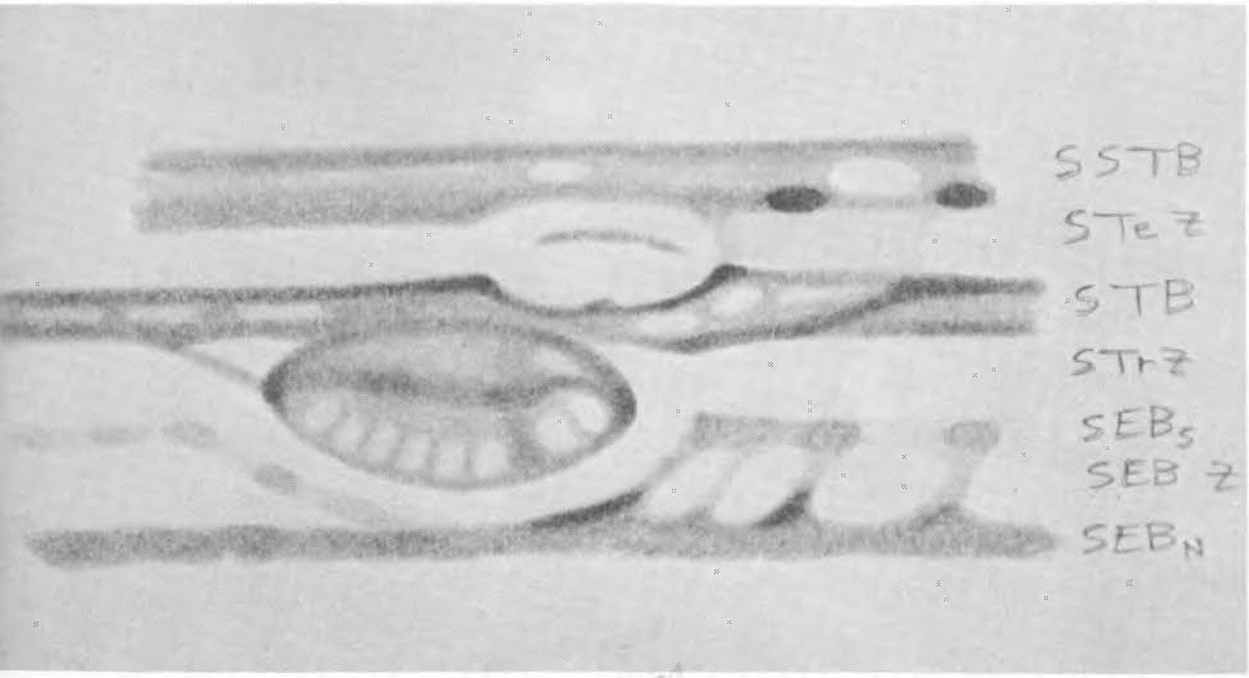

Drawing of Red Spot and vicinity on Jupiter by Elmer J. Reese with Clyde W. Tombaugh's 16-inch Newtonian reflector at 524X. January 21, 1964. 0 hrs., 29 mins., Universal Time. Seeing 7 (Tombaugh-Smith scale), transparency about 6 (twilight). Orientation that of a simply inverted view with south at top. Telescope employed in programs of Research Center, New Mexico State University.

THE STROLLING ASTRONOMER

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in Las Cruces, New Mexico

Founded In 1947

IN THIS ISSUE

PLANETARY OCCULTATIONS AND APPULSES IN 1964

1. The following occultation by Venus has been predicted:

Date	Star	Area of Visibility	Station	Disappearance U.T. h	m	P °	Reappearance U.T. h	m	P °
Aug. 21	B.D. +19° 1559 (7ᵐ.4)	S. Asia	Hyderabad	23	20	63	23	31	297

2. The following occultation by Mars has been predicted:

Date	Star	Area of Visibility	Station	Disappearance U.T. h	m	P °	Reappearance U.T. h	m	P °
Nov. 23	B.D. +10° 2199 (8ᵐ.6)	Europe	Greenwich	03	36	87	03	41	313

3. The following occultation by Saturn's Rings has been predicted:

Date	Star	Area of Visibility	Station	Disappearance U.T. h	m	P °	Reappearance U.T. h	m	P °
May 6	54 Aquarii (6ᵐ.9)	New Zealand	Wellington*	-			14	14	267

* The disappearance behind the ball of the planet occurs before the planet rises.

4. Venus may pass in front of the radio source 3C(160)A on May 19ᵈ.9

5. Mars may pass in front of the radio source MSH 21-125 on Feb.16ᵈ.8

6. Jupiter may pass in front of the radio source 3C(85)A on July 26ᵈ.2

7. The following appulses may be of interest to observers:

Planet	Date	U.T. Time of Conjunction		Star	Mag.	Geocentric Separation*	Horizontal Parallax
Venus	Apr. 21	08^h	06^m	B.D. +26° 775m	6.9	-19"	14"
	Aug. 21	23	31	B.D. +19 1559	7.4	- 4	14
Mars	Aug. 12	22	31	B.D. +23°1451	8.6	+ 7	4
	Aug. 29	22	14	B.D. +22 1689	8.0	-10	4
	Sept. 2	10	03	B.D. +22 1735	7.1	+17	4
	Sept. 2	17	48	B.D. +22 1741	8.6	+25	4
	Nov. 23	03	41	B.D. +10 2199	8.6	+ 2	6
Saturn	Apr. 29	06	26	B.D. -12°6260	8.6	+12	1
	May 6	12	55	54 Aquarii	6.9	- 4	1
	June 16+			B.D. -11°5855	7.3	-	1

* The geocentric separation given here is in the sense declination planet minus declination star.

\+ No actual conjunction occurs in R.A.; but Saturn is at a stationary point, and its slow movement southwards in declination carries the edge of the Rings to within 2" of the star around June 16^d 18^h, U.T.

We are again indebted to Dr. Gordon E. Taylor of H. M. Nautical Almanac Office, Royal Greenwich Observatory for these predictions. Once again also Dr. Taylor and the Editor hope that favorably placed members will observe these phenomena and report upon them.

A STUDY OF THE PHASE OF VENUS, 1960 - 62.

By: Klaus R. Brasch, Montreal Centre

(Revision of a paper read at the Eleventh A.L.P.O. Convention at San Diego, California, August 22-24,1963)

There has been a marked renewal of interest recently in the old, but so far unsolved, problem of the unpredictability of the phase of the planet Venus. In 1793 the astronomer Schröter noted that the observed date of dichotomy or half phase varied from the theoretical date by several days.[1] The phenomenon has since been called the Schröter Effect, and has been suspected on Mercury as well.[2] Theoretically, it is possible accurately to predict, to within a few hours, the time when exactly half of Venus (as seen from the Earth) is lit by the Sun. This aspect should occur near greatest elongation, when the angle between the Earth and the Sun is exactly 90° as seen from Venus. In fact, however, this is found almost never to be an accurate prediction; for there is usually a discrepancy of a number of days both in eastern and western elongations.[3] No satisfactory explanation for this difference has been found as yet.

In the past two evening apparitions, 1960-61 and 1962, a special study of the planet's phase was undertaken by the Montreal Centre of the R.A.S.C.. In the absence of micrometer measurements, the phase had to be measured directly from drawings. Although this method seems crude, it was found that the more experienced observers were so consistent among themselves in their portrayal of the phase that it seemed worthwhile to use these values. In a large number of observations, the average difference in estimation among three observers, G. Wedge, G. Gaherty, and K. Brasch, was between 1% and 4%. The observed values for both years were plotted on graphs, alongside the curves of the predicted phases for the respective apparitions. (See Figs. 1 and 2).

In 1960-61, from a total of 32 observations made before dichotomy it was found that the observed phase was about 5% less than the theoretical value, with a standard deviation of 2.6% in the values obtained. This would correspond to a dichotomy 8 days earlier than the predicted date of Jan. 31, 1961. The actual observed half phase at that time was indeed 7 to 10 days early, occurring between Jan. 21 and 23, 1961.[4] This result was confirmed by A.L.P.O. observations for 1960-61. There is a fair amount of scatter in the values obtained early in this apparition. This scatter can probably be attributed to a lack of experience of the observers at that time.

In 1962, on the other hand, there is a higher degree of mutual agreement in the values; and the curve is consequently smoothed out. This time, from a total of 42 observations before dichotomy, the observed phase was 6.7% less than predicted, with a standard deviation of 2.2%. This means that the actual half phase would occur approximately 14 days early. Actually it was found to be about 11 to 13 days early, occurring between Aug. 21 and 23, instead of on Sept. 1, 1962.

Several months after this analysis had been completed, results of work done along parallel lines in Britain in 1962 were published by the British Astronomical Association.[5] As a result of this happy coincidence, several facts whose reality was at first open to question became definitely established. For one thing, both groups found dichotomy to be unusually early in 1962, occurring between August 19 and 23, instead of on Sept.1, 1962. More important, however, is the fact that when the graphs of all observations are compared, it becomes obvious that the deviation of the observed phase from the predicted phase grows less as the crescent narrows. In fact, at about $k = 0.25$ to 0.30, the two values become uniform. This fact was suspected in the Montreal observations and was definitely confirmed by the British ones.

When the deviation decrease is plotted against the date during the two apparitions, it is found in 1961 to be rather steep, whereas in 1962 there is a gradual decrease over several months from a maximum value of 10%. (See Fig. 3). It seems, then, that when the phase is greatest, so also is the difference between observed and predicted phases, with a gradual decrease in the discrepancy toward minimum phase. The deviation at dichotomy is thus merely a stage in a larger process, becoming quite secondary in itself; for the important question is no longer why there is discrepancy at dichotomy, but rather why the variation exists throughout the entire apparition.

Upon further inspection of the graphs, it becomes apparent that the

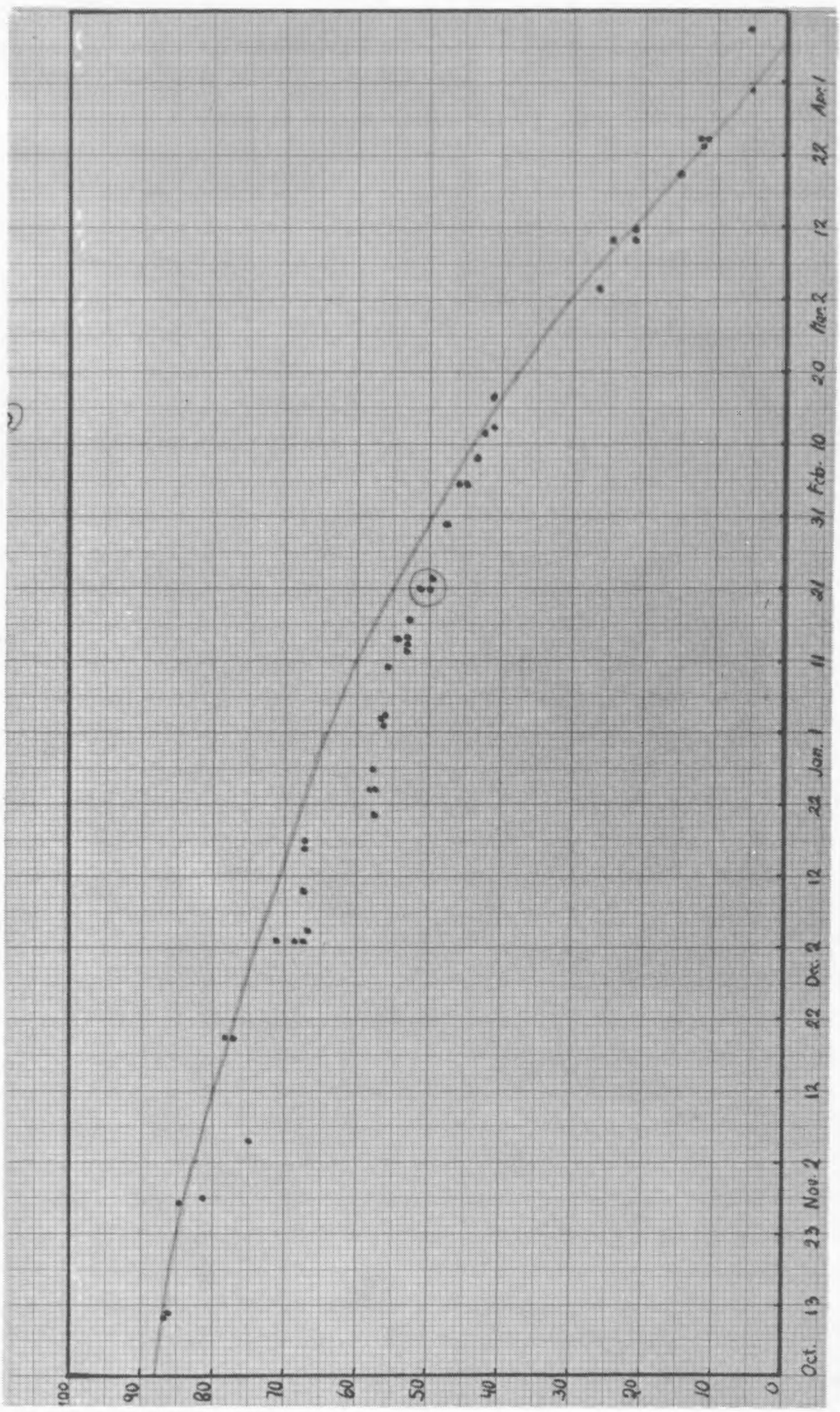


FIGURE 1. Theoretical phase of Venus (solid curve) and observed phase (plotted points) determined by Klaus Brasch and others in the Montreal Centre for the 1960-61 evening apparition. Graph contributed by Klaus Brasch. The illuminated percentage of the disc of Venus regarded as circular is plotted against date.

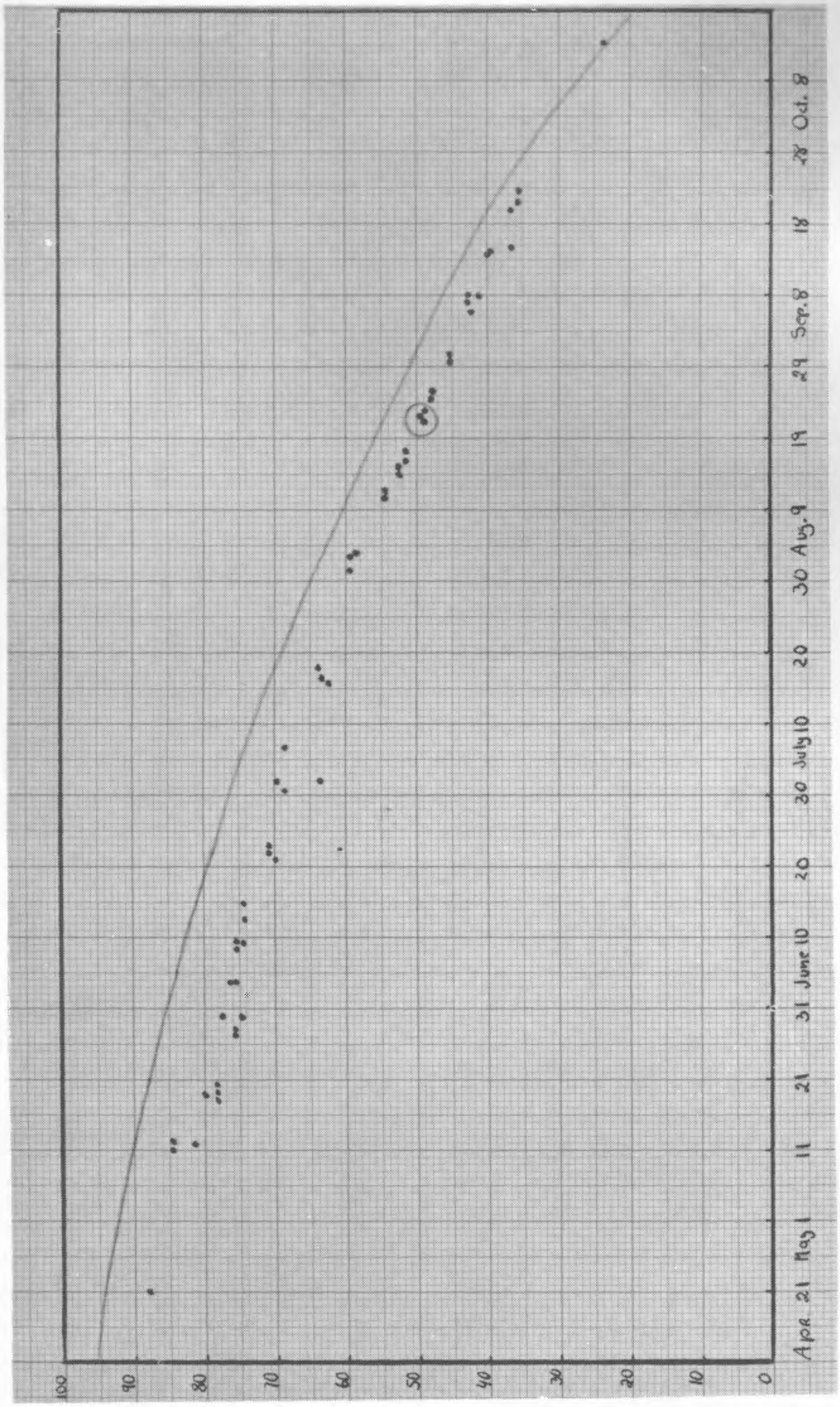
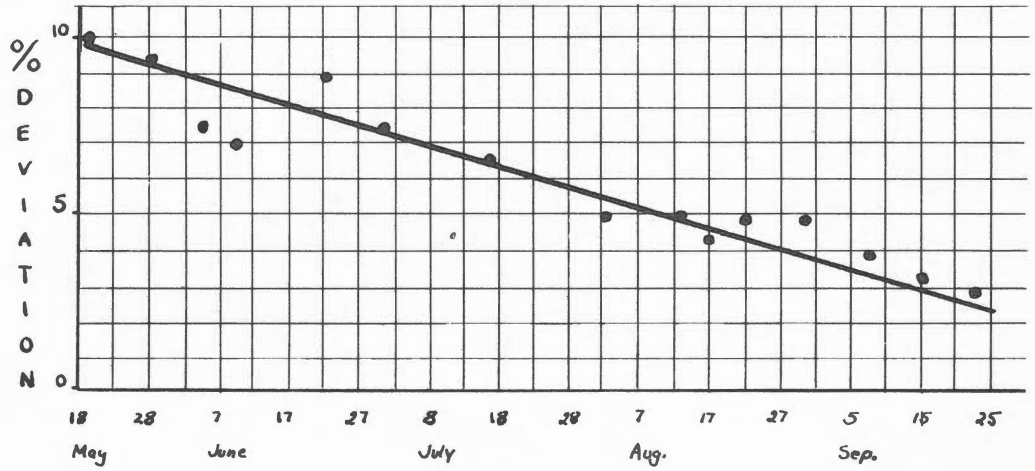


FIGURE 2. Theoretical phase of Venus (solid curve) and observed phase (plotted points) determined by Klaus Brasch and others in the Montreal Centre for the 1962 evening apparition. Graph contributed by Klaus Brasch. The illuminated percentage of the disc regarded as circular is plotted against date.

