
REPORT ON MARS, NO. 44

 By **WILLIAM H. PICKERING.**

THE AXIS OF MARS.

The position of the axis of Mars is a matter of primary importance to the Martian investigator. The drawings of practically all the most active observers of that planet throughout the world, twelve in all, are given in Reports Nos. **41** and **43**. Nearly all of them show that considerable and easily measurable changes have occurred in the shapes and positions of a few of the markings. In order to know definitely quantitatively what these changes are, and in what directions the markings move, it is necessary that we should know the position of the planet's axis with all possible accuracy. Three astronomers during the present century have located the axis by measures of the planet, but by three entirely distinct methods, and from their investigations it is certain that the position at present given in the *Ephemeris* is at least as much as one degree, measured on a great circle, out of the way. By meaning their results in different ways it appears that the position of the axis is now known within less than fifteen minutes of arc, or 9 miles measured upon the surface of the planet. It does not appear plausible that all three observers should be liable to the same systematic errors.

Lowell in 1912 employed the classical method of Schiaparelli, which depends on determining the position angles of the two polar caps. He gives four different results differing in the extreme cases by less than a degree of arc in right ascension, and by only $0^{\circ}.3$ in declination. He does not state which one of the four he prefers, but we have selected the one which combined his latest published observations in 1911 with all his previous ones. (*Lowell Observatory Bulletin*, **56**, 32.) The writer in 1927 combined the drawings of detail of 10 particularly well seen points out of 100, all located near the central meridian on 187 selected drawings, made at six of the seven apparitions between the years 1914 and 1926 inclusive, and from their varying positions on the disk, caused by the apparent shifting of the planet's axis, deduced its position. (Report on Mars, **40**, 8.) In the choice of these points particular attention was given to select those which the measures showed as giving the least evidence of proper motion over the surface of the planet. (Reports Nos. **33**, 7, and **40**, 7.) Trumpler also in 1927, determined the position of the axis from measures of planetary detail, but necessarily remote from the central meridian, as shown upon photographs taken during 1924, the year of closest approach of the planet. (*Lick Observatory Bulletin*, **387**, 33.) Trumpler on that page gives a reference to Lowell's observations as being published in *Lowell Bulletin* No. **2**. They were actually published, and will be found as stated above in *Bulletin* No. **56**.

We will now consider first what we may call the Polar Cap method. During the nine years between 1877 and 1886 Schiaparelli made four determinations of the location of the axis. They ranged over 3° of right ascension and 2° of declination, and were centered at $319^\circ.9$, $+54^\circ.9$. These and the following positions are given for the epoch of 1905. (See map, Figure 2.) Schiaparelli's position, however, lies off the map, and the following positions are not marked upon it. Lohse on the ten years between 1884 and 1894 also made four determinations of the location of the axis, ranging between 4° of right ascension and 2° of declination, centering at $317^\circ.2$ and $+54^\circ.4$. Cerulli made a determination dependent on the apparitions of 1896 and 1898, giving the position as in $318^\circ.6$, $+54^\circ.1$. Between 1901 and 1905, by combining his own observations in different ways, Lowell made four determinations of the position ranging between 2° of right ascension and $1^\circ.5$ of declination, and centered at $316^\circ.0$, $+54^\circ.6$. These must not be confused with his later four above mentioned. H. Struve in 1895 also made a determination based on the orbits of the satellites, locating the pole in 1880 in $317^\circ.0$, $+52^\circ.5$. Trumpler gives Struve's position in 1925 as in $312^\circ.6$, $+52^\circ.8$, a result which I have not been able to verify, and is doubtless a misprint. From all these determinations Lowell located the probable position of the pole in 1905 in $317^\circ.5$, $+54^\circ.5$ (*Lowell Bulletin*, 24, 160), and this value was adopted by the *American Ephemeris* in 1909, and has been in use ever since. The shift in the position of the pole in twenty years due to precession amounts to only $+0^\circ.13$ in α , and $+0^\circ.07$ in δ , hence for the *Ephemeris* pole in 1925 we have α $317^\circ.63$, δ $+54^\circ.57$.

