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CONFERENCE ON THE MICHELSON-MORLEY EXPERIMENT[1]

HELD AT THE MOUNT WILSON OBSERVATORY
PASADENA, CALIFORNIA

FEBRUARY 4 AND 5, 1927

The presence of Professor A. A. Michelson and Professor H. A. Lorentz in Pasadena in the early months of 1927 offered an exceptional opportunity for a conference on the theoretical and practical aspects of the Michelson-Morley experiment. Since Professor Michelson had planned, with the co-operation of the Mount Wilson Observatory, to repeat the experiment, such a conference was especially desirable. This was arranged largely on the initiative of Dr. Charles E. St. John. The experimental side was further presented by Dr. Roy S. Kennedy. This was supplemented by a mathematical treatment of the light-path by Professor E. R. Hedrick, as developed by himself and Professor L. Ingold, and by an account presented by Professor P. S. Epstein of the Trouton-Noble experiment, recently repeated at the California Institute of Technology by Chase, and of other recent experimental investigations. Illuminating discussion followed the presentation of the general reports. The shorthand notes were taken by Dr. Fritz Zwicky and Glenn H. Palmer, of the California Institute. These have been reviewed by the authors.

[1] *Contributions from the Mount Wilson Observatory, Carnegie Institution of Washington*, No. 373.

The addresses by Professors Michelson and Lorentz were followed by a detailed account of the results obtained by Professor D. C. Miller, who, fortunately, was also able to be present.

Reports

I. PROFESSOR A. A. MICHELSON (UNIVERSITY OF CHICAGO)

In 1880 I conceived for the first time the idea that it should be possible to measure optically the velocity w of the earth through the solar system. There had been earlier attempts to discover first-order effects, based on the idea of a system moving through a stationary ether. (First-order effects are proportional to w/c, where c = light-velocity.) Talking in terms of the beloved old ether (which is now abandoned, though I personally still cling a little to it), one might have expected that the aberration of light would be different for a telescope filled with air and with water, respectively. The experiments, however, showed, contrary to the then-established theory of light, that no such difference was present.

Fresnel's theory was the first to account for this result. Fresnel assumed that matter was able to drag along the ether partially (entrainment of ether), giving it a velocity w', so that

$$w' = \rho w \, .$$

He was able to determine ρ (Fresnel's coefficient) in terms of the refractive index μ:

$$\rho = \frac{\mu^2 - 1}{\mu^2} \, .$$

This coefficient is easily obtained from the negative result of the following experiment. Two light-beams travel along the path (Fig. 1; 0, 1, 2, 3, 4, 5) in opposite directions and give rise to a set of interference fringes. I is a tube filled with water. Now if the whole system moves with the velocity w through the ether, a shift of fringes would be expected on moving the tube from position I to II.

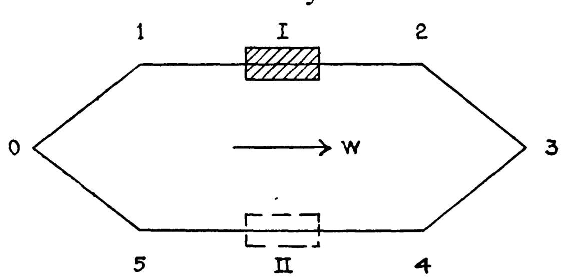

FIG. 1

No displacement is observed. By assuming a partial entrainment of the ether, Fresnel's coefficient ρ may readily be determined from this experiment. It may also be found in a very simple and direct fashion with help of the Lorentz transformation.

Fresnel's result was accepted universally by investigators of his time, including Maxwell, who pointed out that, while there could be no first-order effects, there might, nevertheless, be second-order effects (proportional to w^2/c^2). Now with $w \sim 30$ km/sec. for the motion of the earth, $w/c = 10^{-4}$, and $w^2/c^2 = 10^{-8}$, a quantity too small to be measured, according to Maxwell.

It seemed to me, however, that by making use of light-waves, one might devise an adequate arrangement for measuring such second-

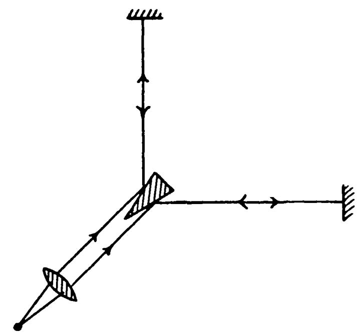

FIG. 2

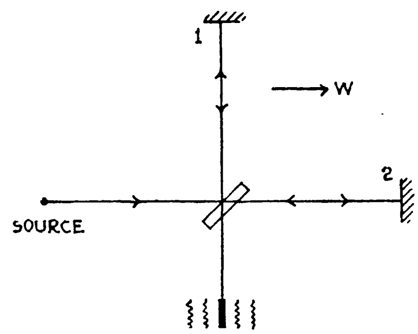

FIG. 3

order effects. Consider an apparatus, including mirrors, moving with the velocity w through the ether. Suppose two light-beams to travel back and forth in the apparatus, one parallel to w, the other at right angles to w. According to the classical theory, the change in light-path resulting from w should be different for the two beams and produce an appreciable shift of the interference fringes. The first device tried for the measurement of second-order effects is indicated in Figure 2. This arrangement, however, involved very great difficulties and was soon abandoned; and fortunately, because it led to the construction of the interferometer, which has proved of great value in many subsequent experiments.

The interferometer (Fig. 3) is known to all of you. A set of fringes is obtained by superposition of the two beams traveling from the source to a glass plate and then to mirrors 1 and 2, respectively, and back. If white light is used, the central fringe will be white and the side fringes will be colored. A motion of the apparatus with the velocity w through the ether should have much the same effect on the light as a stream of water would have on a boat trying to go once forth and back across the stream, and once down and then back up the stream. The time for getting forth and back a given distance will be different for the two cases. This is easily seen, because, however swift the current, the boat in its transverse journey could always return to the bank from which it started, whereas, in the second case, it might be unable to get back up stream against the current.

I tried the experiment at Berlin in Helmholtz' laboratory, but the vibrations of the city traffic made it impossible to get steady fringes. The apparatus was transferred to the observatory at Potsdam. I have forgotten the name of the director (I think it was Vogel), but I remember with pleasure that he was immediately interested in my experiment. Though he had never seen me before, he put the whole observatory with its staff at my disposition. I got a zero result in Potsdam. The accuracy was not very high, because I had a light-path of only about 1 m. Still it is interesting that the results were quite good. Coming back to America, I had in Cleveland the good fortune to secure the co-operation of Professor Morley. The apparatus then used was the same in principle as that used in Berlin, although the light-path was made longer by introducing a number of reflections instead of a single one. The path was in fact about 10–11 m long, which should have yielded a displacement of half a fringe, due to the orbital motion of the earth. But no displacement was found. The shift of fringes was certainly less than 1/20 and may be even 1/40 of that predicted by the theory. The result could be accounted for by the assumption that the earth drags the ether along nearly at its full speed, so that the relative velocity between the ether and the earth at the surface is zero or very small. This assumption, however, is a very dubious one because it contradicts some other important theoretical considerations. Lorentz then suggested another explanation (Lorentz contraction) which in its final form yielded as a result the famous Lorentz transformation equations. These contain the gist of the whole relativity theory. The Michelson-Morley experiment was continued by Morley and

Miller, who again obtained a negative result. Miller then continued alone, and seems now to get some positive effect. This effect, however, has nothing to do with the orbital motion of the earth. It seems to be due to a velocity of the solar system relative to stellar space, which may be much greater than the orbital velocity.

The observations of Mr. Miller have stimulated new interest in the problem. An excellent piece of work has already been done by Mr. Kennedy, whose report you will hear. I intend myself to go over the experiments again, but several months may pass before I shall be able to give my results, which, I hope, will shed more light on the subject.

II. PROFESSOR H. A. LORENTZ (LEIDEN, HOLLAND)

The motion of the earth through a hypothetical ether (talking in historical terms) might have an effect on different phenomena. The first relevant phenomenon found experimentally was the aberration of light. It was discussed on the basis of the emission theory and also on the wave theory in the form Fresnel had given it. From Fresnel's point of view we may argue as follows: We draw our diagrams in a system of co-ordinates which is fixed to the earth. In this system all ponderable matter is at rest. But the ether may move through it. Say the velocity of the ether is w. If the ether does not move, then the velocity of light through matter would be $u=c/\mu$ (μ = index of refraction; c = velocity of light). Now let an elementary wave be formed around P. This after the time dt will be a sphere of radius udt. The center O of this wave will, however, not coincide with P, being displaced over a distance $kwdt$, where $1-k$ is Fresnel's coefficient $1-1/\mu^2=\rho$. Thus $k=1/\mu^2$. PQ is a ray of light. (We denote by v the velocity of the *rays* of light.)

We have then from Figure 4, in which $PQ=vdt$, $PO=kwdt$, and $OQ=udt$, the relation

$$PQ:PO:OQ=v:kw:u\,.$$

Thus

$$u^2=v^2+k^2w^2-2kv\cos\vartheta \qquad (1)$$

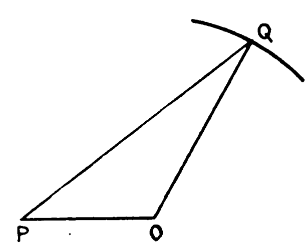

FIG. 4

The derivation of this formula is based on Huyghens' principle and Fresnel's entrainment. Huyghens' principle can be used in any case. One has simply to follow the elementary waves and to construct the successive wave fronts. As to the coefficient of entrainment, I mention that Fresnel found it at first on a mechanical basis from his elastic theory of light. This was a very remarkable performance at that time.

If we neglect terms in w^2 we find

$$v=u+kw\cos\vartheta\ ,$$

$$\frac{1}{v}=\frac{1}{u}-\frac{kw}{u^2}\cos\vartheta\ .$$

The course of a ray of light between given points is determined by the condition (Fermat's principle)

$$0=\delta\int\frac{ds}{v}$$

or

$$0=\delta\int\left(\frac{ds}{u}-\frac{kw}{u^2}\cos\vartheta\ ds\right)\ . \qquad (2)$$

Suppose now

$$\frac{k}{u^2}=\text{const.},$$

that is, k is inversely proportional to μ^2. For $\mu=1$, there is necessarily $k=1$. Thus it follows that

$$k=\frac{1}{\mu^2}\ .$$

The second term in (2) becomes then

$$\frac{1}{c^2}\int w\cos\vartheta\ ds\ .$$

Now let the motion of the ether in our diagram be irrotational, so that w depends on a velocity potential φ,

$$w=\text{grad}\ \varphi\ .$$

Then the integral

$$\int w\cos\vartheta\ ds$$

for a path between two given points P and P' becomes

$$\int_P^{P'}\frac{\partial\varphi}{\partial s}\,ds=\varphi_{P'}-\varphi_P\ .$$

This has the same value for all paths, and the condition (2) becomes simply

$$\delta\int\frac{ds}{u}=0\ ,$$

as if there were no ether motion. Thus we conclude that the course of the ray is not altered by the motion of the ether.

The considerations given above also include cases of reflection and diffraction.

Now let there be two paths, 1 and 2, for a ray of light from a given point P to another given point P'. The time required for light going over them is, for the first path,

$$\int_1\frac{ds}{v}=\int_1\frac{ds}{u}-\frac{1}{c^2}\int_1 w\cos\vartheta\, ds\ ,$$

and, for the second path,

$$\int_2\frac{ds}{v}=\int_2\frac{ds}{u}-\frac{1}{c^2}\int_2 w\cos\vartheta\, ds\ .$$

The last terms in these expressions are equal. Therefore, the difference between the two times is not altered by the motion of the ether. This motion, then, has no influence on phenomena either of interference or of diffraction.