Mean Percent Deviation of Observed from Theoretical Phase

1. Based on 44 Observations of Venus of the 1962 Evening Apparition



2. Based on 30 Observations of Venus of the 1960-61 Evening Apparition

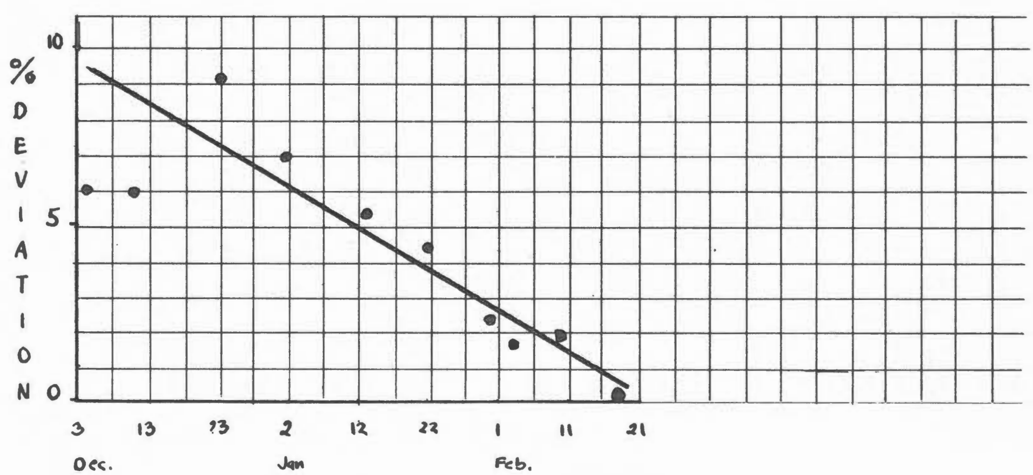


FIGURE 3. Graphs by Klaus Brasch of mean percentage deviation of observed phase from theoretical phase as a function of date during the 1960-61 and 1962 evening apparitions of Venus. See also text of his article.

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deviation before dichotomy in 1961 was less (4.8%) than in 1962 (6.7%). Dichotomy in 1961 was correspondingly only 7 to 10 days early, while in 1962 it was 11 to 13 days early. It is unfortunate that in both years there were not enough observations from morning apparitions for comparison with the above evening apparition values.

A point should be mentioned in connection with filter observations. When observed with a Wratten 47 b blue filter, the phase always appeared narrower than when observed directly. In 1962, for example, drawings made a few days before dichotomy showed the terminator straight in blue light, whereas it appeared slightly gibbous when seen directly. Experiments with filters carried out by Cruikshank appear to indicate that this effect is due to the density of the filter rather than to the transmission wavelength of the filter.[6]

Much controversy exists as to the nature of the Schröter Effect. It is obviously not due to observational error since too much evidence is found in favor of its reality as a physical phenomenon. If the difference between the predicted and observed phases were entirely due to observational error or to an optical illusion, why does it vary from one apparition to another, since an optical effect would have to be consistent and the same at all times?

It should be pointed out here that a study carried out by Hartmann of A.L.P.O. observations made over several years appears to indicate that variations of the Schröter Effect from one apparition to the next are not statistically significant.[7] Rather, there appears to exist an average discrepancy of ± 7.8 days, for both eastern and western elongations of Venus. However, in view of the large standard deviation obtained from those results (± 5.5 days), the possibility that small scale fluctuations about an overall mean of ± 7.8 days may exist should not be dismissed. This result may be indicated in Fig. 3, where in the 1962 graph (which includes all observations before and after dichotomy) the mean deviation for the apparition (about 6%) corresponds to the actual deviation some time in July of that year. In the 1960-61 graph (which is admittedly less accurate) the mean (about 4.5%) corresponds closely to dichotomy. In short, when the values for an entire apparition are averaged, the value obtained represents the mean between the maximum and minimum deviations; and since the rate of decrease from maximum to minimum appears to differ between different apparitions, an average value might correspondingly vary as well. This point can obviously only be settled through an increase in the precision of the observations, with a consequent reduction of their scatter.

Although the reality of the Schröter Effect can no longer be doubted, a satisfactory physical explanation for it has not as yet been found either, although most people probably agree that it is due, in part at least, to the atmosphere of Venus. The foregoing discussion points out rather emphatically that many more accurate phase observations are needed during both morning and evening apparitions in the hope that through them the problem can eventually be solved.

References

1. Moore, P.A., The Planet Venus, The Macmillan Co., New York, 1958, p. 81.
2. Hartmann, W.K., Venus Section Report, Str. A., Vol.16, p. 229.
3. Hartmann, W. K., Venus Section Report, Str. A., Vol.17, p. 103, Figure 8.
4. Hartmann, W. K., Venus Section Report, Str. A., Vol.16, pp. 172- 4.
5. Moore, P.A., Journal of the B.A.A., Vo. 73, p. 184.
6. Cruikshank, D.P., Str. A., Vol. 17, p. 2.
7. Hartmann, W. K., Venus Section Report, Str. A., Vol.16, p. 225.

OBSERVATIONS OF COMET PEREYRA

By: Charles F. Capen, Table Mountain Observatory

Comet Pereyra was observed at the Table Mountain Observatory on September 20, 1963 low in the east in Hydra near Alphard, 20 minutes prior

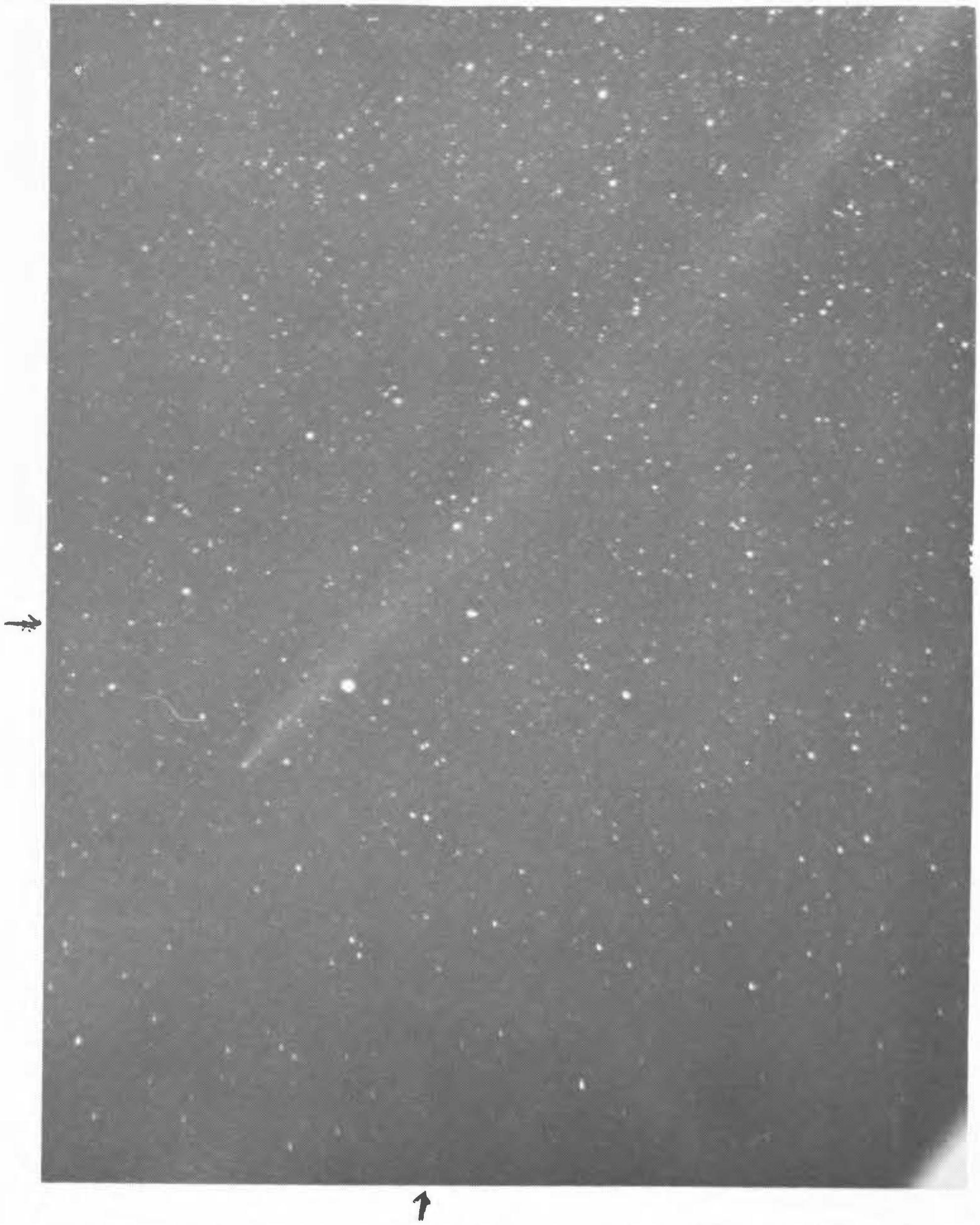

FIGURE 4. Photograph of Comet Pereyra at Table Mountain Observatory on September 23, 1963. Plus X Film. Exposure 14 mins., 35 mm., f: 1.4 camera. Other data in text of Mr. Capen's paper. Arrows point to tail jets, recorded in originals of several photographs on this date but probably difficult to see in this reproduction.

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to the coming of civil twilight, and on the south edge of the luminous Zodiacal Light. These observing conditions made the determination of Comet Pereyra's magnitude difficult indeed. The visual magnitude was approximated to be 5.5 to 5.8. The visual tail length was about 15° or 16°. An ill-defined nucleus in a small head was observed with 10 x 50 binoculars,

FIGURE 5. Photograph of Comet Pereyra at Table Mountain Observatory on September 27, 1963. Plus X Film. Exposure 33 mins. 35 mm., f:1.4 camera. Other data in text.

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a 6-inch refractor employing a 30 power ocular, and a 16-inch Cassegrain reflector using a 106 power ocular for best light grasp and definition. A crude visual position was obtained.

During the morning of September 21, photographic observation of Comet Pereyra was initiated with a Miranda 35 mm. reflex camera employing a telephoto lens of 135 mms. focal length, f/3.5 and using new improved Tri-X film.

A long straight tail of about 14° was photographed. A small fairly bright head with a very weak nucleus was recorded, and the photographic magnitude was noted to be 6.

On each following night the comet's magnitude has become fainter and the tail shorter. On September 26 the comet could barely be seen in a very transparent sky in which a 6.8 magnitude star was on the threshold of naked-eye visibility. The tail was recorded as between 10° and 11° long. A small head with a weak coma and with a possible nucleus was noted.

The following table gives Pereyra's nightly positions:

Date	Time	R.A.	Decl.	Visual Magnitude	Remarks
September 20, 1963	1215 U.T.	$9^h\ 32^m.8$	$-6°\ 35'$.	5.5-5.8	Tail length ≃ 15° - 16° straight and dusty appearance. Small head with no definite nucleus.
				Photographic Mag.	
September 21, 1963	1220 U.T.	9 32.5	-7 06 .	6	Tail length ≃ 14° - 15° from a 10^m exposure on Tri-X f/3.5. A possible weak nucleus?
September 22, 1963	1220 U.T.	9 32.2	-7 25 .	6	Tail ≃ 13° from an 11^m exposure on Tri-X at f/3.5. Weak necleus? 16-inch Cass. with 105X & 10 X 50 binocs.
September 23, 1963	1220 U.T.	9 31.9	-7 43 .	6.2	Tail 12° from a 14^m exposure on Tri-X. Definite weak necleus noted with 16-inch Cass. with 105X.
September 24, 1963	1215 U.T.	9 31.8	-8 10	6.5	Tail ≃ 11½° - 12° from a 20^m exposure on Tri-X & Plus-X employing f/3.5 and f/1.4, respectively. Weak nucleus observed.
September 25, 1963	1220 U.T.	9 31.4	-8 28	≃ 6.8	Tail length 13° photographically from a 25^m exposure on Tri-X f/3.5.
September 26, 1963	1145 U.T.	9 31.1	-8 50	≃ 7	Tail length 11°.2 from a 26^m exposure on Tri-X f/3.5. Weak nucleus.
September 27, 1963	1150 U.T.	9 30.8	-9 18	7+	Tail length 11°.75 from a 33^m exposure on Plus-X f/1.4.

SCIENTIFIC RESULTS OF A TOTAL SOLAR ECLIPSE EXPEDITION TO MOOSE RIVER, ONTARIO ON JULY 20, 1963

By: David D. Meisel, A.L.P.O. Comets Section

Abstract. A brief summary of the final results of the experiments carried out during the July 20 eclipse are given. These include a list of features observed in the solar atmosphere, photographic photometry of portions of the corona, sky illumination photometry, photographic measurements of the shadow bands, and radiowave monitoring. All observations appear to agree well with previously obtained results where such results are available.

The purpose of this report is to give the specific, final results obtained from a reduction of the data obtained by the author's private eclipse expedition to Moose River, Ontario, Canada. The expedition itself has already been described elsewhere (Str. A., Vol. 17, 120, 1963 and Sky and Telescope, Vol. 26, 188, 1963). Because of space limitations, little detail of the methods used or the interpretations of the data are included. Investigators interested in such details can write to the author.

Results Obtained from Direct Photography

Measurements of the black-and-white and color photographs and slides enabled the author to compile the following list of confirmed coronal features and chromospheric phenomena. A total of 37 slides and negatives were used.