We see that this method of location gives rather divergent results. This is due in part to the fact that when the polar caps are large, and extend into comparatively low latitudes they melt in the noonday hours, so that at sunset their rims are in an appreciably higher latitude than at sunrise. Moreover, both caps are often irregular in shape, and marked irregularities of outline sometimes occur. This is especially noticeable in the case of the southern cap, which is quite eccentrically located with regard to the planet's pole. Another cause further limiting the range of the measurements is the sometimes gibbous shape of the planet, when a portion of the cap may remain unilluminated on the dark limb. Even the mere approach of the terminator by darkening the disk is found to involve considerable subjective errors. (Report No. 32, 1.) As a result of these limitations we have found that the position angle of the northern cap can only be accurately measured between solar longitudes $\odot 60^\circ$ and 130° , and the position angle of the southern one between $\odot 230^\circ$ and 295° . Only 135° out of the whole orbit of the planet can therefore properly be used, and often not that. The observations are therefore confined to the times when the planet is near its solstices. But at these times when the caps are small, it is sometimes impossible to tell whether they are due to snow or cloud, and as Lowell and others have shown, they are then sometimes subject to considerable shifts of position, in

one case from longitude 30° to 54° according to *Lowell Bulletin* **33**, 201.

By the Photographic method as used by Trumpler the photographic plate was placed in the direct focus of the telescope, and the planet's image was therefore never more than 2 millimeters in diameter. The pole was located by means of points situated on the boundary between the light and dark regions, and also by measures of the location of numerous lakes, some situated in the light, and others in the darker regions of the planet. In order to be of any particular use at the present day, we should be able to determine the position of the pole within at least one degree, or 37 miles measured on the surface of the planet, or 0.019 millimeters on the scale of Trumpler's plates when Mars was nearest us, and $25''$ in diameter. This distance corresponds to a distance of $0''.22$. There is no question but that a star can be readily measured with this degree of accuracy, because its image is circular, and is for instruments of long focus about $1''$ in diameter, but it is not so clear that it is possible in the case of a relatively faint, indistinct, sometimes irregular, planetary detail. (See Trumpler's enlarged photographs.) These prints are 25 millimeters in diameter, and the distance between the silver grains ranges between 0.5 and 1.0 millimeter, average distance 0.7 millimeter. Dividing this last figure by 12.5 gives us the sine of $3^\circ.2$, or 120 miles as the distance between the grains measured on a great circle on the planet even when Mars was nearest to us. Trumpler made so many measures, however, that the resulting error of the mean is much less than this implies.

With regard to what we may call the Apparitional method of locating the planet's axis, we probably cannot do better than refer to Dr. Trumpler's criticism of some of my earlier results. He mentions the well-known fact that the estimates of the center of a round disk may be, and usually are, either too high or too low, and thinks that this should affect my measures. As I have pointed out in Report No. **40**, 8, which was in the printer's hands when Trumpler's paper was published, and which therefore he had not seen, this systematic error, unless my eye had changed, would have no effect whatever upon the computed position of the axis, but would only affect the latitude of the assumed equator. This effect was measured, and found to amount to a deviation of $-0^\circ.4$ of latitude, or 15 miles measured on the surface of the planet. The present investigation reduces this figure to $+0^\circ.3$, or 11 miles.

It may be mentioned here that Table II in Report No. **40** is not very clear, partly because it is too much condensed, and partly because the spacing of the successive columns as printed is liable to confuse the reader. It was published in order to show that even including the measures made in 1922 in the general computation, these results could not be properly combined with those of the six other apparitions. Table I of the present Report is computed in precisely the same manner, but 1922 is left out entirely in the general computation, and only shown in the special columns devoted to that year. Why we believe that it should

TABLE I.
LATITUDES OF THE STATIONS.

