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**A SUMMARY OF PLANETARY WORK AT THE LOWELL OBSER-
VATORY AND THE CONDITIONS UNDER WHICH
IT HAS BEEN PERFORMED.**

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A SUMMARY OF PLANETARY WORK AT THE LOWELL OBSERVATORY AND THE CONDITIONS UNDER WHICH IT HAS BEEN PERFORMED.*

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In accordance with the title, the present paper divides itself into two parts, of which the first will be a brief *resumé* of the planetary work done here, with a few details and announcements which have not heretofore been given to the public, and the second will present the writer's personal opinions of why it has been possible to reach these results. The latter must necessarily be more in the form of suggestions than facts, for to give facts one must have tried personally many widely separated localities over long periods of time.

Mercury was observed in the summer of 1896 immediately after work was begun with the 24-inch telescope. The observations were made by Mr. Lowell, assisted by Mr. Drew. Lines of various widths were seen and Schiaparelli's result was confirmed, that Mercury rotates once in its revolution about the Sun. Mr. Lowell describes the appearance of the planet as a chiaroscuro of black and white resembling the Moon or the satellites of Jupiter, more than planets holding atmospheres, such as Mars or Venus. He likens the dark markings to the areas on the Moon, known as seas.

Venus was observed at the same time by the same two observers and was also found to present continuously the same face towards the Sun. This result had been obtained by Schiaparelli and others, with less certainty than in the case of Mercury and had been most earnestly disputed by many other observers, so that Stanley Williams in 1893 considered the balance of evidence in favor of the 24-hour period. Mr. Lowell describes the markings on Venus as faint but yet readily seen on a reasonably good day. He made a good map of the planet. A straw color overlies the planet which he attributes to its atmosphere. He thinks there are few mountains. As there seem to be no indications of water vapor on the illuminated side, he argues that if such exists on the planet, it must have all passed to the night side and been deposited in the form of great ice sheets, leaving the bright side a complete desert. Mr. Lowell attributes his ease in seeing these markings to good atmosphere, daytime observations, and low powers; he also used colored glasses. The writer's experience has

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been that he never saw these markings with satisfaction until small apertures, from 2 to 5 inches, were tried. Certain lines were then unmistakable and were readily seen under varying conditions, such as different lenses, apertures, eye-pieces, position of eye, etc.

Mars, during its favorable season, has been a special object of attention. In 1894 Mr. Lowell found in its light regions a large number of new canals, and greatly increased the known number of lakes and oases, first recognized by Professor W. H. Pickering at Arequipa in 1892. His other most important work in 1894, was in tracing the seasonal changes taking place on the planet, facts which have the most direct bearing on its physical condition and its adaptability to habitation. The south pole of Mars was at that time towards us, and the south polar cap was observed to diminish as the Sun crossed the Martian equator toward it. The cap disappeared shortly after the equinox. Simultaneously with the diminution of the cap a dark line formed about it and the tint of the grey areas of the south temperate zone deepened to a beautiful bluish-green, directly suggesting vegetation. This darkening travelled progressively toward the north, reaching eventually in the northern hemisphere almost to the Arctic circle. Meanwhile the large southern bluish-green areas turned a distinct brown and finally after the complete melting of the polar cap faded to a slowly lightening yellow. All this is highly indicative of vegetation and its importance cannot be overestimated.

While Mr. Lowell was carrying on these observations Professor W. H. Pickering succeeded in detecting polarization in some of the larger markings bordering the melting cap but found none in the other large dark markings, an indication that the former are liquid and that the latter are not. And at the same time the writer discovered that all the dark patches in the southern hemisphere consist of a net-work of canals precisely similar to those in the northern hemisphere, save that the intervening spaces are filled with apparently the same coloring matter that gives the dark tint to the canals. "Dark-region canals" had been seen in succession by Professor W. H. Pickering, the writer and Professor J. M. Schaeberle, in 1892 but their character was not definitely decided upon.