It may be remarked that the difference between the times just considered will be altered by the motion of the ether if this motion is not irrotational. The change is given by the difference of the two integrals

$$\int_1 w\cos\vartheta\, ds\qquad\text{and}\qquad\int_2 w\cos\vartheta\, ds$$

taken for the two paths between P and P'. For this difference one can write the line integral of the velocity w taken over the closed circuit formed by the two lines.

Let us consider, for instance, the earth's rotation. If the ether is stationary, its motion relative to the earth will be a rotation in the opposite direction. If now a large horizontal circuit fixed to the earth, e.g., a rectangular one, is traveled over in opposite directions by two beams of light, the relative motion of the ether will change the position of the fringes produced by the interference of these beams. This effect has been observed by Professors Michelson and Gale.

In the following there will be no question of the rotation of the earth; the annual aberration only will be considered. For the explanation of this the foregoing considerations suffice. If, at a point at some distance from the earth, the direction of the rays coming from a star is given in a system of co-ordinates in which the earth is moving, one can deduce from that the direction of the rays in a system of co-ordinates fixed to the earth, and the further course of these relative rays is determined by the ordinary laws of optics.

We proceed with the discussion of some special theories. In Fresnel's theory the ether is supposed to be at rest; its motion relative to the earth may be considered as a uniform translation, which, obviously, is irrotational. It is necessary to introduce the dragging coefficient because the ether moves through the ponderable bodies (lenses) contained in our instruments of observation.

Stokes proposed a theory in which the ether was supposed to have an irrotational motion, such that at all points of the earth's surface its velocity is equal to that of the earth. By this latter assumption he could avoid the introduction of Fresnel's coefficient.

However, at least when the ether is supposed to be incompressible, Stokes's assumptions contradict each other. If a sphere moves with a constant velocity in an incompressible medium, the motion of the medium is completely determined by the condition that it is irrotational and that, in the direction of the normal to the surface, a point of the sphere and the adjacent medium have the same velocity. In a tangential direction the two velocities will necessarily be different.

So far as aberration is concerned, a modification of Fresnel's theory is certainly admissible. When we admit his value of the dragging coefficient, we may assume the existence of any motion of the ether, provided that it be irrotational. In fact, this is a necessary condition. Suppose, for instance, that over a part of the earth's surface which may be considered as plane the ether flows in a horizontal direction x with a velocity w_x increasing with the height y above the earth. This motion would not be irrotational and would not lead to the observed aberration. Since the existence of a velocity potential requires the equality of the derivatives $\partial w_x/\partial y$ and $\partial w_y/\partial x$, the observed aberration can exist only when, in addition to the supposed motion in a horizontal direction, there is a vertical velocity of the ether of sufficient magnitude, varying from one point of the surface to the other.

So far there was question of first-order effects only, i.e., of effects that would be proportional to the first power of the ratio between the velocity of the earth and the speed of light. In almost all cases in which astronomers and physicists have tried to detect an influence of the earth's motion on optical and electromagnetic phenomena, only effects of this order of magnitude could have been observed. The fact that all these attempts have been fruitless, and that this could be accounted for by theoretical considerations of the kind just preceding, led by and by to the conviction that the motion of the earth can never produce a first-order effect. This conviction was greatly strengthened when Einstein developed his theory of relativity and simply postulated that the result of all experiments which we perform in our laboratories must be independent of the motion of the earth, whatever may be the refinement of our measurements and the order of the effects which we can reach by them. To the experimental evidence which we already had, the charm of a beautiful and self-consistent theory was then added.

Historically, I might add that before the relativity theory was developed the situation was somewhat similar to that which now characterizes the quantum problem. There were, of course, not so many people working in the field as there are now. Nevertheless, we had often very lively discussions about the subject. I remember especially the assembly of the German Society of Natural Sciences in Düsseldorf in 1898, at which numerous German physicists were present, Planck, W. Wien, Drude, and many others. We discussed especially the question of the first-order effects. Some devices with which such an effect might be observed were proposed, but none of these attempts was ever made, so far as I know. The conviction that first-order effects do not exist became by and by too strong. We even got, finally, into the habit of looking only at the summary of experimental papers which dealt with such effects. In case the result was properly negative we felt perfectly satisfied.

As to the second-order effect, the situation was much more difficult. The experimental results could be accounted for by transforming the co-ordinates in a certain manner from one system of co-ordinates to another. A transformation of the time was also necessary. So I introduced the conception of a local time which is different for different systems of reference which are in motion relative to each other. But I never thought that this had anything to do with the real time. This real time for me was still represented by the old classical notion of an absolute time, which is independent of any reference to special frames of co-ordinates. There existed for me only this one true time. I considered my time transformation only as a heuristic working hypothesis. So the theory of relativity is really solely Einstein's work. And there can be no doubt that he would have conceived it even if the work of all his predecessors in the theory of this field had not been done at all. His work is in this respect independent of the previous theories.

I shall have little to say about the theory of the Michelson-Morley experiment, which was the first ever made of those in which we are concerned with effects of the second order. That here again the result must be negative is immediately clear if we follow the theory of relativity. If, instead of that, we apply to the experiment our old stationary ether, we must carefully consider the paths of the interfering rays of light and the time in which the light is propagated along each of them from the source of the point where the interference takes place.

For this purpose we can again use the fundamental equation (1). Confining ourselves to the propagation in ether, we may put $u=c$, $k=1$ so that the equation becomes

$$c^2=v^2+w^2-2vw\cos\vartheta$$

Taking into account terms of the second order w^2/c^2, we deduce from it

$$v=c\left(1+\frac{w}{c}\cos\vartheta-\frac{w^2}{2c^2}\sin^2\vartheta\right),$$

$$\frac{1}{v}=\frac{1}{c}\left\{1-\frac{w}{c}\cos\vartheta+\frac{w^2}{c^2}(\cos^2\vartheta+\tfrac{1}{2}\sin^2\vartheta)\right\}.$$

Now let there be two paths, 1 and 2, along which light can go from the point P to the point P' (Fig. 5). For each of them the time required for propagation will be represented by expressions of the form

$$\int\frac{ds}{v}=\frac{1}{c}\int ds-\frac{1}{c^2}\int w\cos\vartheta\,ds+\frac{1}{c^3}\int w^2\left(\cos^2\vartheta+\tfrac{1}{2}\sin^2\vartheta\right)ds\,, \quad (3)$$

and we shall be able to calculate the two times, if we know the lines along which the integrals are to be taken. Let the lines l_1 and l_2 (Fig. 5) represent the paths of the two rays as they would be if the ether did not move through the diagram. As has been shown, these lines are not altered by the motion so long as we confine ourselves to terms of the order w/c. They may, however, be somewhat changed when, as is now proposed, quantities of the second order are taken into account. We shall then have, for instance, the dotted lines l_1' and l_2' whose distances from l_1 and l_2, reckoned along the normals to these lines, are of the second order. We must now calculate the times of propagation for the paths l_1' and l_2', say Tl_1' and Tl_2'. Since, however, T is a minimum for l_1, as compared with neighboring lines, and since the displacements from l_1 to l_1' are of the second order, the difference between Tl_1 and Tl_1' will be of the fourth order. This may be neglected when we confine ourselves to quantities of the second order. Similarly, we may replace Tl_2' by Tl_2. This means that, in the determination of the phase differences, we may use the values of (3) for the rays, such as they would be according to the ordinary laws of optics in the absence of the earth's motion.

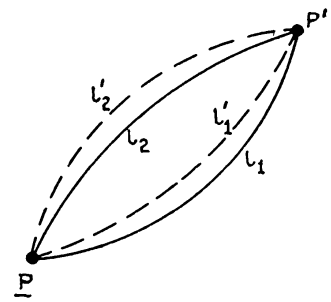

FIG. 5

We are thus led to the ordinary theory of the experiment, which would make us expect a displacement of the fringes, the absence of which is accounted for by the well-known contraction hypothesis (Lorentz contraction).

Asked if I consider this contraction as a real one, I should answer "yes." It is as real as anything that we can observe.

III. PROFESSOR DAYTON C. MILLER (CASE SCHOOL OF APPLIED SCIENCE)

The experiments on which I shall report today seem to lead to conclusions which are in contradiction to the common interpretation of the Michelson-Morley experiment. To make the story complete, I shall start with the conclusion of the experiments performed by Michelson and Morley in 1887, in Cleveland, which were interpreted as giving no indication of an ether drift. Dr. Lorentz, in 1895, proposed the first explanation for this unexpected result by assuming that the motion of translation of a solid through the ether might produce a contraction in the direction of the motion, with extension transversely, the amount of which is proportional to the square of the ratio of the velocities of translation and of light, and which might have a magnitude such as to annul the effect of the ether drift in the Michelson-Morley interferometer. The optical dimensions of this instrument were determined by the base of sandstone on which the mirrors were supported. If the contraction depends upon the physical properties of the solid, it was suggested that pine timber would suffer greater compression than sandstone, while steel might be compressed in a lesser degree. If the compression annuls the expected effect in one apparatus, it might in another apparatus give place to an effect other than zero, perhaps with the contrary sign.

At the International Congress of Physics, held in Paris in 1900, Lord Kelvin gave an address in which he considered theories of the ether. He remarked that "the only cloud in the clear sky of the theory was the null result of the Michelson-Morley experiment." Professor Morley and the writer were present, and in conversation Lord Kelvin expressed the conviction that the experiment should be repeated with a more sensitive apparatus. The writer, in collaboration with Professor Morley, constructed an interferometer about four times as sensitive as the one used in the first experiment, having a light-path of 214 feet, equal to about 130,000,000 wavelengths. In this instrument a relative velocity of the earth and ether equal to the earth's orbital velocity would be indicated by a displacement of the interference fringes equal to 1.1 fringes. This is the size of the instrument which has been used ever since. The optical parts were all new, and nothing was used from the original apparatus excepting the mercury tank and its wooden float.

Such an instrument with a base made of planks of pine wood was used at Cleveland, in 1902, 1903, and 1904, for the purpose of directly testing the Lorentz-FitzGerald effect, but the changes in the wooden frame due to the variations in humidity and temperature made it difficult to obtain accurate observations. A new supporting frame was designed by Professor F. H. Neff, of the department of civil engineering of the Case School of Applied Science, the purpose being to secure both symmetry and rigidity. This frame, or base, was made of structural steel and was so arranged that the optical dimensions could be made to depend upon distance-pieces of wood, or upon the steel frame itself. Observations were made with this apparatus in 1904. The procedure was based upon the effect to be expected from the combination of the diurnal and annual motions of the earth, together with the presumed motion of the solar system toward the constellation Hercules with a velocity of 19 km/sec. On the dates chosen for the observations there were two times of the day when the resultant of these motions would lie in the plane of the interferometer, about 11:30 A.M. and 9:00 P.M. The calculated azimuths of the motion would be different for these two times. The observations at these two times were, therefore, combined in such a way that the presumed azimuth for the morning observations coincided with that for the evening. The observations for the two times of day gave results having positive magnitudes but nearly opposite phases; when these were combined, the result was nearly zero. The result, therefore, was opposed to the theory then under consideration; but according to the ideas which will be set forth later in this address it now seems that the superposition of the two sets of observations of different phases was based upon an erroneous hypothesis and that the positive results then obtained are in accordance with a new hypothesis as to the solar motion. Our report of these experiments published in the *Philosophical Magazine* for May, 1905, concludes with the following statement: "Some have thought that this experiment only proves that the ether in a certain basement room is carried along with it. We desire therefore to place the apparatus on a hill to see if an effect can be there detected."

As an important factor I may mention the state of mind in which we then performed the experiment. It was proposed to look for a certain effect in order to check a certain theory. We had definite pictures in our minds as to what should happen. We calculated the magnitude and azimuth of the effect from the theory and discussed our experimental results in relation to these specific expectations. In every case we found that the result was negative as to these expectations. But it was never numerically zero, not even in the original Michelson and Morley experiment. It was zero in so far as the motion of the earth in its orbit is concerned. The remaining effect, however, was large enough to be measured. Experiments were performed to prove that it was not due to magnetic deformation of the frame, nor to temperature disturbances, since the effect was systematic. It was suggested that the ether might be entrained differently inside and outside of a masonry building.