Sta.	Name	M	1914			1916			1918		
			☉	L	L—M	☉	L	L—M	☉	L	L—M
7	Thymiamata f.	— 2°0	8°6	— 2°0	0°0	53°0	— 2°2	—0°2	89°4	— 2°1	—0°1
8	Aromatum S. p.	— 2.8	6.4	— 3.7	—0.9	55.8	— 3.2	—0.4	83.1	— 1.7	+1.1
23	Maesia	—11.9	14.7	—14.8	—2.9	58.8	—10.1	+1.8	86.0	—11.5	+0.4
24	Solis N.	—22.9	14.2	—22.9	0.0	58.8	—24.2	—1.3	86.0	—21.6	+1.3
43	Titanum N.	—19.8	17.5	—19.8	0.0	50.6	—21.5	—1.7	87.2	—18.6	+1.2
46	Aesculapius	— 0.3	10.9	+ 2.3	+2.6	53.6	— 0.7	—0.4	97.1	— 3.1	—2.8
50	Laestrigonum N.	—14.8	24.1	—19.5	—4.7	70.2	—16.5	—1.7	95.7	—13.8	+1.0
58	Cimmerium N.	— 2.7	14.6	— 1.7	+1.0	44.2	— 3.0	—0.3	88.2	— 2.8	—0.1
94	Edom S.	— 3.0	17.7	— 5.2	—2.2	47.7	— 2.1	+0.9	90.2	— 2.7	+0.3
98	Furca N. f.	+ 8.0	4.7	+ 5.5	—2.5	63.2	+ 9.5	+1.5	89.5	+ 6.9	—1.1
	Mean		13.3		—0.96	55.6		—0.18	89.2		+0.12

TABLE I—CONTINUED.
LATITUDES OF THE STATIONS.

Sta.	1920			1922			1924			1926		
	☉	L	L—M	☉	L	L—M	☉	L	L—M	☉	L	L—M
7	130°1	— 1°8	+0°2	181°4	+ 1°6	+3°6	246°7	— 1°4	+0°6	300°4	— 1°4	+0°6
8	122.2	— 2.4	+0.4	192.8	+ 2.6	+5.4	244.2	— 3.4	—0.6	305.1	— 5.8	—3.0
23	130.2	—11.1	+0.8	182.6	—10.7	+1.2	257.9	—10.5	+1.4	326.0	— 8.2	+3.7
24	111.7	—22.9	0.0	181.5	—18.6	+4.3	237.0	—22.5	+0.4	[309.8	—17.4	+5.5]
43	143.6	—18.5	+1.3	170.1	—13.9	+5.9	241.7	—14.1	+5.7	310.9	—21.6	—1.8
46	143.6	+ 0.3	+0.6	197.8	+ 3.8	+4.1	234.3	— 2.5	—2.2	316.4	— 0.6	—0.3
50	122.9	— 9.4	+5.4	173.5	—12.6	+2.2	244.7	—12.2	+2.6	300.2	—16.7	—1.9
58	132.5	— 3.4	—0.7	183.4	— 0.5	+2.2	243.6	+ 0.8	+3.5	[308.1	—11.7	—9.0]
94	129.8	— 2.1	+0.9	168.6	— 0.6	+2.4	247.3	— 2.8	+0.2	311.9	— 0.6	+2.4
98	174.0	+10.1	+2.1	167.0	+10.9	+2.9	246.7	+ 5.5	—2.5	320.8	+ 3.6	—4.4
	134.1		+1.10	179.9		+3.42	244.4		+0.91	311.5		—0.59

TABLE II.
LATITUDE OF THE LAKES.

Sta.	Name	M	1914			1916			1918		
			☉	L	L—M	☉	L	L—M	☉	L	L—M
23	Maesia	—11°3	14°7	—14°8	—3°5	58°8	—10°1	+1°2	86°0	—11°5	—0°2
32	Phoenicis	—13.3	14.2	—14.3	—1.0	33.4	—13.1	+0.2
91	Ismenius	+45.5	24.0	+45.9	+0.4	59.3	+45.5	0.0	99.1	+45.2	—0.3
	Mean		17.6		—1.37	50.5		+0.47	92.6		—0.25

TABLE II—CONTINUED.
LATITUDE OF THE LAKES.