Table 1. Confirmed Features

Feature	Position Angle Relative to the Northern Solar Rotation Axis	Extent of Feature from Apparent Solar Center
North Polar	13° E to 14° W (through 0°).	2.1 solar radii
South Polar	139 E to 138 W (through 180°)	1.8
F corona	Spherical	to at least 3.2
Points along large ray	44° E	1.0
	48	2.0
	52	3.0
Brightest Rays (non-polar)	# 1 85° E	1.5
	93 E	2.5
	# 2 71 E	1.5
	82 E	2.5
	# 3 20 W (tilted 3° west of radius)	1.4
	# 4 22 W (tilted 4° west of radius)	1.4
	# 5 57 E	2.0
	# 6 100 E	2.0
Brightest Rays (polar)	# 1 21° E	1.4
	# 2 18 E	1.4
	# 3 7 E	1.4
	# 4 2 W	1.4
	# 5 10 W	1.4
	# 6 155 E	1.6
	# 7 163 E	1.3
	# 8 169 E	1.3
	# 9 175 E	1.3
	#10 178 W	1.3
Coronal Condensations	47° E	1.2
	34 E	1.3
	84 W	1.3

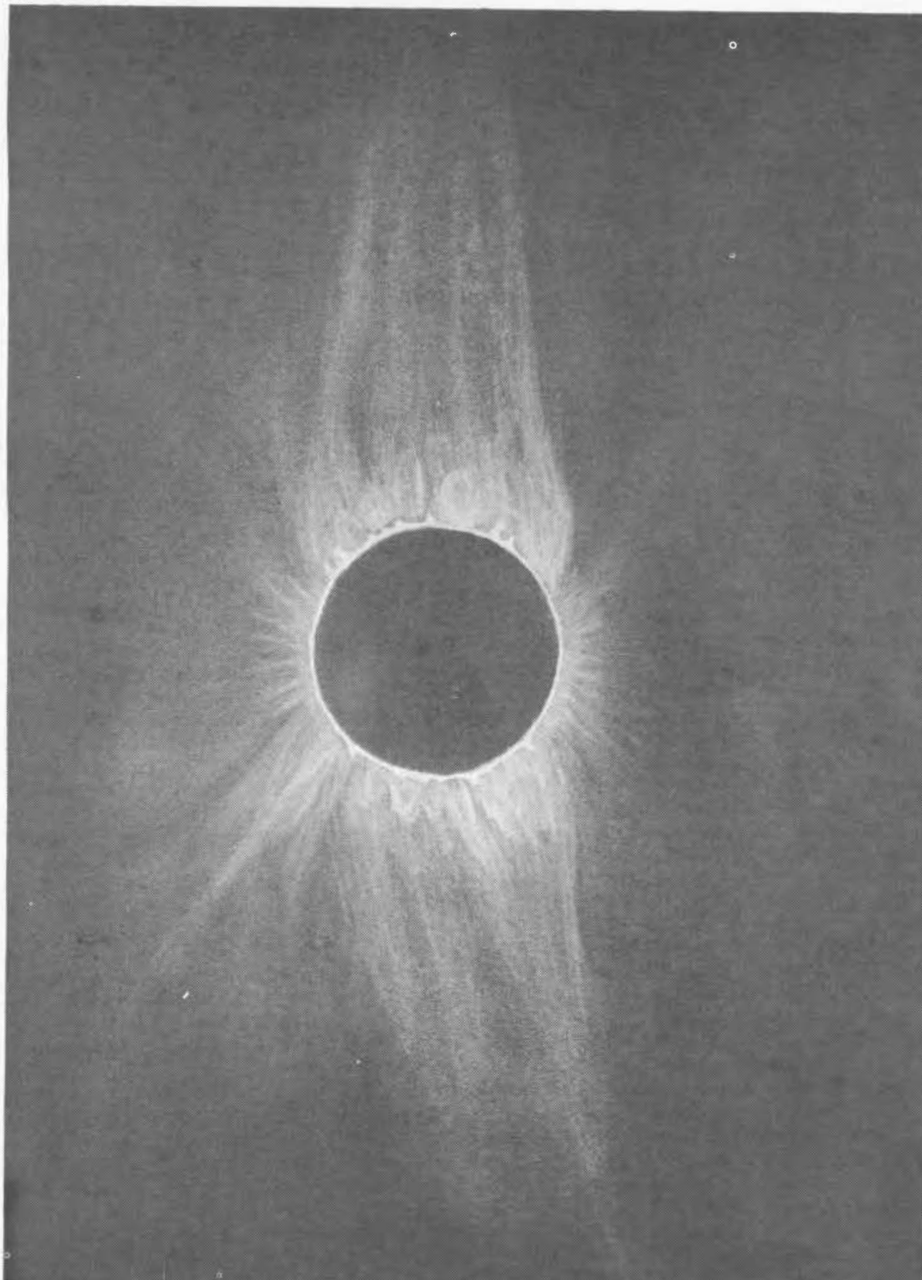

FIGURE 6. Composite sketch by David Meisel of coronal and chromospheric features during total solar eclipse of July 20, 1963. Moon made same size as photosphere for completeness of detail. Sketch based on 37 slides and photographs secured by Mr. Meisel's expedition. North at left, east at bottom. See also text.

Feature	Position Angle Relative to the Northern Solar Rotation Axis	Extent of Feature from Apparent Solar Center
Prominences	40° W	1.07
	45 W	1.08
	116 W	1.08
	122 W	1.03
	132 E	1.05
	120 E	1.03
	117 E	1.05
Eastern Coronal Extension	146° E to 97° E	3.6
Western Coronal Extension	138° W to 14° W	4.1
Coronal Arches	110° E	1.8
	89 E	1.8
	79 E	1.8
	69 E	1.8
	56 E	1.6
	34 E	1.6
	61 W	2.0
	72 W	1.9
	84 W	1.5
	85 W	1.8
	94 W	1.7
	104 W	1.4
	96 W	1.7
	119 W	1.4

These major features as well as some smaller ones are shown in the drawing published as Figure 6. The relative diameter of the moon has been reduced to the size of the solar photosphere in order to show the detail in the lower atmosphere.

The average shape of the corona was computed according to the Ludendorff ϵ criterion* for two average distances.

	ϵ	Average Distance
Inner Corona	= 0.14 ± 0.03	1.42 solar radii
Outer Corona	= 0.27 ± 0.04	2.83 " "

The corona appeared to be typical for this phase of the solar cycle. Of particular note is the very large coronal ray. On one negative this could be traced to at least 7½ solar radii from the center.

Photographic Photometry of Selected Frames

A set of four selected negatives, including a calibration partial phase exposure, was photometered using a transmission densitometer. The transmission was converted into intensity by means of a series of step wedge exposures. The solar image for the partial phase was used as the intensity standard for the other negatives. Scattering by sky light and internal reflection was evaluated empirically, and corrections were applied. Corrections for the part of the sun obscured by the limb of the moon were also applied where necessary. In the case of the coronal features the aperture position was chosen so that the area exposed would be equal to the area of the solar disk visible during the partial phase exposure. The results obtained using a 30' of arc diameter integration aperture are summarized in the table on page 185.

Interested investigators may wish to utilize the lunar disk value to obtain an estimate of the total brightness of the earth at full phase at 1 A.U. (The details of this calculation are too complicated to go into here.) A value for the albedo of the earth could also be obtained.

* See C. W. Allen, Astrophysical Quantities, p. 176, 1963.

Location of the center of the integrating disk

Position Angle	Distance from Solar Center	Intensity
135° E	1.3 solar radii	1.09×10^{-6} mean solar intensity
135° W	1.3	1.14×10^{-6}
180°	1.3	0.6×10^{-6}
0°	1.3	0.5×10^{-6}
45° E	1.3	1.08×10^{-6}
45° W	1.3	0.8×10^{-6}
Cloud Area 4° south of the sun		2.2×10^{-7}
Sky Area 4° west of the sun		1.5×10^{-7}
Sky Area 4° east of the sun		1.0×10^{-7}
Lunar Disk (with sky and area corrections)		0.84×10^{-7}
Mean of all areas of corona		0.87×10^{-6}
Mean of polar regions		0.55×10^{-6}
Mean of non-polar regions		1.03×10^{-6}

The average internal error of the determination is about 10%.

The lunar value corresponds to $m_{pg} = -8^{m}.5 \pm 0.2$.

Total Sky Illumination Photometry

The results and light curve obtained from this experiment have already been published (Sky and Telescope, Vol. 26, 119, 1963), but it would be useful to utilize the result of mean illumination of 0.04 foot candles. Assuming a reflectivity of 0.8 for the white sphere an average sky surface brightness of $(1.3 \pm 0.1) \times 10^{-11}$ mean solar surface is obtained. This is a residual intensity about 300 times greater than the night sky mean brightness for the observing station latitude for no aurora conditions. This shows that the skylight in the photographs was due almost entirely to internal scattering.

Shadow Band Photographs

Members of our expedition succeeded in obtaining what are believed to be the first motion pictures of the elusive shadow band phenomenon. The bands are evident under close inspection on several hundred of the movie frames. Except for changes in intensity no systematic difference could be found between the bands before and after totality. Reduction of the best frames gives the following values:

Time of maximum intensity of bands: $21^{h} 25^{m} 50^{s}$ U.T., July 20, 1963.
Direction of travel (projected on ground from angles observed on screen directed toward the sun):
from 320° (NW) to 140° (SE), ($\pm 1^{\circ}$).
Speed of Travel along ground: 33.4 inches per second (±0.2 ins/sec).
Band width: 2.59 inches (±0.08 ins.). "Wavelength": 5.35 inches (±0.04 ins.).
Wind Direction (from cloud motion): NNW to NW, going to SSE to SE.
Wind Speed: 8 to 10 knots with gusts up to 20 knots.

The presence of the bands on film was confirmed by densitometry of the negatives. The band intensity was found to be about 3% down from the background intensity of the screen.

Radio Reception Results

Shortwave signals from the Canadian time station CHU at 7.33 mc were tape recorded for an interval running through totality. The receiver was a Heathkit Mohican Portable equipped with ground and 20' long horizontal antenna oriented directly north and south and situated at a height of 8' from the ground. It was found that the receiver had a temperature coefficient which affected the initial set of readings. The readings were

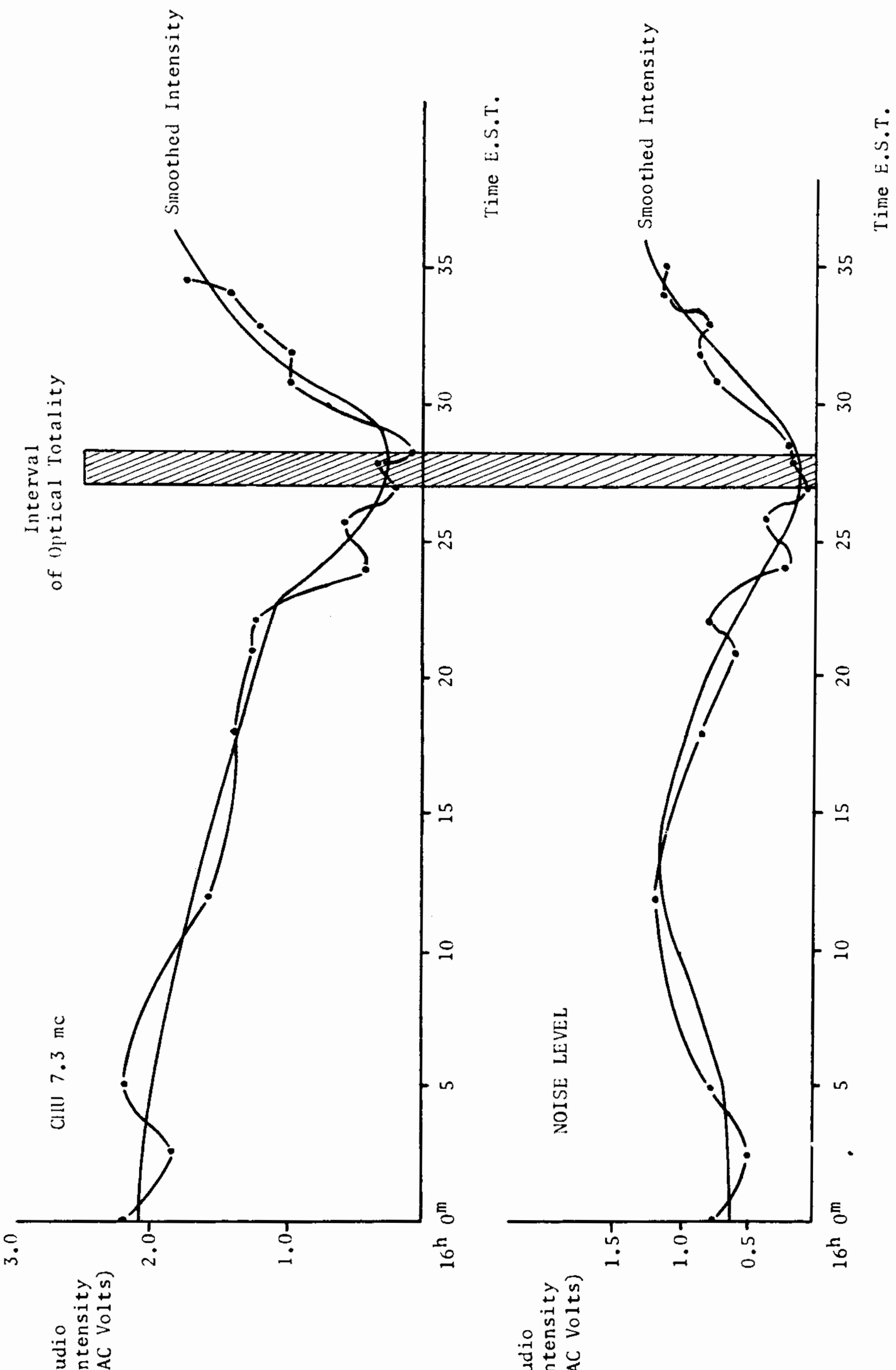

FIGURE 7. Intensity of radio shortwave signals near totality during solar eclipse of July 20, 1963. Add 5 hours to E.S.T. to convert to U.T. Graphs constructed by David Meisel. See also text of his article in this issue. The curves here plotted are not corrected for the effect of temperature upon the receiver. Prepared for publication by Ray Montes.

taken with the automatic volume control off, and the initial adjustment was for maximum gain. The uncorrected radio curve is shown in Figure 7. Correction for temperature results in a nearly linear curve until $21^h\ 29^m_.5$ U. T., when a rapid increase is noted. If the linear portion is extrapolated forward in time, a no-signal condition is obtained at $21^h\ 38^m$ U.T. Subtraction of the linear fit gives the non-linear component, assuming the two to be independent effects. Assuming also that signal strength is proportional to ion density for the linear curve, the following results are obtained:

Linear Trend:

Mean height of layer in shadow: 220 kms. ± 10 kms.
Recombination coefficient: $\alpha = 6.1 \times 10^{-10}\ cm^3\ sec^{-1}$
Critical Frequency at $21^h\ 38^m$ U.T. = 7.33 mc

For the non-linear trend the increase is interpreted as a decrease of wave absorption. Hence no estimate can be made of the ion density. The following data can be secured:

Non-linear Trend

Mean height of the layer in shadow: 80 kms. ± 5 kms.
Time constant $1/N\alpha = 3.2 \times 10^2$

The latter data correspond to the D layer. Assuming $N = 6 \times 10^2$ as the ion density for the D layer (C. W. Allen, Astrophysical Quantities, p.133, 1963), the recombination coefficient can be found: $\alpha = 5.1 \times 10^{-6}\ cm^3/sec$.

Conclusions

It can be seen from the above that definite results can be obtained from the work of inexperienced observers under the direction of a professional scientist. The experiments were rehearsed well in advance; and although the equipment was crude in many cases, several firsts were achieved. In most cases, however, no useful estimate of the error can be made. The values quoted will be useful for planning future investigations, but no claim is made for accuracy beyond the next to the last significant figure quoted if no error is given.

The author would again like to thank the many people, too numerous to mention, who have contributed in some way to this effort. Although complete success of all experiments planned was not obtained, the amount of data that was recovered is considered significant. The author would welcome inquiries from other investigators.

Orientation of Meisel Expedition Eclipse Photographs Published in May-June, 1963 Issue. This kind of information is important for the scientific interpretation of any total solar eclipse photograph. The Editor has tried to use an eclipse print with marked directions supplied by Mr. Meisel to determine the orientation for the three eclipse photographs in our May-June, 1963 issue. Conclusions are: on the front cover north is to the right (east at the top), on Figure 17 on pg.122 north is at the top, and on Figure 18 on pg. 122 north is toward the upper right.

Some photographic prints of the total eclipse sent by Mr. Meisel in a fairly recent letter constitute instructive examples of the range of details revealed by "dodging". Two prints made from the same negative as the May-June, 1963 issue front cover photograph vary greatly among themselves in the shape, extent, intensity, and structure of the corona. One does well to be aware of how much printing techniques can do.

TEMPERATURE STRUCTURE OF THE ATMOSPHERE OF MARS

By: George W. Rippen, Department of Meteorology,
University of Wisconsin

(Paper read at the Eleventh A.L.P.O. Convention
at San Diego, Calif., August 22-24, 1963.)

Abstract. This paper considers the methods and problems involved in determining the temperature structure of the atmosphere of the planet Mars. Use is made of the global radiation budget in conjunction with other radiation laws to describe the vertical temperature structure of the atmosphere. The possible existence of a tropopause and its significance is also discussed.

List of Symbols

1. T = temperature.
2. C = Centigrade temperature scale, in degrees.
3. K = absolute temperature scale, in degrees.
4. p = pressure (mb a unit of pressure: 1013mb = 29.92 inches of mercury).
5. H = heat.
6. Z = height.
7. g = gravity.
8. C_p, C_v = gas constants.
9. a = specific volume.
10. km = kilometers, 10^3 meters (m), 10^5centimeters (cm).
11. "∂" = mathematical notation -- differential operator.

If meteorological theory were sufficiently advanced and if we had a knowledge of certain planetary parameters, it would be possible to derive from basic principles the temperature and circulation characteristics of any planetary atmosphere. Unfortunately, meteorological theory is not sufficiently advanced; and the required planetary parameters, especially in the case of Venus, are not completely available. At the present time, all that we can do is to make inferences based upon available theory and observational data, parameterizing our schemes to cover uncertainties in our data. Of the two planets that might be considered for discussion -- Mars and Venus -- Mars is more favorable. Even with the results of the recent U.S. deep space probe to Venus, so much more is known about Mars.

