

conflicting reports received from the various witnesses of every great meteor testify to the need of observers prepared to gather trustworthy data from such rare phenomena.

It is true, of course, that the estimations still depend on the individual judgment, but with the constant standard of comparison and the rapid familiarity with brief durations which the apparatus affords it appears that more accurate estimates are certain.

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NOTE:—Mr. Lincoln LaPaz, of Wichita, Kansas, observed during 1915 over 1500 meteors, which he reported to the American Meteor Society. The last 600 of these were observed, using the instrument, described in the above article, to estimate their durations.

The details of these observations, which will appear in full in the 1914-1915 report, now about to be printed, were of such interest, that Mr. LaPaz, at my request, prepared a short account of his instrument for POPULAR ASTRONOMY.

Judging by the consistency of his results and their evident accuracy, all members are urged to have a similar one made for their use in future. The estimate of a meteor's duration is certainly the most uncertain of all the data our observers try to obtain, and the use of this simple device should make their estimates several times more accurate.

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REPORT ON MARS, No. 16.

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After a brief reference to a suggestion of Professor Russell, which will serve excellently as an Introduction, this Report will be devoted to a *resumé* of a few of the more important conclusions that we have reached as the result of a preliminary study of our observations during the past opposition.

PROFESSOR RUSSELL'S SUGGESTION.

In the *Scientific American*, March 4, p. 248, Professor Russell devotes a considerable portion of his space to a discussion of the appearance of the canals of Mars as described by various observers. As one of the four specialists, to whose work he particularly refers, the writer may say that he considers the article an eminently fair one, and that the explanation given for the fact that Dr. Lowell draws the canals very narrow, while some of the other observers draw the majority of them fairly wide is probably the true one. This explanation in brief is that it is merely a question of the personality of the observer.

Professor Russell then goes on to suggest that a series of disks should be prepared by a committee, and these disks observed by the four specialists through their telescopes, none of the observers having previously seen the disks near at hand. By means of a comparison of these drawings Professor Russell thinks it might be possible to determine the real nature of the markings on Mars. The writer, having used in the course of his observations various artificial disks, from the 8-foot round canvas screen erected in 1892 at an altitude of something over 16,000 feet in the Andes, and which was observed from a distance of eleven miles,* to the 2-inch glass plate placed in front of an oil lamp and condensing lens, and viewed from a distance of 1100 feet in Jamaica,† would like to make a few remarks on this suggestion.

Suppose that one of the disks prepared by the committee contained simply two lines, one very narrow and black, the other wider and fainter. It is quite possible that Dr. Lowell would represent these markings by two narrow dark lines exactly alike. It is possible that Professor Barnard, who is placed by Professor Russell at the other extreme of the observers, would represent them by two faint broad bands, while M. Jarry Desloges and the writer who in Professor Russell's opinion occupy an intermediate position, might, if the markings were faint enough, represent them both as lines, and if they were darker represent them both as bands.

Now what would the committee learn by a comparison of these results? In the first place they would learn that all four of the observers had represented the markings incorrectly. In the next place they would learn the peculiarity of each observer's method of representation, which would in this case be what we have here assumed. But would the committee learn anything about the canals on Mars? They would in part have explained the discrepancies in the observer's work, but I believe that is all. The same would be true of any other markings that the committee might have prepared.

Among the four or five larger and more conspicuous canals one can often see that there is a real difference in width, but we can also occasionally see that there is a real difference in blackness. Now what can we say about the fainter canals that are so difficult that they can only just be distinguished? Differences in width seem to be more frequent than differences in blackness, and are therefore generally assumed by the writer to be the real cause of the difference in appearance of the canals, but it is quite likely that in some cases both causes affect them. To come back to the final, and also the initial question,

* *Annals of the Harvard College Observatory* 32, 117.

† *Report on Mars*, No. 11.

however, are the markings in reality canals at all, that is continuous narrow dark bands or lines, or is the appearance due simply to comparatively broad irregular areas of unequal density and size? Some evidence bearing on this question has been collected during the past opposition.

