frequently sees statements about radiants being only deducible within ±2° for even good cases and ±5° for poorer ones. That the radiants were so well determined in each case comes largely from the fact that 80 minutes was the extreme interval between the plotting of the first and last Aquarid on any night. This should call the attention of observers to the necessity, as far as possible, of determining their radiants from observations of one night and also by meteors seen within a limited portion of that night. This is most important for radiants so situated that their zenith distances change rapidly from hour to hour.

While physical appearances are difficult to classify in any useful way, still the Aquarids may be said to have generally exhibited the following. Their duration was longer than the average meteor, and hence their apparent velocity slower, when bright—or sometimes even when faint—persistent trains were left; they exhibited a variety of colours and their brightness varied from 0 mag. to 6 mag. It seems certain that an observer near the equator, where the radiant has a higher altitude, would have seen a good shower this year. The fact that they were still fairly frequent on May 8 proves that the width of the stream is several million miles, although the parent comet passed the Earth's orbit eleven years ago, and we are therefore several hundred million miles behind it. The radiants deduced this year agree excellently with those observed by C. P. Olivier in 1910, 1911, and 1913. Using all the data secured in the three years mentioned with that of 1921 we find a slow motion of the radiant during the interval May 2 to May 11 inclusive, in the direction of increasing right ascension.

C. P. Olivier and R. M. Dole.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Relativity Shift in the Solar Spectrum.

Gentlemen,—

Your note on page 159 of the May number of *The Observatory* conveys, I think, a wrong impression of the value of the evidence obtained at Kodaikanal, and I ask permission therefore to state some facts about our instrumental equipment which may retrieve the results of our work from being classed as "inferior evidence."

Before designing and building our large spectrograph, I had, through the kindness of Prof. Hale, the great advantage of working with the spectrographic equipment at Mount Wilson as it existed in 1906 as a part of the Snow telescope installation. My experience with this instrument, which is in essentials similar to the 30-foot Littrow spectrograph used by Dr. St. John in his research on the Relativity shift, led me to start work at Kodaikanal on entirely different lines in the design of our spectrograph.

Without in any way questioning the excellence of the equipment at Mount Wilson, or of St. John's work with it, which is probably the best attainable with present means, I may, nevertheless, call attention to the relative advantage which our equipment possesses owing to its much greater economy of light, so that with equal linear dispersion the exposure-times are very greatly reduced. The discovery of the shift of the lines due to radial motion in sun-spots, before it was detected on Mount Wilson, was one early result due very largely to this economy in light, combined with the high dispersion of the instrument. In resolving-power, our Anderson grating with 75,000 lines is probably proportionately less powerful than the Mount Wilson grating with 95,000 lines. It is difficult to judge of this, however, without a critical comparison of photographs. Probably there would not be a very significant difference between them. In any case, in our plates we realize a photographic resolution which is entirely adequate for the measurement of line shifts as small as $\frac{1}{8}$ of the Einstein displacement.

We have found that, owing to the width and indefinite edges of the lines at the limb of the Sun, little or nothing is gained by increasing the scale beyond 2 mm. to the Angstrom unit. It is far better to use a moderate dispersion and double the intervals measured by means of the positive-on-negative method; and this has the additional advantage of immediately detecting any want of symmetry in a line, which, as you point out, may become a source of error in estimating the shifts.

The smaller solar image we work with, as compared with Mount Wilson, is no disadvantage, but a positive gain in this particular research, because the small irregular line-shifts which I have shown to exist over the disc of the Sun, even at its centre, are compressed into a smaller space, and are, therefore, less likely to have the same sign along the very short section of the image that is measured.

The difficulty of deciding for or against the Einstein shift in the Sun lies in the conflicting nature of the evidence itself; it is not a question of weighing the relative accuracy of the measures made at Mount Wilson and at Kodaikanal. There is a general shift of the lines of many, if not all, substances identified in the Sun, and this is of the right sign and order of magnitude, so that we have an initial argument strongly in favour of the Einstein effect; but, if the cyanogen band-lines were shown conclusively to have a shift towards red at the limb of the Sun equivalent to 0·634 km./sec., the problem would not be solved. There are many apparent anomalies which require to be investigated in the lines of other substances. I have shown that pressure shifts in the reversing layer may be ruled out of consideration, and this has recently received confirmation in Perot's measures of the magnesium lines b_1 and b_2 *. That being so, it seems now desirable to make a more detailed and intensive study of the iron lines at the centre and limb of the Sun, instead of wasting more time on the much more difficult cyanogen band-lines, which, according to the work of King, are likely to prove untrustworthy.

It is hoped that by co-operation with Mount Wilson the various issues involved may eventually be made clearer, so that a definite conclusion may be reached as to the cause or causes of the line-shifts.

Recent work at Kodaikanal on the iron lines in the red and in the ultra-violet regions of the spectrum is not altogether unfavourable to an Einstein effect, superposed on a motion shift which has its greatest effect at the centre of the disc. The measures of Venus spectra, showing a zero or minus shift at the back of the Sun, are, of course, unfavourable to the Relativity effect; but I am quite prepared to find that the result of all my work during five successive apparitions of the planet is an illusion, and that the very excellent spectra obtained in July 1918, showing a zero shift, are affected by some as yet undetected source of error. It is very difficult to believe that they are affected by an unequal illumination of the slit, as has been suggested by St. John †. I hope to get a new series of Venus spectra during the later months of this year.

I am, Gentlemen,
Yours faithfully,
J. Evershed.

Kodaikanal, 1921, June 7.

OBSERVATORIES.

Reports of Observatories for 1919

(concluded from p. 191).

Heidelberg (Königstuhl).—Prof. Max Wolf reports diminished activity, partly on account of excessive expense and partly on account of the extremely arduous and difficult conditions under which the work had to be conducted. Coal was lacking, transport difficult, and workmen were not available to help with the necessary repairs to the buildings. Dr. Kopff has taken up with the meridian circle the parallax work left unfinished in 1914. Twenty-eight new asteroids were discovered, mostly by Dr. Reinmuth, including a sixth Jupiter planet 1919 FD. A search was made for the Trans-Neptunian planet indicated by

* *Nature*, cvii. p. 182.
† *Nature*, cvi. p. 790.