In the autumn of 1905, Morley and Miller removed the interferometer from the laboratory basement to a site on Euclid Heights, Cleveland, free from obstruction by buildings, and having an altitude of about three hundred feet above Lake Erie and about eight hundred and seventy feet above sea-level. The house was purposely of very light construction, and was transparent (glass windows) in the direction of expected drift. Five sets of observations were made in 1905–1906, which give a definite positive effect of about one-tenth of the then-expected drift. Professor Morley retired from active work in 1906, and it devolved upon the present speaker to continue the experiments. It seemed desirable that further observations should be carried out at a much higher altitude, but numerous causes prevented the resumption of observations.

The deflection of the light from the stars by the sun, as predicted by the theory of relativity, was put to test at the solar eclipse of 1919. The results were widely accepted as confirming the theory. This revived the writer's interest in the ether-drift experiments, the interpretation of which had never been acceptable to him. Through the kindness of President Merriam and Directors Hale and Adams, a site was provided at the Mount Wilson Observatory on the top of Mount Wilson, at an elevation of about six thousand feet. The ether-drift interferometer was set up here in February, 1921, and observations were carried on during the succeeding five years.

Observations were begun in March, 1921, using the apparatus and methods employed by Morley and Miller in 1904, 1905, and 1906, with certain modifications and developments in details. The very first observation gave a positive effect such as would be produced by a real ether drift, corresponding to a relative motion of the earth and ether of about 10 km/sec. But before announcing such a result it seemed necessary to study every possible cause which might produce a displacement of fringes similar to that caused by ether drift; among the causes suggested were magneto-striction and radiant heat. In order to test the latter, the metal parts of the interferometer were completely covered with cork about one inch thick, and fifty sets of observations were made showing a periodic displacement of the fringes, as in the first observations, thus showing that radiant heat is not the cause of the observed effect.

In the summer of 1921 the steel frame of the interferometer was dismounted and a base of one piece of concrete, reinforced with brass, was cast in place on the mercury float. All the metal parts were made of aluminum or brass; thus the entire apparatus was free from magnetic effects and the possible effects due to heat were much reduced. In December, 1921, forty-two sets of observations were made with the non-magnetic interferometer. These show a positive effect as of an ether drift, which is entirely consistent with the observations of April, 1921. Many variations of incidental conditions were tried at this epoch. Observations were made with rotations of the interferometer clockwise and counter-clockwise, with a rapid rotation and a very slow rotation, with the interferometer extremely out of level, due to the loading of the float on one side. Many variations of procedure in observing and recording were tried. The results of the observations were not affected by any of these changes.

The entire apparatus was returned to the laboratory in Cleveland. During the years 1922 and 1923 many trials were made under various conditions which could be controlled and with many modifications of the arrangements of parts in the apparatus. An arrangement of prisms and mirrors was made so that the source of light could be placed outside of the observing-room, and a further complication of mirrors was tried for observing the fringes from a stationary telescope. Methods of photographic registration by means of a motion-picture camera were tried. Various sources of light were employed, including sunlight and the electric arc. Finally an arrangement was perfected for making observations with an astronomical telescope having an objective of five inches aperture and a magnification of fifty diameters. The source of light adopted was a large acetylene lamp of the kind commonly used for automobile headlights. An extended series of experiments was made to determine the influence of inequality of temperature and of radiant heat, and various insulating covers were provided for the base of the interferometer and for the light-path. These experiments proved that under the conditions of actual observation the periodic displacement could not possibly be produced by temperature effects. An extended investigation in the laboratory demonstrated that the full-period effect mentioned in the preliminary report of the Mount Wilson observations is a necessary geometrical consequence of the adjustment of mirrors when fringes of finite width are used and that the effect vanishes only for fringes of infinite width, as is presumed in the simple theory of the experiment.

In July, 1924, the interferometer was taken again to Mount Wilson and mounted on a new site where the temperature conditions were more favorable than those of 1921. The interferometer house was also mounted with a different orientation. Again the observations showed a real periodic displacement of the fringes, as in all the observations previously made at Mount Wilson and at Cleveland.

In spite of long-continued efforts, it was impossible to account for these effects as being due to terrestrial causes or to experimental errors. Very extended calculations were made in the effort to reconcile the observed effects with the accepted theories of the ether and of the presumed motions of the earth in space. The observations were repeated at certain epochs to test, one after another, the hypotheses which were suggested. At the end of the year 1924, when a solution seemed impossible, a complete calculation of the then-expected effects, for each month of the year, was made for the first time. This indicated that the effect should be a maximum about April 1, and further, that the direction of the effect should, in the course of the twenty-four hours of the day, rotate completely around the horizon. Observations were made for verifying these predictions in March and April, 1925. The effect was equal in magnitude to the largest so far observed; but it did not point successively to all points of the compass, that is, it did not point in directions 90° apart at intervals of six hours. Instead of this, the direction merely oscillated back and forth through an angle of about 60°, having, in general, a northwesterly direction.

Previous to 1925, the Michelson-Morley experiment has always been applied to test a specific hypothesis. The only theory of the ether which has been put to the test is that of the absolutely stationary ether through which the earth moves without in any way disturbing it. To this hypothesis the experiment gave a negative answer. The experiment was applied to test the question only in connection with specific assumed motions of the earth, namely, the axial and orbital motions combined with a constant motion of the solar system toward the constellation Hercules with the velocity of about 19 km/sec. The results of the experiment did not agree with these presumed motions. The experiment was applied to test the Lorentz-FitzGerald hypothesis that the dimensions of bodies are changed by their motions through the ether; it was applied to test the effects of magneto-striction, of radiant heat, and of gravitational deformation of the frame of the interferometer. Throughout all these observations, extending over a period of years, while the answers to the various questions have been "no," there has persisted a constant and consistent small effect which has not been explained.

The ether-drift interferometer is an instrument which is generally admitted to be suitable for determining the relative motion of the earth and the ether; that is, it is capable of indicating the direction and the magnitude of the absolute motion of the earth and the solar system in space. If observations were made for the determination of such an absolute motion, what would be the result, independent of any "expected" result? For the purpose of answering this general question, it was decided to make more extended observations at other epochs in 1925, and this was done in the months of July, August, and September.

It may be asked: Why was not such a procedure adopted before? The answer is, in part, that we were concerned with the verification of certain predictions of the so-called "classical" theories, and in part that it is not easy to develop a new hypothesis, however simple, in the absence of direct indication. Probably a considerable reason for the failure is the great difficulty involved in making the observations at all times of day at any one epoch. I think I am not egotistical, but am merely stating a fact when it is remarked that the ether-drift observations are the most trying and fatiguing, as regards physical, mental, and nervous strain, of any scientific work with which I am acquainted. The mere adjustment of an interferometer for white-light fringes and the keeping of it in adjustment, when the light-path is 214 feet, made up of sixteen different parts, and when it is in effect in the open air, requires patience as well as a steady "nerve" and a steady hand. Professor Morley once said, "Patience is a possession without which no one is likely to begin observation of this kind." The observations must be made in the dark; in the daytime, the interferometer house is darkened with black paper shades; the observations must be made in a temperature which is exactly that of the out-of-door air; the observer has to walk around a circle about twenty feet in diameter, keeping his eye at the moving eyepiece of the telescope attached to the interferometer, which is floating on mercury and is turning on its axis steadily, at the rate of about one turn a minute; the observer must not touch the interferometer in any way, and yet he must never lose sight of the interference fringes, which are seen only through the small aperture of the eyepiece of the telescope, about a quarter of an inch in diameter; the observer makes sixteen readings of the position of the interference fringes in each turn, at times indicated by an electrical clicker; these operations must be continued without a break through a set of observations, which usually lasts for about fifteen or twenty minutes, and this is repeated continuously during the several hours of the working period.

When observations are in progress, the interferometer to which the observing telescope is attached is caused to rotate on the mercury float so that the telescope points successively to all points of the compass, that is, it points to all azimuths. A relative motion of the earth and the ether should cause a periodic displacement of the interference fringes, the fringes moving first to one side and then to the other as referred to a fiducial point in the field of view, with two complete periods in each rotation of the instrument. Beginning when the telescope points north, the position of the fringes is noted at sixteen equidistant points around the horizon. The azimuth of the line of sight when the displacement is a maximum having been noted at two different times of day, it is a simple operation to calculate the right ascension and declination, or the "apex" of the presumed "absolute" motion of the earth in space. The determination of the direction of the earth's motion is dependent only upon the direction in which the telescope points when the observed displacement of the fringes is a maximum; it is in no way dependent upon the amount of this displacement or upon the adjustment of the fringes to any particular zero position. As the readings are taken at intervals of about three seconds, the position of the maximum is dependent upon observations covering an interval of less than ten seconds. A whole period of the displacement extends over only about twenty-five seconds. Thus the observations for the direction of the absolute motion are largely independent of ordinary temperature disturbances. The observation is a differential one, and can be made with considerable certainty under all conditions. A set of readings usually consists of twenty turns of the interferometer made in about fifteen minutes' time; this gives forty determinations of the periodic effect. The forty values are simply averaged to give one "observation." Any temperature effect, or other disturbing cause, which is not regularly periodic in each twenty seconds over an interval of fifteen minutes would largely be canceled out in the process of averaging. The periodic effect remaining in the final average must be real.

The position of the fringe system is noted in units of a tenth of a fringe-width. The actual velocity of the earth's motion is determined by the amplitude of the periodic displacement, which is proportional to the square of the relative velocity of the earth and the ether and to the length of the light-path in the interferometer. A relative motion of 30 km/sec., equal to the velocity of the earth in its orbit, would produce a displacement of the fringes from one extreme to the other, of 1.1 fringes. Disturbances due to temperature or other causes lasting for a few seconds or for a few minutes might affect the actual amount of the observed displacement and thus give less certain values for the velocity of relative motion, while at the same time the position of maximum displacement is not disturbed. Thus it is to be expected that the observations for the velocity of motion will not be as precise as the observations for the direction of motion. The two things, magnitude and azimuth of observed relative motion, are quite independent of each other.

It is desirable to have observations equally distributed over the twenty-four hours of the day; since one set requires about fifteen minutes of time, ninety-six sets, properly distributed, will suffice. The making of such a series usually occupies a period of ten days. The observations are finally reduced to one group, and the mean date is considered the date of the epoch. The observations made at Mount Wilson in 1925 correspond to the three epochs, April 1, August 1, and September 15, and are more than twice as numerous as all the other ether-drift observations made since 1881. The total number of observations made at Cleveland represents about one thousand turns of the interferometer, while all the observations made at Mount Wilson previous to 1925 correspond to 1200 turns. The 1925 observations consist of 4400 turns of the interferometer, in which over 100,000 readings were made. A group of eight readings gives a value for the magnitude and direction of the ether-drift function, so that 12,500 single measures of the drift were obtained. This required that the observer should walk, in the dark, in a small circle, for a total distance of one hundred miles, while making the readings. Throughout these observations the conditions were exceptionally good. At times there was a fog which rendered the temperature very uniform. Four precision thermometers were hung on the outside walls of the house; often the extreme variation of temperature was not more than one-tenth of a degree, and usually it was less than four-tenths of a degree. Such variations did not at all affect the periodic displacement of the fringes. It may be added that while the readings are being taken, neither the observer nor the recorder can form the slightest opinion as to whether any periodicity is present, much less as to the amount or direction of any periodic effect.

The hundred thousand readings are added in groups of twenty, are averaged, and then plotted in curves. These curves are subjected to mechanical harmonic analysis for the purpose of determining the azimuth and magnitude of the drift. In this work all the original observations have been used, without any omissions and without the assignment of weights; furthermore, there are no corrections of any kind to be applied to the observed values. The results of the analyses are finally charted in such a way as to show the variation in the azimuth of the drift throughout the day of twenty-four hours for each epoch, and the variation in magnitude is similarly charted.