Sta.	1920			1922			1924			1926		
	☉	L	L—M									
23	130°2	—11°1	+0°2	182°6	—10°7	+0°6	247°9	—10°2	+1°1	326°0	—10°8	+0°5
32	120.0	— 9.0	+4.3	191.8	—11.8	+1.5	245.4	—15.3	—2.0	310.0	—16.3	—3.0
91	154.4	+44.3	—1.2	173.9	+47.8	+2.3	248.2	+44.2	—1.3
	134.9		+1.10	182.8		+1.47	247.2		—0.73	318.0		—1.25

be left out will be explained presently. The first three columns give the numbers and names of the stations as recorded on my map (Report No. 36), and their mean latitudes as given in *Harvard Annals*, 82, No. 5, and Report No. 40, based on my observations made at the apparitions of 1914, '16, '18, '20, '24, and '26, but omitting those made in 1922. This naturally makes some small differences in the third column. The remaining 21 columns in the two portions of the table are devoted to the seven apparitions. In 1926 only eight stations were used, as fully explained in Report No. 40, on account of the obvious extended changes exhibited in the two omitted regions. Under each apparition the first column gives the mean solar longitude \odot of the planet when the draw-

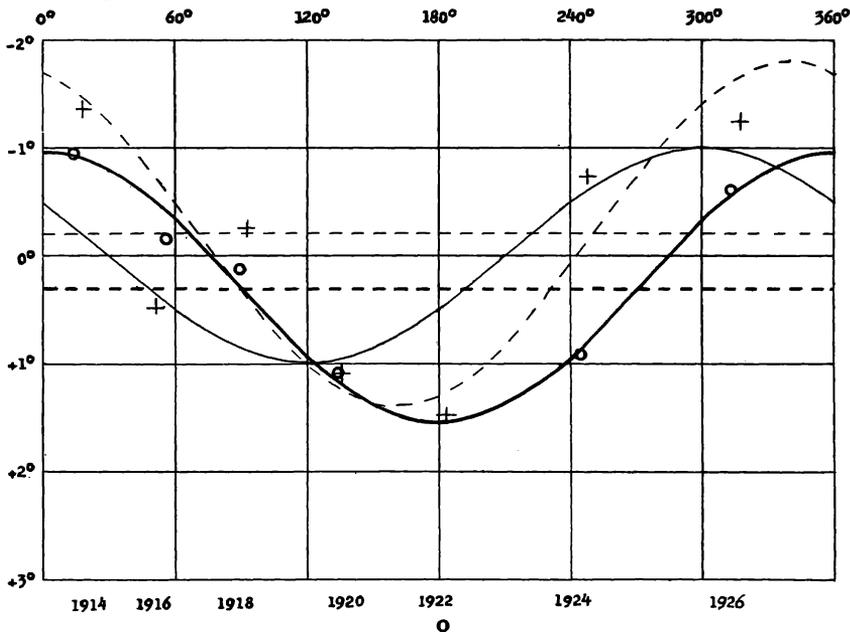


FIGURE 1.

APPARENT LATITUDE OF THE PLANET'S EQUATOR AT DIFFERENT SEASONS.

ings were made, the second the mean latitude of the station at that apparition, as measured on the drawings and published in the *Harvard Annals* through the year 1922, and the third the difference between that latitude and the mean latitude for the six apparitions, as given in the third column of the table. The numbers in the first of these columns are identical with those given in Report No. 40, the numbers in the third necessarily differ somewhat from them, but the resulting location of the pole is nearly identical.

