When the paper was read, it appears that Professor Stokes objected that, although the contributing area of the gas would be increased in the way indicated, yet other stellar masses would at the same time be crowded into the line of sight, and that it need not therefore follow that the spectrum should become linear with increase of distance.

So long as the gaseous envelopes are considered to be isolated round each individual star, and to be of comparatively speaking small dimensions, the objection is a valid one; but the clusters which I had under contemplation were physical clusters, and this isolation of the gaseous surroundings would be, under suitable conditions, destroyed by the relative gravitation and heating effects of the several members of the system. It is to such a cluster only that my reasoning applies; and I think such will be seen to be the case if my paper is carefully read.

I need hardly say that, after I heard of Professor Stokes' objections, I carefully reconsidered the theory; but after the best consideration I can give the subject, I believe my conclusion is sound, and there the matter must rest for the present so far as I am concerned.

With respect to Mr. Proctor's own views: If he thinks that he has proved that, because a stellar cluster is irresolvable with a telescope of small aperture and small power, it must follow that "it would be of constant intrinsic brightness at all greater distances," I think there would be no great difficulty in proving that he is mistaken. I have introduced the words "small aperture" and "small power" as presenting the case in an exaggerated form, and merely to indicate the point upon which I think his theory fails.

I must also, as bearing on the same point, call attention to a very general statement of Mr. Proctor's, that "he has shown that by no optical contrivance can apparent brightness be increased, though quantity of light can of course be greatly increased." This statement certainly requires qualification; for, if it were true, why could not the satellites of *Mars* be as easily seen with a 4-inch as with the 26-inch of the Washington Observatory? I know, of course, what Mr. Proctor should, and probably does, mean; and I should not have called attention to this point had it not been the fact that the principal cases of exception are those which have been under consideration in the present paper.

Note on Specular Reflexion from Venus.

By W. H. M. Christie, Esq.

In the *Monthly Notices* for December 1876, I gave some observations on the gradation of light on the disk of *Venus*, made with the object of testing Mr. Brett's hypothesis of specular reflexion

modified by atmospheric diffusion. The position of the brightest point of the disk, determined with all possible care, agreed with that assigned by this hypothesis, whilst it was incompatible with any theory of reflexion from an unpolished surface. The disk was then somewhat gibbous, and the whole question turned on the estimate of position, about which, however, I feel pretty confident. I have now made some observations of *Venus* in the crescent form with the Greenwich Equatoreal, the result being entirely to confirm Mr. Brett's original statement, that the brightest part of the crescent is a little within the limb. On December 31, 1877, I found, on reducing the intensity of light by means of the polarising eye-piece previously referred to, that the breadth of the crescent diminished from about 0·8 of the angular radius of the disk to 0·2, fading off on both sides as well as towards the cusps; and in the *middle* of the breadth of this narrow strip I was able to detect a very fine line, the length of which I estimated at about one-fourth of the radius. This observation was made in the evening between 5^h and 6^h. Allowing for the loss of a small portion on the side of the limb (when the light was reduced to one 3000th part), of which I satisfied myself by repeated comparisons, I estimated this brightest portion of the disk to be 0·2 of the radius within the limb. The calculated position on the hypothesis of specular reflexion, as afterwards found, would be 0·22 of the radius from the limb. On 1878, January 9 and 10, I had an opportunity of examining *Venus* in the daytime, and was fortunate in securing Capt. Tupman's co-operation on these occasions. As seen with an ordinary eye-piece (power 500) *Venus* appeared as a crescent, having a breadth equal to about two-thirds of the radius, with a bright arc near the limb. This arc shaded off perceptibly towards the cusps. On applying the solar eye-piece, which reduces the light (by virtue of the three reflexions from glass) to one 3000th part, both Capt. Tupman and I satisfied ourselves that there was a bright elongated patch distinctly within the limb. In the centre of this I caught glimpses of a small point of light, the distance of which form the *apparent* limb I estimated as rather more than 0·1 of the radius. The position of the point at which specular reflexion would take place, I afterwards found by calculation to be 0·18 of the radius within the limb.

Although the estimates of position of the bright point are, from the nature of the case, rough, the agreement with the specular reflexion hypothesis is very striking; and these observations would establish the fact that there is a gradation of light on the disk of *Venus*, which can only be explained on that theory. On the supposition of reflexion from a rough or unpolished surface, there would, according to Mr. Neison, be a point of maximum illumination, but it would fall on the farther side of the limb, and as the limb itself, at the time of the second observation (January 9), was distant 20° of longitude from this point, the increase of brightness would be very marked right up to the limb. Now the actual circumstances are very different. There

is a portion of the disk near the limb which at a first glance appears to be uniformly brighter than the part near the terminator. The breadth of this bright arc was, on January 9, about one-fourth of the radius, so that at its inner edge the Sun's zenith distance must have been 60°, whilst at the limb it would be 20°. Thus, if we had to do with reflexion from a rough surface, there ought to be a decided gradation up to the very limb, the brightness at that part being, on this supposition, twice as great as at the inner edge of the bright arc. This is emphatically not the case.

The bright patch here referred to was described by Capt. Noble in the *Monthly Notices*, June 1876, when *Venus* was under similar conditions of illumination, and his failure to detect any gradation whatever towards the limb appears conclusive against the theory of reflexion from a rough surface. Had he looked a little more closely, he would, perhaps, have detected in the middle of the bright patch a small bright point, as I did. At any rate, the existence of a patch of sensible area, and "without the slightest sign, trace, or indication of one part being in the smallest degree brighter than another," seems utterly incompatible with any theory whatever. But on the hypothesis of specular reflexion modified by diffusion in a dense atmosphere, the point of maximum brightness would be in the middle of this patch, and the gradation on either side, being comparatively slight in the neighbourhood of the maximum, might easily be overlooked by a hasty observer. But I believe that, by patient gazing under favourable circumstances, the actual image of the Sun may be seen as a minute speck. A high power, however, must be used, as the speck would hardly exceed $0''\cdot1$ in diameter. I have used thoughout a power of 500. The reduction of the light by a dark glass or other means is important, as it removes any disturbing effect of irradiation and enables the eye to appreciate more readily small variations of brightness. It may be well to add, that the position of plane of polarisation in the eye-piece relatively to the line of cusps appeared to have no appreciable effect on the gradation of light. This would be consistent with the theory of reflexion from a metallic surface; but the observation is so delicate, that I would not lay much stress on the circumstance that no trace of polarisation was noticed.

1878, *January* 10.

P.S.—On 1878, January 11, after the above was written, Mr. Maunder examined *Venus* both with the polarising eye-piece and a dark wedge, and noted a bright line or elongated patch, about $7''$ or $8''$ in length and $2''$ in breadth, the middle of which was from $2\frac{1}{2}''$ to $3''$ within the limb, i.e. about 0·15 of the radius. As the brightness was still further reduced the ends of the bright line disappeared, and the last phase was a tiny faint blur. These observations were quite independent, and Mr. Maunder had, in fact, no knowledge of my results.