[The observations of 1925 were described and the details of the results were shown by means of lantern-slide diagrams. A similar report constituted the address of the President of the American Physical Society read at Kansas City on December 29, 1925. This address is printed in full in *Science*, **63**, 433–443, April 30, 1926.]

A calculation based only on the observations of 1925 was made to determine the absolute motion of the earth. The result of this, as reported at the Kansas City meeting, indicated that the solar system is moving toward an apex in the constellation Draco with a velocity which is in excess of 200 km/sec. In order to confirm the Kansas City report, a set of observations consisting of 2020 turns of the interferometer was made at Mount Wilson, corresponding to the epoch February 10, 1926. A complete calculation has now been made, including the observations of both 1925 and 1926, which leads to the following conclusion: The ether-drift experiments at Mount Wilson show, first, that there is a systematic displacement of the interference fringes of the interferometer corresponding to a constant relative motion of the earth and the ether at this observatory of 10 km/sec., with a probable error of 0.5 km/sec.; and, second, that the variations in the direction and magnitude of the indicated motion are just such as would be produced by a constant motion of the solar system in space, with a velocity of 200 km/sec., or more, toward an apex in the constellation Draco, near the pole of the ecliptic, which has a right ascension of 255° (17 hours) and a declination of +68°; and, third, that the axis across which the observed azimuth of drift fluctuates, because of the rotation of the earth on its axis, points in a northwesterly direction, whereas the simple theory indicates that this axis should coincide with the north and south meridian.

The arguments which have led to these conclusions may be illustrated by means of Figures 6 and 7. In the lower part of Figure 6 the four light-line curves represent the average azimuths for the four epochs of observation, plotted with respect to Mount Wilson local or civil time. The curves all have the values for midnight on the

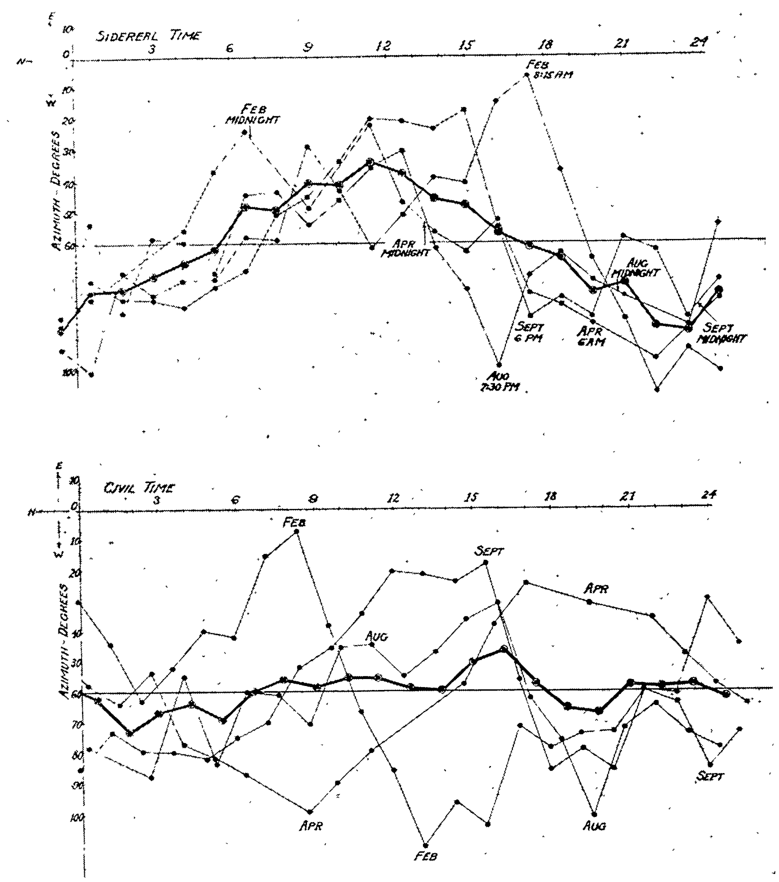

FIG. 6

ordinate for zero hours and the noon values on the ordinate for twelve hours, etc. The heavy curve represents the average of the four sets of observations and is clearly seen to be irregular and nearly zero in value. In the upper part of Figure 6, the four azimuth curves are plotted against sidereal time. The heavy-line curve representing the average is clearly a periodic curve. If the effect is due to a motion of the earth through space, the sidereal time at which this curve crosses the time axis is the right ascension of the apex of the motion. This occurs at seventeen hours. The declination of the apex is determined from the amplitude of the curve and the cosine of the latitude of the observatory, and is equal to $+68°$. Figure 7 shows, at the bottom, the average diurnal variation in the azimuth

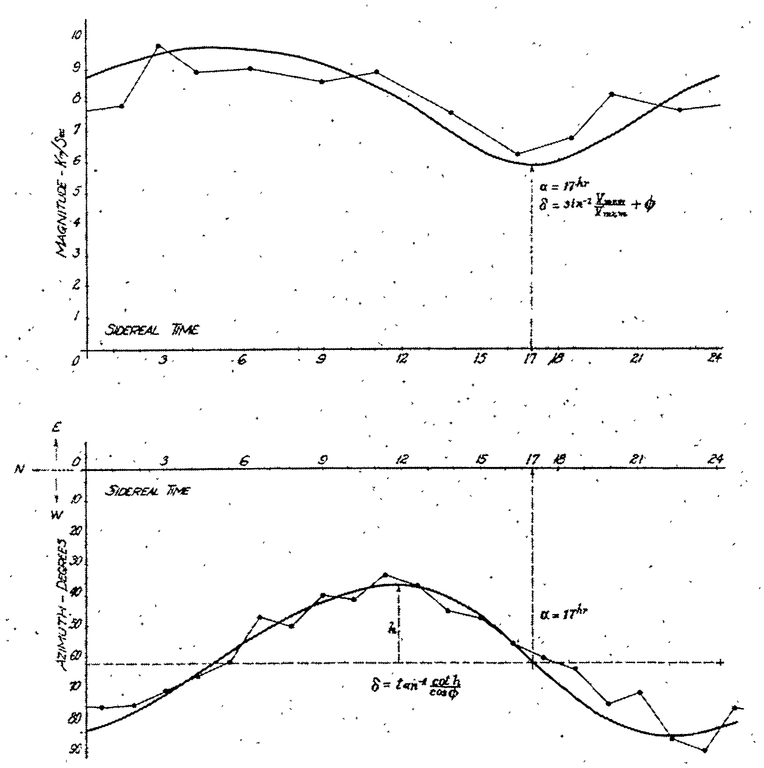

FIG. 7

(the broken line) as compared with the theoretical variation shown by the smooth curve. The upper part of Figure 7 shows, in the broken line, the average diurnal variation in the observed magnitude of the effect, while the smooth curve shows the theoretical variation. If this is due to an ether drift, the sidereal time of minimum magnitude is the right ascension of the apex. This is seventeen hours, in agreement with the right ascension obtained from the azimuth curve. The declination of the apex is dependent upon the minimum and maximum values of the effect and upon the latitude of the observatory. The computed value is about $+69°$, agreeing with that obtained from the azimuth curve. As far as instrumental considerations are concerned, the azimuth and magnitude are independent of each other; it is only when they are produced by the same cause that there is any necessary connection between them. The agreement of the calculated and observed effects for both magnitude and azimuth surely points to a real, cosmical cause. The result cannot be considered as a "null" effect; neither can it be due to instrumental or local disturbances.

The fact that the direction and magnitude of the observed ether drift are independent of local time and are constant with respect to sidereal time implies that the effect of the earth's orbital motion is imperceptible in the observations. The present experiments show no effect of the orbital motion, and hence they are no more consistent with the old theory of a stagnant ether than were the experiments of Michelson and Morley. In order to account for the absence of the orbital effect, it is assumed that the constant motion of the earth in space is more than 200 km/sec., but that for some unexplained reason the relative motion of the earth and the ether in the interferometer at Mount Wilson is reduced to 10 km/sec.; under those conditions a component motion equal to the earth's orbital motion would produce an effect on the resultant which is just below the limit of the smallest quantity which can be measured by the present interferometer. It is for this reason that it is concluded that the velocity of the motion of the solar system is at least 200 km/sec., and it may be much greater.

Several critics seem to be under the impression that the earlier Cleveland observations gave a real zero effect and that it is claimed that the present positive effect is due to the greater elevation at Mount Wilson. This is not true. The numerical values of the positive effect at Cleveland and at Mount Wilson are so nearly equal that with the observations now available (those at Cleveland being relatively few in number) it is impossible to state that there is any effect due to altitude. If there is any influence of altitude, it is certainly small; further observations at Cleveland are now being made to determine this matter.

In order to account for these effects as the result of an ether drift, it seems necessary to assume that, in effect, the earth drags the ether so that the apparent relative motion at the point of observation is reduced from 200, or more, to 10 km/sec., and further that this drag also displaces the apparent azimuth of the motion about 60° to the west of north. It is possible that the westerly deflection is influenced by the trend of the Mount Wilson range of mountains from southeast to northwest. The reduction of the indicated velocity of 200 km/sec., or more, to the observed value of 10 km/sec. may be explained by the theory of the Lorentz-FitzGerald contraction without assuming a drag of the ether. This contraction may or may not depend upon the physical properties of the solid, and it may or may not be exactly proportional to the square of the relative velocities of the earth and the ether. A very slight departure of the contraction from the amount calculated by Lorentz would account for the observed effect.

The values of the quantities defining the absolute motion of the solar system as obtained from these ether-drift observations are in general agreement with the results obtained by other methods. The recent study of proper motions of stars by Ralph Wilson, of the Dudley Observatory, and of the radial motions of the stars by Campbell and Moore, of the Lick Observatory, gives the apex of the sun's way in the constellation Hercules with a right ascension of 270° and a declination of about +30°, with a velocity of about 19 km/sec. Dr. G. Strömberg, of the Mount Wilson Observatory, from a study of globular clusters and spiral nebulae, finds evidence of a motion of the solar system toward a point having a right ascension of 307° and a declination of +56°, with a velocity of 300 km/sec. Lundmark, studying the spiral nebulae, finds evidence of a motion having a velocity of 400 km/sec. The various determinations of the motion of the solar system are all in the same general direction and lie within a circle having a radius of 20°. Our assumed velocity of 200 km/sec. is simply a lower limit; it might equally well be 300 or 400 km/sec. The first assumption therefore seems to offer no difficulty. The location of the apex in the constellation Draco, at right ascension 255° and declination +68°, is within 6° of the pole of the ecliptic, that is, the indicated motion of the solar system is almost perpendicular to the plane of the ecliptic. The sun's axis of rotation points to within 12° of this apex. One cannot help wondering whether there may be some dynamic significance in these facts.

The argument now being presented can be demonstrated only by means of observations extending over the whole twenty-four hours of the day, in order to determine the exact form of the daily variation in magnitude and azimuth of the effect, and by means of observations made at different times of year, in order to prove that the effect is dependent on sidereal time. The earlier observations of 1887 and 1905 are not sufficiently numerous and are not distributed throughout the day in such a manner as to make it possible to calculate the direction of the drift. These earlier observations were made for the purpose of detecting the earth's orbital motion and consequently were made at two selected times of day, such that at one time the magnitude of this particular effect would be a maximum and at the other time it would be zero; or, two times of day were chosen in which the azimuth of the orbital component of motion would have very different values. Furthermore, until the year 1925 the experiments have never been carried out at intervals of six months. The reason that a second set of experiments has not been made after this interval before is simply that in no instance has the expected effect been found in a first set.

