

the German "Vierteljahrsschrift des Astronomischen Gesellschaft" within the past four or five years, and see that only a single one of them all is expressly mentioned, and to note the values of the parallax adopted in the astronomical ephemerides of France, Spain, Portugal, and Germany, and see that not one of them can be traced in Mr. Proctor's history. If, as he once said, he had not room to describe the recent researches, I should have supposed he would have condensed or omitted the older ones, which these recent ones have superseded, instead of doing the contrary. The importance of this matter arises from the fact that these discussions and researches put a different face on a number of questions connected with the determination of the solar parallax from that given by Mr. Proctor, and I do not think the latter can successfully argue that the astronomical world of to-day is nearly all wrong in the views to which it has been led by five years of discussion, experiment, and research.

On Nos. 3, 4, and 7, of Mr. Proctor's defence, it is only needful to remark (1) that I did not write No. 3 till I had verified Foucault's result by a careful calculation not made on my thumbnail; (2) that Mr. Proctor leaves it to be logically inferred that the discussion alluded to in No. 4 was an unpublished one; (3) that, having disclaimed my interpretation of No. 7, his book gives no explanation of the reason why Mr. Stone's parallax was so much greater than those of Encke and Ferrer. It is only necessary to refer to the paper of the latter in vol. v. of the *Memoirs of the Royal Astronomical Society* at pages 254 and 264 to find a very full discussion of the apparent, and the so-called true contacts.

No. 6 involves one of the most important questions connected with the determination of the solar parallax from transits of Venus, and I am sorry to see that Mr. Proctor simply evades the issue, as the misinterpretation to which he refers consists in supposing him less erroneous than he really is. Let one make a drawing representing the limbs of Venus and the sun in mathematical contact. On each side of the point of tangency there will be an exceedingly thin thread of light, vanishing at that point. How much of this thread will be visible by an ordinary telescope? We must remember that the sun is viewed through a dark glass, which reduces its light to that of an ordinarily illuminated object. The narrowest visible line so illuminated subtends an angle of about  $20''$ . With a power of 120 this would correspond to a breadth of one-sixth of a second. But it is well known that atmospheric tremors, and, with most old instruments, imperfect corrections of the object-glass, prevent our seeing an object at all approaching the minimum visible, and that the same cause prevents the increase of magnifying power from giving a corresponding increase of seeing power. It is probable that the thickness of the least visible thread may have varied with the telescope, the observer, the dark glass, and the atmosphere, from one or two tenths of a second to one or even two seconds. Let us take the more favourable cases in which a thread of  $0''.2$  is visible. A simple calculation will show that there is a space of  $3''.4$  on each side of the point of tangency, in which the thread will be thinner than this, and therefore invisible, and that the visible cusps will be about  $7''$  apart. How different this  $7''$  from Mr. Proctor's invisibly thin ligament! This explains the observations of Wolf and André, who found that the black drop when seen at all continued after internal contact at ingress and preceded it at egress.

In answer to Mr. Proctor's letter of October 5, I beg leave to reply, if the "fringe" is something actually produced by the telescope or the atmosphere, it is simply bad definition. If it is not so produced, it is an optical illusion, of which the laws are obscure, and the very existence problematical under the circumstances in question. See, for instance, the celebrated paper of Prof. Baden Powell on Irradiation. Mr. Proctor's intimation that the great mass of astronomers who have observed transits of Mercury within the past forty years, among whom are included Bessel, Airy, and the Struves, were careless and inferior observers, because they did not see an optical illusion according to his view of it, is as good a *reductio ad absurdum* of his theory as I could ask for. It is comforting to know that one of his careless observers can be turned into a careful and attentive one by giving him a telescope with plenty of irradiating power.

To prevent misapprehension, allow me to say that the theory set forth in my letter of September 28 is in no way my own, but was promulgated by Bessel nearly forty years ago, and has, I believe, been since universally received on the continent of Europe.

SIMON NEWCOMB

Washington, Oct. 23

### The Aurora of Nov. 9th and 10th

I WITNESSED on the night of Nov. 9, at about 7.30 P.M., an aurora which, for symmetry of form and other features, was very remarkable; and unless, as is very likely, some more able observer has already sent you a description of it, you may like to put my account on record.

In the magnetic north horizon was the usual segment of auroral light, very brilliant, and stretching considerably to the east and west, its altitude being  $20^\circ$  or more. High above this, and extending in a complete arch from the east to the west horizon, was a remarkable and well-defined band of still brighter light, about  $7^\circ$  in breadth, and passing about  $30^\circ$  from the zenith.

Filling the space between these two arcs of light was what I can call nothing else than a dark shadow, which had somewhat of a mysterious appearance; for, though decidedly darker to the eye than other parts of the heavens, it did not in the least obscure even small stars, nor do I think this darkness was the effect of contrast. In this dark space faint auroral streamers occasionally shot up to the upper arch, but did not pass it. This shadow was what the French observers speak of as the *nube*.

The light of the upper arched band was silvery, and increased much in intensity towards the horizon both east and west; the points of greatest intensity being about  $5^\circ$  above the horizon, as would be expected in the direction in which the arch appeared most foreshortened.

While watching this phenomenon I was impressed by the conviction that, to an observer in space, the north magnetic pole of our planet would have presented the appearance of being surmounted by a symmetrical cap of light, streaked by one or more bands, and terminated at its greatest distance from the pole by a well-defined brilliant margin.

In the hope that an observer in some other locality might have made similar observations, I was preparing to measure the distance of the upper arch of light from the zenith, as well as the positions in azimuth of the points where it touched the horizon, when the whole phenomenon was obscured by dense clouds.

Stretton Rectory, Hereford, Nov. 15

H. C. KEY

THE following brief extract from our observatory note book may be interesting:—

"Nov. 10.—For about  $20^\circ$  on each side of north, at 9.30—9.40 P.M., brilliant waves of light followed one another rapidly, from two to four in a second, moving upwards, following the direction of the streamers, fading away at about  $40^\circ$  from the horizon. Three or four waves could be seen at once, measuring about  $5^\circ$  to  $8^\circ$  by estimation, from crest to crest."

I heard some of the boys remark "How close it must be; it looks like puffs of steam from behind those houses."

Rugby

J. M. W.

As none of your correspondents who described the brilliant auroræ of Nov. 9th and 10th last week, speak of their being seen earlier than from 7 to 10 P.M., it may be interesting to note that in the Midland Counties the latter was visible at a considerably earlier period of the evening. On the evening of the 10th I was walking from Reading in Berkshire to Caversham in Oxfordshire, from 5.45 to 6.5 P.M. During the whole of that time I had before me the steady white light of the coming aurora, extending perhaps  $25^\circ$  to  $30^\circ$  in width, and  $20^\circ$  in height, its centre being immediately beneath Polaris. The appearance was exactly that of the departing twilight in a clear winter sky, for which, indeed, but for its position and the time of the evening, it would have been mistaken. As I noticed the light immediately on leaving the railway station above the lights of the town, I have little doubt that it had been visible since sunset. I had no opportunity of watching its progress after 6.5 P.M.; up to that time there were no coloured streamers, nothing but the white light I have described.

ALFRED W. BENNETT

### The Ghost of Flamstead

I OUGHT earlier to have thanked this venerated shade for a communication which will enable me to correct (at some future time) an omission in my treatise on the Sun. Let me hasten to