APPEARANCE AND NATURE OF THE CANALS.

About March 1 the character of some of the canals observed changed very markedly. Prior to that date most of them had existed as broad smoothly curved lines. While many of these remained visible, there now suddenly appeared a number of new canals, very narrow, and some of them strikingly straight and artificial looking. Their advent had been foreseen, and indeed referred to in our first report, as well as in several others. Thus in No. 8 we say "at the next opposition he [the writer] expects to draw them appreciably more narrow than in his present series of sketches." Although anticipated therefore, from earlier studies in Peru and elsewhere, it must be admitted that the suddenness of their appearance was a little startling. On February 28, ω 30°, M. D. May 18, seeing 10, no canals were detected on the disk save Deuteronilus, which appeared as a broad faint band some 300 miles in width. Two nights later March 1, ω 35°, M. D. May 19, seeing 8 and 7, the band had narrowed to about 0.3 its former width, and five other canals had appeared, all of them comparatively narrow and sharply defined. While the seeing was inferior to that on the night of the earlier observation, the canals were both sharper and distinctly narrower. What was particularly striking in some instances was their straightness. It gave them an artificial appearance that should be seen to be fully appreciated.

March 8, ω 332, the canal known in different sections as Protonilus and Deuteronilus stretched in a nearly due east and west direction, as an apparently absolutely straight line, from Coloe Palus to Siloe Fons, where it was joined nearly at right angles by the equally straight Gehon, slightly wider, and about three-fifths as long. Doubtless both canals were arcs of large radius, but they appeared absolutely straight to the eye. The width of Protonilus was measured as we shall see later, and found to be 94 miles. Its length was 1860 miles, or practically just 20 times its width. If the reader will draw on paper a rectangle five inches long by one-quarter of an inch in width, he will get somewhat the effect that this canal produces. Save the lake, Ismenius, located at its middle point, it did not seem possible that it contained any irregularities or sinuosities equal to its own breadth, nor perhaps even half as great.

On March 15 Mars was in conjunction with the Moon, the two bodies being at the same altitude, and some 5° apart. The Moon was 3.5 days short of being full, and the coarser lunar canals visible with a field or opera glass, and represented in Report No. 6, Figure 1, were conspicuous. Mars was examined with a power of 660, which brought it up within a distance of about 100,000 miles. The Moon was examined with a power of 4, which brought it within a distance of 60,000 miles. Objects of the same size upon their surfaces would therefore have about the same angular dimensions. It is recorded after alternately examining first one and then the other, that the two northern canals of Imbrium were just about as distinct as Thoth and Nilosyrtris. These are at present the two most conspicuous canals on Mars. They were seen on a much darker background, but had about the same apparent breadth as the two lunar canals. The Copernicus canals were not so strongly marked at this phase, and the components of the narrow double in the south-western quadrant were much narrower.

On March 21 it is recorded that Cerberus, another very distinct canal, was just about as clear and wide as those near Copernicus with the field glass. The Moon was then two days past the full. The writer takes this opportunity to state, that since Report No. 6 was written he has found that certain of the lunar canals there mentioned had been previously described and pictured by Dr. Cerulli, Flammarion II, 317. Dr. Cerulli's drawing is a little difficult to understand at first, as he does not show conspicuously the generally recognized features of the Moon, but by means of the description in the text, it is evident that several of his canals are identical with those shown in our report.

If instead of viewing the Moon with a field or opera glass, we use a small telescope magnifying only ten or twelve times, these lunar canals entirely disappear. Even with magnification eight they are hardly visible. In their place we find a more or less continuous line of broad shaded areas of irregular shape, and of varying and irregular density. If we could use double the magnification on Mars, let us say 1300, would the same result be obtained? The writer uses habitually a magnification of 660, half as high again as anybody else, but to increase the magnification further would be worse than useless, less instead of more would be seen.