The observations made at Cleveland by Michelson and Morley in 1887, and later repeated by Morley and Miller, have just been recomputed on the present hypothesis; while the earlier observations are not sufficient to determine the direction of the drift, they are nevertheless shown to be entirely consistent with the conclusions drawn from the Mount Wilson experiments. Or, to state the converse, the present result wholly confirms the earlier experiments of Michelson and Morley, giving no evidence of the effect of the earth's orbital motion. In addition to this, the recent experiments, by a thorough study of the residual effects, have shown that there is a systematic cosmical effect as of a true ether drift. This conclusion introduces a new question, "Why is the magnitude of the effect less than would be expected on the classical theories and why is the direction of the effect at Mount Wilson deflected to the westward?" This question certainly is no more difficult than are many others now awaiting solution.

The interferometer is being set up again on the campus of Case School of Applied Science in Cleveland, near the place where the original Michelson-Morley experiment was performed in 1887. It is proposed to make a series of observations for four epochs of the year, comparable in every way with the Mount Wilson series. This will give information as to the possible effects of local conditions; it is hoped that it will show more definitely whether there is any effect due to altitude, and whether the orbital motion is appreciable.

IV. DR. ROY J. KENNEDY (CALIFORNIA INSTITUTE OF TECHNOLOGY)

When Professor Miller published the conclusions that he presented to us yesterday, it became necessary, or at least very desirable, that the experiment be repeated independently. It is such a performance of the experiment that I shall discuss this morning.

In this experiment the light-paths were reduced to about 4 m, and the required sensitiveness was obtained by an arrangement capable of detecting a very slight displacement of the interference pattern. The whole optical system was inclosed in a sealed metal case containing helium at atmospheric pressure. Because of its small size, the apparatus could be effectively insulated, and circulation and variations in density of the gas in the light-paths nearly eliminated. Furthermore, since the value of $\mu-1$ for helium is only about one-tenth that for air at the same pressure, it will be seen that the disturbing effects of changes in density of the gas correspond to those in air at only a tenth of an atmosphere of pressure. Actually it was found that any wavering of the interference pattern was imperceptible, and when temperature equilibrium had been reached there was no steady shift.

The plan of the apparatus is sketched in Figure 8. The optical parts are mounted on a marble slab 122 cm square by 10.5 cm thick, which rests on an annular float in a pan of mercury 77 cm in diameter. This is simply a reduced copy of Michelson's original mounting. The mirrors M_1, M_4, and M_5 are fixed in position; such adjustments of the compensating plate C and mirror M_2 as are necessary after the cover is in place can be made from the observer's position at the telescope. The green light λ 5461 is separated by the lens and prism system from the radiation of a small mercury arc lamp

S attached to the slab, and passed through a small hole in the screen Z. The pencils of light are carefully limited by screens and by focusing in order to prevent stray light from reaching the eye and thereby reducing its sensitiveness. Adjustments are made so that broad fringes are formed at the surface of M_1 and M_2, on which the telescope is focused. Final adjustments are made by rotating the compensating plate C by means of a fine differential screw, and by placing small weights near the corner of the slab; under proper condi-

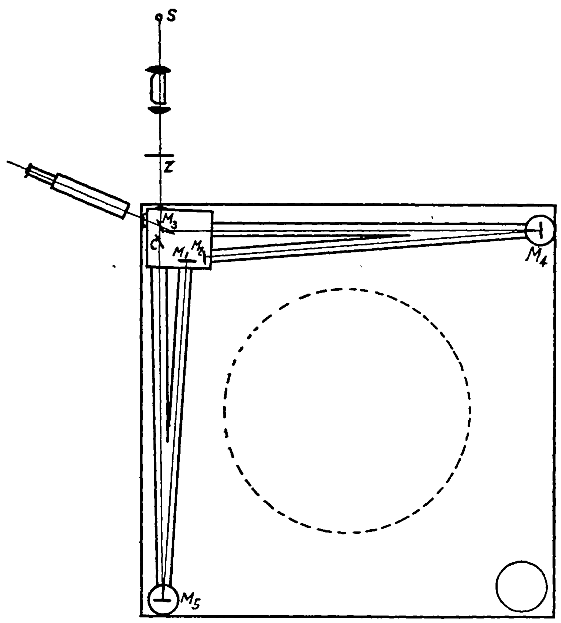

FIG. 8

tions a 5-g weight deflects the heavy slab just perceptibly. The adjusting screws are manipulated by means of spindles passing through short flexible tubes in such a way as to be freely rotatable but air tight. After the mirrors are given preliminary alignment, the cover is carefully lowered into place, sealed to the slab, and then filled with helium.

Schematically, the arrangement of the interferometer is shown in Figure 9. A beam of practically plane-parallel, homogeneous light, plane-polarized so that its electric vector lies in the plane of the paper, moves to the right and falls on the mirror M_3 at the polarizing angle for the given wave-length. At the upper face the beam is split by a thin platinum film into two parts of nearly equal intensity, one passing to the mirror M_1 and the other to M_2. From there they are reflected back to M_3, where they recombine and pass to the eye through a telescope focused on M_1 and M_2. Two purposes are accomplished by the use of plane-polarized light: first, the non-interfering rays indicated by the dotted lines, which would be produced with natural light, are completely eliminated; and, second, the recombining beams can be adjusted to perfect equality of intensity by varying the relative reflecting powers of M_1 and M_2. Be-

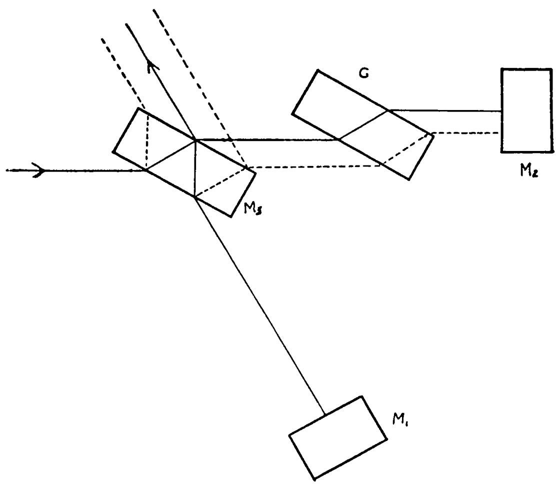

FIG. 9

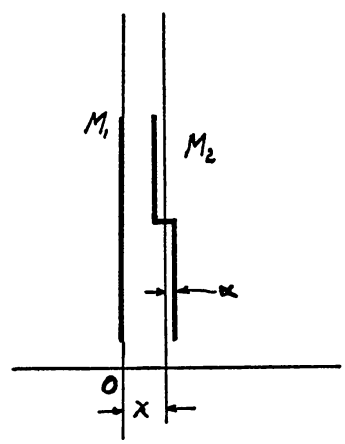

FIG. 10

cause there are two more glass-air interfaces to be traversed by the upper beam than by the lower, it is impossible to equalize both components of natural light in this way.

The high sensibility necessary because of the short paths is secured chiefly by the simple device of raising one-half of the surface of mirror M_2 a small fraction of a wave-length above the other, the dividing line between the two levels being straight and as sharp as possible. The mirror used was made by covering part of a plane plate with a flat sharp-edged microscope cover-glass and applying the extra thickness by cathode deposition of platinum, thereafter giving the whole plate a fully reflecting coat. I ran across the suggestion of using such a divided mirror in interferometry some years ago, but am unaware to whom the credit for it belongs.

The theory of the arrangement is as follows: The interference phenomena will be the same as if the mirror M_2 were replaced by its image in M_3. Under the conditions of the experiment, where the paths are nearly equal, M_1 is perpendicular to the beam incident on it, and the reflected beams are brought nearly to parallelism, the image of M_2 will be nearly parallel and coincident with the face of M_1. Elementary theory shows that the resulting interference pattern then practically coincides with M_1. It would needlessly complicate this discussion to develop the general theory of interference for all inclinations of the mirrors; the experimentally realized case of near parallelism alone is necessary.

Let Figure 10 represent a greatly exaggerated cross-section of M_1 and the image of M_2, normal to their planes and to the dividing line in M_2. M_1 lies in the plane $x=0$, and the levels of M_2 are at equal distances on opposite sides of a parallel plane at the distance x from M_1. Let a monochromatic wave, in which the displacement is given by

$$\xi = a \cos \omega\left(t+\epsilon-\frac{x}{c}\right) ,$$

fall on M_1 and M_2 from the left. At the surface of M_1 the displacement in the reflected wave is then given by

$$\xi_1 = a \cos \omega(t+\epsilon)$$

if we ignore the loss through imperfect reflection. The displacement in the plane of M_1 in the wave reflected from the upper part of M_2 is

$$\xi_2 = a \cos \omega\left[t+\epsilon-\frac{2(x-\alpha)}{c}\right] .$$

The square of the resultant displacement is then

$$(\xi_1+\xi_2)^2 = a^2 \left\{ \cos \omega(t+\epsilon)+\cos \omega\left[t+\epsilon-\frac{2(x-\alpha)}{c}\right] \right\} .$$

This can be reduced to the form

$$2a^2\left[1+\cos \frac{2\omega}{c}(x-\alpha)\right] \cos^2 \omega(t-\delta) .$$

Similarly, the square of the resultant displacement in the interfering beams below the dividing line is found to be

$$2a^2\left[1+\cos\frac{2\omega}{c}(x+a)\right]\cos^2\omega(t-\delta)\ .$$

The intensities, being proportional to the squares of the amplitudes, can be represented by

$$I_1=ka^2\left[1+\cos\frac{2\omega}{c}(x-a)\right]$$

and

$$I_2=ka^2\left[1+\cos\frac{2\omega}{c}(x+a)\right]\ .$$

Now $\omega=2\pi\nu$ where ν=frequency of the light. Hence $\omega/c=2\pi/\lambda$. Therefore

$$I_1=ka^2\left[1+\cos\frac{4\pi}{\lambda}(x-a)\right]$$

and

$$I_2=ka^2\left[1+\cos\frac{4\pi}{\lambda}(x+a)\right]\ .$$

For values of $x=n\lambda/4$, where n is an integer,

$$I_1=ka^2\left(1\pm\cos\frac{4\pi a}{\lambda}\right)\ ,$$

the sign being positive for even values of n and negative for odd values. The same expression holds for I_2; hence, under these conditions,

$$I_1=I_2\ .$$

To the observer, then, the field of view is equally intense on both sides of the dividing line when $x=n\lambda/4$.

We have now to determine the least change in x from this value which will produce a perceptible difference in illumination in the two sides of the field. If x is given the variation δx while a is kept constant, the difference in intensity will be

$$\delta I=\left(\frac{\partial I_1}{\partial x}-\frac{\partial I_2}{\partial x}\right)\delta x\ .$$

Now

$$\frac{\partial I_1}{\partial x} = \pm \frac{4\pi k a^2}{\lambda} \sin \frac{4\pi a}{\lambda}\,.$$

Similarly,

$$\frac{\partial I_2}{\partial x} = \mp \frac{4\pi k a^2}{\lambda} \sin \frac{4\pi a}{\lambda}\,.$$

Therefore

$$\delta I = \pm \left[\frac{8\pi k a^2}{\lambda} \sin \frac{4\pi a}{\lambda}\right] \delta x\,,$$

the sign being of no importance.

The perceptibility of the variation is determined not by δI alone, but by the ratio of δI to the total intensity, I_1 or I_2. According to the Weber-Fechner law, if δI is taken to be the least perceptible variation in intensity, the foregoing ratio is nearly constant for a considerable range of intensities. With this meaning of δI, δx becomes the least detectable change of position of M_2.

