and I wish to make out a perfect vindication, hoping never again to be obliged to recur to it. If you will refer to my reply to Peters, you will see that I speak of our difference in R.A. and stop, coming to a full pause. I then take up the subject of Dec., and when through with that, make another period. Then I say, "Thus the matter rested until NATURE pointed out the error, &c." Is not your language about as unlike this as can well be? In response to your wish to be able to tell your readers "how this sudden illumination caused the scales to fall from my eyes," I hope the above explanation will prove full and clear to all.

Your second charge, "hesitancy about the matter," is a new one, and so at variance with truth that necessity, even at the expense of being prolix, compels me to refute it, and to show to the world that this charge is as baseless as the other. How long did I hesitate? I answer, from the time of the eclipse until just two minutes after my arrival at home, when, though very weary and ill, and before I was seated, I consulted "Webb's Celestial Objects" to see how far Alcor was from Mizar. Then and only then was I able to fix on a definite distance between θ Cancri and, as I then supposed, the planet Vulcan, viz., about $7'$. I left Denver the next morning after the eclipse, coming homeward, both by night and day, as fast as steam could bring me, arriving at home on the P.M. of Saturday, before most of the astronomers had left Denver. I immediately despatched a messenger to the Editor of the *Rochester Sunday Morning Herald*, notifying him of my arrival. I was at once interviewed by him, and a full account was laid before his readers by daylight the next morning. Sunday P.M. I was interviewed by a reporter of the *Rochester Democrat and Chronicle*, which paper, the next morning, contained a long account of my observations, a considerable part of which was published in NATURE. As soon as possible I wrote the facts to the Astronomer-Royal, to the *Observatory*, to Admiral Mouchez, and made out my report to Prof. Colbert, of Chicago (the chief of the party to which I belonged), which, with those of the other members, was published in pamphlet form, also a more extended one to Admiral Rodgers, not yet published. Very little hesitancy in this I think.

I left Denver with Professors Colbert and Hough. On the way Prof. Hough asked me several questions regarding the distance between the two stars. I told him I was unable to give their distance in arc, neither could I think of two stars whose apparent distance was the same. I also said to him that the nearest approach to a resemblance which I could then recall were α^1 and α^2 Capricorni, but, not having observed them with such an object in view, would not say that they were sensibly the same. After they had left me—changing to another road—and before my arrival at Kansas City, and before night of the day of starting, the thought came suddenly to my mind that their distance apart was about equal to a little more than half that between Mizar and Alcor, whatever that might be, which could not be ascertained until my arrival at home.

Since the eclipse I have made many observations of θ Cancri and regions adjacent, to see if my judgment would allow me to modify in any particular my observations as made and published. I have even gone to a part of this city where the streets run parallel with and at right angles to the meridian, as they did at our camp, in Denver, and then wait until an imaginary sun some $30'$ west of δ Cancri had the same altitude and azimuth as had the real sun during totality. And, while I am not inclined to make any changes whatever, I will say that it cannot be denied that, as regards the distance and direction from the sun, they can only be considered as rough guesses, though this does not militate in the least against the existence of the new objects. That they are new I know, for they are not there now. I have never made a more valid observation, nor one more free from doubt regarding the genuineness of the objects seen, which, in my opinion, were circumsolar bodies, unquestionably intra-Mercurial planets. The view of them was as beautiful as it was unexpected, and it was with great reluctance that I could break away from the captivating scene. It must be borne in mind that my telescope was filled with a flood of light, with not an object for reference visible, and therefore, when I ran upon these two round red disks, equally bright, and so near together, it is not surprising that they made an impression upon my mind that never will be effaced.

The great field for future astronomical discovery will, without doubt, be the sun and his immediate surroundings. Let no man's prejudice deter him from taking part in such prospective discoveries, for the field promises rich rewards.

Though I have said above that I am not inclined to modify my published estimations, yet I am willing to say as follows:—If I were compelled to change the brightness of the two stars one magnitude, and say whether they were of the fourth or sixth, I should answer, the former. If I were compelled to change their distance from the sun half a degree, and say whether they were $2\frac{1}{2}''$ or $3\frac{1}{2}''$, I should say the latter. Again, if I were compelled to change their direction from the sun, and say a little farther south or north, I should unhesitatingly say the latter, or, as I said in my report to the Naval Observatory, south of west, instead of south-west. And, finally, were I obliged to change their distance apart, and declare whether they were $6'$ or $8'$, I should, without a moment's hesitation, say the former, or about the distance between α^1 and α^2 Capricorni.

LEWIS SWIFT

Rochester, N.Y., December 10, 1879

The Transverse Propagation of Light

IN NATURE, vol. xxi, p. 256, appeared a paper by Mr. Tolver Preston, on which I wish to make a few remarks.

The author does not make himself very clear as to what he supposes the effect of the vibrating molecules of gross matter on the ether atoms to be. From what I can gather, the effect on a small plane receiving the light from an illuminated "point" would be of the following nature:—When the molecule of gross matter was not vibrating, there would be a more or less shaded spot on the plane, but if the molecule vibrated, then this shaded spot would also vibrate in the same time, which would be possible, since during one vibration of the molecule an extremely large number of ether atoms would impinge on it, and therefore, a large number at each portion of its vibration. In what follows I shall suppose that this is the manner in which the light is supposed to be propagated.

1. The atoms are very small; the free paths are very long. In order that the acceleration of the sun on all the planets must be inversely proportional to the squares of their distances, this mean path must be comparable with the radius of Neptune's orbit; and in order that the light of the stars may be visible, it must be comparable with the distance of the furthest visible star. Again, since, as Mr. Preston says, the automatic adjustment to equality of direction is "of such a rigid character, that if the atoms were imagined to be disturbed or made to move in the most chaotic manner, they would, when left to themselves, instantly correct the irregularity," it follows that the time of describing the mean free path must be very much smaller than the "instantly" small time in which they "correct the irregularity." Their velocity, therefore, must be enormous. They must move to the farthest visible star in a very small fraction of a second. That they have a very large velocity also follows from the smallness of the atoms and the magnitude of gravitation. Now the velocity of light on Mr. Preston's theory must be the velocity with which the atoms move, a velocity which, as has been shown, must be enormously greater than 200,000 miles a second.

2. The above supposes the velocity of all atoms the same, which would not be true. If they varied in the same way as in a gas composed of atoms which do not influence one another, then at a distance from the illuminated point, after a few vibrations of the gross molecule, the shaded spot would not vibrate, but would become an elongated shaded spot without motion, and there would be no light at all.

3. The data of the theory are definite, and it therefore ought to be capable of explaining the laws of refraction and reflection, let alone those of diffraction. This it is incapable of doing; for the light that gets through must be carried by atoms which pass through without striking any of the molecules of gross matter; they must therefore pass through without change of direction or velocity, and therefore cannot be deflected.

These are three reasons, each of which by itself condemns the ingenious explanation offered by Mr. Preston.

W. M. HICKS

St. John's College, Cambridge, January 16

Mountain Ranges

It is to be regretted that Mr. Trelawney W. Saunders should make confusion worse confounded by noticing imaginary discrepancies based upon a mistaken assumption of a natural agreement. In his paper "On the Mountains of the Northern and Western Frontier of India," published in NATURE, vol. xxi, p.