The mean planetary surface temperature for an atmosphere that is transparent to long wave radiation -- IR radiation -- can be computed by equating the solar energy received to the energy emitted by the planet's surface. The upward flux of long-wave radiation must balance the incoming flux of solar radiation. This relation can be written as follows:

$$I_o(1-A)=W,$$

where I_o equals the average influx of radiation at the top of the atmosphere, W equals the total back flux of long-wave radiation at the top of the atmosphere, and A equals the planetary albedo. This W is composed of radiation emanating from the surface and the atmosphere. Assuming certain values for these parameters, one obtains a mean surface temperature of 219K. In this computation the Martian atmosphere is assumed to be completely transparent to long-wave radiation; however, this is not completely true. The semi-opacity of the Martian atmosphere causes a "greenhouse effect" that raises the average surface temperature above 219K. The 219K level may be taken to be the minimum possible value for the mean surface temperature. Realistic estimates of the maximum possible mean surface temperature may be obtained by constructing a model atmosphere which maximizes the greenhouse effect. The magnitude of the greenhouse effect is dependent upon both the vertical temperature distribution and the amount of absorbing gases. To compute the maximum possible mean surface temperature, we must make some reasonable guesses about the maximum possible amounts of CO_2, water vapor, and O_3[1] present in the Martian atmosphere. We must also make some guesses about the slope of a linear temperature profile that would maximize the greenhouse effect.

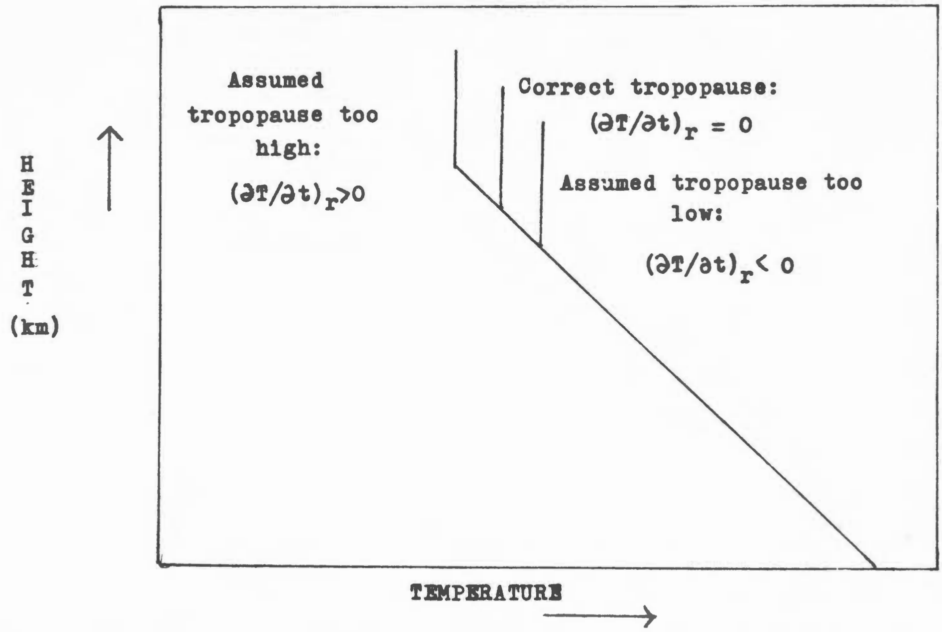

FIGURE 8. Schemetic model of the technique for computing the height and temperature of the tropopause. The partial derivative $(\partial T/\partial t)_r$ is the computed radiational heating or cooling rate. Figures 8 - 14 were prepared and contributed by Mr. George W. Rippen. They can be studied in connection with his paper in this issue.

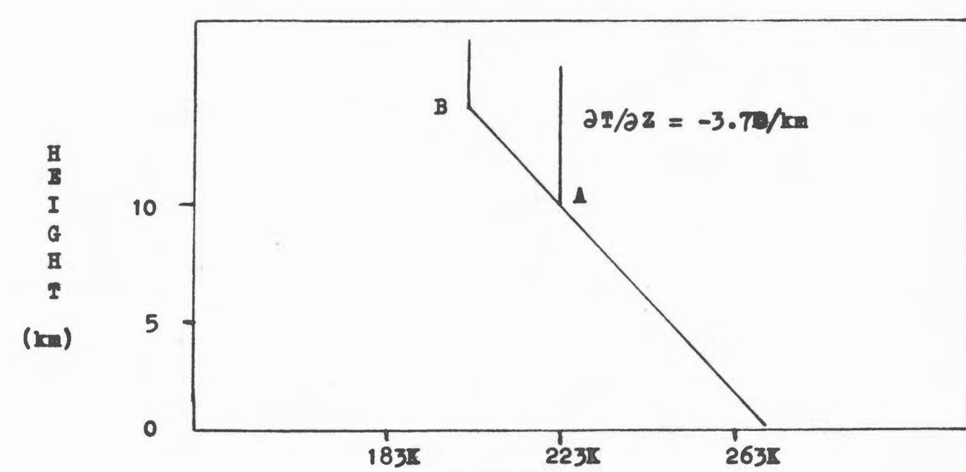

FIGURE 9. The height and temperature of the Martian tropopause for two hypothetical cases: (A) carbon dioxide (2 percent by volume); (B) water vapor only (10^{-2} cms. of precipitable water).

Computations of the maximum possible mean surface temperature can be made on the basis that outgoing long-wave radiation and incoming solar radiation must balance. Then 233K is obtained as the maximum possible mean surface temperature from these calculations. The mean Martian surface temperature lies, therefore, somewhere between 219K and 233K, indicating a small greenhouse effect as compared to the earth.

Theoretical mean surface temperature estimates can be compared to thermal emission observations. Analysis of these observations has yielded mean noontime latitudinal temperature profiles for the different seasons. It is possible to obtain an estimate of the mean surface temperature from these data by extrapolating poleward, averaging seasonal data into an annual curve, correcting the annual curves for the amplitude of the diurnal temperature variation, and computing an area weighted mean temperature from the new annual average curve.[2] Working through this method, we arrive at a mean surface temperature of 233K, which is in good agreement with the theoretical estimates. Analysis of these data also indicates a mean equatorial temperature of 240K and a mean polar temperature of about 205K.

The mean variation of temperature with height in the atmosphere of Mars is controlled, for the most part, by radiative and convective processes. We can compute the mean temperature profile, using a simple formulation in which convection is assumed to extend to a height above which the radiative equilibrium temperature lapse rate is just stable.[3]

IR cooling rates can be determined from the vertical divergence of the net flux of radiation.[4] These rates are applied to the initial temperature distribution for a certain time step, resulting in a new temperature-height relationship. This process is repeated several times until the resulting radiational computed rates are very small -- < 0.1C per day. If the computed lapse rate is greater than the adiabatic lapse rate, it is adjusted downward. The necessary adjustments are, for the most part, limited to the lower portions of the atmosphere -- the lower troposphere. As a computational check, a similar set of iterations can be performed with an initial adiabatic temperature distribution.

The final temperature distribution obtained is characterized by an adiabatic troposphere and a stratosphere in which the lapse rate is slightly less than adiabatic. The computed curve can be thought of as an estimate of the variation of temperature in the Martian atmosphere. The possible presence of O_3 and water vapor must be ascertained before this curve can be adopted with any degree of certainty. Ozone's presence would especially affect the stratospheric temperature distribution through its direct absorption of solar radiation.

Of considerable practical and theoretical importance is the possible presence of a tropopause on Mars. The tropopause would separate the atmosphere into two different layers -- a troposphere and a stratosphere.[5] The tropospheric lapse rate would be nearly adiabatic, while the stratospheric lapse rate would be nearly stable. For example, in the earth's atmosphere, the mean tropospheric lapse rate is -6.5°C/km., and in the stratosphere it is zero to slightly positive. Being colder and higher at the equator and warmer and lower at the poles, the earth's tropopause varies with latitude. These latitudinal height variations and even the reason for the existence of a tropopause are not completely understood. While the basic difference between the troposphere and the stratosphere is their vertical thermal structure, there are other meteorologically important differences. Practically all weather occurs in the troposphere. Turbulence is a main feature of this layer. The Martian tropopause, if it exists, probably would act as a similar dividing line.

There are several theoretical height estimates of the Martian tropopause available. One theoretical computation is based on a model atmosphere characterized by tropospheric convective equilibrium and stratospheric radiative equilibrium. The computed height is 8.5 kms. The temperature

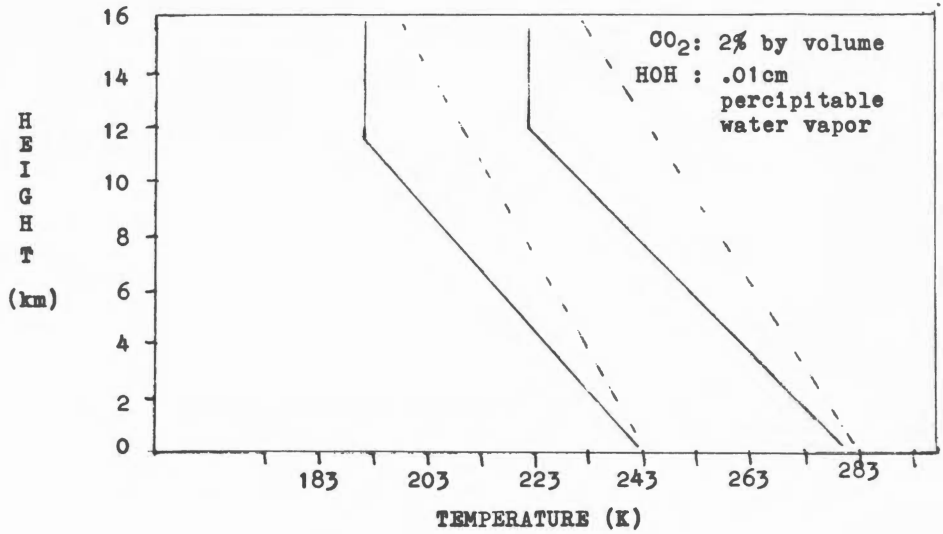

FIGURE 10. The height and temperature of the tropopause of Mars for various surface temperatures and a tropospheric lapse rate of - 3.7 C/km. (solid lines) and also - 2.5 C/km. (dashed lines). See also text of Mr. Rippen's paper.

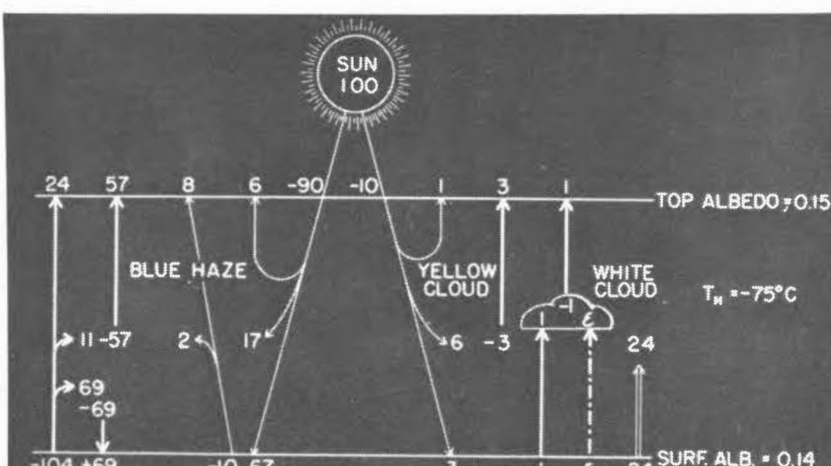

FIGURE 11.

at the tropopause in this model is 196K. Another model leads to a tropopause height of 45 kms. and temperature of 113K. It is not clear which of these models is more realistic, and we shall not know until actual Martian soundings become available. It is useful to make additional estimates founded upon different formulations in the hope that some of them may approach a common point.

The tropopause occurs at that level where the rate of radiational temperature exchange is zero. An isothermal distribution of stratospheric temperatures could be the case provided ozone absorption in the upper layer is sufficient to raise temperatures above those previously calculated. With these assumptions, radiational cooling rates are computed for various height estimates of the tropopause. If the estimate is too high, the radiational temperature change will come out positive. If the estimate is too low, the radiational rate of temperature change will come out negative. After several trials, a height at which the radiational rate of temperature change will be zero is determined (Fig.8).

Since radiational rates of temperature variation depend upon atmospheric parameters -- temperature structure and composition, a model atmosphere is required. The pressure at the surface is taken as 85mb (compares with 1013mb for the earth). In most of our computations, water vapor and carbon dioxide are assumed to be uniformly distributed with height. Computations can be made varying the CO_2 and water vapor concentrations. By our choice of a surface temperature and mean lapse rate, the temperature distribution remains fixed throughout our computations.[6] The mean lapse rate of the Martian atmosphere has never been measured. Because of the atmosphere's high solar radiation transparency, probable lack of processes involving condensation, and tenuous nature as an IR absorber, the Martian lapse rate is probably close to the adiabatic lapse rate of -3.7C/km. In the sense that the actual lapse rate can be no greater, this value is a limiting lapse rate. The actual lapse rate is smaller during the night and in polar regions. The minimum mean lapse rate may be estimated at -2.5C/km.[7]

Tropopause conditions for the case of carbon dioxide acting alone and for the case of water vapor acting alone show some interesting effects. The tropopause of an atmosphere containing both CO_2 and water vapor will fall somewhere between the heights shown in Fig. 9. This is seen to be the case in Fig.10 where tropopause conditions for such compositions are illustrated as functions of various surface temperatures and lapse rates. The[8] tropopause height decreases somewhat with decreasing surface temperature. Most interesting is the result that the tropopause temperature and the surface temperature decrease simultaneously. If lower temperatures are assumed to apply to polar regions and higher temperatures to equatorial regions, the temperature at the tropopause will decrease with increasing latitude. This effect is the opposite of what happens in our atmosphere.

Calculations based upon various radiative models of the atmosphere suggest a height for the tropopause of something on the order of 10kms. Other models based upon other schemes of reasoning suggest heights ranging from between 4kms. and 45kms. If such a phenomenon as a tropopause exists on Mars, its height can only be determined by direct observation.

Planetary atmospheric circulation is thermally driven, with solar radiation being the primary heat source. While local distributions of heat sources are important, experience with the earth's atmosphere has shown that latitudinal variation of the difference between incoming and outgoing long wave radiation is the principal driving force. The problem of the general circulation of a planetary atmosphere is the atmospheric response to this thermal gradient, subject to strong rotational constraint.

By convention the general circulation on earth is divided into three scales.[9] Water vapor is of primary importance on the meso scale. While latent heat contributions are important to the heat budget and thus also to the long-term evolution of circulation, the hypothetical treatment of macro scale motions in the dry atmosphere has been successful in short-term predictions on the earth. A simplification of theoretical studies by a dry atmosphere assumption will hopefully find their counterpart on Mars. The similarity in rotation rate and in the seasonal cycle of insolation offers a set of conditions whose resemblance to earth conditions is exciting. The major difference between the two atmospheres which might affect Martian circulation is the extreme diurnal variation of temperatures on Mars.

Included in any study of Martian circulation is the possible importance of the extreme diurnal variation of temperature. The possible existence of an extensive boundary layer in which longitudinal temperature gradients exist raises many questions about the types of circulation patterns that might form.

While we have estimates on the mean surface temperature, latitudinal variation of mean surface temperature, and vertical mean temperature distribution, considerable work must be done before a suitable temperature climatology of Mars can be worked out. As more temperature and composition data become available, the present estimates must be refined. These refinements must be made also when meteorological theory improves -- as it probably will someday! Many aspects of the temperature field are still unknown. Included in these are latitudinal and seasonal variations, diurnal variations at non-equatorial latitudes, longitudinal variations, and interdiurnal variations.

Of major importance to the general Martian circulation is a knowledge of the radiation budget. To determine the radiation budget, we must have sufficient information about the seasonal, latitudinal, and vertical temperature distribution. Some knowledge of the absorbing gases, cloudiness, and seasonal and latitudinal variation of the planetary albedo is also required. Presently the radiation budget is poorly defined except for some questionable estimations.

The Martian general circulation patterns are only crudely known. Of interest is the large apparent ratio of the amplitude of the diurnal temperature variation to the amplitude of the latitudinal temperature gradient ($\vec{\nabla} T$), and the height to which the diurnal tempeature variation can propagate. What might the effect of this be on Martian dynamics? Careful tailoring and extension of meteorological theory and the results of experiments with rotating fluids along with an analysis of Martian cloud observations are required before we can extend current dynamic theory.

Many problems remain unsolved. Some of these pertain to surface phenomena and some to atmospheric phenomena. Included in these problems are such things as the cloud types, blue haze, and polar caps. These shall be covered in a later paper. When new observations become available, they should be integrated into current atmospheric models. Hopefully those observations will clear up many of the uncertainties currently lingering on.

References

1. Assumed amounts:

CO_2	4% by volume.
water vapor	10^{-2}cms. precipitable H_2O
O_3	0.15 STP.

The gases are assumed to have constant mixing ratios with height. The adiabatic lapse rate maximizes the mean surface temperature, and for Mars this is -3.7C/km.

