paragraph, which is entirely distinct from the rest of the discussion, he uses an expression which is not sufficiently approximate; *e. g.*, if the expression be taken to mean the wave-length as stated above, and accordingly used to compute the number of waves in a given length in the line of vision, it differs from the truth by $\frac{v^2}{V^2}$, precisely doubling the result found otherwise.

We assert, then, that the theory of 1887 is correct to terms of the order retained, which were sufficient; that Dr. Hicks's theory agrees with it precisely as to numerical amount and sign* of the effect, and that a third examination of the theory gives results differing from those of the two others only by negligible terms of the third order.

LXII. *Report of an Experiment to detect the FitzGerald-Lorentz Effect.*

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[This experiment was assisted by a grant from the Rumford Fund of the American Academy of Arts and Sciences; and a fuller account will appear in the Proceedings of the Academy.]

[Plate X.]

A NULL result was obtained in 1887‡ in an experiment to detect, if possible, a difference of velocity of light in different directions, owing to the motion of the apparatus towards or away from waves of light in the stationary æther. FitzGerald and Lorentz then suggested that the dimensions of the apparatus might be modified by its motion through the æther. If this modification depend on the resilience or other physical properties of the materials, it may perhaps be detected by experiment.

We have constructed two apparatus with which to examine this question. In the first, we replaced the sandstone used in 1887 by a structure of white pine. A strong cross was built up of planks, 14 inches wide and 2 inches thick, and 14 feet long. One was laid east and west, then one across it north and south, and so on. They were slightly notched where they crossed. On their intersection was secured a cast-iron bedplate for certain optical parts of the apparatus.

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* Taking into account a note in 'Nature,' vol. lxv. p. 343 (1902).

† Communicated by the Authors: read at the New York Meeting of the National Academy of Sciences.

‡ "On the Relative Motion of the Earth and the Luminiferous Æther." A. A. Michelson and E. W. Morley, Am. Journ. Sci. vol. xxxiv. p. 333.

At the ends, after filling the spaces between the planks, were bolted iron supports for our mirrors. The whole was placed on a round float, which in turn rested in a basin of mercury.

Our sixteen mirrors were each 4 inches in diameter. The mirrors rested each on the points of three adjusting screws, against which they were held by springs. On the bedplate, at the intersection of the arms of the cross, were placed a plane half-silvered mirror and a compensating plate; these had been, as is usual, cut from the same plane-parallel disk.

Fig. 1 is a diagram, not to scale, of the optical arrangements.

Fig. 1.

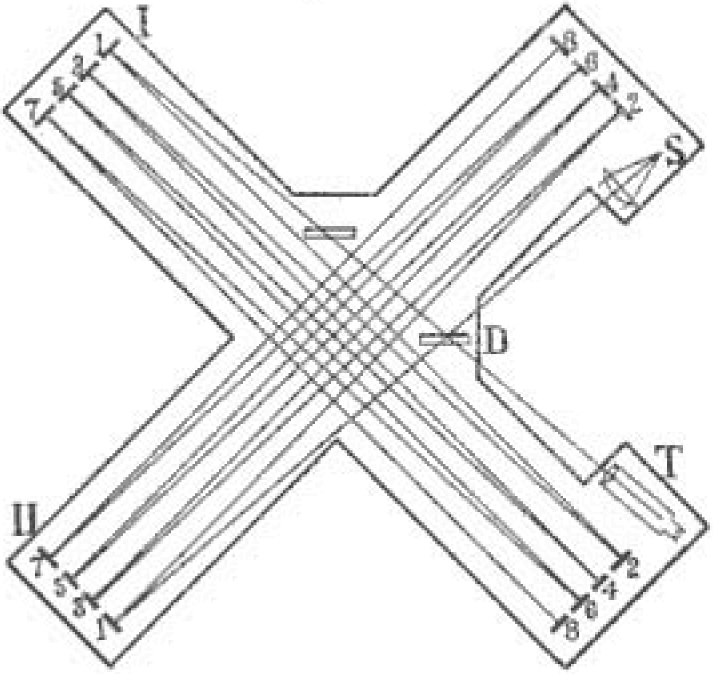
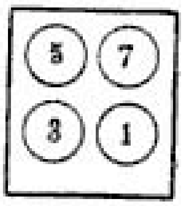


Fig. 2.