Another investigation was pursued by him, namely of the projections and depressions observed on the terminator. A study of over 700 of these irregularities led to the conclusion that the projections are due to clouds and depressions to, simply, the deep

tint of the markings as they pass the terminator; and in a few special cases to cloud shadows and other causes associated with clouds. The peculiar feature of the conclusion is that most of the clouds form on the withdrawal of sunlight and not from the convectional currents as with us. This is made not only possible but probable by the fact that the Martian air decreases in density on ascent much more slowly than on the Earth, so that as the moisture dissipates towards the ground at sundown it is less likely to redissolve, and that from the desert character of the planet dust would be likely to exist in considerable quantities in the air, thus assisting in rapidly chilling it as insolation ceased and condensing the moisture. In particular on November 25, 1894, one immense cloud was observed on the sunrise terminator, having a minimum elevation of 15 miles above the surface. On the following night it had moved nearly 400 miles north and fallen to a minimum elevation of 8 miles, and separated into a number of smaller cloud-masses. On each date it was seen to disappear soon after entering sunlight and before it became projected on the illuminated disk of the planet. This and other observations indicating the absence of clouds by day, their formation at sunset, presumable presence over night and dissipation at sunrise, are of essential importance in explaining the high mean temperature of the planet which must be assumed if we regard the white polar caps as being composed of genuine snow or ice. In 1892 Professor Pickering suggested that such cloud action would account for the high temperature but we believe we have obtained real indications of it.

Throughout the observing season many measures of diameter were made. Upon reducing a large number, Mr. Lowell found that those somewhat separated from opposition gave, after correction, slightly larger values for the equatorial diameter than others exactly at opposition. This he attributes to a twilight effect by which in the gibbous phase the terminator set on by the micrometer and apparent to the eye was actually farther from the planet's center than its theoretical position.

In the opposition of 1896 many of these phenomena were observed but nothing specially novel has yet been deduced.

Of the asteroids, Vesta only has been observed and that very recently. It shows a polar compression of $\frac{1}{10}$, the major diameter being at present apparently within a few degrees of the direction of its orbit. If not exactly in the orbital path, it lies somewhere between that path and a line having 10 degrees greater position angle. Detail has been suspected which would

indicate a direct rotation in less than 30 hours. These observations were by the writer assisted by Mr. G. A. Waterbury, Jr. During the months of May and June the diameter of this minor planet was measured and found to be closely $0''.5$.

Jupiter itself has not been observed but its satellites have received much attention. To understand the full significance of the results, we must review the early observations of them. In 1892, Professor Pickering at Arequipa discovered their peculiarities of form. He found that the first rotated in $13^h 3^m$ and was egg-shaped, having the polar and one equatorial diameter of equal size while the equatorial diameter perpendicular to this one was some 10 per cent greater. It was by the change in form that he determined the rotation. The other satellites he also found of curious and almost inexplicable forms. He estimated that the second rotated in $41^h 24^m$ and that the third and fourth kept the same face towards Jupiter; but in regard to the last he subsequently expressed doubt, and was puzzled in regard to the third satellite. In the summer and autumn of 1894 Professor Pickering made some observations at this Observatory which have not yet received more than a passing inspection; as is the case also with the author's work with the 18-inch in the spring of 1895.

The writer's systematic work began in Mexico in February, 1897. During three weeks of superb seeing and on occasional good nights in the following months of May and June, enough material was obtained to show:—1st, that the first satellite was a spheroid of three unequal axes and that its ellipticity of form had perceptibly increased since 1892 and probably since 1894, that the mean diameter had probably decreased, that the rotation was direct and that its period had certainly been reduced to $12^h 25^m.8$; and a map was made exhibiting detail symmetrical with respect to its axis, that is, alike on opposite sides; 2d, that detail could frequently be seen on the second satellite, and a direct rotation is probable but no map was made; 3d, that the third satellite presents the same face towards Jupiter, thus having a direct rotation; a map was made of the markings; 4th, that the fourth satellite also presents the same face towards Jupiter; its markings are similar in character to those on the third and in addition appear to be symmetrical with respect to its axis. In the desire to verify this rotation period, no final map has yet been made.