But suppose the canals of Mars are of irregular outline and density, and not smooth and straight, what of it? So too our cultivated fields would appear to an observer on the Moon. The lunar canals act in a very different manner from the Martian ones, and are not subject to such singular, irregular, and unaccountable changes of appearance and position. The writer wishes distinctly to state that it is not so much the *straightness* of the canals of Mars, as the public generally

imagines, and as Professor Russell in his article suggests, as it is their *arbitrary and unaccountable changes*, that make us believe them artificial. Even if the canals were really perfectly straight and uniform, it would certainly be impossible for us to prove it.

FUTURE POSSIBLE MAGNIFICATION.

It may now perhaps be asked is there any hope then that we may ever use a power of 1300 on Mars. Probably not at any observatory with the telescopes now existing, because these telescopes have all been carefully tried, and 660 is the highest power that has proved practicable anywhere. It has however been shown here that it is perfectly possible to use to advantage a magnification of 60 per inch of aperture on Mars, whenever the seeing reaches 8, even for an 11-inch lens. It is not believed that the seeing in Jamaica is any better than in many other portions of the tropics, where uniform temperature and atmospheric pressure prevail. It seems perfectly possible theoretically to use a power of 60 per inch of aperture even for lenses 20 or 25 inches in diameter, provided the seeing reaches as high as 10. In summer seeing 10 is often reached in the tropics, and 12 is not uncommon. But in the August opposition Mars is at only two-thirds the distance that it was this year, so that with twice the aperture, used at that season of the year, it might even now be perfectly practicable to obtain the desired enlargement of its surface details. In order to compute the necessary aperture of our telescope we will start with the fact that the lunar canals are all resolved into irregular areas when the magnification is such as to bring the Moon within 30,000 miles of us. To bring Mars within this distance at an August opposition would require a magnification of 1,200. Using a magnification of 60 per inch of aperture would require a 20 inch glass. To be on the safe side we will say a 24-inch telescope.

August oppositions occur once every thirty-two years, and the next one will come in 1924. Twenty-four inch telescopes are not now uncommon, and as the advantages of the tropical atmosphere for astronomical research gradually become more and more widely known, it does not seem improbable that some day someone will locate a telescope of this size somewhere in the tropics, and we may then add materially to our present knowledge in all those directions where the best of seeing is a matter of vital consequence.

WIDTH AND VISIBILITY OF THE CANALS.

On March 14 and 15 the widths of several of the canals were measured by means of the Canal Scale, Report No. 7. A magnification of 660 was used in all cases, two measures of each canal being made

when possible. The corrected mean diameter of the planet expressed in arc was $11''.67$, and the equivalent of $1''$ was 361 miles, or 580 kilometers. The widest canals measured, Nilosyrtris and Thoth were found to have a breadth of $0''.30$, and the narrowest Astusapes $0''.08$. The mean average deviation was $0''.025$, 9 miles, or 14.4 kilometers. The following widths were deduced, Protonilus 94 miles, Astusapes 30 ± 6 , Nilosyrtris 106 ± 12 , Thoth 106 ± 12 , Hyblaeus 42 ± 6 , Cerberus (mean) 94, Hades 94. An early measure of Cerberus was rejected as obviously erroneous. Its breadth varied appreciably in different places, being widest near Charontis. Astapus and Nasamon were both visible and rather difficult. They were the same width as Astusapes, 30 miles. The seeing varied from 9 to 11, so that this width may be considered about the narrowest visible when the planet is at the mean distance computed for these dates, 74,000,000 miles. When nearest us the minimum breadth would be reduced to about 15 miles.

The density of Nilosyrtris and Thoth was equal to the darkest band or stripe one could make on white paper with a number 4 pencil. It is thought that a month or two earlier in the season they were somewhat darker, but not so dark as the marking of a number 3 pencil. These measures may be compared with some made last November, see Report No. 13, and also with some made upon an artificial disk, Report No. 11, from which it was concluded that with moderately good seeing we should be able to detect a canal whose width was only 9 miles, if it were absolutely black.

