

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Observations of Mars.

IN NATURE of November 10, 1910, Mr. J. H. Worthington gives his interesting observations of the fine straight lines which he saw on Mars at Flagstaff, and expresses his belief that these "telegraph wires" are objective realities in the focal image. Although I have not seen Mr. Worthington's paper, yet I shall reply to it, basing myself on the accuracy of the abstract given of it in the Journal of the British Astronomical Association, vol. xxi., p. 130.

Now Mr. Worthington's very brief experience of the appearance of Mars during the few days spent at the Lowell Observatory is necessarily outweighed by that of an astronomer like Prof. A. E. Douglass, who spent several years in the planet's study at Flagstaff. But what was the conclusion of Prof. Douglass from his observations of the straight "canals," of which he saw more than anyone else? That they are optical "illusions" having "worked serious injury to our observations" (*Popular Science Monthly*, vol. lxx., May, 1907). It would be difficult to conceive a more decisive symptom of frailty in the "canal" question than this surrender to truth of its ablest exponent.

In discussing my work rather than the collective evidence of great telescopes (of which my results form an integral part), Mr. Worthington seems to show some misapprehension in the very object of his criticism, for my conclusions are identical with those arrived at at Lick, Yerkes, and Mount Wilson. Thus, in 1895, Prof. Barnard, summarising his evidence with the 36-inch at Mount Hamilton, said:—"No straight, hard, sharp lines were seen on the continents, such as have been shown in the average drawings of recent years" (*Monthly Notices, R.A.S.*, vol. lvi., January, 1896, p. 166). On September 21, 1909, I state that "those geometrical spider's webs . . . do not exist" (*Journal of the British Astronomical Association*, vol. xx., p. 141). A fortnight later Prof. Frost telegraphs:—"Yerkes telescope too powerful for canals." Lastly, on January 3, 1910, Prof. Hale proclaims "the perfectly 'natural' appearance of the planet" in the 60-inch reflector, by far the most perfect and powerful instrument ever made, "and the total absence of straight lines" (*Journal of the British Astronomical Association*, vol. xx., p. 192).

It would thus appear that Mr. Worthington is perhaps attempting to revive the old controversy on the relative merits of large and small telescopes. But that question has been settled long ago, so that any attempt to renew it can no longer deserve serious consideration. The overwhelming superiority of large instruments has been often demonstrated on double stars, for the two discs seen in a great aperture will be blended, by increased diffraction, into a single mass of light with an 18-inch; and, as the smaller star is observed to revolve in perfect harmony with Newton's law, there can be no doubt whatever as to its objective existence. The same fundamental principle holds good for planetary detail. Two contiguous, irregular, bright spots on Mars in a 33-inch will appear as a single round spot in an 18-inch. Hence delicate objective markings, which are quite plain in large glasses, cannot be defined at all with inadequate instruments, and this well-known rigid demonstration establishes for ever the hopeless inferiority of small telescopes.

The advantage of great objectives I have further shown on Mars when stating (December 23, 1909) that the geometrical network vanished in perihelic opposition of the planet, while much more delicate detail was quite plain (*Journal of the British Astronomical Society*, vol. xx., p. 141). On September 20, 1909, under perfect seeing, I can discover no straight lines, but draw Lacus Moeris as a vast shading, and Deltoton Sinus triple (letter to Schiaparelli, dated September 21, 1909). A fortnight later

the same region of Mars is photographed at Mount Wilson, and Lacus Moeris comes out likewise as a vast shading, while the triple structure of Deltoton Sinus is also confirmed. On November 3, 1909, at Flagstaff, the "lake" is missed (although covering fully one-sixth of the diameter of the planet), and Deltoton Sinus appears single, while a host of lines furrow the surface (*Journal of the British Astronomical Association*, vol. xx., pp. 376-7). But the fact that straight lines are drawn when more delicate detail, confirmed by photography, is missed, constitutes another proof, not only of the inadequacy of the 18-inch as compared with the 33-inch, but also of the inanity of the "telegraph wires."

Yet my position in the "canal" question should not be misunderstood. If by "canals" be meant straight lines, then I think the "canals" do not exist; if we mean irregular, more or less streaky markings, then the "canals" exist. Of course, it would be utterly illegitimate to speak of genuine canals on Mars. But in the positions of Schiaparelli's lines I often saw, with the large telescope, either (a) complex, irregular, knotted, or winding bands; or (b) jagged, isolated, dark spots; or (c) indented edges of differential shadings. Under good seeing, the irregularities of these objects were held steadily from five seconds to several minutes. From my experience of the "canals" since 1894, with various apertures, I am led to account for the single and double straight of lines of Schiaparelli as follows: over the objective substratum of irregular, sinuous corrugations diversifying the Martian surface, a tired eye will discover by flashes a geometrical appearance. Impressions of single lines will fleet now and then either over a narrow objective streak or over the jagged border of a half-tone, while double parallel lines will flash in the position of a broader band. But, as pointed out by Mr. Maunder, the straight lines (which, so far as my evidence goes, are usually glimpsed severally, and not collectively) are merely optical summations of groups of minute irregularities beyond the reach of the instrument used. Prof. Lowell may justly feel proud upon having succeeded where all his predecessors failed, and upon having photographed the irregular streaks of Mars by ingenious methods, devised at his observatory.

A new notion was recently introduced in science by the "born-good" and "born-bad" air of some localities; but the splendid results of Dawes, Lockyer, Burton, Green, Denning, and others in the British Isles (a country most unfavourable to telescopic work), prove that the difference between the best and worst observing stations is largely a difference of duration of good seeing. Transparency of air, which is indispensable in detecting faint stars or nebulae, seems to be of little moment in planetary detail. When minute Martian irregularities, beyond the reach of an 18-inch at Flagstaff, are held steadily near Paris with a 33-inch; when such detail is corroborated by the unanswerable testimony of photography; and when the blue cap of Saturn is a most conspicuous feature at Meudon a whole year before the recent Solar Congress, we are bound to admit that any point on the earth's surface may give us short spells of perfect seeing.

E. M. ANTONIADI.

Paris, December 28, 1910.

Sir Ray Lankester's Book on the Okapi.

SIR HARRY JOHNSTON is wrong in suggesting (*NATURE*, December 15) that the incompleteness of my monograph of the okapi is due to the "financial control" (presumably he means the trustees of the British Museum) disliking the expense of publishing a volume of text. The full expenditure required was approved by the trustees when I was director of the museum. The absence of any further text than that which accompanies the plates and figures in the volume, as issued, is solely due to the fact that I have not provided such further text.

It would have been better to call the book "Contributions to a Knowledge of the Okapi" rather than a "monograph" of that animal, since although it is in the strict sense a monograph, it does not profess to give (as Sir Harry Johnston seems to think that word implies) a *résumé* of all that is known and has been written on