2. Assumed to be 30C at the equator and to decrease with latitude according to the cosine law.

3. The model atmosphere used here has the following characteristics:

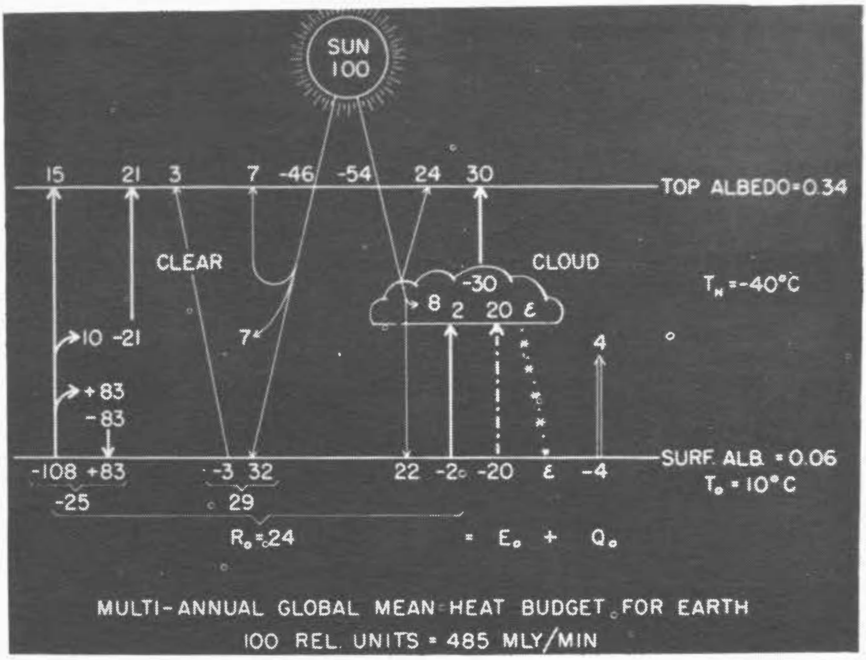

FIGURE 12.

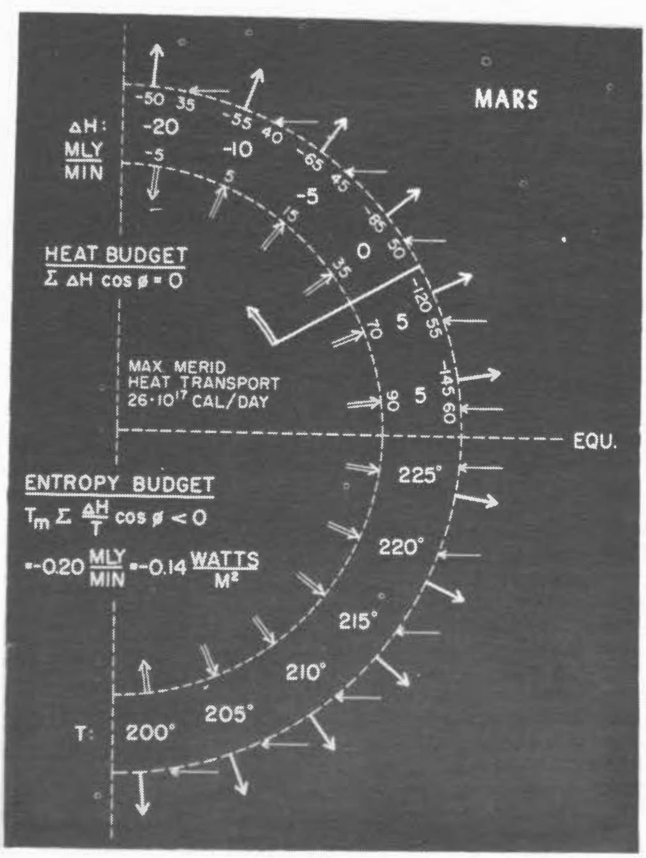

FIGURE 13.

a) mean surface temperature -- about 230K.
b) no absorption of solar radiation.
c) CO_2 is the only important radiating gas (2% by volume).

4. This is computed from radiation tables developed in 1960 for the 15 micron CO_2 band.

5. The troposphere is a convective layer, and the stratosphere is a stable layer.

6. Surface temperatures ranging from 243K to 283K are used.

7. On earth the mean lapse rate is -6.5C/km and is about 65% of the adiabatic lapse rate. On Mars the mean lapse rate should be closer to the adiabatic value.

8. A variation of plus or minus fifty percent in CO_2 content has no effect on the tropopause temperature. Lesser water vapor content yields a slight increase in tropopause temperature.

9. The scale in question is as follows: micro, meso, and macro. They increase or decrease in scope of coverage depending on how one looks at it.

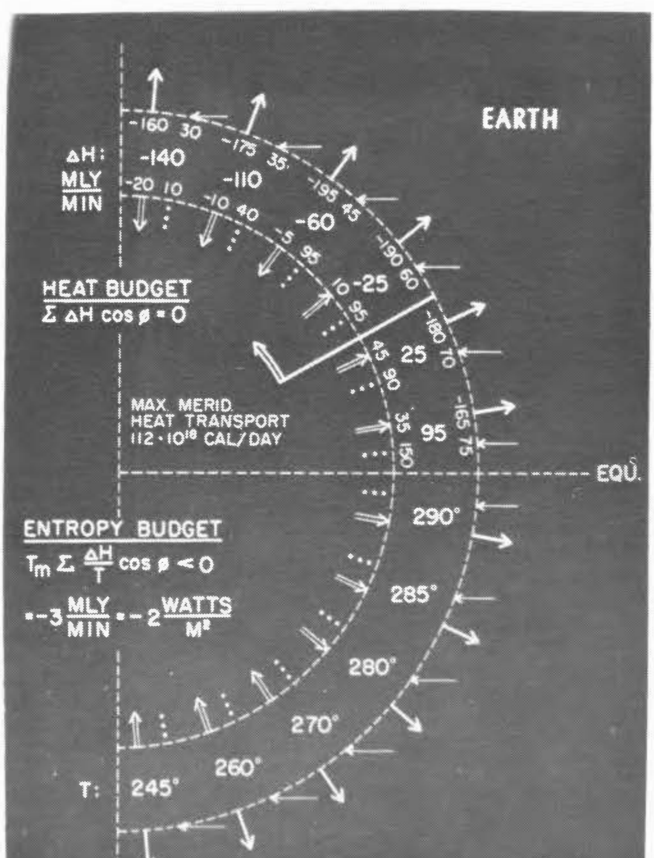

FIGURE 14.

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Appendix

1. Long-wave radiation:

Long-wave radiation and IR are frequently interchanged. IR in general refers to infra-red radiation which is the long-wave radiation of most interest.

2. Insolation:

Insolation is the rate of delivery of all direct solar energy per unit of horizontal surface.

3. Iteration:

An iteration process is a repetitive process.

4. Adiabatic process and adiabatic lapse rate:

Let us consider a system -- a given volume or box -- located in some given environment. In an adiabatic process there is no heat exchange between the system or box and the environment. With this in mind consider the following form of the First Law of Thermodynamics:

$$dH = C_v dT + pda,$$

which can also be written as the following:

$$dH = C_p dT - adp.$$

With our definition of an adiabatic process in mind we see that dH = 0, and thus the following is true:

$C_p dT - adp = 0$; and thus if we consider the hydrostatic equation ($dp = -gdZ/a$) and make the correct set of substitutions, the following results:

$\frac{dT}{dZ} = -\frac{g}{C_p} = -9.8C/km.$ This is the so called adiabatic lapse rate. This value is for the case of our own atmosphere.

5. Divergence:

The "divergence" of any vector is generally defined by the dot product of the del operator and the vector.

6. Heat budget of an atmosphere:

The sun is the primary source of energy that drives the atmospheric heat engine. Over long time periods short— and long-wave heat fluxes are about equal. Over shorter periods of time, however, there may be a definite unbalance in the two. If this unbalance were allowed to persist for any length of time, a state in which the polar temperatures would fall and the equatorial temperatures -- mean temperatures-- would rise would exist. This is contrary to observation though. A transport of the excess energy from equator to pole must arise in order for this radiation unbalance to be redistributed. The conditions of equilibrium require a circulation in the atmosphere that will transport enough heat to all regions of the planet. A quantitative evaluation of the radiation balance and an estimation of heat transport is difficult to make in the case of Mars. One must consider the nature of the atmosphere as an absorber. Problems also arise with regard to the nature of the clouds as absorbers and reflectors. Different types of clouds will give different values of reflectivity and absorptivity. Nevertheless, attempts have been made; and one such attempt is shown in this paper.

ON THE LATITUDE ABNORMALITY OF THE SOUTH EQUATORIAL BELT NORTH ON JUPITER IN 1961

By: Takeshi Sato, Director Jupiter Section Oriental Astronomical Assoc.

(Paper read at the Eleventh A.L.P.O. Convention at San Diego, Calif., August 22 - 24, 1963)

Abstract

The conspicuous dark belt observed in 1961 near the equator of Jupiter is here considered to be the SEB_n. The writer presents a number of reasons for this conclusion.

Observations

In 1961 the north component of the South Equatorial Belt on Jupiter was abnormally displaced northward, and hence much confusion arose among students of the Giant Planet about the nomenclature of the surface markings.

The writer has measured five photographs taken at the Rakurakuen Planetarium Observatory (10-inch reflector, the writer and K. Yamamoto), Hiroshima and two photographs taken at the Sapporo City Observatory (8-inch refractor and an orange filter, K. Hayashi), Hokkaido; and from them the writer has determined the mean latitudes of the north and south edges of the belt in the interval from July 12 to October 23, 1961, inclusive as being + 1°.9 and - 5°.8 respectively[2]. Though the measurements of the north edge cannot be accepted strictly because some of them may or may not have been affected by the neighboring Equatorial Band or by southern parts of festoons, even the south edge was farther north than the normal latitude of the center of the SEB_n; and it is certainly the most northerly recorded latitude of the SEB_n, according to the data available to the writer.[3]

The beginning of the northerly displacement of the belt was not observed by the Oriental Astronomical Association observers, and hence it presumably occurred during the conjunction of the planet with the sun early in 1961. The belt was almost always very far north during the 1961 apparition; but in the middle of November of that year a number of the O.A.A. observers, namely, M. Joya (6-inch reflector), I. Hirabayashi (4-inch reflector), both of Tokyo, and K. Yamamoto (10-inch reflector), of Hiroshima, suspected that the belt was beginning to move south toward the normal position of the SEB_n. In the early months of the next apparition, that of 1962-3, the belt was still unusually placed though definitely farther south than in the preceding apparition. However, observers of the O.A.A. continued to record the southerly motion of the belt; it finally became normal in latitude in or near August, 1962.

On the Nomenclature Controversy

P. R. Glaser and E. J. Reese have considered that the belt which the writer called "SEB_n" in the preceding section was not actually the SEB_n but rather a new belt which developed in the southern parts of the Equatorial Zone, and which they call the "EZ_s Belt".[5,6,7] However, the O.A.A. observations of the latitudinal motion of the belt very clearly show that the belt was the true SEB_n. Glaser and Reese pointed out that the rotation periods of the spots on the south edge of the belt in question are too short for spots on the south edge of the SEB_n. However, their evidence is only indirect; on the other hand, the O.A.A.'s evidence is direct and hence much stronger. The importance of the fact which Glaser and Reese pointed out might be that, unless some additional way to interpret the data is found, it weakens the theory[8] that a belt on Jupiter is attributable to turbulence at the boundary of two different atmospheric currents of different rotation periods. In other words, when the SEB_n was displaced, the belt was <u>not</u> located at the boundary of the ordinarily adjacent currents.

W. E. Fox and B. M. Peek have suggested another interpretation of the belt; i.e. the belt was the remarkably developed Equatorial Band.[9,10] However, the motion of the belt as mentioned above also clearly shows that it was not the EB; and moreover the EB was observed by Reese, C. R. Chapman, the writer, and others quite separately from the belt under discussion.

In 1961 Glaser, Reese, and others in the A.L.P.O. and Yamamoto, I. Ikeya (Shizuoka Pref., 4-inch and 6-inch reflectors) Hirabayashi, and the writer (6-inch and 10-inch reflectors) in the O.A.A. observed a very faint belt near the normal position of the SEB_n. Glaser and Reese considered it to be the remarkably faded SEB_n. The writer rather thinks that it was a central component of the SEB and might be called either "SEB_c" or "SEB Z Belt". Past observations show that it is not too rare that one or two faint belts have been observed between the SEB_n and the SEB_s.[11] Another interpretation, though probably much less acceptable, might be that the faint belt was a subcomponent of the SEB_n, which is normally hidden below or above the usually visible SEB_n, and that this faint belt did not participate in the displacement of the main body of the SEB_n. For this interpretation, however, a much more detailed analysis of observations during the first half of the 1962-3 apparition is very definitely required.

Conclusion

What is mentioned above is the writer's personal opinion with which others may disagree. The writer would like to have the pleasure of discussing this problem with any interested person. His address is Rakurakuen Planetarium, Itsukaichi, Hiroshima, Japan. In conclusion the writer wishes to express his most hearty thanks to each and every member of the O.A.A. Jupiter Section, though naturally errors or misinterpretations anywhere in this paper must be attributed to the writer.

References and Notes

1. This paper is based largely upon the writer's paper published in <u>The Heavens</u>, Vol. 43, No. 447 (1962 Aug.), pp. 223-227.

2. The measurements were made, with the aid of a scale, on enlarged positive prints on which Jupiter's polar diameter varied from 2.3 to 4.1 cms.

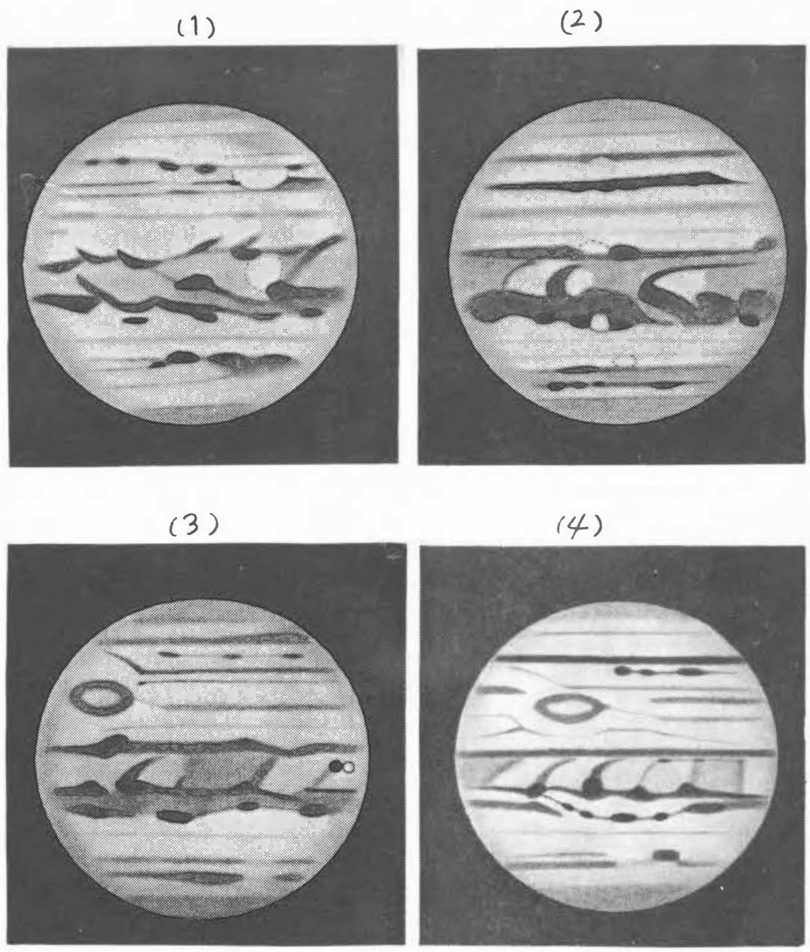

FIGURE 15. Selected drawings of Jupiter in 1961 by Takeshi Sato. Seeing (S) on a scale of 0 to 10, with 10 best, and transparency (T) on old scale of 0 to 5, with 5 best.

(1). July 7, 1961. $14^h 45^m$, U.T. 10-inch refl. 278X. C.M.$_1$ = 180°. C.M.$_2$ = 130°. S = 6. T = 4. Note that SEB$_n$ is not parallel to equator of Jupiter.

(2). July 8, 1961. $14^h 20^m$, U.T. 10-inch refl. 278X. C.M.$_1$ = 323°. C.M.$_2$ = 266°. S = 6 - 8. T = 2 - 4.

(3). July 21, 1961. $14^h 0^m$, U.T. 6-inch refl. 188X. C.M.$_1$ = 205°. C.M.$_2$ = 49°. S = 8. T = 1 - 4 (very variable). Note Jupiter I and its shadow beside each other near right limb.

(4). August 31, 1961. $12^h 4^m$, U.T. 10-inch refl. 278X. C.M.$_1$ = 132°. C.M.$_2$ = 23°. S = 6. T = 3.5 - 4.

3. Latitudes of various belts on Jupiter from 1908-09 to 1947, inclusive, are tabulated in B. M. Peek's The Planet Jupiter, London (1958), p. 63. Latitudes for other apparitions can be found in such publications as The Strolling Astronomer, the B.A.A. Memoirs, and The Heavens.

4. More recently (personal communication, May 5, 1963), Hirabayashi reported that his measurements of drawings show that the SEB_n reached its maximum northerly latitude in the last half of July, 1961 and that thereafter it rather rapidly moved southward until the end of August. In this connection, it is interesting to note (Figure 15) in the writer's drawing on July 7 at $14^h\ 45^m$, U.T. that the belt was then not parallel to the equator of Jupiter; it probably supports the idea that northerly motion was still in progress at that time. After the beginning of September the belt was almost stationary, with probable maximum northern latitudes in the last weeks of both October and November, after which time the belt again began to move southward, according to Hirabayashi's measurements. Of course, great credence cannot be placed in all of these results, as Hirabayashi said himself; but they are little different from the results given in this paper.