Light from a source S reaches the mirror D. Part is transmitted, reaching the mirror II. It is successively reflected to 2, 3, 4, 5, 6, 7, and 8. From 8 it returns by the same path to D, where part is reflected to the observer at T. Another part of the incident ray is reflected along the other arm of the cross, is similarly passed to and fro, returned, and at last transmitted to the observer. In the apparatus actually used, mirror 5 lay above 3, rather than to one side of it; fig. 2 shows this arrangement. The whole path of the light along these mirrors was enclosed and covered, to lessen the effect of air-currents and other disturbances. An acetylene flame was carried as a source of light. A telescope magnifying thirty-five diameters gave distinct vision of mirror 8, at whose surface the interference-fringes are apparently localized.

The mirrors being silvered and polished were put in place, and the lengths of the two paths were measured with a split rod and then made nearly equal. Establishing interferences in sodium light, we found the central part of a series of some 700 interferences which are brighter than the adjoining 300. With no long search, we could see interferences in white light, although we had provided no screw for moving a mirror with its surface always parallel to a given surface. This we had avoided, in order to have everything about the two arms as symmetrical as possible.

We now computed the direction and velocity of the motion of the centre of the apparatus by compounding the annual motion in the orbit of the earth with the motion of the solar system towards a certain point in the heavens. During part of August, the whole of September, and nearly all of October, this motion never coincides with the plane of our apparatus. For other dates, there are two hours in each day when the motion is in the desired plane, except for two days when the two hours coalesce into one. At the beginning of June, the two hours are about $11^h\ 20^m$ A.M., mean solar time, and $9^h\ 50^m$ P.M. At the time of our last set of observations, July 5th to July 9th, the hours were $11^h\ 40^m$ A.M. and $8^h\ 20^m$ P.M., local mean time.

After many trials, with filar micrometer, and with a scale on mirror 8, we found it advisable to accumulate a great number of observations made as rapidly as might be. What we had to do, in presence of all the local disturbances of density of the air which sometimes made observation impossible and always made it difficult, was as if we were trying to measure the diurnal solar atmospheric tide. If we could vary the period of this tide at will by controlling the revolutions of the earth, we should doubtless get a result sooner by accelerating the latter and making a great number of observations in a given time, rather than by retarding the period in order to measure with very great precision the hourly height of a barometer. We therefore proceeded as follows:—One observer walked around with the moving apparatus, his eye at the telescope, while he maintained the rotation by an occasional gentle pull on a cord so fixed as not to bring any strain to bear on the cross arms of the apparatus. The room was darkened. The other observer also went around with the apparatus; as an index showed the azimuth of the apparatus to be that indicated by one of 16 equidistant marks, he called out the number or some other signal. The first observer replied with the reading for the given azimuth, which the second observer recorded. The next azimuth was called at the proper instant, the reading given, and so on. Half the time, perhaps, the observations were interrupted before they became numerous enough to be useful, being stopped by excessive displacement of fringes owing to temperature changes and the like. But patience is a possession without which no one is likely to begin observations of this kind. Runs of twenty and thirty turns, involving 320 or 480 readings, were not uncommon. A run of thirty turns meant that the observer, who could sometimes make a turn of sixteen readings in 65 or 75 seconds, walked half a mile while making the severe effort involved in keeping his eye at the moving eyepiece without the least interruption for half an hour. The work is, of course, somewhat exhausting.

Observation with this apparatus could not begin till the month of August, and we had to stop without having accomplished as much as was desirable. During the busy season of the school year, observation is impossible. We had therefore expected to resume our work in June. But we then found that our pine apparatus had so much suffered from the dry air of the building, that we could not maintain the adjustment of our fringes. We could not, in the time, build another apparatus of timber which had not been dried all winter; nor was it thought well to construct another apparatus closely resembling the first. While planning a new apparatus, we made a couple of experiments to show, what was well enough known, that difference of magnetic attraction on the iron parts of our apparatus could not disturb our observations. We suspended two massive pieces of iron at the ends of one arm, so that one should be in the lines of magnetic force of the earth's field, and the other transverse to them, these relations being reversed on reversing the position of the apparatus. But observations with this load of iron gave the same result as before. Next we placed an analytical balance on one arm, with which to weigh a bar of iron at the extremity of that arm. It was so placed that at one azimuth the bar was nearly in the lines of force, and at another was transverse to them. If there were a difference of half a milligramme in twelve hundred grammes, it would have been detected; but no such difference existed. We found by trial how much a weight of a hundred grammes displaced our fringes, and so learned, as was known before, that the influence of the earth's magnetism could not be a disturbing factor.