Comparing these results again with observations made on the same night upon the two canals visible on the outer slopes of the lunar crater Aristillus, colongitude $39^\circ.8$, we find that with our 3-inch finder and magnification 240, the Aristillus canals are a trifle easier than Astusapes in the 11-inch. The breadth as measured with the finder was $0''.18$ or 1100 feet. As measured with the 11-inch $0''.13$, 800 feet, or 240 meters. This may indicate that with a still more powerful telescope than the 11-inch, the equivalent breadth of Astusapes might be also reduced, so as not to much exceed 20 miles. The density of the Aristillus canals was the same as those of Mars, that of a No. 4 pencil. Both Nilosyrtris and Thoth could be detected, although rather faintly in the finder. They were easier early in the season, and were not difficult at that time. With the 11-inch, Nilosyrtris and Thoth were both much easier than the Aristillus outer canals, while Astusapes was clearly more difficult.

ARYN, ELYSIUM, THE SNOW CAP, AND VEGETATION.

While for many years changes on Mars have been recorded by all careful observers, it was not until the last two years that they have

been recognized as such a constant feature of the planet, and expected to occur anywhere at any time. Accordingly a universally accepted feature such as Aryn was formerly believed to be constantly present. The feeling was, that if it was not seen, it ought to have been, and so it was put down in the drawing whenever that part of the planet was visible, whether it was really seen or not. This, it is believed, accounts for its appearance on so many of the drawings made during the opposition of 1913-14, when it is now practically certain that it was not really visible at all.

In point of fact Aryn did not appear this year before March. On the first it was strongly suspected, but not surely seen. ω $3^{\circ}.9$, \odot $61^{\circ}.5$ M. D. May 19, seeing 8, 7. March 3 it was recorded as pretty certainly there, but even on March 5, ω $0^{\circ}.5$, although certain, it was only very faintly marked, seeing 9. The appearance was little more than an indication of Aryn, for it consisted in a very shallow notch in the northern border of the bay of Sabaeus, with a scarcely noticeable lightening extending about one-third way towards the southern border. Indeed it would scarcely have been seen had not a special search been made for it. In 1914 it was not until April 14, three months after opposition, that Mars reached the same solar longitude \odot , that it had this year March 1.

The same remark is true of the pentagonal form of Elysium. Doubtless at certain seasons Elysium is pentagonal. Observers believed it was always so, and drew it accordingly, as a matter of habit. One cause of these errors is that observers have heretofore used too low powers,—possibly all that their aperture or their atmosphere would permit, but still too low. Mars is a very small object at the best, and the details of its surface minute. To see them well, and make no gross errors, we must have a reasonably large aperture, and we must have good seeing, and high magnification. Under these conditions, and a magnification of 600 or 700, the planet may be less distinct than with a power of 400, and certainly less pretty as a show object, but the observer familiar with its surface will see all its generally accepted details, such as the two above mentioned, more certainly, and can draw them more accurately, than when they are so small as to be on the very limit of distinct vision. If he is interested in hunting for fine canals, he may later use the low power, and insert what he sees on his previous sketch, but that is the only way that it should be used.

There is another interesting feature about Elysium as it has been seen at the past opposition. Its outline this year has been nearly circular, and the inner edges of the bounding canals, Cerberus, Styx, and Eunostos were all sharply defined, while their outer edges were less so. This is precisely the reverse of what is shown in Schiaparelli's

drawing in May 1888, when the outside is sharp, and the interior hazy. Flammarion I, 423. We have already seen in Report No. 13 that the advancing or eastern side of the north and south band was sharp while the other side was hazy. We have also seen by Report No. 12 that there was evidence that Elysium was subject to changes in size. It therefore appears possible that Elysium has been changing its size and growing smaller, while in 1888 it was growing larger. This suggestion is corroborated by measures of the drawings made hitherto this year, although the change found is so small that it can not be called conclusive. The mean inner meridional diameter in January measured 859 miles, in February 806, and in March 778. Both inner and outer edges are now fairly sharp, so that it seems likely that the minimum size has been attained. It is now clearly smaller as well as rounder than it was early in the previous opposition of 1913-14.