Hirabayashi also suggested that the NEB, and probably the STB also, appeared to move with the SEB_n synchronously but in a much less degree. It is a very interesting and, if confirmed, a very important result. But skepticism must arise. Did the abnormal position of the SEB_n lead observers to place the other belts erroneously on drawings? Confirmation using measurements made on photographs or with a filar micrometer is most desirable.

5. P. R. Glaser, The Strolling Astronomer, Vol. 16, Nos. 3 - 4 (1962 Mar.-Apr.), pp. 91-92.

6. E. J. Reese, The Strolling Astronomer, Vol. 16, Nos. 9-10 (1962 Sept.-Oct.), pp. 193-196.

7. C. R. Chapman, The Strolling Astronomer, Vol. 15, Nos. 11-12 (1961 Nov.-Dec.), pp. 214-216. (Chapman's own opinion is that the belt was the SEB_n, but Reese's opinion is also quoted.)

8. S. Murayama, Astronomical Magazine (The Heavens), Vol. 26, No. 289 (1947), pp. 41-43.

9. J. B.A.A., Vol. 71, No. 8 (1961), pp. 337-338.

10. J. B.A.A., Vol. 73, No. 3 (1963), p. 107.

11. See, for example, the drawings by S. Ebisawa in The Heavens, Vol. 32, No. 328 (1951 Apr.), Front Cover; in The Strolling Astronomer, Vol. 5 (1951), No. 5, p. 1; in Sky and Telescope, Vol. 24, No. 2 (1962 Aug.), p. 72; and the Palomar Frontispiece photograph in B. M. Peek's The Planet Jupiter.

A STUDY OF POLYGONALITY AMONG SMALL LUNAR CRATERS AND A POSSIBLE INTERPRETATION IN TERMS OF LOCAL LUNAR TERRAIN

By: Francis John Manasek

Abstract: When small lunar craters were divided into two groups based upon their location on either the maria or in the mountainous regions, a clear relationship between diameter and polygonal outline was observed. The possibility that the nature of the local terrain influenced the shape of the crater is examined.

THE RELATIONSHIP BETWEEN THE DIAMETERS AND OUTLINES OF SMALL CRATERS AND THEIR LOCATION ON THE LUNAR SURFACE

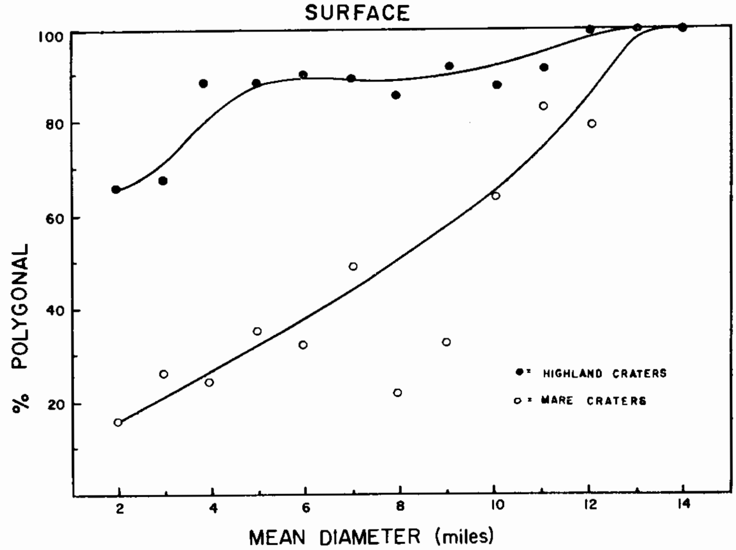

FIGURE 16. Histogram to show frequency of polygonal lunar craters of different diameters in both maria and highlands Prepared and contributed by Francis J. Manasek. Refer also to text of his article. The study shown embraces 1250 craters selected at random from the central regions of the moon. Each crater is shown clearly on at least one *Photographic Lunar Atlas* plate.

Almost all lunar craters with mean diameters greater than about 13 miles present boundaries which are polygonal [1,3,5], quite frequently hexagonal. Comparatively little attention has been given to polygonal craters smaller than 13 miles in diameter. Two principal mechanisms responsible for the formation of the polygonal outlines have been postulated:

1. The deformation of circular or nearly circular craters by the combined action of lateral forces.

2. The action of a polygonal meshwork of faults and fracture lines as natural boundaries for the growing crater.

The first mechanism has been discussed at great length by some investigators, notably Spurr[2], while some more recent views on the subject may be found in the works of Fielder[3], Firsoff[4], and Warner[5].

The current study was undertaken with the intention of obtaining data on the relation of polygonal craters to the surrounding terrain. For the purposes of this paper, all craters whose boundaries contained any angular portions were termed "polygonal" craters; all others, round, elliptical, or oval, were termed "round" craters. The data presented here were obtained by measuring structures represented in the *Photographic Lunar Atlas*[6] and were confirmed, in the case of the smaller craters, by visual observation. The visual work was helpful in differentiating between the two classes established. All diameter measurements were made on PLA plates. A histogram was constructed with an interval of 1 mile, the points on the graph representing each interval. All of the craters which were examined were also divided into the following two classes:

1. Mare Class. This class consists of craters which are found on the relatively level lunabase areas of the moon's surface, e.g., maria, flooded crater bottoms, etc. and which do not impinge upon any other landform.

2. Highland Class. This class consists of craters found in the more rugged lunarite areas and whose boundaries do impinge upon surrounding formations.

All lunar craters with diameters greater than about 14 miles which were examined were found to be effectively polygonal. An analysis of a population of 1250 craters with mean diameters less than about 14 miles revealed that the smaller diameter groupings are composed of both polygonal and round craters. It was also found that as the diameter group decreases the percentage of polygonal craters decreases. (Figure 16.)

If the polygonal aspect of lunar craters is in part due, as is suggested here, to a natural boundary effect of the grid system, such a relationship would be expected. With a grid system having a given spacing between adjacent faults, the larger the crater the greater is its probability of intercepting a segrement of the grid and achieving a polygonal outline. Conversely, the smaller the crater, the less likely it is to encounter a portion of the grid; and hence it will tend to grow more nearly uniformly in all directions and to maintain a more regular boundary. The same would be true if the polygonal aspect is due to the physical limitations to crater growth which would be offered by an encroaching landform.

As is indicated in Figure 16, the incidence of polygonal craters is higher for highland craters than for mare craters. At about the 4 mile diameter range, the percentage of polygonal highland craters rises sharply. This effect can possibly be attributed to the physical interference of surrounding raised areas to symmetrical expansion of the crater. Such a sudden increase is absent in the mare crater class, where none of the craters had surrounding landforms interfering with its growth.

If polygonal characteristics of some craters are due to the confining action of the grid system, these data might suggest that the system has a smaller spacing between adjacent faults in lunarite regions than in lunabase regions. Thus the greater density of faults in the lunar highland regions coupled with the effects of neighboring land masses would then explain the greater incidence of polygonal craters in the highland regions.

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A REVIEW OF SOME ALPO VENUS STUDIES

By: Dale P. Cruikshank, A.L.P.O. Venus Recorder

In the seventeen years of the ALPO journal there has been published a rather large amount of speculation as to the conditions on the surface of Venus and in its atmosphere, based on visual and photographic observations.

Much of this speculation has been completely unencumbered by the restrictions of physical possibility. This condition reflects somewhat on the degree of self-criticism of American and British amateur astronomers. One may write scores of articles criticizing the work of others, but it is quite another thing -- and a more fundamental attribute -- to criticize one's own work. Along these lines, less is expected from beginning observers. Indeed, many of the drawings and reports in the Venus Section files were made by observers with negligible experience. However, it is depressing to see the continued efforts of some observers of long standing and considerable experience. There are notable exceptions, of course.

In this article we will examine a few topics that have been discussed by various persons through the pages of this journal, not to supply definitive solutions to the problems, but to help keep the unwary off dark and dead-end paths. The discussions herein should also help to justify the programs suggested in an earlier article on the aims of the Venus Section in 1963-1964 (Cruikshank, Str.A.,17,pp. 151-153, 1963).

1. Dusky Markings

Do dusky markings exist on the visible cloud layer of Venus? Most observers say that they do, and I agree. In 1928, Frank Ross (Ap. J., 68, p. 57) reported his photographs of them in the ultraviolet at Mt. Wilson; and a number of others (G. Kuiper, R. S. Richardson, B. Smith, W. K. Hartmann, F. J. Eastman, B. Lyot, A. Dollfus, H. Camichel, etc.) have likewise photographed them, often with small instruments. W. K. Hartmann (Str. A., 16, 7-8, p.171, 1962) showed that bands visible on ultraviolet photographs seldom correspond to those shown on drawings made at the same time. It is also clear from his report that markings on simultaneous drawings by different observers do not agree well. The implication is obvious; the bands and shadings are on the threshold of visibility at the contrast level and spectral response region in which the eye works, and observers cannot be absolutely objective in recording them on a sketch.

Clearly, these remarks apply to dusky shadings which are real. A wide variety of markings on the sketches in the section files are not in this category and appear to be results of contrast effects encountered when observing the bright image of Venus against a moderately dark, or worse, a very dark sky. Some would explain all shadings seen visually as contrast effects, but I don't believe it. My own observations are always made in daylight, and I can distinguish shadings about half the time. Further, very recent photographs in visual blue light show markings on Venus now and then and generally do not confirm sketches made by two persons with the same instrument at the same time (in an article to be published by W. K. Hartmann).

2. Ashen Light

What about the ashen light? Too many good observers have seen the illumination of the night hemisphere of Venus for us to write it off as an optical illusion. N. Kozyrev in the Societ Union has spectra of the emission which suggest auroral activity involving the excitation of molecular nitrogen (Publ. Crimean Astrophys. Obs. 12, p. 169). I have seen the ashen light only once -- when Venus was two hours from inferior conjunction. Some ALPO observers report the ashen light at almost any phase, sometimes as great as k = 0.82. I strongly doubt the reliability of the observations.

If the ashen light is auroral in character, then we should expect it to be most conspicuous and frequent during periods of intense solar activity. Further, since the Mariner II space probe observations indicate absence of an intense dipolar magnetic field associated with Venus, we might expect auroral activity to be more uniformly distributed around the planet as compared to the strong polar concentration observed on Earth. ALPO records are generally consistent with this interpretation, which some may consider ad hoc. The ALPO records should be compared to solar activity fluctuations to see if any degree of correspondence exists.

The above remarks pertain only to observations of the night hemisphere's appearing brighter than the background sky. Sometimes we hear reports of the night hemisphere's appearing darker than the background sky. Clearly, this difference is physically impossible, regardless of whether Venus is observed in daytime or at night; the planet can never subtract light from the sky but can only enhance it. James Bartlett's explanation of the appearance of the night hemisphere (Str. A., 16, 1-2, 1962, p. 10) at large phases as the result of the planet's being silhouetted against the Zodiacal Light and/or a coronal streamer from the sun is impossible as a passing glance at the geometry of the situation will show, and on the basis of what is known of the Zodiacal Light and the corona. Anyone can, however, easily demonstrate to himself how these misleading observations come about. If one looks at Venus when k is less than about 0.6 with very low magnification (about 5 or 10 per inch of aperture), it is at once seen that the portion of the night hemisphere adjacent to the terminator appears darker than the sky. The conclusion is obvious; the steep contrast gradient from planet to sky "darkens" the portion of the planet in question. Indeed, many sketches in the ALPO files show that just a fraction of the night hemisphere is so darkened -- that portion nearest the terminator.

3. Limb Band

What is the nature of the bright limb band of Venus? James Bartlett (Str. A., 15, 7-8, p. 133, 1961) recently discussed his observations of a bright band along the limb of the planet. He found that the band developed shortly after superior conjunction, was most conspicuous around dichotomy, and then diminished as the crescent became more narrow. Bartlett also reported a dusky inner band which was sometimes seen adjacent to, and parallel to, the bright limb band. He notes that this dusky band was usually brownish in color. He interpreted these bright and dark bands as a high cloud bank (or a propinquitously united group of several cloud banks) and the shadow cast by the clouds, respectively. The brownish tint was attributed to a scattering effect in the Venusian atmosphere.

Joseph Eyer (Str. A., 15, 9-10, p. 148, 1961) criticized Bartlett's article, saying that while the bright limb band at dichotomy is reasonable (assuming that scattering is maximum at an angle of 90° to the incoming light) the dark band cannot be a shadow of a cloud ridge. Even though the Rayleigh scattering function (which Eyer mentions) is strongly forward and rearward thrown and not at 90°, Eyer correctly showed that on the basis of Rayleigh scattering, the dusky band should be blue and not brown or red. Unfortunately, Eyer neglects the fact that Rayleigh scattering does not hold in the upper atmosphere of Venus, except perhaps in the very short ($\lambda <$ 3600 Å) wavelengths (T. Gehrels and T. Teska, Applied Optics, 2, p. 67, 1963). That Rayleigh scattering does not occur has been known for over half a century (see, for a discussion relating to this, Russell, Ap. J., 9, p. 294, 1899).

We now question the point of whether there does indeed exist a bright limb band on Venus. My own visual observations and those of most others indicate that the limb of Venus is far brighter than the terminator, as one would expect from the cosine dependence of brightness distribution across a diffuse reflecting sphere as given by Lambert in the eighteenth century. But, as with other complex phenomena where the physical senses of humans are insufficient to distinguish the true picture amid the subtleties presented, we must turn to physical (as opposed to visual) observations. In the surface photometry of planets, Venus in particular, significant strides have been made. Among the important papers relating to this topic, V. I. Yezerskii's work "Photographic Photometry of Venus" (Trans. Gor'ki Astr. Obs., Kharkov State Univ., 12, pp. 73-165, 1957) is fundamental. Yezerskii's photometry done in 1932, 1951, 1953, and 1954, with phase angles from 26° to 139° and in four spectral regions, shows that the position of the brightness maximum along the intensity equator (the equator reckoned from the line of cusps) and in parallel directions, is located near the portion of the disk where the angle of incident sunlight equals the angle of "reflection". Clearly then, the limb band as Bartlett describes it is not a real feature of the planet. If the Venus atmosphere had an orthotropic scattering indicatrix as Eyer suggests, the brightness would be proportional to the cosine of the angle of incidence; and its maximum would then occur at the subsolar point. This is not observed.

It is probable that the actual brightness distribution across the Venus disk is a result of the combined effects of reflection and anisotropic scattering. It should be added that Yezerskii's observations confirm earlier photometry by R. S. Richardson (P.A.S.P., 67, p. 304, 1955) and N. P. Barabashev (Pub. Kharkov. Astr. Obs., No. 2, pp. 3-10, 1928).

While it is true that the eye perceives a bright limb on Venus, I contend on the basis of the observations reported above that this is a pitfall of contrast effects to which the eye cannot help but succumb. Fortunately, then, we need no enormous cloud banks, orthotropic scattering (which is hard to come by), or suchlike to explain our drawings of this phenomenon.

4. Radial Spoke System

In 1955 and 1956 there occurred in this journal a lively debate on the matter of a system of radial dusky markings with their center at the subsolar point. J. C. Bartlett began (Str. A., 9, 1-2, p. 2, 1955) by reviewing old observations by Percival Lowell, more recent confirming observations by R. M. Baum, and to some degree, those of O. C. Ranck, D. Avigliano, and Bartlett himself. He concluded that both hard linear markings and dusky streaked markings exist, and that each has some degree of permanency on the disk of Venus. Patrick Moore countered (Str. A., 9, 5-6, p. 50, 1955) Bartlett's remarks by doubting the general authenticity of spoke systems, writing them off as illusions. He cited the observations of E. E. Barnard and E. M. Antoniadi with large telescopes, and U.V. photographic observations of streaks in support of his thesis. R. M. Baum (Str. A., 9, 7-8, p.82, 1955) proceeded to correct "errors" in the Bartlett paper, flail Moore with some rather frail arguments, re-inflate Lowell's observations, and outline evidence "proving" the reality of the spoke system. In my view, experiments with artificial planetary disks, color sensitivity of the eye, and such prove nothing of the kind. Moore came forward again (Str. A., 9, 9-10, p.112, 1955) to restate his views on the superiority of large versus small apertures, question again the observations of Lowell, and cite results of his own artificial disk experiments. Bartlett returned (Str. A., 10, 9-10, p. 102,1956) to reemphasize the value of Baum's independent "recovery" of the Lowellian spoke system, and to assert its general reality.

Generally, I concur with Moore in that large apertures are better than small ones (given good seeing and high optical quality), and that the observations of Barnard and Antoniadi must carry considerable weight (and in the use of the word "Venusian"). With respect to Lowell's observations, to say that his sketches and maps of Venus, Mercury, and the Jovian satellites are highly suspect is a gross understatement.