In Figure 1 the zero ordinate represents the mean position of the planet's equator. The ordinates L—M marked on the left indicate its apparent shift north or south as determined by the *American Ephemeris*, at different times of the planet's year, that is when the planet is in different

portions of its orbit. The abscissas give the solar longitudes \odot which are always $87^{\circ}.5$ to $88^{\circ}.0$ less than the heliocentric longitudes, and indicate the season, counting from the northern vernal equinox at 0° . The small circles give the deviation of the equator at each apparition. These are plotted, both abscissas and ordinates, from the last line of the table. The dates of the former are given at the bottom of the figure. Now if the *Ephemeris* position of the pole were correct, if the stations were stationary, and the observations without error, it is clear that the result would be a series of circles lying in a straight horizontal line coinciding with ordinate 0° . Instead, however, we find that one circle, that of 1922, lies far below the others, and that the remaining six lie closely along a sinusoid whose node is in longitude 90° , and whose amplitude is $1^{\circ}.25$. Their deviations from it are $-0^{\circ}.04$, $+0^{\circ}.23$, $-0^{\circ}.20$, $-0^{\circ}.07$, $+0^{\circ}.07$, and $-0^{\circ}.06$, mean deviation $\pm 0^{\circ}.11$, or 4 miles. Trumpler suggested in his criticism of my earlier work that the reason that I obtained the large deviation from the *Ephemeris* indicated by the observations of 1922 was that my eye had changed, so as to make the center of the disk appear in a different position from that obtained in 1914 and 1918. If this were the case the change would presumably have continued, and not gone back in 1924 and 1926 to the sinusoid curve, and on the latter date to an ordinate lying between those of 1914 and 1916.

It is not believed now that the measures made in 1922 are in error, or are inferior in any respect to those made at the other six apparitions. I believe that an actual seasonal change of location of the vegetation upon the planet occurred. The following is my explanation of it. It is based in part on the measures of temperature made by Coblentz and Lampland at the Lowell Observatory during the summer of 1924, when the planet was exceptionally near the Earth. They showed that the temperatures of the maria were something like 20° F higher than the temperatures of the deserts in their immediate neighborhood, the exact reverse of what we find occurring upon our planet. (POPULAR ASTRONOMY, 1925, **33**, 371, and *Journal of the Franklin Institute*, 1925, June, July, page 67 of the Reprint.) The obvious explanation is an interesting confirmation of the view expressed long ago by the writer that the deserts are located at a higher elevation than the maria. Indeed there seems to be no other possible explanation of the difference. This difference is still another confirmation of our view that the planet possesses a very dense atmosphere,—an atmosphere much denser than that of our Earth, because while the differences of elevation upon it are small, yet the difference of temperature is marked, in spite of the small gravitational constant of the planet. About half-way between the northern summer solstice and the autumnal equinox the northern polar cap has nearly completely melted (Report No. **31**, Figure 1) and the water is travelling southerly, mostly through the planet's atmosphere. It is crossing the cold northern desert plateau, where much of it is doubtless absorbed, without producing much vegetation, save in the great depressions of Acidalium and the Syrtis. The remainder is dropping down to

fertilize the broad level plains of the southern maria lying just south of the equator. Vegetation soon begins to develop on their northern border, where the surface slopes up to the plateau, and which is more suited to vegetation than the colder more elevated regions still farther north.

This intermediate band of vegetation soon extends on an average to a distance of 70 miles farther north than it did before the water reached it. Most of the region thus covered lies well within the tropics, so that the temperature is controlled not by the seasons, but by the elevation. The moisture which at the northern autumnal equinox is so conspicuous as the southern polar cap is probably wholly derived from the southern maria, the moisture from the north never reaching it at all. In the mean time the slopes of the plateau, with the water supply exhausted, dry up, and by ten or twelve weeks after the autumnal equinox the vegetation on them has disappeared, leaving only that visible on the lower plains. At the opposite season of the Martian year, when the southern cap has melted, the moisture never reaches the deserts, but is absorbed by the maria themselves. This appears to me to be a simple and natural explanation of the temporary northerly advance of the maria as observed at the apparition of 1922. Owing to this advance, observations for the purpose of locating the planet's axis by the Apparitional method are not practicable for perhaps one-quarter of the planet's year. It thus appears that the Martian moisture, if the above explanation is correct, does not, as hitherto generally supposed, cross the whole planet from pole to pole, but at both seasons of its year has only to go half as far, to the southern maria and back again. It also appears that a large part of the northern water supply is wasted on the desert plateau, except for the comparatively small northern maria, which it will be noted, unlike the southern ones, turn blue instead of green when moistened. Some water is doubtless deposited on the elevated region north of Sabaeus, and on Elysium itself, on both of which regions clouds have been seen to form. The exceptionally narrow canals leading from them may then well carry the water down hill to Sabaeus, to the Syrtis major, and to the Charontis-Cerberus areas. (See map in Report No. 36, 2.)