The snow cap is now appreciably smaller than it was at the last opposition, at the same season of the year. Since as we have already seen it was much larger earlier in the season than at the former opposition, we can hardly doubt but that they have been having rather warmer weather on Mars this year. It will be of interest to note if this means a warmer summer than usual for our Earth.

It may be mentioned here that although we have watched the planet under very satisfactory circumstances throughout the whole of the spring of its northern hemisphere, yet not a trace of green has been detected in that region. The impression gathered has been that all the dark northern areas, and most of the canals observed have been simply marshes, or moistened soil. Possibly greens will appear in their autumn, or before. On the other hand the greens of the southern hemisphere, during their autumn, have been at times very striking. They have now all disappeared.

SHIFTING FEATURES, AND PERIOD OF ROTATION.

In our last Report we referred to the large bay similar to Titanum which had recently appeared at the junction of Cimmerium and Laestrigon, and was shown on our drawings of February 7, 9, and 12. This bay is also shown in four drawings made by two of our Hawaiian correspondents, Mr. Midkiff and Dr. Romberg, on the evening of February 17. It appears again of large size on two drawings made February 10 and 14 which have recently been received from Mr. L. J. Wilson. On the latter drawing it is just about equal to Titanum, which is also shown not far from the limb. This drawing furthermore agrees in showing Titanum unusually far to the east of Charontis, that is in a very low longitude. In our last Report we gave it as the result of the

mean of the transits taken in December, January, and February as located in longitude $156^{\circ}.4 \pm 0^{\circ}.8$. It was impossible to obtain its position accurately this past month, but on March 29 it was found to be but little over 159° , which indicates that it is now gradually shifting back to the position given to it on the map, in longitude 170° . The shift recorded is equivalent to $13^{\circ}.6$, a distance of 500 miles, or 800 kilometers.

The following determinations of the longitude of Margaritifer at the mouth of the Indus, have been secured by different observers:—

| | | | | | |
|--------------|---------|----------------|----------------|------|----------------|
| Schiaparelli | 1877-79 | $17^{\circ}.3$ | Lowell | 1903 | $20^{\circ}.7$ |
| Wislicenus | 1890 | 23.9 | Lau | 1909 | 24.5 |
| Pickering | 1892 | 17.2 | Jarry Desloges | 1912 | 15.5 |
| Lowell | 1894 | 16.6 | Pickering | 1916 | 15.1 |

This last determination was made March 5, and rests on a single transit only, but serves to confirm the value of M. Jarry Desloges, whose observation indicates that the shift since 1909 has been about 350 miles, corresponding to an interval of 40 minutes in the time of transit. Since this point is near the equator it is readily seen, and its position can be determined with considerable accuracy. Observers do not usually give average deviations, but with careful work they should lie between $0^{\circ}.5$ and $2^{\circ}.0$, the corresponding errors being 2^m and 8^m in the time of transit.

The longitude of the northern tip of the Syrtis Major was determined on the nights of March 9, 11, and 14, results $283^{\circ}.4$, 282.3 , $286^{\circ}.9$, mean $284^{\circ}.2 \pm 1^{\circ}.8$. Professor Barnard at my request made a determination March 15, obtaining 285° . This compares with Dr. Lowell's result obtained in 1903 of $284^{\circ}.77 \pm 0^{\circ}.36$, and that of M. Jarry Desloges in 1912 of $284^{\circ}.0$. These results agree very satisfactorily, and little more could be desired. If we go back twenty years however, after making the correction of $-4^{\circ}.8$ made by Marth in the Ephemeris for 1896, we find that the longitude of the Syrtis according to Wislicenus in 1890 was then $291^{\circ}.0$. By the writer's results at Arequipa in 1892 it was $291^{\circ}.3$, according to Lowell in 1894 it was $287^{\circ}.6$ and finally according to Lau as late as 1909 it was $289^{\circ}.4$. The mean of the first four results is $284^{\circ}.5 \pm 0^{\circ}.4$, the mean of the last four $289^{\circ}.8 \pm 1^{\circ}.3$, difference $5^{\circ}.3$, 183 miles or 293 kilometers. Whether we include Lau's recent determination among the early ones or not makes little difference in the final result. Schiaparelli's determination in 1877, $285^{\circ}.5$ agrees well with the latest figures.