While it would not be proper for me to enter the argument actively at this late date, it is fitting, I believe, to re-examine the so-called spoke system in the light of recent observations and on theoretical grounds. A. Dollfus has made important planetary observations under optimum conditions with large apertures in recent years. He reports that the dusky markings on Venus generally have the pattern of a radial system with the center at the subsolar point (L'Astronomie, 69, p. 413, 1955, or in English, see his chapter in Planets and Satellites, Kuiper and Middlehurst, ed., Chicago, 1962, pp. 534-571). Dollfus finds that the general appearance of the markings varies greatly from day to day but that the radial pattern or portions of it recur and that this represents the undisturbed condition of the markings observed. He offers a general map of the planet showing these permanent features. In many respects, this pattern reported by Dollfus is consistent with a model of the circulation of the Venus atmosphere proposed by Yale Mintz (Plan. and Space Sci., 5, p. 141, 1961). The Mintz model is based on convection in the planet's atmosphere (assuming a very slow rotation) with the principal circulation from the subsolar to the antisolar point.

Klaus Brasch (Str. A., 15, 9-10, p. 156, 1961) published a map of Venus made from composite sketches of the planet by members of the Montreal Centre of the RASC. He assumed, as did Dollfus, that the rotation period is long and that certain markings are semipermanent. While I do not agree in detail, Brasch's approach is clean and praiseworthy, and generally indicative of the good work of which the ALPO is capable.

In contrast with visual observations of the radial pattern of dusky features (and hard, linear features) we have the well-known ultraviolet photographs of Ross, Kuiper, etc. The markings shown in the ultraviolet (and more recently in the visual blue -- W. K. Hartmann, unpublished --) do not correspond to a radial pattern.

While it is possible that visual observers record activity in a cloud level greatly different from that photographed, and while the Mintz model gives some theoretical reason for a radial pattern, I personally believe that it is not a major feature of the visible cloud layer of Venus, the Dollfus and Brasch observations notwithstanding.

5. Cusp Caps

Observers of Venus often note that one or both cusps are abnormally bright compared to the remainder of the disk. James Bartlett (Str. A., 12, 4-6, 1958, p. 43) contributed a very worthwhile paper on his statistical analysis of his own observations, those of Owen Ranck, and those of a group of ALPO observers lumped together. For the present time we will call these anomalous brightenings "cusp caps", though this term suggests a physical interpretation that is unproved. Bartlett showed that one or both cusp caps were visible in 54% of the observations (477 out of 830 observations), that the south cap alone was visible 11% of the time, the north 7%, and both caps simultaneously 35%. These figures correspond closely to those obtained when Bartlett's 221 observations or Ranck's 158 observations are considered alone. I believe that we can attach considerable significance to these values, particularly because both Bartlett and Ranck had such a long and continuous series of observations. This is prerequisite to a statistical study of features like the cusp caps.

Bartlett also found that the cusp caps appeared at virtually all phases of the planet, from which he concluded that seasonal effects on the planet are not important in governing the appearance of the caps, since all positions in the orbit were represented by cusp cap observations. This last statement does not follow, however, since phase is not uniquely related to Venus' heliocentric longitude alone, but also to the difference in heliocentric longitudes of Venus and the Earth.

Bartlett's work indicates that the south cap was most often the larger of the two. Some reports of details seen in the cusp caps were mentioned, but are probably not reliable enough for useful analysis. Dusky cusps were also discussed, but these are reported far less often than bright cusps.

The very fact that bright cusps occur indicates that the Lambert law of diffuse reflection from a sphere is not strictly applicable to Venus, as we also saw in section 3.

Bartlett appears to favor the interpretation that bright cusp caps represent the actual poles of rotation of the planet, and that the inclination of the polar axis to the orbit plane must be small since the caps were seen at a wide variety of heliocentric longitudes. This does not follow, since we can only say (on the basis of a "polar cap" sort of interpretation) that the angle of inclination of the axis to the orbit plane is less than the angular extent of the larger cusp. This is the case with Mars (and with the Earth, correcting for the asymmetry of the caps). In fact, it would be of some interest for ALPO observers to make a continuous and consistently good series of observations of the angular extent of the cusp caps so that a statistical analysis could be made to determine the best value of this quantity. White it is not totally fair to judge Bartlett's 1958 interpretation in the light of 1962 observations, the recent infrared studies of Venus reported in many recent journals make it clear that the surface temperature of Venus precludes any sort of ices which could form the bright polar caps as required by Bartlett. P. Moore dismissed this idea some time ago (J. B. A. A., 65, 6, p. 232, 1955); and most astronomers, knowing that the surface temperatures on Venus are very appreciable by inference from atmospheric temperatures, rejected the idea of ice polar caps many years ago.

C. M. Pither (J. B. A. A., 73, 5, p. 197, 1963)has suggested on the basis of the frequent correspondence of cusp caps to the time of solar flares in 1961 that the caps are auroral in nature. The examples Pither gave, however, correspond to flares of class 1 or 1+, which are not prone to produce terrestrial auroral activity. Furthermore, some of the flares that Pither cites were invisible from Venus at the time of the cusp cap observations (see Cruikshank, J. B. A. A., in press). While statistics such as Pither proposes are of some interest and should be examined for ALPO and individual observers' records, Pither's own are not convincing to me. In fairness, we must admit that it is possible that auroral mechanisms in the Venus atmosphere may be triggered by flares which we ordinarily consider unimportant (1- to 2); the geometrical factor remains that flares must ordinarily occur within about 45° of the center of the solar disk as seen from the planet before intense magnetic storms are induced. Further, Venus does not seem to have an intense magnetic field by which to concentrate auroral activity toward the poles (Mariner II observations).

6. Cusp Bands

In addition to bright cusp caps, observers often report dusky bands or collars near the cusps which define or border the caps. P. Moore, (The Planet Venus, 1st ed., Macmillan, 1958, p. 53) suggests that the bands are simple contrast effects of the bright cusps, saying that his own observations "indicate that the collars can only be seen when the caps are unusually prominent". I suspect that many observers would disagree that they are seen only in the presence of bright cusps; I do. Because I have seen such dusky streaks with and without the adjacent bright cusp caps, I am led to believe that they are not different from the ordinary dusky bands which are often roughly perpendicular to the terminator. At the apparent poles, however, these bands are highly foreshortened, and this accounts for the usually narrow and rather dark appearance. This also may tell us why cusp bands or collars are often seen when no other features are visible (see Figure 16, Str. A., 16, 7-8, p. 179, 1962). Broad bands on the limit of visibility at the center of the disk will be darker and more easily visible near the poles, assuming that they are near the top of the visible portion of the atmosphere. That the dark bands lie at such a level is consistent with the fact that they are most prominent in the bluest regions of the spectrum. Observers might keep this in mind in planning individual programs of visual observations.

7. Phase Anomalies

It is well-known that the phase of Venus as seen in the telescope seldom corresponds to the predicted phase. Brian Warner (J. B. A. A., 73, 2, p. 65, 1963) has shown that the discrepancy between the date of observed and predicted dichotomy (half phase) may be interpreted as an effect of scattering in the upper atmosphere of the planet. On the basis of a report by W. K. Hartmann (Str. A., 16, 9-10, p. 222, 1962) covering a decade of ALPO work, and on my own experiences, I believe that it has not been satisfactorily demonstrated that phase effects are due to anything more complex than the effects of contrast and the expected decrease in brightness of the disk as one approaches the terminator. It is known that the contrast of the image of the planet perceived in the telescope is a function of magnification, aperture, transparency, seeing, cleanliness of optical surfaces, etc. The reader is referred to Hartmann's article (see above) for a good review and detailed analysis of the phase anomaly observed at dichotomy.

8. Concluding Remarks

Now that the above points of view have been aired, we may well ask what can be done by experienced observers with small and moderate sized telescopes in order to advance our state of knowledge of Venus. To a first approximation, the answer is probably "nothing". Visual observations of the planets simply tell us that problems exist and can do little to solve them. Furthermore, the problems made evident by visual observations are generally of a secondary nature, i.e. cusp caps, banded markings, etc. These problems, if they can be solved, shed only a little light on the more fundamental questions as to the atmospheric composition, temperature and density, the geometrical dimensions of the solid body, the presence of a magnetic field, and so on. The solutions of these secondary problems, as should be apparent, are often no less subtle than their more fundamental counterparts and as such may be beyond the capabilities of amateurs with limited instrumental complements and techniques. Our immediate task, then, is to find which of those problems are solvable by ALPO observers and then to initiate a concentrated effort in their directions. To a large extent, this has been the policy of the Venus Section under the Recordership of W. K. Hartmann. The suggested programs for specific attention have been discussed in an earlier article (Cruikshank, Str. A., 17, p. 151, 1963).

I will be pleased to discuss these topics in detail with interested persons as time allows.

A LETTER BY THE A.L.P.O. SECRETARY

In its efforts to be of service to devotees of astronomy the A.L.P.O. must naturally receive all kinds of inquiries and requests from all kinds of people. Much of this correspondence is handled by our Secretary, Mr. David P. Barcroft. We think that the philosophy and contents of the following letter by Mr. Barcroft are worth careful reading by any earnest teenager beginning the study of astronomy. Names are unimportant; the letter which our Secretary answered was probably a request for "anything free" for an intended Science Fair project.

Dear Inquirer:

"In reply to your recent inquiry: The A.L.P.O. as an organization has no literature save its publication, "The Strolling Astronomer", which is somewhat specialized and would likely be of little use to you at the present time.

"Anybody undertaking the study of astronomy must get hold of all the books he can find on the subject. There are any number of these, many of them can be found in public libraries and certainly your high school library ought to have a few of them.

"You can't get any worthwhile information on astronomy simply be making requests that literature be sent to you. You have to do the leg work yourself by finding the literature.

"And when you have found it, the next thing to do is to use it. There is no easy way of assimilating astronomy. It takes a lot of effort and this over many years.

"But don't let this get you down. We all had to start the same way."

With kindest regards,

David P. Barcroft, Secretary
Association of Lunar and Planetary Observers.

This letter came to the Editor's attention by accident, and its appearance here will be as much a surprise to Mr. Barcroft as to our other members.

BOOK REVIEWS

The Photographic Story of Mars, by Dr. Earl C. Slipher. Sky Publishing Corp., Cambridge, Mass., and Northland Press, Flagstaff, Arizona, 1962. 168 pages. Price $8.50.

Reviewed by Rodger W. Gordon

Only one word is needed to describe this masterpiece and that is "magnificent". Dr. Slipher's book begins where most other Martian texts leave off. Dr. Slipher begins by giving 70 pages of general information about Mars -- its polar caps, atmosphere, and surface features, including his 28 conclusions supported by over 60 years of observational evidence. The remainder of the text is devoted to over 500 photographs. Each plate is preceded by a page of explanation, which altogether give a concise description of all the phenomena on Mars of which we are aware. Especially important in this respect are Dr. Slipher's conclusions concerning the "violet layer", which was discovered by him in 1939. This book is a remarkable record of over 50 years of dedicated observations by the world's greatest Mars specialist. Dr. Slipher gives a description of each phase of Martian phenomena and then groups his photographs in such a way that the reader can easily follow the logical development of thought. Using the photographs, graphs, and charts, he clears up many false notions about the polar caps and violet layer which, needless to say, still appear in many standard texts. In the years to come, this volume perhaps will be compared to Antoniadi's classic "La Planète Mars", because both works are quite definitive in their scope. Those who still doubt the reality of the Martian canals need only look at page 103, photographs 37-42, or pages 143 and 162 to see the reality of these markings. Dr. Slipher remarks that much is unavoidably lost in reproduction, but enough remains to verify the existence of many of the canals. To sum up, anyone who is interested in Mars or who considers himself a Mars specialist would do well to become thoroughly familiar with this tremendous observational work.

The Planet Mercury, by Dr. Werner Sandner. Translated by Alex Helm, F.R.A.S. The Macmillan Co., New York, 1963. 94 pages. Price $3.95.

Reviewed by Rodger W. Gordon

Only 94 pages might not seem a lot to devote to a major planet; but since we know so little about the planet Mercury, it is amazing that Dr. Sandner was able to produce a book of 94 pages about the elusive planet. Those who have attempted to observe Mercury will greatly appreciate this volume. Dr. Sandner shows that much can be accomplished with amateur equipment. In fact, the author cites many of the observations by A.L.P.O. Director Walter Haas and A.L.P.O. observer Gary Wegner to prove his point. Included among the plates is Gary Wegner's map of Mercury plus additional drawings of the planet by noted amateurs. Evidently Dr. Sandner regards amateur observations of Mercury as very valuable. Professional results are not neglected either -- Antoniadi's and Schiaparelli's maps are given full treatment, and attempts by professionals to detect a Mercurian atmosphere are given fairly good treatment. The book makes exciting reading because the manner in which the material is presented will make one want to try to observe Mercury more often. To sum up, A.L.P.O. members will undoubtedly want to add this little book to their collection of planetary literature.

[Mr. Gordon's two reviews above were edited and submitted by Mr. J. Russell Smith. - Editor]

RECENT LUNAR AND PLANETARY WORK IN SWITZERLAND

By: Joseph Ashbrook

The Swiss Astronomical Society (SAS)contains an active group of amateur planetary observers in Switzerland and neighboring countries. Their results are reported mainly in the journal Orion. Though limited in quantity, this work is of excellent quality, and a look at some of it should interest ALPO'ers.

Mars during the 1960-61 apparition is the subject of a summary in Orion for April-June, 1961, pages 110-115. Here are analyzed 42 drawings (10 of them reproduced) obtained with 6-to 10-inch telescopes. There is a good map.

An important Jupiter study by S. Cortesi (Orion, April-June, 1962, pages 106-122) deals with the three persisting white ovals FA, BC, and DE in the STeZ. During 1941-61, the average rotation period of these markings was $9^h\ 55^m\ 08^s.5$. Extensive tables of visibilities, longitudes, and rotation periods are provided, partly from data supplied by Elmer Reese of ALPO.

Jupiter's appearance in 1962 is described in Orion, April-June, 1963, pages 92-114, from the work of nine observers (192 drawings, 95 CM transits, 349 intensity estimates). Because of the abnormal state of Jupiter that year, the 32 sketches reproduced are a useful reference. The rotation period of the Great Red Spot was $9^h\ 55^m\ 42^s.0$, from 27 transits between June 2 and December 27.

In the same paper, zenographic latitudes of Jovian belts have been deduced by Cortesi from 11 photos by himself and J. Dragesco, and from direct estimates on six nights by himself and D. Courvoisier. He finds: center SSTB = $-43^\circ.5$; center STB = $-30^\circ.3$; north edge Red Spot = $-15^\circ.8$; south edge SEB_n = $-9^\circ.6$; north edge NEB = $+17^\circ.7$; center NNTB = $+40^\circ.9$.

L. Dall'Ara reports his findings on several historic lunar observing problems in the Mare Nectaris region (Orion, January-March, 1963, pages 38-41). These problems are some of those listed in Sky and Telescope, May,1958, page 237. With his 7-inch reflector, 230X, Dall'Ara ascertained that the central eminence in Beaumont A is a dome. The mysterious Bohnenberger B was seen as a wall craterlet on Bohnenberger A, rather than a gap in the latter's wall. His drawing of Fracastorius Y shows it to be a chain of three strongly distorted craters, worthy of further study.

A valuable investigation of Linné has been carried out by Cortesi (Bolletino della Societa Astronomica Ticinese, 2, 30-42, 1962.) He has analyzed micrometer measurements of the Linné craterlet by G. Fournier (R. Jarry-Desloges, Observations des Surfaces Planetaires, 10, 199-201, 1946), and deduces the following dimensions:

Exterior diameter, east-west	2500 meters
Rim-to-rim diameter	1700 meters
West rim height above plain	100 meters
East rim height above plain	40 meters
Average slope of outer west wall	10 degrees
Average slope of outer east wall	15 degrees
Horizontal extent of west rim, at plain level	700 meters
Horizontal extent of east rim, at plain level	160 meters

These numbers can be compared with the independent study by J. Ashbrook in The Strolling Astronomer, 17, 26-28, 85-89, 1963. Cortesi assumed a depth of 160 meters for the craterlet, and then computed the volume of the cavity as 0.15 km^3, the volume of the ramparts as 0.12 km^3.

Next he constructed a 1:50,000 scale model in plaster of Paris, and by photographing it under various illuminations he was able to duplicate beautifully the changing telescopic appearance of the Linné craterlet from sunrise to sunset. From a careful review of earlier observations (18 references), Cortesi finds no trustworthy indication of physical change.

[Persons wishing more information about the work of the SAS planetary group may write Sergio Cortesi, Locarno-Monti, Switzerland.]

ANNOUNCEMENTS

Sustaining Members and Sponsors. As of January 16, 1964 there are these new classes of membership:

Sponsors - W. O. Roberts, Jr.; David P. Barcroft; Philip and Virginia Glaser; Charles H. Giffen; John E. Westfall; Joel W. Goodman; the National Amateur Astronomers, Inc.

Sustaining Members - Grace E. Fox; Ken Thomson; Sky Publishing Corporation; Joseph Ashbrook; Charles F. Capen; Kenneth J. Delano; Craig L. Johnson.

Sustaining members pay $10 per year; Sponsors, $25. Of these amounts, $4 pays for a subscription to this magazine; and the remainder is a gift to help support the work and activities of the A.L.P.O. We express our thanks to the donors listed above for their generous and timely aid.

Corrections to July-August, 1963 Issue. The listing of the A.L.P.O. staff on the back inside cover of this issue was wrongly made the same as in the March-April, 1963 issue. In reality there had been no changes from the May-June, 1963 issue.