A simple trigonometrical solution based on the above mentioned curve, Figure 1, places the pole P^1 in 1925.0 in $\alpha 316^\circ.18$, $\delta +53^\circ.66$, see Figure 2. The *Ephemeris* position for the pole for 1925.0 is, as we have already seen, in $317^\circ.63$, $+54^\circ.57$. Trumpler's position is in $315^\circ.90$, $+54^\circ.70$, and Lowell's latest one in $316^\circ.09$, $+54^\circ.34$. H. Struve's position, which was used for the ephemerides computed by Crommelin prior to those published by the *American Ephemeris* and was based exclusively on the orbits of the satellites, is shown for 1925 in $317^\circ.43$, $+52^\circ.72$. Lowell's criticism of it is given in his *Bulletin* 56. The mean position M^1 of Struve's, Lowell's, Trumpler's, and my locations, $316^\circ.40$, $+53^\circ.85$, lies within 13' of my position. The corresponding inclination of the axis of Mars to the pole of its orbit is $24^\circ.1$. If we

disregard Struve's position, and take the mean of Trumpler's, Lowell's, and my locations, we shall obtain the position M^2 in $316^{\circ}.06$, $+54^{\circ}.23$, lying within $6'$ of Lowell's position. The arrow indicates the direction, at an inclination to the meridian of $45^{\circ}.2$, and at a distance of $23^{\circ}.6$ of the pole of the planet's orbit.

If my explanation of the shift of the boundaries in 1922 is correct, there is certainly no reason to expect them to shift either continuously or irregularly at other times, but if any points on the planet do shift, we might think that those lakes situated on canals lying in an east and west direction should be as exempt as any markings could be from such latitudinal change. Indeed they would seem to be the ideal bodies to use in an investigation of this sort. The trouble with them is that there

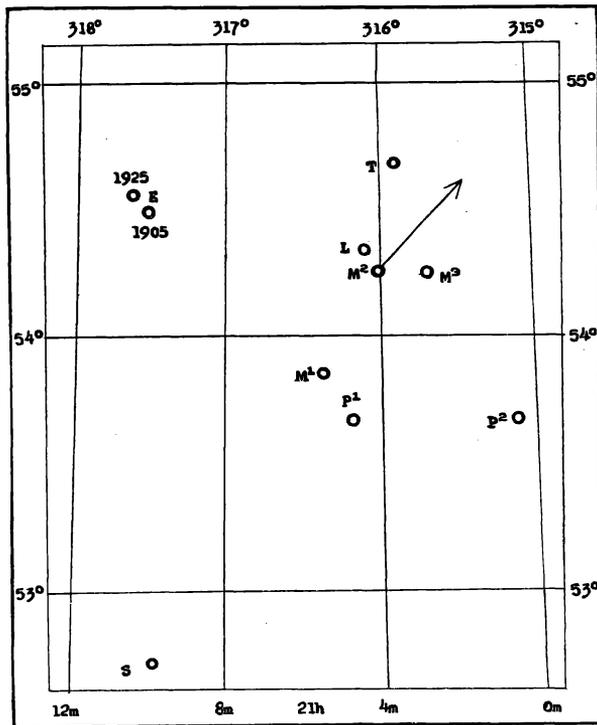


FIGURE 2.
LOCATION OF THE MARTIAN NORTH POLE.