If initially we have uniformity of illumination, we have from the equations above,

$$\frac{\delta I}{I} = \frac{8\pi}{\lambda}\,\delta x \frac{\sin \frac{4\pi a}{\lambda}}{1 \pm \cos \frac{4\pi a}{\lambda}}\,,$$

or

$$\delta x = \frac{\lambda}{8\pi}\,\frac{\delta I}{I}\,\frac{1 \pm \cos \frac{4\pi a}{\lambda}}{\sin \frac{4\pi a}{\lambda}}\,,$$

If now $\delta I/I$ were a true constant, we should have for the case of negative sign, which corresponds to dim illumination of the field, the sensibility of the apparatus increasing indefinitely as the factor a was made smaller. I decreases with a, however, and the Fechner "constant" soon diminishes rapidly. Nevertheless, the conditions of illumination and contrast here are similar to those in the half-shade polariscope, and from the theory of the Lippich instrument it appears that $\delta I/I$ equals about 8×10^{-3}. The lack of perfect planeness in the mirrors and of equality of intensity in the interfering beams is a further limiting factor; a little experimenting indicated that a should not be much less than $0.025\ \lambda$, which was the value finally used. Substituting these values in the last equation, we get

$$\delta x = 5 \times 10^{-5}\ \lambda$$

as the least detectable change in position of one of the mirrors. This corresponds to a change of optical length of path

$$\delta l = 2\delta x = 10^{-4}\ \lambda\ .$$

To take full advantage of the possibilities of the arrangement would have required perfect mirrors and an intenser and, therefore, hotter source of light than would have been desirable near the sensitive apparatus, as well as lengthening the interval between observations, thus allowing greater chance for any steady temperature shift to show itself. No attempt was made in the experiment, therefore, to go below values of δl equal to $2 \times 10^{-3}\ \lambda$; such variations were detectable without the least uncertainty.

With this apparatus the velocity of 10 km/sec. found by Professor Miller would produce a shift corresponding to 8×10^{-3} wavelengths of green light, which is four times the least detectable value.

The experiment was performed in a constant-temperature room in the Norman Bridge Laboratory at various times of day, but oftenest at the time when Miller's conclusions require the greatest effect. The sensitiveness of the eye was tested for each trial by the placing or removal of a small weight on the slab before and after rotating it. There being no fluctuations in the field of view, it was unnecessary to take the average of a number of readings. As has been shown, a shift as small as one-fourth that corresponding to Miller's would have been perceived. The result was perfectly definite. There was no sign of a shift depending on the orientation.

Because an ether drift might conceivably depend on altitude, the experiment was repeated on Mount Wilson, in the 100-inch telescope building. Here again the effect was null.

[*Note added April, 1928.*—Illingworth at the California Institute of Technology has continued the work with Kennedy's apparatus, using improved optical surfaces and a method of averaging. He concludes[1] that no ether drift as great as 1 km/sec. exists.]

[1] *Physical Review*, **30**, 692, 1927.

V. PROFESSOR E. R. HEDRICK (UNIVERSITY OF CALIFORNIA AT LOS ANGELES)

[Because of lack of time Professor Hedrick presented only a summary of the following contribution, prepared by himself and Professor Ingold of the University of Missouri.]

I. INTRODUCTION

The celebrated experiment by Michelson to determine the relative motion of the earth and the luminiferous ether was first made in 1881.[1] Objection to the mathematical theory was raised by H. A. Lorentz in 1886,[2] and in 1887 the theory was modified by Michelson and Morley to meet this objection.[3] It is the theory accompanying the account of their 1887 experiment that is usually given and that is now generally accepted.

Until about 1898 it does not appear that any further serious objections were raised against the theory. From that time on, however, numerous papers[4] dealing with the matter have appeared, which, in many instances, contain objections to one feature or another of the theory. The differences of opinion appear to arise mainly from different conceptions regarding the mechanism of interference phenomena.

In view of the wide diversity of opinion on the subject, it has seemed worth while to work out the theory anew, on the basis of some reasonable hypothesis that has been employed in dealing with other phases of interference phenomena.

Some portions of the present investigation appear to be closely related to part of the work of Righi as reported by Stein,[5] and

[1] *American Journal of Science*, **22**, 120, 1881.

[2] *Archives Néerlandaises*, **31**, 2^me livre, 1886.

[3] *Philosophical Magazine* (5), **24**, 449, 1887.

[4] We mention the following: Sutherland, *ibid.* (5), **46**, 23, 1898; Hicks, *ibid.* (6), **3**, 9, 1902; Sutherland, *Nature*, **63**, 205, 1900; Luroth, *Ber. d. Bayr. Ak. d. W.*, **7**, 1909; Kohl, *Annalen der Physick*, **28**, 259, 1909; Budde, *Physikalische Zeitschrift*, **12**, 979, 1911, and **13**, 825, 1912; Righi, *Sessions of the Royal Institute of Bologna*, 1919 and 1920.

For replies to some of these articles consult the following: Lodge, *Philosophical Magazine* (5), **46**, 1898; Morley and Miller, *ibid.* (6), **9**, 669, 1905; Laue, *Annalen d. Physik*, **33**, 186, 1910, and *Physikalische Zeitschrift*, **13**, 501, 1912; Debye, *Beiblätter zu den Annalen der Physik*, **34**, 1910.

[5] "Michelson's Experiment and Its Interpretation according to Righi," *Memorie della Societa Astronomica Italiana*, **1**, 283, 1920.

confirms, by an independent calculation, some of Righi's results, which is a matter of great importance, since the accuracy of his work has been called in question.[1]

2. REFLECTION FROM A MOVING MIRROR

We begin by obtaining certain general formulae for the reflection of light from a moving mirror. Two cases are considered: (*a*) the direction of motion of the mirror coincides with the direction of the rays of light before reflection; (*b*) the direction of motion of the mirror makes an angle Θ with the direction of the rays of light.

a) Denote the velocity of light by c and the velocity of the

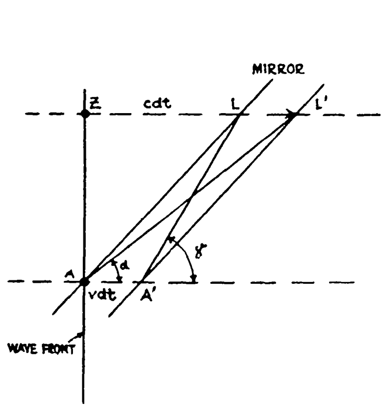

FIG. 11

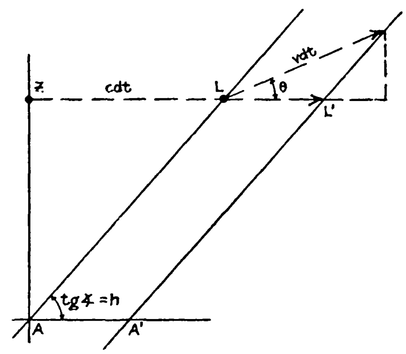

FIG. 12

mirror by v. Let h represent the tangent of the angle of inclination of the mirror to the direction of motion.

In Figure 11, AZ represents the front of a wave advancing on the mirror at A. While the mirror moves from AL to $A'L'$, the portion of the wave at Z traverses the distance ZL'. Therefore, denoting the angle $A'AL'$ by α, we have

$$\tan \alpha = \frac{AZ}{ZL'} = \left(1 - \frac{v}{c}\right) h = (1-\beta) h ,$$

which gives the position of the equivalent fixed mirror.

Similarly, $A'L$ is the position of the equivalent fixed mirror for a ray coming from the opposite direction CA; and if we denote $CA'L$ by γ, we have

$$\tan \gamma = (1+\beta) h .$$

[1] See *Observatory*, 44, 340–341, 1921.

b) If the direction of motion of the mirror makes an angle with the direction of the rays, then from Figure 12 it is clear that the mirror really advances with a velocity

$$v \cos \Theta - \frac{v \sin \Theta}{h} ,$$

so that the formulae for this case may be obtained from those of the previous case by putting

$$\beta\left(\cos \Theta - \frac{\sin \Theta}{h}\right)$$

in place of β.

If the mirror is inclined at an angle of 45° to the direction of the rays of light, $h=1$ and

$$\tan \alpha = 1 - \beta(\cos \Theta - \sin \Theta) ,$$

while

$$\tan \gamma = 1 + \beta(\cos \Theta - \sin \Theta) .$$

3. APPLICATION TO THE MICHELSON-MORLEY EXPERIMENT

In the Michelson-Morley experiment a ray of light from a source S (Fig. 13) meets a half-silvered glass plate, inclined at 45° to its path, at A. A portion is reflected to a mirror at B, parallel to SA, from which it is again reflected to pass through the plate at A' and finally into a telescope at T. Another portion is transmitted through the glass plate at A to a mirror at C, perpendicular to SA, from which it is returned to the glass plate at A' and from there a further portion is reflected into the telescope at T. When the mirrors are set as described, with absolute accuracy, we call the experiment the "ideal Michelson-Morley experiment." We wish to compute the angle $T'A'T$.

We assume that the earth and the apparatus are moving through the ether in a direction making an angle Θ with the path of the rays SA.

It will be necessary to determine the position of the equivalent fixed mirror at B.

For convenience denote $\beta(\cos \Theta - \sin \Theta)$ by ξ. Then the angle $CAB = 2\alpha$ where $\tan \alpha = 1 - \xi$.

In Figure 14, if BE is the wave front of the ray reflected from A and if the mirror at B advances from BM to $B'M'$ (a distance r in

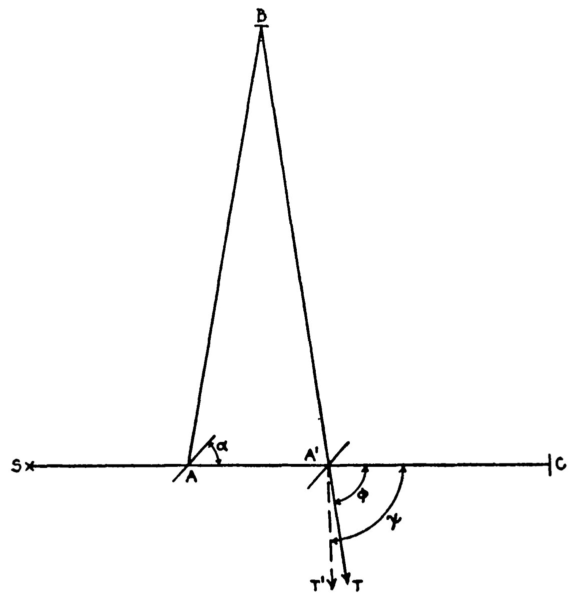

FIG. 13

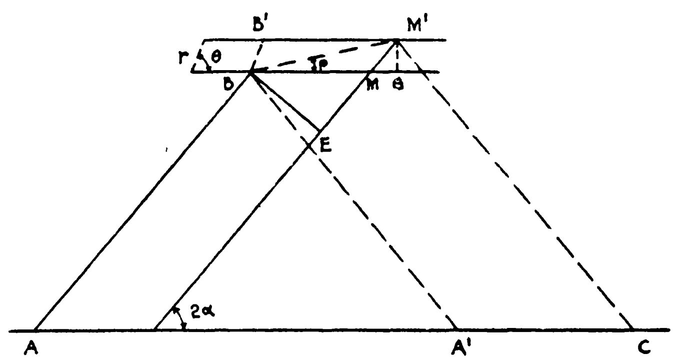

FIG. 14

the direction Θ) while the portion of the wave front at E advances to M', then BM' is the position of the equivalent fixed mirror. Denote the angle MBM' by ρ; then

$$\tan \rho = \frac{GM'}{BG}$$

where GM' is perpendicular to BM.

$$GM' = MM' \sin 2\alpha = r \sin \Theta ; \qquad BG = BM + MM' \cos 2\alpha ;$$

$$BM = \frac{EM}{\cos 2\alpha} ; \qquad \beta = \frac{r}{EM + MM'} .$$

Therefore

$$\tan \rho = \frac{r \sin \Theta}{BM + MM' \cos 2\alpha} = \frac{r\beta \sin \Theta}{\dfrac{EM\beta}{\cos 2\alpha} + MM'\beta \cos 2\alpha}$$

$$= \frac{r\beta \sin \Theta \cos 2\alpha}{r - r\beta \sin \Theta \sin 2\alpha} .$$

But we have $\tan \alpha = 1 - \xi$; hence, to terms of the second order,

$$\sin \alpha = 1 - \frac{\xi^2}{2} , \qquad \cos 2\alpha = \xi + \frac{\xi^2}{2} .$$