During the present opposition attention has chiefly been given to the first and second. The second may briefly be dismissed by saying that many drawings of detail have been made, which may give the period and map. Observations of the first show that the

period is now $12^h 24^m.0$ and that in other respects it is practically unchanged from last year. Its elliptical form has been measured by comparison with a scale on every favorable night. Apparently during five hours it retains a ratio of equatorial to polar diameter of closely 119 to 100, usually called by us, simply, ellipticity 119. It then begins to decrease in ellipticity, at first slowly and then quickly until about 112 is reached when its curve of ellipticity turns a sharp angle, rising first abruptly and then slowly until at the end of an hour it has returned to 119. This minimum phase therefore becomes a phenomenon capable of very exact observation. In most cases it has been obtained to within a minute; yet sometimes it occurs as much as 20 minutes from its predicted time. The weather has prevented very regular observations on this point but there are indications that it depends on the relative positions of the first, second and third satellites.

Saturn has received a small amount of scrutiny, chiefly from Mr. S. L. Boothroyd and Mr. Waterbury, and Titan has been measured for diameter by the writer a number of times, but no systematic work has been done on this beautiful system.

Uranus has been an object of special interest to us during the past spring but announcements in regard to it will be deferred until its next opposition.

The only observations of Neptune have been measures of its satellites by Mr. D. A. Drew.

Such is our four year's work at Flagstaff and in Mexico.

The remainder of my paper, concerning the conditions under which this work has been performed, is less easily disposed of. The explanation that is usually offered for successful effort is that the seeing is better. But such statements in regard to the seeing are very loose because we judge the seeing by the number of things seen. The sentence "We see more at a certain place because the seeing is better," if translated into non-astronomical English, becomes "We see more at a certain place because we see more." We lack technical terms with which to describe the seeing with precision.

For some six years I have been interested in this matter of standard scales of seeing and since 1894 I have been making some attempts to investigate the matter systematically and this year some results have been published giving standard methods of measuring the conditions of the atmosphere, so far as yet

developed.* In 1897 I had adopted as the standard, the scale long since devised by Professor W. H. Pickering, which depends directly upon the form of the stellar image, and had made it more precise by limiting it to the form seen in the six-inch aperture. This is as follows:

In a 6-inch aperture,

- Seeing 0 Means a confused, enlarged mass.
2 means a confused mass not enlarged.
4 means disk well defined, no evidence of rings.
6 means disk well defined, rings broken but traceable.
8 means disk well defined, rings complete but moving.
10 means disk well defined, rings motionless.

With each record is given the vibratory motion of the image, in terms of the angular size of the disk, or in seconds of arc.

If I may be permitted I will digress so far as to give one extra refinement of this scale, which is in prospect. The atmospheric waves have been studied until it can be told which ones are inside the tube, which come from the dome, which come from the local surroundings and which are high up in the air and not to be avoided, and from this study there arises the conclusion that a standard aperture can be found in which the form of the image will roughly indicate the strictly local conditions of the air, and the motions of the image will indicate the general condition of the currents high overhead. The size will probably be between two and four inches. I will add this, that the twinkling of the stars is almost exclusively dependent on the general conditions of the upper air. That has been a valuable fact to remember when judging of the night from the appearance of the stars to the naked eye.

To return to our subject, it may be stated that during last November, December and January, which are among our worst months, the mean seeing at the zenith at Flagstaff, was 7.0 on this standard scale on clear nights and the mean vibratory motion of the image was 1".0. It was found that profitable work on Jupiter's satellites required seeing 8 with a motion of not over 1".0 or, rarely a seeing of 7 with reduced motion. Seeing 8 or over was recorded on 70 per cent of the nights included in these means. These measures of our average seeing during three

* POPULAR ASTRONOMY, June, 1898. In the several articles sent from here to the *Astronomische Nachrichten* of the last year, on this general subject of atmospheric currents, scintillation, etc., nearly every idea was obtained from the writer. The articles were all published during his absence from the Observatory.

comparatively poor months is a quantitative statement of one general condition under which we work. It will serve as a basis for comparison with any other place in which enough interest is felt to get similar measures.

Another general condition which is of great advantage to us is the fact that in the spring and autumn we are nearly always able to obtain from four to eight weeks in succession in which almost every night is of high quality. This consecutiveness of observation is of far more importance in planetary than in stellar work, because planetary changes are much more rapid. One may state as an axiom that the frequency of observation must be proportioned to the rapidity of change in the object studied. With double stars it makes no difference whether the given series of measures is made on five successive nights or on five nights separated by irregular intervals. But in planetary work the succession of changes is so rapid that if the observations are scattered at irregular and perhaps long intervals, the results of such observation, even though correct, convey to the mind only a chaotic confusion. It becomes very difficult to coördinate them and derive general laws. The withdrawal of confidence thus produced becomes a strong psychologic effect and explains why people, whose only experience has been of this kind, are so skeptical.