The Rumford Committee of the American Academy now came to our aid, and we carried out our original plan of making a steel structure which should permit easy and satisfactory observation. In this apparatus, all weights are carried by two steel girders which intersect in a cross. With steel, we could have perfect symmetry in the two arms, which is impossible with wood. On the steel framework, two sets of four mirrors each are fastened by bolts through their supporting frames. Against these holders rest eight slender pine rods, supported throughout their lengths by enclosing them within the tubes of a brass truss. Against the further ends of these rods are held the freely suspended holders of the other two sets of mirrors. Springs apply a certain force to hold the freely suspended holders against the rods, and so against the first fixed mirror-holders, so that the distance between the mirrors depends entirely on the pine rods. In fig. 3 (Pl. X.) the apparatus is shown in an incomplete state; the telescope is shown in its final position, but the lamp and condensing-lens are not in place. Fig. 4 (Pl. X.) shows the apparatus ready for observations; with the lamp and lens placed as far as may be from the optical parts of the apparatus, and shut off somewhat by screens. The whole path of the light in the apparatus is enclosed, and the observer is protected by a dark cloth from the light which is necessary for the recorder.

With this apparatus, we adjusted our fringes on a certain Monday, and found that they remained in adjustment throughout the week during which we were occupied in observing them.

With this apparatus, observations were made precisely as before. We obtained 260 complete observations consisting each of readings at sixteen azimuths around a circumference. At the date of the observations, the annual motion of the earth together with the motion of the solar system may be taken as 33·5 kilometres a second. The velocity of light being 300,000 kilometres a second, the ratio of the squares of the velocities is $0{\cdot}72\,.\,10^{8}$. The length of path of a ray in our apparatus was 3224 centimetres, in which distance there are contained $5{\cdot}5\,.\,10^{7}$ wave-lengths of sodium light. The expected effect being doubled by rotation through 90°, the displacement of fringes expected on the simple kinematic theory will be $11\,.\,10^{7} \div 0{\cdot}72\,.\,10^{8}$. This is 1·5 wave-length.

As was indicated, there were two times in the day when observation was advisable. The direction of the motion with reference to a fixed line on the floor of the room being computed for the two hours, we were able to superimpose those observations which coincided with the line of drift for the two hours of observation. Doing this, and subtracting a constant so as to make the algebraic sum of the observations equal to zero, we get a certain result. Then adding the first term to the ninth, and so on, since the effect repeats itself after a half revolution, we get our final result, as follows :—

Result of observations at various azimuths.

Azimuths	8.	7.	6.	5.	4.	3.	2.	1.
Wave-lengths...	+0·0075	+0·0088	+0·0113	−0·0102	−0·0123	+0·0027	−0·0021	−0·0062

Azimuth mark 1 denotes that the telescope of the apparatus was directed N. 29° E., 3, N. 16° W., 5, N. 61° W., &c.

These numbers may be confidently pronounced to be due to errors of observation. We computed from them several curves of the theoretical form, having their origins at sixteen equidistant points in the half circumference; this was done by the method of least squares. The most probable of these curves had an amplitude of 0·0073 wave-length, and its zero was half way between the azimuths marked 4 and 5. The average of the given observations is 0·0076 wave-length: after subtracting the ordinates of the computed curve, the mean residual was 0·0066 wave-length. The sum of the squares of the residuals before was $565 \,.\, 10^{-4}$; afterwards it was $329 \,.\, 10^{-4}$.

We may therefore declare that the experiment shows that if there is any effect of the nature expected, it is less than the hundredth part of the computed value. If pine is affected at all as has been suggested, it is affected to the same amount as is sandstone. If the æther near the apparatus did not move with it, the difference in velocity was less than 3·5 kilometres a second, unless the effect on the materials annulled the effect sought.

Some have thought that the former experiment only proved that the æther *in a certain basement-room* was carried along with it. We desire to place the apparatus on a hill, covered only with a transparent covering, to see if an effect could be there detected. As the Rumford Committee have allowed us thus to utilize an unexpended balance, we hope to make the experiment in this form, should it be possible to make observations in trying conditions.