Substituting these values in the expression for $\tan \rho$, we have

$$\tan \rho = \beta^2 \sin \Theta (\cos \Theta - \sin \Theta) \text{ , q.p.}$$

Now if we denote the angle $CA'T$ by ϕ and the angle $CA'T'$ by ψ, we have (remembering that ϕ and ψ are negative angles)

$$\phi + \rho = 2\alpha - \rho \qquad \text{or} \qquad \phi = 2(\rho - \alpha)$$

$$\psi = 2\gamma - 180^\circ .$$

Thus the positive angle

$$T'A'T = \phi - \psi = 2\rho - 2\alpha - 2\gamma + 180^\circ .$$

To determine the tangent of this angle, we find

$$\tan (-2\alpha) = -\frac{2(1-\xi)}{1-(1+\xi)^2} ,$$

$$\tan (2\gamma - 180^\circ) = \frac{2(1+\xi)}{1-(1+\xi)^2} ,$$

and therefore

$$\tan (-2\alpha - 2\gamma + 180^\circ) = \frac{4\xi^2}{4 - \xi^4} \quad \text{q.p.}$$

From this we obtain

$$\tan (\phi - \psi) = \frac{\xi^2 + 2\beta^2 \sin \Theta (\cos \Theta - \sin \Theta)}{1 - 2\beta^2\xi^2 \sin \Theta (\cos \Theta - \sin \Theta)} \quad \text{q.p.} ,$$

since $\tan 2\rho = 2\beta^2 \sin \Theta(\cos \Theta - \sin \Theta)$ to terms of the second order. Substituting for ξ and reducing, we have finally

$$\tan (\phi - \psi) = \beta^2 \cos 2\Theta .$$

This formula was obtained by Righi, who concluded from it that a rotation of the apparatus (in the ideal experiment) through 90° would produce absolutely no effect, since, although the distances traversed by the two rays are exchanged, yet at the same time their positions are also exchanged; that is, the ray having the longer path occupies the same relative position with respect to that one having the shorter path, after the rotation as before. It follows that the pattern of the interference fringes after the rotation cannot be distinguished from that before the rotation.[1]

4. THE USUAL THEORY

A careful computation of the difference in the length of path traversed by the two rays yields precisely the same result as is given in the usual theory, namely, $\beta^2 \cos 2\Theta$. As a matter of fact, it was also known that, under ideal conditions, there exists a second-order difference in the directions of the final rays.[2] The view has been, however, that this difference in direction could affect the difference in time, up to the telescope, and therefore the difference in phase, only by an amount of the third order in β. Thus it was thought that this difference had no appreciable effect on the position of the interference fringes, although it might modify the width of the fringes.

In the next section we investigate, as far as we can, the legitimacy of this older view. As a basis for this investigation, we use a conception of the mechanism of interference phenomena which has been employed in other connections. Whether its application in the present instance is legitimate is perhaps a matter to be decided by experiment, but there does not seem to be any very evident reason why it cannot be employed with safety.

We may mention here that, quite apart from any special hypothesis concerning interference phenomena, the argument of Righi given at the conclusion of the preceding section proves absolutely that the second-order change in the angle between the final rays is by no means negligible, since in the ideal experiment the expected shift for a rotation through 90° is proportional to $2\beta^2$ if that angle is not taken into account, but is zero when it is taken into account.

[1] See, for example, Larmor, *Aether and Matter*, p. 53.

[2] See Michelson and Morley, *loc. cit.;* also Larmor, p. 48.

5. POSSIBLE EFFECT OF DIFFERENCE OF ANGLE ON THE POSITION OF THE INTERFERENCE BANDS

Figure 15 represents the network of wave fronts of the two interfering rays. The space between F_1 and F_2 represents the central bright fringe.

Let the ray s change its direction (relative to the ray t) by the amount $\Delta\alpha$. If the new wave front f_2 meets the old wave front f_1 near

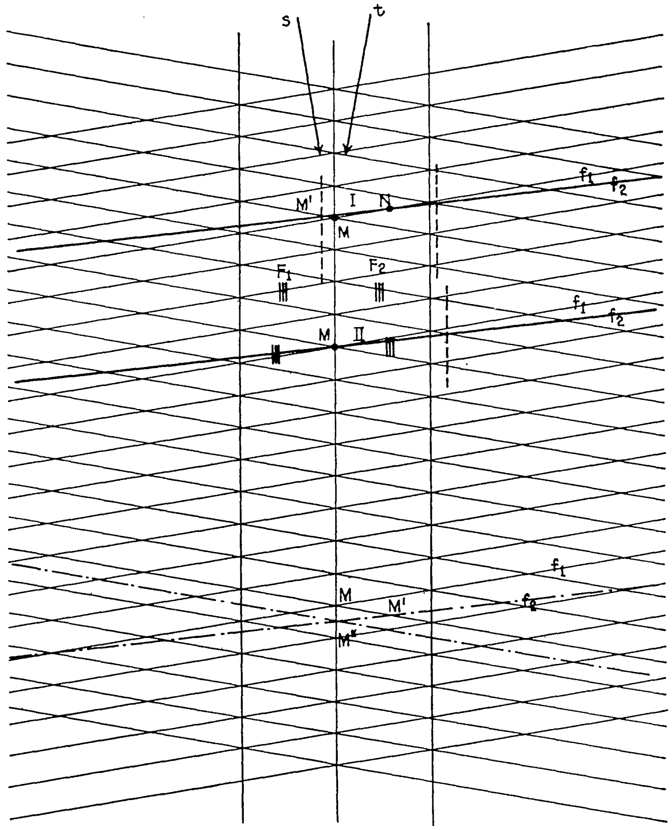

FIG. 15

the edge of the fringe at N, the center of the fringe will be shifted to the left from M to M'. The amount of this shift, which is due wholly to the change of angle between the interfering rays, will depend on the distance of the point of intersection of the consecutive wave fronts from the edge of the fringe. As this point approaches the center of the fringe, the distance MM' becomes negligible. In this case the effect is to widen the fringe without appreciably altering the position of its center.

The foregoing is based, of course, on the hypothesis that the distances traversed by the two rays are not changing. If the distance traversed by t changes, then the wave front LM takes a new position indicated by the dotted line.

Now actually, the two changes occur simultaneously; and as both are periodic it seems inevitable that the point of intersection of f_1 and f_2 should at times come near enough to produce an appreciable displacement.

It is conceivable, of course, that the two effects might neutralize each other, as indicated at the bottom of the figure, where the point of intersection of consecutive rays is supposed to come outside of the central fringe.

6. FORMULA FOR THE SHIFT OF THE FRINGES

It seems to be impossible to obtain a formula for the amount of the shift of the fringes without making certain assumptions concerning the nature of interference phenomena.

The simplest procedure seems to be to study the network of parallelograms so drawn that each system of parallel sides represents the successive positions of some definite phase of the waves of the corresponding ray.

Let Figure 15 represent this network of parallelograms, and let a denote the distance of the middle of the central fringe to the right of some convenient origin. This distance will depend upon the initial adjustment between the distances traversed by the two rays.

If it is agreed that only the relative positions and lengths of paths of the two rays s and t are involved, we may suppose that one of the rays remains fixed in length while the other remains fixed in direction.

Let the ray t be supposed to rotate about a point in the neighborhood of its image. Then one of the lines f representing a certain phase of t may be supposed to envelop a circle. Let b denote the distance to the right of the origin of the point of contact of this circle with f in its initial position.

Use the following notations: a' equals the new value of a due to change of length of s, b' equals the new value of b due to change of direction of t, M' denotes the middle of central fringe after s has changed length, and M'' denotes the final position of the central fringe.

After the apparatus has rotated through an angle Θ, we have

$$a' = a - D\beta^2(1-\cos 2\Theta) \; ,$$

$$b' = b + D\beta^2(1-\cos 2\Theta) \; ,$$

$$\frac{M'M''}{1-\cos 2\Theta} = \frac{a-b-2D\beta^2(1-\cos 2\Theta)}{r+\cos 2\Theta} \; .$$

Adding $M'M''$ to a', we have for the position of M'', the new middle point of the central fringe,

$$a'' = \frac{a(r+1)+b\cos 2\Theta - D\beta^2[r+2-(r+3)\cos 2\Theta+\cos^2 2\Theta]}{r+\cos 2\Theta} \; .$$

7. POSITION OF MAXIMUM SHIFT

The formula of the last section shows that the fringes have a periodic motion across the field of the telescope. The maximum and minimum positions of M, however, depend upon the values of the quantities a, b, and r. The values of a and r depend upon the initial adjustments, and the value of b would very likely be different for experiments performed at different times.

If, then, no effort is made to control the values of these quantities, we must suppose that the maximum and minimum positions for a series of experiments will have an entirely random distribution. It will not be legitimate, therefore, simply to average the readings of a series of observations, as was done in the Michelson-Morley experiment. In fact, there would seem to be a high degree of probability that this procedure would lead to a quite small result in case it is applied to a large number of observations.

[Professor Hedrick remarked at the end of his report that his results had been discussed by Professor Epstein from the physical point of view. This discussion has kindly been supplied for publication here.]

V. PROFESSOR PAUL S. EPSTEIN (CALIFORNIA INSTITUTE OF TECHNOLOGY)

The result of Professor Hedrick's analysis is that the two beams of light acquire a difference of phase

$$\delta-\delta'=h\beta^2 \cos 2\vartheta$$

and a difference in direction

$$\Delta\alpha=\beta^2 \cos 2\vartheta \qquad \left(\beta=\frac{v}{c}\right),$$

in which terms of the fourth order are neglected.

Let us now choose the plane in which we observe the fringes as $x=0$ of a cartesian system (Fig. 16). We can then represent the two waves by the formulae (s = light-vector)

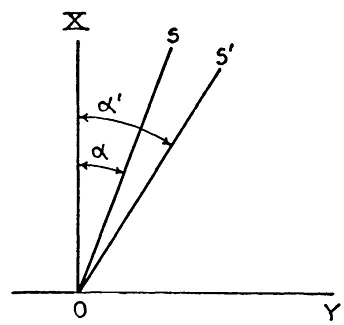

FIG. 16

$$s=A \cos\left[\frac{2\pi}{\lambda}(x \cos \alpha+y \sin \alpha+ct)+\delta\right],$$

$$s'=A \cos\left[\frac{2\pi}{\lambda}(x \cos \alpha'+y \sin \alpha'+ct)+\delta'\right].$$

The illumination of the screen is then ($x=0$, $\sin \alpha=\alpha$)

$$(s+s')^2=4A^2 \cos^2\left[\frac{2\pi}{\lambda} y(\alpha-\alpha')+\delta-\delta'\right] \cos^2\left[\frac{2\pi}{\lambda} y(\alpha+\alpha')+2ct+\delta+\delta'\right]$$

$$=2A^2 \cos^2\left[\frac{2\pi}{\lambda} y(\alpha-\alpha')+\delta-\delta'\right].$$

We have maxima, where the argument of the cosine is a multiple of π.