In Dr. Joseph Ashbrook's article, "Steep Places on the Moon", on pg. 137 the coordinates of the abnormally deep crater Langrenus M should read Xi = +.903, Eta = - .170.

A Suggestion about Lunar Directions. The following proposal from the Reverend Kenneth J. Delano of Taunton, Mass. in a letter dated November 13, 1963 appears worthy of adoption:

"I wrote to you before concerning the use of east and west on the moon, and am glad that the ALPO has adopted the IAU designation. I have noticed that all astronomical publications have to take care to make clear what designations are being used in articles about lunar features. To avoid unnecessary verbosity I would like to suggest that the letters (I A U) in parentheses should follow immediately after the very first use of 'east' and 'west' in reports or articles when the I A U designations are being used. The mere appearance of (IAU) after the words east and west should make it clear what interpretation of east and west is being used."

A.L.P.O. Staff Changes. We have two new Lunar Recorders, namely:

Harry D. Jamieson 2706 Ethel St., Muncie, Indiana	José Oliverez 804 St. Marie Mission, Texas

Mr. Jamieson is in charge of our A.L.P.O. Lunar Dome Survey, and Mr. Olivarez is his assistant. This project will be described in the next issue; interested lunar observers are meanwhile invited to write to the two new Recorders, whom we cordially welcome to our staff.

Mr. Elmer J. Reese desires that correspondents address him at:

Research Center, New Mexico State University
University Park, New Mexico

Mr. Reese is on the staff of the Planetary Astrophysics Group, directed by Dr. Clyde Tombaugh and Mr. Bradford Smith.

Twelfth A.L.P.O. Convention. This meeting will be held at Denver, Colorado during the last week of August, 1964 as part of the second National Amateur Astronomers Convention. The first one was held at Denver in 1959, and those who attended found it very enjoyable and highly successful. Besides the A.L.P.O., the Astronomical League, the Western Amateur Astronomers, and the A.A.V.S.O. will take part in the coming meeting. Further details will be carried in future issues, but it is already not too early to start making plans to be in Denver for an outstanding astronomical get-together. Mark your calendar now!

Papers at Twelfth A.L.P.O. Convention. Mr. Andrew R. Gassman, acting for Convention Chairman Ken Steinmetz, has written us of some general policies about papers at Denver. The A.L.P.O. has been allotted four hours for presenting papers. Those planning the Convention will need to have from each author the title and an outline of his paper by July 1, 1964 at the latest. They will need a complete and reproducible copy of the paper before the Convention for inclusion in the Proceedings to be published.

Qualified A.L.P.O. members are heartily invited to begin working on suitable papers very soon; indeed, their contributions are definitely needed. With time on the program rather limited, papers should not be unduly long; and some screening may be necessary. All A.L.P.O. papers should be sent directly to the Editor, who has accepted responsibility for our portion of the program.

In Praise of Abstracts. Mr. Francis J. Manasek recommends that writers of the longer articles in this periodical include a short abstract at the beginning. His own article elsewhere in this issue is a good example of this plan. Mr. Manasek feels that abstracts will helpfully guide readers and will in addition aid the author in developing his own thoughts.

Location of Red Glow East (IAU) of Schroeter's Valley. Harry Jamieson and one or two others have pointed out that the "hill west of Cobra Head", which looked red to Lowell Observatory Air Force mappers on October 30, 1963, was really at: Xi = -.681, Eta = +.426, not where stated on pg. 171 of our last issue. Nevertheless, we did correctly quote the Harvard Announcement Card reporting the observation.

Request for Observations of December 30, 1963 Total Lunar Eclipse. The Editor is planning an article on this eclipse for the next issue. He accordingly requests all persons who observed the eclipse to communicate their results to him at Box 26, University Park, New Mexico. He is especially interested in studies to detect possible physical effects of the shadow's passage, and one would think that Aristarchus and vicinity should have been closely watched for such possible changes. Types of eclypse observations adequately processed by Sky and Telescope , notably umbral contact times, need not be reported.

We shall state right now that it was an amazingly dark eclipse.

An Offer of JPL Observational Data: Recorders Please Notice. Mr. Charles F. Capen, the Chief Resident Observer at the Jet Propulsion Laboratory's new Table Mountain Observatory in southern California, is well known to many of our readers as a frequent contributor to this periodical. We quote a portion of a letter from Mr. Capen dated September 15, 1963:

> "The papers given at San Diego were of good quality and interest; and because of the attention given by several of the JPL staff and the interest shown by the JPL Photographic Section regarding my color photo paper, more recognition has been given to the ALPO by our senior staff scientist, which means in short that I have been given authority to cooperate more closely with the ALPO in the exchange of observational data. All that is essentially necessary is a request from yourself or any Recorder for observational data on a given date or dates, and I will notify our section chief scientist Dr. Drake and the JPL Public Information Office and will send the requested data - providing, of course, that we have the observation and have time to prepare it".

We thank JPL very much for this generous offer of cooperation. It will be most practical if the Recorders themselves initiate requests for observations since they will know best what information is needed. Such requests should be addressed to C. F. Capen, Table Mountain Observatory, Box 367, Wrightwood, Calif. We invite our staff members to make some considered and useful requests of this kind soon, for offers tend to be forgot when no action is taken on them for a long time.

A.L.P.O. Headquarters Zip Code. In this new system our address has become Box 26, University Park, New Mexico, 88070. We are glad gradually to learn the zip codes of our subscribers, but a complete, immediate changeover to their use in our mailing and in correspondence is not feasible.

COLORS IN THE ARISTARCHUS REGION: NEW OBSERVATIONS, OLD OBSERVATIONS, AND COMMENTS

By: Walter H. Haas, Director A.L.P.O.

We mentioned on pp. 171-172 of our July-August, 1963 issue the Lowell Observatory report of three transient red areas on the moon on October 30, 1963, one in the Cobra Head of Schroeter's Valley, the second on a hill east of this Valley, and the third on the southwest rim of Aristarchus (east and west in IAU sense). A detailed account will be found in Sky and Telescope, Vol. 26, pp. 316-317, 1963 (December). Then Harvard Announcement Card 1631 of December 6, 1963 carried the rather surprising news of a second Lowell Observatory observation of red in this lunar region:

"Unusual Lunar Colors. - Dr. John S. Hall writes, 'Lowell Observatory reports that pink colors have again been observed on the southwest rim of the lunar crater Aristarchus. Observations were made by four people with the 24-inch Lowell refractor. Event lasted from U.T. 0030 to 0145, 28 November, 1963. Coordinates of event were Xi = -.682 to -.685, Eta = + .391 to + .398 scaled from gridded sheet E 3-A of the orthographic lunar atlas'".

There are additional details in Sky and Telescope, Vol. 27, pg. 3, 1964. It is especially important that an observer at a 69-inch reflector on November 28 suspected a red spot in almost exactly the same location as the Lowell observers with only the information that a color phenomenon was occurring near Aristarchus. These observations have all been visual; there is so far no photographic confirmation.

There can be little doubt that a fair number of A.L.P.O. members have made a substantial number of color observations of Aristarchus and vicinity since early November, 1963. The list below is unquestionably extremely incomplete but may still be of interest. So far I know of no satisfactory recent positive color observations other than those mentioned above. The colongitude in the table is the lunar western longitude of the sunrise terminator at the moon's equator. On Aristarchus sunrise occurs at about 47°; local noon, at 137°. The colongitude at the Lowell color observation on October 30 was 60°.3 - 60°.5, at the one on November 28, 52°.6 - 53°.2.

Date 1963	Time (U.T.)	Observer	Telescope	Colongitude	Remarks
Nov. 6	6:25-6:35	Haas	6-in. refl.	147°.7	
Nov.28	3:00-3:15	Haas	12.5-in. refl.	53.9	Red filter used.
Nov.28	7:45-7:55	Haas	12.5-in. refl.	56.3	
Nov.29	5:00-5:08	Haas	6-in. refl.	67.0	Red filter used.
Dec.27	5:55-6:10	Haas	12.5-in. refl.	48.1	Note 1
Dec.28	0:15-0:18	Reese	8-in. refl.	57.3	
Dec.28	0:36-0:41	Haas	12.5-in. refl.	57.5	
Dec.28	0:40-0:45	Reese	8-in. refl.	57.5	
Dec.28	0:53-0:56	Reese	8-in. refl.	57.6	
Dec.28	1:15-2:00	Olivarez	17-in. refl.	57.7-58.1	Note 2.
Dec.28	1:30-1:32	Reese	8-in. refl.	57.9	
Dec.28	1:50-1:56	Reese	8-in. refl.	58.1	
Dec.28	2:02-2:13	Haas	12.5-in. refl.	58.2	Color filters used.
Dec.28	2:25-2:28	Reese	8-in. refl.	58.4	

Date	Time (U.T.)	Observer	Telescope	Colongitude	Remarks
1963					
Dec.28	2:38-2:48	Haas	12.5-in. refl.	58°.5	
Dec.28	2:55-2:57	Reese	8-in. refl.	58.6	
Dec.28	3:11-3:42	Haas	12.5-in. refl.	58.9	
Dec.28	3:30-3:33	Reese	12.5-in. refl.	58.9	
Dec.28	3:52-3:55	Reese	16-in. refl.	59.1	
Dec.28	4:04	Reese	8-in. refl.	59.2	
Dec.28	4:13-4:25	Haas	12.5-in. refl.	59.3	Color filters used.
Dec.28	4:46-4:54	Haas	12.5-in. refl.	59.6	
Dec.28	5:14-5:33	Haas	12.5-in. refl.	59.9	
Dec.28	5:35-5:45	Reese	8-in. refl.	60.0	
Dec.28	5:58-6:06	Haas	12.5-in. refl.	60.2	
Dec.28	6:30	Reese	8-in. refl.	60.5	
Dec.28	6:30-6:43	Haas	12.5-in. refl.	60.5	
Dec.29	0:30-0:37	Reese	8-in. refl.	69.5	
Dec.29	2:40-2:45	Reese	8-in. refl.	70.6	
Dec.29	6:08-6.13	Reese	8-in. refl.	72.4	
Dec.30	2:00-4:00	Olivarez	17-in. refl.	82.4-83.4	

Note 1. The brilliant east (IAU sense) outer wall of Herodotus sometimes looked reddish or brownish in the poorer moments, rather clearly a spurious spectral effect.

Note 2. Reddish-orange hues on the eastern bright rims of Aristarchus, Herodotus, and Schroeter's Valley were probably spurious eyepiece colorations. When the moon was in a clearing in thick high cirrus clouds for about 3 minutes, there was recorded a tiny red spot on the Aristarchus rim and a reddish orange color on the hill east of Schroeter's Valley (the one called red on October 30).

The intensive study on December 28 was motivated by the similarity of the solar lighting to that on October 30. Except as noted, absolutely no peculiar colors were even suspected in the observations listed in the table.

Color filters can be a valuable aid in visual lunar color observations. Of course, one should use only commercial filters whose transmissions at different wavelengths are known, such as Eastman Kodak Wratten Filters or Schott Filters. In general, a red area will look brighter relative to other features with a red filter and darker compared to such features with filters of other colors, such as blue or green.

One interesting question is that of the aperture necessary to see these red lunar spots. It may be significant that Mr. Greenacre and others at the Lowell Observatory could not confirm the colors with a 6-inch finder on October 30 nor with a 12-inch guide telescope on November 28. If it be true that apertures above 12 inches are necessary for such observations, then their infrequency in lunar literature is sufficiently explained. Mr. Alika Herring is strongly of this opinion; in a letter on January 6, 1964 he commented on how vivid he once found colors on Jupiter in a giant reflecting telescope compared to the aspect in his own 12.5-inch reflector, in spite of mediocre seeing in the view with the giant telescope. Mr. Herring concludes: "I think this indicates that color contrast is indeed purely a function of aperture; and assuming the Aristarchus phenomena to be typical, they therefore are not likely to be observable in a six-inch, despite the fact that the angular size might be well within the resolution of such an aperture. What appears to be a 'ruby red' color in a 24-inch telescope might well be completely undetectable in a 6-inch". Mr. Elmer Reese in personal conversations in recent months has similarly frequently described how much richer and clearer planetary colors are with 16 inches of apertures, especially in good seeing, than with a 6 to 12 inches. Certainly A.L.P.O. members owning large apertures or having access to them will possess a significant advantage in lunar color searches in the Aristarchus region and elsewhere.

It may be worth remembering that peculiar colors have been recorded in the Aristarchus region by a number of past lunar observers. R. M. Baum of Chester, England directs attention to some observations by Professor H. Klein described in Popular Astronomy, X, 2, No. 92, pp. 63-64, 1902. Professor Klein wrote in part: "First of all is the intensely green color of the whole surface, which is surrounded by the great Rill [Schroeter's Valley], a coloring which is recognized, immediately, even by an unpracticed eye, and has not its equal in intensity on the whole Moon . . . . Another phenomenon still appears at the mouth of the Herodotus Rill and towards Aristarchus on the plateau there, namely a violet glimmer, which begins to become visible immediately after sunrise and spreads itself out farther and farther with the rising Sun. On the 6th of August, 1881, as the wall of Herodotus had just emerged from the lunar night, the whole region between Aristarchus and Herodotus and the southern part of the great Rill appeared to me in a strong violet light, as if covered with fog. It did not lie around the brightest mountain portions, for the eastern slope of Aristarchus which shines so brightly in the interior, showed no vestige of it, nor was there the slightest trace of it shown there where the shadow of the western wall in the interior of Aristarchus contrasts with the bright eastern slope. In order to be perfectly sure I examined all the other bright spots especially the brilliantly lighted, but did not find even a single trace of violet light. . . . If Aristarchus was placed out of the field the intensity of the violet color was not altered in the least".

As Mr. Baum says, it is difficult to evaluate historical observations of this kind. It is perhaps a little curious that the scientific community of 1902 pretty much rejected Klein's color observations out of hand, while the current scientific world is inclined to accept the recent Lowell Observatory reports. It is obvious from the passage quoted that Professor Klein was aware of some of the pitfalls of lunar color observation. If the current Lowell report is valid, perhaps various past reports of bright if transient colors on the moon deserved more attention than they received. An objective evaluation would require more knowledge of the elements involved in a telescopic visual observation of lunar color and perhaps even of the psychology of the observers. These remarks must also apply to some A.L.P.O. reports of colors in the Aristarchus region in recent years. J. C. Bartlett and one or two others have occasionally reported a violet glow in Aristarchus. On September 13, 1951 T. Osawa with a 6-inch reflector observed a brownish orange or deep yellowish carmine tone over a large area south of Aristarchus and a faint blue on the northwest rim of this crater.

The interpretation of the Aristarchus reds must at present be highly conjectural. Since both positive Lowell observations were soon after sunrise, one guess might be that gases congealed during the long lunar night are responsible. If so, similar effects could well follow eclipses of the moon. (Observations of the December 30, 1963 lunar eclipse for unusual colors in and around Aristarchus were, however, negative - more details in the next issue). One may also wonder whether there is a resemblance to the activity in Alphonsus on November 3, 1958; refer to Sky and Telescope, Vol. 18, pp. 184-186, 1959 (February). In brief, spectrograms of the central peak of Alphonsus taken by N. A. Kozyrev at the Crimean Astrophysical Observatory revealed an emission spectrum probably best interpreted as a volcanic degassing. The central peak looked red at one stage of the phenomenon. Certainly spectra of possible future red spots in Aristarchus and elsewhere should be secured. Amateurs will very seldom have the necessary equipment, but they can watch for such outbreaks and can quickly notify interested professional observatories.

Finally, we invite A.L.P.O. members to join in a systematic patrol of Aristarchus and vicinity. Lunar Recorder Clark Chapman has already initiated such a project; interested persons should write to him at 2343 Kensington Ave., Buffalo 26, New York. As indicated above, the assistance of observers with apertures above 12 inches is particularly wanted. However, those with more modest instruments should also participate with a realistic view of their capabilities, as in all scientific work. Presumably results with any aperture will be almost wholly negative, but they are not less valuable on that account; time spent in searching must be carefully logged even when nothing is seen. Perseverance and carefulness will enable the A.L.P.O. in time to establish sound statistical data upon the frequency of red colors on the moon to replace present pure guesswork. Of course, reflectors are greatly to be preferred to refractors in all studies of colors. Since looking for what one does not see can get extremely monotonous, it might prove advantageous to combine this survey with other lunar projects described in this periodical in recent months. If and when unusual lunar colors are seen, the observer should watch them with the closest possible attention and should also alert other observers to attempt confirmation (more convincing when the information supplied to them is not too detailed).

Suggestions from readers about this proposed patrol in selected regions will be appreciated.

NOTE ON FRONT COVER DRAWING

The drawing of the Red Spot of Jupiter and its environs on the front cover is an encouraging example of how very much planetary detail a skilled and experienced observer can record with a moderately large aperture (16 inches) and extremely good seeing. Most of the internal structure in the Red Spot was not visible to the observer in a view with the same telescope some days earlier but with seeing several grades worse (3-5 Tombaugh-Smith scale). The South Temperate Zone oval in partial conjunction with the Red Spot is DE. Note the impression, shown on the drawing, that the Red Spot partly overlay the South Temperate Belt, as if at a higher level in the atmosphere of Jupiter.

The Strolling Astronomer

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