While lack of consecutiveness of observation thus explains rational slowness in accepting our results, the existence of this consecutiveness explains an important part of the certainty with which we speak of our work. Let us suppose an observer suspects or glimpses a new marking. If he sees the same thing on the following night it brings an assurance to his mind that would not occur if bad weather prevented his seeing the same faint glimpse till the third or fourth night after. By that the first impression had faded from his mind and he attributes the whole to imagination or imperfect vision. If in the first case he sees it not only the second night but the third and fourth and many more, his assurance is very strong.

Another example of the value of this consecutiveness of observation will not be amiss. At Flagstaff in fair seeing it is usually easy to become sure that there is detail on the third satellite of Jupiter, but the single view is not good enough to positively identify it. The detail is therefore usually drawn and located as accurately as possible on several successive nights in order to eliminate the possible misidentification that might occur in single observations.

It seems to be a frequent characteristic of our seeing that we obtain in or near the focus several overlapping images of small planetary objects and it is possible to connect at one instant the detail of one with the outline of another. Therefore one of the chief sources of wrong identification is in erroneous location of detail on this apparent disk. So much does this happen that after getting the most probable configuration it is next to impossible to accurately determine its location. Thus both for form and position of detail a succession of good nights is generally necessary. I have had a similar experience with Uranus. Occasionally the correct position angle of the belts has been recognized at the first glance, but more often several successive nights must be used so that each may corroborate the other.

Attention is called to a third point in this connection. An observer gets used to the idiosyncrasies of his own locality. He knows better than others how and when to catch the moments of favorable seeing and I believe that each astronomer should be regarded as presumably the highest authority on the capabilities of his own atmosphere in his special line.

My final remarks deal with our special methods of handling the instrument. In the first place we commonly use low magnifying powers. For Mercury and Venus we have used a power of 150, for Mars from 370 to 750, for Vesta 1000, for Jupiter's third and fourth satellites 500, for the first and second 750, for Uranus 370. With a low power there seems to be a great deal more variability in the appearances of planetary detail from moment to moment than with a high power, but the views are more frequent and easier. Mr. Lowell, who used low powers almost exclusively, has remarked to me that he saw the detail on Mars either exceedingly well or not at all. At that time he attributed this to a psychological effect but now it seems to be the characteristic effect of using low power. Again in using a power of 500 on the third satellite of Jupiter I usually see the detail intensely black and sharp or not at all. This has a marked similarity in its impression on ones mind to twinkling and it might be called the twinkling effect. With a power of 750 I occasionally see the detail very much better than in the 500 but such moments are much rarer and unless the night is very good, the use of 750 diameters requires far more constant and wearisome attention.

Although the detail in the lower power is far more easily seen, it is, I believe, not so well seen. For to me it sometimes appears distorted, wide at places, narrow elsewhere, and difficult to locate on the planet and in general less reliable than in the higher power.

A special advantage in using low powers arises from the possibility of decreasing the aperture and thus improving the seeing. This effect is one to be sought in any case and the reason of it is very simple but cannot be discussed at present. I have found that the most effective work can be done when the average apparent size of the markings is approximately the same as small type under ordinary conditions of reading. If the detail is complex it may be necessary to increase the power. Therefore the other custom of ours in handling the telescope is the use of diaphragms as small as possible. This is especially applicable in the case of Mercury and Venus whose light is much too bright for a 24-inch telescope. A newspaper or book page with so great an illumination would blind us at once. Diaphragming to three or four inches not only reduces the light of these interior planets to a better intensity but vastly improves the seeing. The amount of diaphragming depends on the exact work required. For coarse planetary detail the aperture can be very small. For fine detail more light is required, while for micrometer measuring the aperture must be larger, not to produce more light, but to keep the image steady in the field. In obtaining this steadiness, perfection of definition is lost and one must make the compromise between the two that seems to him best. The diaphragm must never be made so small as to produce obvious diffraction effects.