The Syrtis Major is the most conspicuous marking upon the planet. There is probably no point on the whole surface whose longitude is more readily determined than its northern tip. A difference of $5^{\circ}.3$ corresponds to a difference of 22 minutes in the time of transit of the central meridian. It is inconceivable that either set of observers should

have made such a mistake. That the difference is not due to an erroneous rate of rotation is shown by the fact that after an interval of 39 years, Schiaparelli's determination in 1877 of $285^{\circ}.5$ is but $1^{\circ}.0$ above the present mean value. Indeed the only possible explanation of these discrepancies seems to be that the point has shifted across the planet's surface.

A very valuable piece of evidence on this question may be obtained from Dr. Lowell's careful measures of Aryn in 1894 and 1916. In the *Annals of the Lowell Observatory* I, 53 he states that as the result of his observations in 1894 it appeared that Fastigium Aryn passed the central meridian by "the unmistakable amount of twenty minutes behind time." In consequence, on the next page, he offers the suggestion "that the received time of rotation of the planet is a trifle too small." Twenty minutes of time corresponds to a correction of $4^{\circ}.8$ of longitude, and in 1896 Marth added this 20^m to his ephemeris. In consequence of this change, when we wish to compare earlier determinations of longitude with later ones, we must, as above stated, reduce the earlier values by subtracting $4^{\circ}.8$. In *Bulletin No. 60* Dr. Lowell says that his observations in 1916 show "the received ephemeris to be $11.73 \pm .12$ minutes behind time." This corresponds to a difference of longitude of $2^{\circ}.9$.

We must note that before it was the planet that was behind time, now it is the ephemeris. The correction in longitude must therefore be reversed. He accordingly in this bulletin computes a shorter period of rotation than the accepted value, instead of a longer one as he formerly suggested. According to Schiaparelli in 1879, the longitude of Aryn was $0^{\circ}.9$. Correcting this by $4^{\circ}.8$ gives us $356^{\circ}.1$. It appears therefore that in the 15 years between 1879 and 1894 Aryn increased its longitude by $3^{\circ}.9$ or 144 miles, and that during the 20 years between 1894 and 1914 its longitude diminished by $2^{\circ}.9$ or 107 miles. Lowell's latest results therefore very closely confirm Schiaparelli's original position. Indeed his final position differs but 37 miles from that found by the great Italian astronomer 35 years before. It is quite impossible that deviations of 20^m and 12^m should be due to accidental errors, for as Dr. Lowell very truly remarks in referring to the former, it is a quantity "raised well above the possibility of accident, since it is twenty-five times as great as the probable error of the observations."

In Flammarion's *Mars I*, 506 is given a list of the various determinations of the period of rotation of the planet published prior to 1887. From these Flammarion has selected the six which he considers the best, and takes their mean, arriving at the value $24^h 37^m 22^s.65$. The extreme values range between $22^s.603$ and $22^s.715$. These values are all based on a comparison of the early drawings of Huyghens and

Maraldi with later observations, the extreme range of dates extending from 1659 to 1881. This value of Flammarion's is the one hitherto used for computing the ephemeris, and is evidently very nearly correct. A shift in position of 1° at the end of 10 years will make a difference in the computed period of $0^{\circ}.0754$. An increase of $0^{\circ}.02$ in the adopted period to $24^{\text{h}} 37^{\text{m}} 22^{\text{s}}.67$ would bring Schiaparelli's and Lowell's latest observations into exact agreement, but it is clear that really reliable results will only be obtained by locating several points upon the surface of the planet, instead of using a single one as has been done hitherto.

DOUBLE CANALS.

