most favourable estimate. In the first place, the image whose position is to be measured to within one-hundredth of a millimetre is the result of seventy-nine reflections from concave mirrors!

Secondly, one of these mirrors is to be 2 decimetres in diameter. Such a mirror used in a reflecting telescope would show signs of distortion if not carefully mounted—even at rest. But this mirror is required to make fifty revolutions per second, and the distortion is multiplied by forty reflections from its surface!

Finally, notwithstanding the avowed purpose of diminishing the path of the light ("*sans augmenter le trajet de la lumière*"), the distance required is *greater* than in my own experiments in the proportion of 1600 to 1200, and hence atmospheric disturbances would come into play in the same proportion—unless especial precautions were taken to guard against them.

And here, I am free to concede, is an important advantage, but one which is by no means limited to M. Wolf's arrangement, but is universally applicable—for by repeated reflection by plane or by concave reflectors the whole path, either in Fizeau's method or in Foucault's, may be confined to a limited space. But I think the chief object of such an arrangement—namely, to control easily the homogeneity of the air-column—could be more advantageously effected by a long underground tunnel containing a pipe, surrounded, if necessary, by running water, or, better still, exhausted of air.

At Prof. Newcomb's request I have repeated, with some alterations, the experiments described in the paper referred to, and occasionally the appearance of the image was better than in that work. On one occasion the width of the image was carefully measured, and found to be 0·25 mm. Evidently there is nothing remarkable in measuring the position of the centre of an image of this width within a hundredth of a millimetre.

Again, the "probable error" of my final result, 5 kilometres, would seem to show a somewhat greater degree of consistency than would be possible had I only a "*tache lumineuse*" to bisect.

I cannot forbear remarking that by astronomical methods—if M. Wolf entirely mistrusts the results obtained by Cornu, Newcomb, and myself—the velocity of light is known certainly within 1 per cent., and that it would, therefore, denote rather an excess of caution to deduce a formula for the elimination of a possible uncertainty of from 5 to 10 per cent., as M. Wolf does in determining "*l'ordre* M *de cette déviation*."

In conclusion, I think M. Wolf is to be congratulated on the very happy combination he has devised for the solution of this most fascinating problem—a problem which, notwithstanding its difficulties, will ultimately yield a result correct not merely to one part in 3500, but, I firmly believe, one in 300,000—perhaps one in 1,000,000.

ALBERT A. MICHELSON

SELF-INDUCTION IN RELATION TO CERTAIN EXPERIMENTS OF MR. WILLOUGHBY SMITH, AND TO THE DETERMINATION OF THE OHM

IN a lecture delivered by Mr. Willoughby Smith before the Royal Institution in June last (see *Proceedings*) some experiments are detailed, which are considered to afford an explanation of discrepancies in the results of various investigators relating to the ohm, or absolute unit of electrical resistance. As having given more attention than probably any one else in recent years to this subject, I should like to make a few remarks upon Mr. Willoughby Smith's views, which naturally carry weight corresponding to the good service done by the author in this branch of science.

In the first series of experiments a primary circuit is arranged in connection with a battery and interrupter, and a secondary circuit in connection with a galvanometer and commutator of such a character that the make and break induced currents pass in the same direction through the instrument. Under these circumstances it is found that at high speeds the insertion of a copper plate between the primary and secondary spirals entails a notable diminution in the galvanometer deflection, and this result is regarded as an indication that the molecules of copper need to be polarised by the lines of force—an operation for which there is not time at the higher speeds. The orthodox explanation of the experiment would be that currents are developed by induction in the copper sheet, which thus screens the secondary spiral from the action of the primary, and the result is exactly what might have been anticipated from known electrical principles. I have the less hesitation in saying this, because as a matter of fact I did anticipate from theory the action of a combination very similar in character. The experiment is described in the *Philosophical Magazine* for May, 1882, and differs from Mr. W. Smith's only in the substitution of a telephone for the galvanometer, and of a microphone for the interruptor, no reverser in the secondary circuit being required. By the interposition of a thick copper sheet the sound is greatly enfeebled.

The second series of experiments were made with Faraday's "new magneto-electric machine," in which a copper disk rotates about its centre between the poles of a horse-shoe magnet. The currents developed are examined with a galvanometer whose electrodes touch two points upon the disk—in Mr. W. Smith's experiments, one at the centre, and the other at the circumference. At low speeds the distribution is symmetrical with respect to that diameter of the disk which is passing at any moment between the poles; but, as the speed is increased, a certain "drag" is observed, disturbing the symmetry. This drag, or lagging, was noticed by Nobili in a very similar arrangement as long ago as 1833 ("Wiedemann's Electricity," third edition, vol. iv., § 374), and is no doubt to be attributed to the induction of the currents upon themselves.

This question of self-induction is indeed a very important one in respect of certain methods for determining the ohm; but it certainly cannot be said to have been neglected, as Mr. W. Smith seems to suggest. Both in the original experiments of the British Association Committee with a coil revolving about a vertical axis, and in my own recent repetition of them, the self-induction of the coil is a most important feature, and may cause a displacement of the position of maximum current from the plane of the magnetic meridian through as much as 20°. In my paper (*Phil. Trans.*, 1882, p. 661) I thought I had discussed the question at almost tedious length.

It is possible that Mr. W. Smith had in his mind rather determinations by the method of Lorenz, in which Faraday's disk is used. The arrangement here, however, differs in one very important respect from that of Mr. W. Smith's experiments in that the lines of force are symmetrically arranged in relation to the axis of rotation. The consequence is that, however great the speed of rotation, there are no currents circulating in the disk, and therefore no question arises as to the self-induction of such currents. What is observed is simply the difference of electrical potential between the centre and the circumference. It is impossible to discuss the matter fully here, but the reader will find all that is necessary by way of explanation in the paper published in the *Phil. Trans.* ("Experiments by the Method of Lorenz for the further Determination of the Absolute Value of the British Association Unit of Resistance," &c.). My object in writing is to correct the inference, suggested by W. Smith's remarks, that the question of self-induction has been neglected by workers upon this subject.

RAYLEIGH