It seems to me that the habitual use of certain powers, coupled with certain illumination of the object studied, is an important cause of differences in drawings of planetary details among observers. Dr. Barnard, I understand, used powers of 500 and over on Mars and Professor Pickering and I certainly did. Between all three there is a specific similarity in representing detail. Mr. Lowell used low powers, 370 or below, and shows differences from us in representing detail, many of which differences seem to me to be due to the characteristic low power effects.

A criterion for determining the proper powers and aperture for planetary markings is under investigation. It depends upon what seems to me a rational analogy between the act of looking at markings on planets and that of looking at certain markings on white paper, namely printed letters, as one has been accustomed to do from childhood. The criterion may be expressed in this way: Whereas, in any atmosphere except a perfect one, the smaller the aperture, the better the seeing; therefore, for maximum effectiveness, simulate the ordinary conditions of reading to which the eye and brain have become accustomed, by the use of such powers as make the detail about the apparent size of small

type, and such apertures as make the apparent illumination of the planetary disk about that of printed matter under ordinary reading conditions.

This plan is not wholly new. Astronomers have long been accustomed to use lower powers when the seeing becomes worse. Now it appears that the really important thing is to reduce the aperture as well.

While we are thus guided in adapting ourselves to changing conditions, this criterion also saves experiment by supplying us at once with a constant relation between the power and aperture and size of detail. So that in search for detail of a given size we can assign at once the proper power and aperture, and if we see that detail easily and wish to try for other markings of, perhaps, half the average size, we have only to double the power and aperture; as will appear from the following investigation.

I find by measurement that the average size of newspaper type which I have been reading during the past three months is about $10'$ of arc or about one-third the apparent size of the Moon; and I have found by trial that a good reading illumination is $\frac{1}{1000}$ of full sunlight on the printed matter. Therefore putting:

a = aperture of telescope in inches

$e = \frac{1}{8}$ inch = pupil of eye under good reading illumination.

p = power

d = distance of planet from Sun in terms of Earth's distance

$s = 600''$ = apparent size of small type

z = largest dimensions of markings examined in seconds of arc

l = albedo of planet

$m = .7$ = approximate albedo of reading matter and

$i = \frac{1}{1000}$ = desirable illumination of reading matter in terms of the illumination of such matter held in full sunlight at distance unity from Sun.

We have, neglecting the absorption of the lenses,

$$(1) \quad p = \frac{s}{z}$$

$$(2) \quad i = \frac{1}{d^2} \cdot \frac{l}{m} \cdot \left(\frac{a/e}{p} \right)^2$$

in which $\frac{a}{e}$ gives the power at which the object theoretically has the same brilliancy as to the naked eye, because the emergent pencil exactly fills the pupil. The square of this power divided by the square of the power employed therefore is directly proportional to the apparent brilliancy of the object.

Transposing and taking the square root, we have:

$$a = dep \sqrt{\frac{im}{l}} \text{ or, putting } \frac{s}{z} \text{ for } p$$

$$(3) \quad a = es \sqrt{im} \cdot \frac{d}{z\sqrt{l}}$$

$$(4) \quad a = \frac{2d}{z\sqrt{l}} \quad \text{very closely.}$$

In accordance with (1) and (4) the following table has been worked out, in which l and d are taken from Young's Astronomy and Barnard's measurements of the asteroids, and z is assigned as carefully as possible from experience. It is undoubtedly also some function of the actual contrast between the markings and their surroundings.

Planet.	d	l	z "	Power.	Aperture. in.
Mercury	$\frac{1}{3}$.13	8	75	$\frac{1}{4}$
Venus	$\frac{2}{3}$.50	12	50	$\frac{1}{7}$
Mars	$1\frac{1}{2}$.26	1.0	600	6
Vesta	3	.7	.5	1200	14
Pallas and Ceres	3	.2	.6	1000	22
Jupiter's Sat.'s	5	.6	1.0	600	13
Titan	9	.5	.5	1200	51
Uranus	19	.64	4.0	150	12
Neptune	30	46	2.6	230	34

I cannot go more into details at present but while none of these results are with certainty in precise accord with experience, yet nearly all that have been tried are good approximations. Of course some of these smaller apertures are inadmissible in any case from diffraction effects.

So far as I know this is the first attempt to establish a principle upon which to determine the proper powers and apertures for planetary markings.

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