On several occasions during the past opposition the double effect has been fairly evident here, particularly during the month of March, when the canals were very narrow and often straight. Thus on March 3, $\omega 335^\circ$, $\odot 62^\circ.3$, seeing 9, magnification 660, Protonilus and Deuteronilus stretched as a strikingly straight narrow band across the disk, and the double effect was clearly seen at intervals. This does not however mean that the canal was clearly double. Such certainly was not the case, but the effect that other observers have described was clearly seen. The canal possessed a geometrical straightness, and a small but perceptible breadth. Once in five or ten seconds it appeared for perhaps half a second to be composed of two extremely sharp and extremely delicate parallel lines. The rest of the time it was a uniform band. One of these appearances was evidently an illusion, the question was which. For several reasons it was concluded that the brief double effect was the illusion, and the band the genuine appearance. Perhaps the most convincing argument (*a*) was derived from this same canal a month later April 7, when it again took on the double appearance. Its breadth was measured at this time by means of the Canal Scale, and was found to be only $0''.13$, or 58 miles. With an 11-inch telescope even two stars must be $0''.41$ apart in order to be separated. Two dark lines require a somewhat greater separation, $0''.58$. Next (*b*) the two lines were separated by a space greater than either of them, such as we are all familiar with in the drawings of double canals. Such defining power is theoretically entirely out of the question with any lens. (*c*) Very good seeing 8 to 10 is absolutely necessary in order to show the double effect, but with our very best seeing 11 to 12, the effect entirely vanishes.

In Lowell Observatory Bulletin No. 5 Dr. Lowell explains how he observed the double canals Euphrates, Hiddekel and Gehon with a 6-inch aperture, and found the separation to be $0''.27$, $0''.26$, and $0''.28$ respectively. From this we gather that the same phenomenon was

seen by both observers. In order to separate a real double canal with a 6-inch aperture the separation must be at least $1''.06$. He also points out that apertures of 12 and 24 inches gave him approximately the same separation, that with the latter ranging from $0''.35$ to $0''.19$. In Bulletin No. 15 he states that "The doubles, like all delicate detail, appear not continuously, but by flashes of revelation according as the atmospheric waves permit of passage undisturbed." That is precisely the way they appeared to me. He further states that the breadth of the components is "about a third of the breadth of the light land between them." That is about my impression of them. From these facts it would appear that we both saw the same phenomenon, in spite of the fact that according to my measures the separation was less.

To explain the effect satisfactorily has hitherto proved a very difficult matter. My impression is that it is due to a slight shifting of the image due to the air currents. Usually this shifting is more or less continuous, irregular, and rapid, producing merely a slight blurring of the image. It is believed that occasionally this shifting momentarily becomes fairly regular like a vibration, owing to the action of the atmospheric waves, and that this lasts let us say for half a second or more at a time. We shall then get two images produced side by side. Such an appearance would be visible only for extremely narrow lines, such as are found on Mars, and under such circumstances both images might be distinct even if separated by but little over $0''.1$.

The amount of the shift would doubtless be different on different days and at different places, so that uniformity of result is hardly to be expected. In this connection Dr. Lowell's remark "Under faulty visual conditions the doubles appear wider than they otherwise would, for some reason not easy to explain" is of interest. Clearly that is exactly the effect we should expect to find on this hypothesis. See (c) above. We may readily demonstrate the effect mechanically, by viewing an ink line in a vibrating mirror attached to a tuning fork, or more simply by tying a very small mirror to the stem of a T-square placed horizontally, and projecting a few inches over the edge of a table. On setting the mirror in vibration, the reflection of the line, or of a pencil held a few inches above the mirror, will be clearly seen to be doubled.

The writer is not aware that this explanation has been previously proposed, but it seems to him after having studied the appearance with some care, that it accounts satisfactorily for the facts observed with regard to this singular and obscure phenomenon. It certainly appears more plausible than that the canals are really double, and separated by an amount theoretically incapable of detection in the telescope. It is of course possible that wide doubles may exist on Mars like the one

on Aristillus. That is however a divergent pair, widely separated at one end. None such have been seen on Mars by the writer as yet, and he believes that if they exist they must be of very rare occurrence. It has been thought best on account of the length of this paper to defer the record of our observations in March and April until the next Report.

