LETTERS TO THE EDITOR.

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Radiation in a Magnetic Field.

THE application by Prof. Michelson of his interferometer to the study of the structure of the spectral lines has raised two important questions regarding the performance of this instrument; and it is to be hoped that perfectly satisfactory answers to both of them may be forthcoming in the near future. These questions are:

(1) Is the complex structure of the lines indicated by the interferometer a real structure existing in the light emitted by the source; or is it imposed, in part or altogether, by the apparatus employed?

(2) Supposing the structure referred to in (1) not to be spurious, but to be real, or partly real and partly spurious, can the interferometer be relied on as a measuring instrument for the purpose of determining the distribution of light in the complex line by estimation from the visibility curve?

With regard to (1), I may say that although it has been suggested that the structure indicated by the interferometer is entirely due to diffraction effects (or other unknown instrumental troubles), yet I personally am of opinion (from the study of Prof. Michelson's work) that the structure indicated is in the main real. It is possible, and indeed probable, that diffraction effects influence the final results in some small degree; but the main character of the indicated structure agrees, no doubt, with a real structure existing in the light emitted by the source.

The modifications introduced by diffraction (if any) ought to be detected from the fact that such effects are the same in character for light of all wave-lengths, and their magnitudes for different spectral lines depend on the wave-length only, and in no way on the nature of the radiating substance. For this reason I believe, with Prof. Michelson, that the structure indicated by the interferometer as existing in certain spectral lines, even when uninfluenced by the magnetic field, is a real structure; but as to whether it is all real, or to some small extent spurious, has not yet been placed beyond all doubt. The discovery of this structure adds one more to the already long list of achievements in the advance of science for which we are indebted to Prof. Michelson, and I trust he will place it beyond all doubt as to whether diffraction, or other causes, exert any appreciable influence in the instrument, or in any way mask the true structure. It is not sufficient to reply, as he does on p. 440 of this journal (March 9), that the explanation of this structure by diffraction effects "would be very difficult to accept, in view of the very great constancy of the results, with instruments of different construction and dimensions, with different observers, and with different forms of vacuum tubes employed"; for whether the effects are due to diffraction or not, they ought to remain the same under the circumstances here related, unless the "instruments of different construction" differ in principle and are not all interferometers. Diffraction cannot be the main cause if the character of the effect differs for different wave-lengths, and Prof. Michelson finds that it does differ for different spectral lines; and in the same way, I think, it might be determined if it intrudes itself as a modifying influence.

With regard to question (2) above, the charge against the interferometer remains most serious; nor is it diminished in any way by Prof. Michelson's further explanations given on p. 440. The case is this—the interferometer, when applied to the study of the splitting up of the spectral lines by the magnetic field, yields the law that the magnetic separation of the constituents "is *approximately* the same for all colours and for all substances." Now the facts of the case are that no such law holds, even as the roughest *approximation*. The magnetic separation is quite large for some lines, and very small, almost unobservable, in the case of others, and this even in the case of lines of nearly the same wave-length in the same substance. In fact the law yielded by the interferometer is not at all supported by the facts, and what remains to be done is to determine the causes of error in the instrument, or to standardise it, so that it may be employed as a measuring instrument. Of course, as I have already mentioned (NATURE, January 5, p. 228), the interferometer might have yielded this law without censure if by chance Prof. Michelson had happened to observe lines which suffer approximately the same amount of resolution in the magnetic field. But this is not the case, for in the case of cadmium the separation for the blue line is more than 30 per cent. greater than for the green line, yet the interferometer gives 0·41 for the green line and 0·40 for the blue. Similar remarks apply to the corresponding lines of zinc and magnesium; and what person, who has had even the slightest survey of these effects, can have any doubt as to the great difference in the magnitudes of the magnetic effects in the case of the green lines of magnesium and the green lines of copper?—and so on *ad infinitum*.

In conclusion, it may be well to mention that the relative intensities of the light in the components of the magnetically resolved lines, as observed by the eye in a good spectroscope (21·5-foot grating), are not by any means the same as those indicated by the interferometer. Thus in the figures reproduced on p. 440, the central components A (Fig. 1) are shown by the interferometer as possessing greater intensity than the lateral components B. But when the resolved line is observed by the eye (or photographed), it is at once seen that the illumination in the lateral components B (types II. and III.) is very much greater than that in the central components A. Type II. shows as a quartet (in which A is double) if the field is not very intense; but this quartet becomes resolved into a sextet, owing to the side lines B splitting up into doublets when the field becomes very intense. There is no trace of the further little "humps" pictured by Prof. Michelson, but the lines are clear and sharp—and it is possible that these little humps may be due to diffraction effects (?). Similar remarks apply to the relative illumination in type III.; and in this connection I may mention that although I did not give an illustration of the general type on p. 226, viz. that in which each constituent of the normal triplet is itself a triplet (figured by Prof. Michelson, p. 441, Fig. 3), yet I stated in the text of my article (p. 226) that "all the variations so far noted may be embraced in the general statement that each line of the normal triplet may itself become a doublet or a triplet." Indeed, these various types of effect were observed by me as early as November 1897, and have been communicated to the Royal Dublin Society from time to time.

THOMAS PRESTON.

Bardowie, Orwell Park, Dublin, March 16.

The Phenomena of Skating and Prof. J. Thomson's Thermodynamic Relation.

IN connection with Prof. Osborne Reynolds's "Notes on the Slipperiness of Ice," read before the Manchester Literary and Philosophical Society (NATURE, March 9, p. 455), the following extract from a brief paper, read by me before the Royal Dublin Society in 1886 (*Proc. R.D.S.*, vol. v. p. 453), may not be without interest.

"To the many phenomena which have found an explanation in Prof. J. Thomson's thermodynamic relation connecting melting-point with pressure, might be added those attending skating, *i.e.* the freedom of motion and, to a great extent, the 'biting' of the skate.

"The pressure under the edge of a skate is very great. The blade touches for a short length of the hog-back curve, and, in the case of smooth ice, along a line of indefinite thinness, so that until the skate has penetrated some distance into the ice the pressure obtaining is great; in the first instance, theoretically infinite. But this pressure involves the liquefaction, *to some extent*, of the ice beneath the skate, and penetration or 'bite' follows as a matter of course. As the blade sinks, an area is reached at which the pressure is inoperative, *i.e.* inadequate to reduce the melting-point below the temperature of the surroundings. Thus, estimating the pressure for that position of the edge when the bearing area has become 1/50 of a square inch, and assuming the weight of the skater as 140 lbs., and also that no other forces act to urge the blade, we find a pressure of 7000 lbs. to the square inch, sufficient to ensure the melting of the ice at $-3{\cdot}5°$ C. With very cold ice, the pressure will rapidly attain the inoperative intensity, so that it will be found difficult to obtain 'bite'—a state of things skaters are familiar with. But it would appear that *some* penetration must ensue. On very cold ice, 'hollow-ground' skates will have the advantage.

"This explanation of the phenomena attending skating