are very few of them sufficiently conspicuous at all seasons of the year for our purpose. Only one, Maesia, was drawn at all seven of the apparitions. Phoenicia and Ismenia were drawn at six of them, but none of the others at more than five, and often only one or two drawings were made each year. In Table II these three lakes have been treated in precisely the same manner as the ten points in Table I, with one exception, namely that having found that the lakes exhibited no northerly

irregular shift in 1922, we have therefore included this apparition in taking the means given in the third column. This causes the mean latitude of Maeisia to differ slightly from that given in Table I. Owing also to the fact that the positions of all three of the lakes are based on more measures than were used in the original survey, they differ somewhat, as do the positions of those points recorded in Table I, from the values given in the *Harvard Annals*, **82**, No. 5. In 1924 the number of observations of Maeisia, Phoenicis, and Ismenius with the Harvard University refractor was 4, 4, and 1. In 1926 with my reflector 1, 2, and 0. The dotted curve in Figure 1, node 70° , amplitude $1^\circ.60$, shows the result of this study, based on the crosses derived from the table. The computed pole $315^\circ.12$, $+53^\circ.67$ is entered on the map, Figure 2, and marked P^2 . Combining it with Lowell's and Trumpler's positions, we get the point M^3 in $315^\circ.70$, $+54^\circ.24$. The measures on which this curve is based are so few, and the results so scattering that I do not put much confidence in its accuracy. Three conclusions may be drawn from it, however. It shows clearly that the marked irregular advance of the boundaries toward the north in 1922 does not occur in the case of the lakes. Secondly, it fully confirms the more accurate results derived from the boundaries P^1 , and the measures of both Lowell and Trumpler in showing that the *Ephemeris* pole is seriously in error, and so much so that its position should be corrected at as early a date as possible. Thirdly, the latitude of P^2 , confirming that of P^1 , further contradicts Trumpler's northerly position, which appears to me to be distinctly too far to the north. Indeed it is farther north than that of any other observer since Schiaparelli. The other two mean positions seem to me to be more probable than M^3 .

The question now arises at once, is Struve's position reliable? We accept as accurate of course his theory and computations, but what of his data? The computation depends on the observed distances and directions of the satellites from the center of the planet, and it must be remembered that during the greater part of the observations, unlike the other outer planets of our system, Mars presents a very marked phase, the satellite's orbits lying nearly parallel to its equator, and therefore strongly inclined to its orbit and to the direction of its phase. Also that by irradiation the image extends out beyond the limb on one side, and owing to the refraction of the planet's atmosphere, to a usually greater, but somewhat uncertain extent beyond the terminator on the other. With a marked phase, variations of seeing must strongly affect the result, to say nothing of the varying brightness of the surface markings, and the frequent presence of cloud, perhaps half the time, either on the limb or terminator. I have already stated my criticism of Trumpler's results, but must say here that it does not seem plausible to me that he could be more than a degree in error. Whether Struve could be more than that we obviously have no means of judging.

At first I was inclined therefore to reject Struve's position altogether. The only possible use of knowing the position of the axis of

Mars more accurately than we do is to study the relative and absolute *motions* of the markings on its surface, and the location of the new ones that may from time to time appear. We want an axis that will give stationary results for stationary markings at different seasons, and that we shall now get approximately, as is shown for the first time by the present investigation, by using either of the three mean poles M^1 , M^2 , or M^3 , as located from the work of the four Martian astronomers above mentioned. The heavy curve of Figure 1 indicates that we should get a still smaller shift throughout three-quarters of the orbit of Mars if we used P^1 for the pole. I have drawn in Figure 1 a similar sinusoid corresponding to the position of Trumpler's pole relatively to that of the *Ephemeris*. It is represented as a fine continuous line. It would indicate, if it was correct, that the average of my markings was anything but stationary in different years, that is at different seasons. On the other hand, in case most of the more important of the Martian markings are really stationary for the greater part of the year, then either his pole or my drawings are wrongly located, on the average by some 35 miles measured on the planet. But that is after all not very much, at a minimum distance of 35,000,000 miles. There is not any great error on either side with a total difference of $0''.2$. With Struve's pole every well-known point on the planet would have a marked annual (Martian) swing, averaging 70 miles, or a total range of 140 miles, in a direction nearly opposite to that indicated by Trumpler's pole. In my recent work in correcting the positions of my 100 known points on Mars, I rejected Struve's result, and adopted the middle mean position M^2 . During the course of the present investigation, however, I decided to include Struve's position in this Report, for the purpose of obtaining an alternative location of the pole. Indeed those sidereal astronomers unfamiliar with Mars would probably consider his determination as the best of all. It has the advantage that it draws the pole somewhat to the south, partially neutralizing Trumpler's extreme position, and I now feel that M^1 is probably more likely to be correct than M^2 . The distance between the two is $25'$ or 15 miles,—about the width of an extremely narrow canal. Their mean value for 1925.0 is $316^\circ.23$, $+54^\circ.04$. This position in any case cannot be far wrong.