PLANET NOTES.

 CLIFFORD C. CRUMP.

Occultations Visible at Washington.

| Date 1916 | Star's Name | Magni- tude | IMMERSION. | | | EMERSION. | | | Dura- tion | |
|--------------|------------------|----------------|----------------------|----|-----------------|----------------------|----|-----------------|---------------|----|
| | | | Washing- ton M.T. | | Angle f'm N. | Washing- ton M.T. | | Angle f'm N. | | |
| | | | h | m | ° | h | m | ° | | |
| July 16 | 96 B Aquarii | 6.5 | 18 | 29 | 339 | 18 | 40 | 318 | 0 | 11 |
| 23 | 17 Tauri | 3.8 | 13 | 34 | 25 | 14 | 14 | 297 | 0 | 40 |
| 23 | 23 Tauri | 4.3 | 13 | 58 | 90 | 14 | 54 | 230 | 0 | 58 |
| 23 | γ Tauri | 3.0 | 14 | 30 | 76 | 15 | 36 | 243 | 1 | 5 |
| 23 | 28 Tauri | 5.2 | 15 | 23 | 101 | 16 | 23 | 216 | 1 | 1 |
| 23 | 27 Tauri | 3.7 | 15 | 29 | 126 | 16 | 8 | 192 | 0 | 39 |
| Aug. 7 | b Scorpii | 4.7 | 7 | 7 | 115 | 8 | 27 | 268 | 1 | 20 |
| 10 | 127 G Sagittarii | 6.4 | 4 | 48 | 61 | 5 | 44 | 292 | 0 | 56 |
| 10 | 172 B Sagittarii | 5.8 | 5 | 46 | 44 | 6 | 38 | 305 | 0 | 52 |
| 10 | 189 B Sagittarii | 6.1 | 8 | 38 | 48 | 9 | 49 | 282 | 1 | 10 |
| 10 | 208 B Sagittarii | 6.1 | 12 | 30 | 69 | 13 | 35 | 246 | 1 | 5 |
| 12 | 29 Capricorni | 5.5 | 10 | 47 | 61 | 12 | 2 | 235 | 1 | 15 |
| 13 | ρ Aquarii | 5.3 | 15 | 57 | 106 | 16 | 38 | 188 | 0 | 40 |
| 13 | 170 B Aquarii | 6.0 | 17 | 44 | 84 | 18 | 34 | 218 | 0 | 50 |
| 15 | 22 Piscium | 5.8 | 7 | 37 | 93 | 8 | 24 | 213 | 0 | 47 |
| 17 | 101 Piscium | 6.2 | 8 | 46 | 75 | 9 | 38 | 235 | 0 | 51 |
| 24 | Saturn | 0.4 | 15 | 3 | 104 | 16 | 5 | 272 | 1 | 2 |

Planet Notes for July and August, 1916.

During the months of July and August the sun will move south and east. It will move from Gemini through Cancer, and into Leo. During this interval its declination will change from $+23^{\circ} 7' 34''.5$, to $+8^{\circ} 40' 35''.0$. Near the end of August the sun will be near the bright star Regulus. On July 29 there will be an annular eclipse of the sun. This eclipse will be visible in the southern part of Australia and Tasmania.

The phases of the moon for July and August are as follows:

| | | | |
|---------------|--------|----|--|
| First Quarter | July 8 | at | ^h 5 ^m 55.0 A.M. C.S.T. |
| Full Moon | 14 | " | 10 40.0 P.M. " |
| Last Quarter | 21 | " | 5 33.0 P.M. " |
| New Moon | 29 | " | 8 15.4 P.M. " |
| First Quarter | Aug. 6 | " | 3 5.6 A.M. " |
| Full Moon | 13 | " | 6 0.3 A.M. " |
| Last Quarter | 20 | " | 6 52.8 A.M. " |
| New Moon | 28 | " | 11 24.7 A.M. " |

The moon will be at apogee on July 28, and again on August 24. On July 14, and on August 12, it will be at perigee.