CORRESPONDENCE.

To the Editors of 'The Observatory.'

Anomalous Dispersion in the Sun.

GENTLEMEN,—

Dr. Albrecht has re-stated the principal facts brought out in his recent investigation on solar and arc wave-lengths in a short paragraph at the beginning of his letter published in the *Observatory* for July 1916. These "facts" are, we agree, strikingly in accord with the requirements of the anomalous dispersion theory.

Dr. Royds and myself have questioned the validity of Dr. Albrecht's results, since our direct comparisons of the spectra concerned do not support them. But in what way are our comparisons superior to those of Dr. Albrecht? Dr. Royds in a letter to me says that he declines the responsibility which Dr. Albrecht seeks to put on him of proving "in what essential way so-called direct comparison methods are 'more direct' for such a purely relative comparison of what may be termed *internal* differences in the two systems." In the discussion in *Kodaikanal Bulletin*, No. 48, he was not concerned with comparing the internal differences in the two systems of wave-lengths, but with the *interpretation* which Albrecht gives to this comparison. When Dr. Albrecht interprets the results of his comparison as solar displacements, then Dr. Royds considers that the so-called direct comparison method is not only the more direct but the only reliable method of obtaining displacements.

I may mention that our measures were made on plates having probably considerably higher photographic resolution than those from which Rowland's wave-lengths were measured, and from two to three times the linear dispersion. That, however, is perhaps of secondary importance: the main advantage is, I think, that in a direct study of the spectra themselves one is able to avoid certain pitfalls, which beset the method of comparing mere tables of wave-length. This is exemplified when one comes to examine in detail Dr. Albrecht's tables of lines with

companions to the red and violet*. In these, for instance, I find many close pairs of lines having the highest weights assigned but which are actually *unresolved* in the solar spectrum. However great the resolving power which may be used, the separation of these pairs cannot be measured accurately, but only guessed at! Rowland, in his preface to the "Preliminary Table of Solar Wave-lengths," does not state precisely how these pairs were measured. It is possible that the arc-line separations were substituted in some cases, as the arc lines are in general narrower than the solar lines, and close doubles are more readily resolved. However this may be, all of these pairs are absolutely inadmissible as evidence for anomalous dispersion in the Sun. In Albrecht's tables of iron lines there are 9 of these unresolved pairs to be deleted, out of a total of 31 having the highest weights (2 and 3). Typical examples are the pairs at $4210\cdot\frac{494}{561}$ and $5202\cdot\frac{439}{516}$.

After these unresolvable pairs, we come to a considerable number which are distinctly separated in the Sun, but yet are difficult to measure, owing to shading or other causes, and unless special precautions are taken reliable measures are impossible, *e.g.*, the pair G of the solar spectrum, $4307\cdot\frac{907}{8\cdot081}$.

I have only succeeded in getting good values of the separation of these lines with greatly over-exposed plates, and it is not to be supposed that Rowland took special plates for measuring this pair.

Now, as to the reliability of Rowland's table generally, where easily measurable double lines are concerned. My attention was recently called to an extraordinary discrepancy in the values obtained by my assistant, Mr. Narayanayyar, who is an expert measurer, in the separation of the pair b_3 in Sun and arc compared with Rowland, and this has led me to make a series of measures of close doubles in high-dispersion solar spectra, with the result that in a large proportion of cases I find Rowland's separations are quite grossly over-estimated.

In Albrecht's tables of *Fe* lines there are 21 measurable pairs, weighted 2 and 3, and of these I have measures of 18. In 11, the separations are greatly over-estimated in Rowland, and the remaining 7 show small differences, and in two of these only I get slightly larger values than Rowland. The mean separation of all the 18 lines is 0·1920 according to Rowland, and 0·1836 according to me, or a mean difference R–E of +0·0084A.

It appears evident from this that in measuring close pairs of lines, barely separated owing to insufficient linear dispersion, the tendency in Rowland is to displace the violet components to the violet, and the red components to the red, thus simulating the effects of anomalous dispersion.

* *Astrophysical Journal*, vol. xli. p. 348.

With regard to the further results obtained by Dr. Albrecht, which seem to support Sir Joseph Larmor's interesting suggestion, or modification of the anomalous dispersion theory, I can only say that he is certainly straining the material quite beyond the breaking point. Only new observations of solar and arc spectra, free from the errors I have pointed out, would be adequate to test the question whether there is a systematic difference between the separation of pairs of lines of which the components belong to different elements, and pairs the components of which are due to vibrations of the same atoms.

But first it is necessary to settle the original question whether or not anomalous dispersion is an effective agent in displacing solar lines. Our measures so far appear to show decidedly that it is not.

I am, Gentlemen,

Yours faithfully,

J. Evershed.

Kashmir Temporary Observatory,
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