It is perhaps a matter of interest to note that all the observers from Struve to Trumpler agree fairly well as to the direction of the Martian pole from the pole of its orbit, as measured in arc on the map at right angles to the line joining them, but that the important difference, about twice as great, consists in their values of the inclination of the axis of rotation, ranging from $25^\circ.3$, as given by Struve's position of the pole late in the last century, to $23^\circ.2$ as taken from Trumpler's position in 1924. The continued rapid decrease in the observed value of the inclination was noted by Lowell as far back as 1908, in *Bulletin* 33, 201. As early determinations, he cites Herschel in 1781, 1783 as $28^\circ.5$, Schroeter in 1798 as $28^\circ.0$, Bessel in 1830 as $27^\circ.3$. Later observers such as Schiaparelli from 1879 to 1886 give the value $24^\circ.7$, Lohse 1884

to 1894 gives $24^{\circ}.0$, Struve in 1895, based on observations of the satellites extending back to 1877 gives $25^{\circ}.2$, Cerulli 1896 and 1898 gives $24^{\circ}.8$, and Lowell himself in 1911 from his later observations together with those of the fourteen years previous gives the value $23^{\circ}.5$. (*Bulletin*, 56, 33.) For the period 1914 to 1926 the writer found the angle $24^{\circ}.1$. Finally, Trumpler in 1924, as above, gives the smallest value of all $23^{\circ}.2$. These results are plotted in Figure 3. In case the diminution were real, and should continue at its present rate, the axis of Mars would stand perpendicular to its orbit within 670 years. This is clearly impossible. No satisfying explanation has yet been found for the

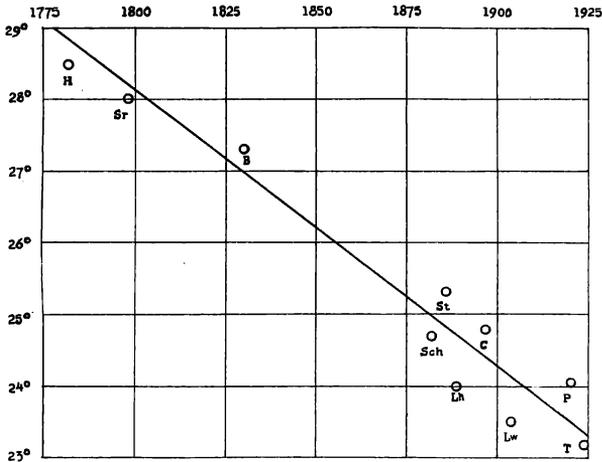


FIGURE 3.
THE INCLINATION OF THE AXIS OF MARS.

change. If real, there must be some reversing action. Polar Inversion, besides being apparently inadequate for such a rapid change, would act through the satellites as portions of a broken ring, only in the one direction of a diminution of the inclination of the axis to the axis of its orbit. (See "Why the Axes of the Planets are Inclined," *POPULAR ASTRONOMY*, 1917, 25, 487.) If due merely to errors of observation, probably the most plausible explanation, the continuance of the diminution at a uniform rate through the centuries is certainly a matter of interest.

With regard to future investigations of the location of the pole, it is probable that still better results could be obtained by confining the observations to a few points situated as near as possible to the equator, so that they should never transit far from the center of the disk, nor near the limb. Possibly some of the promontories between the bays, although they would have no definite longitudes, might prove to be as satisfactory as the bays themselves, especially if the promontory lay in a nearly east and west direction. Their latitudes at the time of transit of the central meridian should be measured with a micrometer, and preferably with a