The position of the central fringe is therefore given by

$$\frac{2\pi}{\lambda} y_0(\alpha-\alpha')+\delta-\delta'=0,$$

$$y_0=-\frac{\lambda}{2\pi}\frac{\delta-\delta'}{\alpha-\alpha'}. \tag{1}$$

The distance between two maxima, or the width of the fringes, is given by the equation

$$\frac{2\pi}{\lambda}\,\Delta y(a-a')+\delta-\delta'=\pi\,,$$

or

$$\Delta y=\frac{\pi-(\delta-\delta')}{a-a'}\cdot\frac{\lambda}{2\pi}\,. \tag{2}$$

Let us first consider the interferometer at rest. We cannot take the ideal adjustment, because then we should have no fringes. Formula (2) shows that we must have a finite difference a_0-a_0' in order to get a finite width of fringes. This width is of the order of 1 mm, so that $(\delta-\delta'=0)$ we have the order of magnitude

$$a_0-a_0'=\frac{\lambda}{2\Delta y}=\frac{5\cdot 10^{-5}}{2\cdot 10^{-1}}=2.\ 5\cdot 10^{-4}\,.$$

In the actual experiment, we have in addition to a_0-a_0' the rotation Δa:

$$a-a'=a_0-a_0'+\Delta a\,,$$

$$y_0=-\frac{\lambda}{2\pi}\,\frac{\delta-\delta'}{a_0-a_0'+\Delta a}\,.$$

The order of magnitude is

$$\Delta a=\left(\frac{v}{c}\right)^2\cos 2\vartheta=\left(\frac{3\cdot 10^5}{3\cdot 10^{10}}\right)^2\cos 2\vartheta=10^{-8}\cos 2\vartheta\,.$$

Therefore an expansion is permissible:

$$y_0=-\frac{\lambda}{2\pi}\left(\frac{\delta-\delta'}{a_0-a_0'}-\frac{\delta-\delta'}{(a_0-a_0')^2}\,\Delta a\right),$$

$$y_0=-\frac{\lambda}{2\pi}\,\frac{\delta-\delta'}{a_0-a_0'}\left(1-\frac{\Delta a}{a_0-a_0'}\right).$$

The first term represents the shift due to the difference of phase; the second term is due to the rotation. We see that it is $0.4\cdot 10^{-4}$ of the first term, that is, quite outside the possibility of observation under the conditions of Michelson, Morley, and Miller's experiment.

It is interesting that in the ideal case

$$y_0 = -\frac{\lambda}{2\pi}\,\frac{\delta-\delta'}{\Delta\alpha} = -\frac{\lambda}{2\pi}\,\frac{h\beta^2\cos 2\vartheta}{\beta^2\cos 2\vartheta} = -\frac{\lambda h}{2\pi}\,.$$

That is, we have a constant position, independent of the orientation of the instrument. If Michelson had devised the experiment so as to have no fringes, but light in a certain position of the ideally adjusted interferometer, expecting to have darkness in another position, because of the phase difference, the experiment would not have proved anything.

Dr. Kennedy's arrangement occupies an intermediate position. He takes fringes of considerably greater width. The width necessary to produce an appreciable error is about 250 cm, however, and it is quite certain that his fringes were not as wide as that. Professor Hedrick's theory is, however, very interesting and important in connection with Kennedy's experiment.

VI. PROFESSOR PAUL S. EPSTEIN (CALIFORNIA INSTITUTE OF TECHNOLOGY)

I cannot report to you today on anything of my own. What I intended was a short review of some recent experiments which relate to Mr. Miller's experiment, and which have been performed mainly outside of Pasadena.

I shall give you a brief account of three experiments, carried out by Tomaschek in Germany, by Chase in Pasadena, and by Piccard in Brussels.

In one of his experiments Tomaschek used the following arrangement. In the immediate neighborhood of a charged condenser *I* (Fig. 17) was suspended a magnetic needle *II*. The experiment was intended to check an old idea of Röntgen's which was as follows: The charged condenser, being in motion, represents a system of two parallel currents moving in opposite directions. These currents produce a magnetic field which should exert a force on the magnetic needle. In case the condenser is in motion relative to the ether, a deflection of the magnetic needle should be found. In reality this device cannot provide a crucial test for a decision between the old and the new theory. An exact analysis shows that both theories lead to the same result because the effect is of the first order. The explanation why no effect exists lies in the fact that it is not the condenser alone which moves, but also the indicating needle. This gives rise to a second torsional moment which just balances the first one. Moreover, Tomaschek tried the experiment with a metal cover around his needle. By this arrangement, cutting out all the magnetic actions between *I* and *II*, he eliminated any effect which might have existed without shielding. So it is not surprising that he did not obtain a positive effect. He might indeed have saved his efforts by not trying the experiment at all.

Tomaschek, and independently Mr. Chase in our laboratory, repeated the old experiment of Trouton and Noble, as they think, in a much more precise way. The underlying idea is the following:

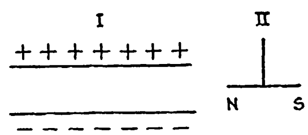

FIG. 17

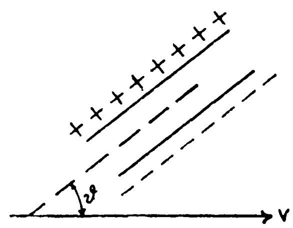

FIG. 18

Suppose *I* (Fig. 17) to be a charged condenser, suspended in such a way that it can rotate. For a condenser at rest there exists only a force of attraction between the two plates due to the charges of opposite sign. Now the apparatus being in motion with the velocity v (Fig. 18) means that the positive charge is moving in a magnetic field originated by the moving negative charge, and vice versa. Hence two additional forces are exerted on the condenser which would manifest themselves as a torque, so that a rotation of the condenser should be expected. It is easy to calculate this torque, M, which is

$$M=\frac{U}{\epsilon}\left(\frac{v}{c}\right)^2 \sin 2\vartheta \cos \varphi \ ,$$

where U equals the energy content of condenser, ϵ the dielectric constant of material filling the condenser, and φ the azimuth characterizing the projection of v on the plane of the condenser in relation to the suspension.

The foregoing formula is derived on the assumption that the dielectric substance may be regarded as a continuum. The structure of ϵ has not been taken into account; but the difference would not be appreciable. Both Tomaschek and Chase used not a single condenser, but a great number of plates in order to obtain a large capacity and thus have a large value for the electric energy.

The torque is practically the same for both the classical theory and the theory of relativity, the difference arising only in some terms of the fourth order, which are of no practical importance. In spite of the existence of a torque, relativity contends that no effect can be observed at all. This follows because the torque is compensated in some way. The explanation of this peculiar fact is to be found in the tensor character of the mass in relativity. In this theory mass has a different value for accelerations in direction of the motion (m_l) and at right angles (m_{tr}) to it. The ratio of the masses is given by the expression

$$\frac{m_l}{m_{tr}}=\frac{1}{1-(v/c)^2}\,.$$

In order to analyze the effect of the torque found above, we must divide the acting forces into two components, one in direction of the motion and one at right angles to it. The first component acts against the heavier mass m_l and causes a relatively smaller acceleration than the second component. It thus happens that the two accelerations (as vectors) point to the center of gravity of the system (condenser) although the two forces do not. In this way the torque appears to be compensated in the end effect. Thus we see that the tensor character of mass is responsible for the lack of an effect. The Lorentz contraction has not to be taken into account at all. Even in case there were no Lorentz contraction, we should not obtain an effect on our condenser. If, however, an effect should really be observed it would be a contradiction of relativity, because the ratio m_l/m_{tr} is a direct consequence of this theory. Tomaschek and Chase both claim to be able to detect an uncompensated torque corresponding to a velocity of the earth of 4 km/sec. For lower velocities no deflection could be observed with their apparatus. This limit of precision is obtained by assuming the whole torque to be in action.

Now this assumption is not quite correct even from the standpoint of the classical theory. As the nuclei are of electrical constitution, we must in the classical theory also take into account a definite relation between the mass and the velocity of the nuclei. Considering the nuclei as rigid spheres, for instance (Abraham), we should find

$$\frac{m_l}{m_{tr}}=\frac{1/(1-\beta^2)}{1/(1-\frac{4}{5}\beta^2)}\,.$$

If we use this formula, the torque will be compensated in part, but not completely as in relativity. It can easily be seen from the formula that 20 per cent of the calculated torque would manifest itself as deflection. The minimum velocity which could be observed by Chase would then be $4\sqrt{5}$ km/sec., which brings us near to Miller's value of 10 km/sec. Although interesting, these experiments cannot therefore decide either for or against Miller's results. On this account it would be of great value if they could be carried out with increased precision.

Now some remarks about the experiment of Mr. A. Piccard at Brussels: Piccard thought that the height above the earth's surface should be of influence on the effect Mr. Miller has found. (This is, in fact, a misunderstanding, because Mr. Miller does not claim any such effect.) If the ether drift may be supposed to be larger on Mount Wilson than at sea-level, it should be still larger in the free atmosphere. So Piccard tried the experiment in a balloon. His interferometer had branches with an optical path 2.8 m long. The steady temperature was controlled by a thermostat. The balloon was rotated about a vertical axis by means of a propeller. A self-recording device was used, and ninety-six rotations were registered. The curves were analyzed harmonically, but it appeared that the thermostat had not functioned as expected. For this reason the accidental errors were too large (the probable error corresponded to a velocity of 7 km/sec.). All that Piccard claims, then, is that the drift in the free atmosphere at 2300 m altitude is not larger than on Mount Wilson. No further conclusions can be drawn from this experiment.

[*Note added April, 1928.*—Both C. T. Chase and A. Piccard have continued their work during the year intervening since the foregoing report was presented. Chase,[1] working at Harvard University, increased the accuracy of his measurements about three times. Even taking into account the factor 1/5 mentioned above, his new apparatus could have detected an ether-drift velocity of 3 km/sec. Within this accuracy his results were negative, thus giving strong support to the theory of relativity. The most accurate and recent work of Piccard's was carried out by him, jointly with E. Stahel,[2] on the summit of the Rigi in Switzerland (altitude, 1800 m). The same self-recording interferometer with thermostatic temperature control was used. The results were completely negative, the ether drift being only one-fortieth part of that expected according to Miller.]

Discussion

[Dr. Walter S. Adams, director of the Observatory, opened the discussion, expressing his hope that Professor Lorentz and Professor Michelson would give their opinions in regard to the considerations of Righi and Hedrick.]

Professor H. A. Lorentz: I feel somewhat guilty in regard to the work of Righi. It was a long time ago that I read his papers, and I do not remember their contents very well, as I have been busy with quite different things these last years. I should have read them again of course for this meeting. But this good intention could not be materialized because of my being entertained so much by the people of Pasadena. After having heard Mr. Hedrick's report, I intend, however, to study these questions again very carefully in relation to Mr. Miller's experiment. Further, the considerations of Brylinski must be taken up again. Offhand, I can only say that the results of Mr. Hedrick are in contradiction with those which I presented yesterday. Until today I felt myself quite satisfied with the considerations which are based on Fermat's principle. After Mr. Hedrick's report, however, I shall have to reconsider these questions carefully. According to Mr. Hedrick's results it appears, indeed, that the result to be expected in the Michelson-Morley experiment may be numerically different from that which we ordinarily expect on the basis of the classical theory. The numerical value of the second-

[1] *Physical Review*, **30**, 516, 1927.

[2] *Die Naturwissenschaften*, **16**, 25, 1928.

order effect would be different from that which Michelson calculated. My procedure seems to me still to be the easiest and most straightforward one. Still it must be found out where the discrepancy between the two ways lies. In case a method other than Fermat's is chosen, one has to do considerable work. One must distinguish, for instance, very carefully between the rays of light and the normals to the wave trains. Another difficult point is involved in the treatment of the reflection from moving mirrors. Fermat's principle, of which I have made use, gives in any case a much simpler treatment. But as there exists a discrepancy between the results obtained by the two methods, I intend to go through the detailed calculations as soon as possible. In the meantime I still hope, of course, that my general considerations are right.

I should now like to make some remarks on Mr. Miller's experiment. It seems to me that there is a serious problem connected with the effect which is periodic for a full turn of the apparatus and which is discarded by Mr. Miller, who emphasizes the importance of the half-period effect (periodic with half-turns of the apparatus) in regard to the question of an ether drift. In many cases the full-period effect is much larger than the half-period effect. According to Mr. Miller, the full-period effect is dependent on the width of the fringes and would become zero for infinitely wide fringes.

Although Mr. Miller says that he was able to eliminate this effect to a great extent in his Cleveland measurements, and that it is to be explained easily by the experimental arrangement, I should like to understand its cause somewhat more clearly. Speaking now for a moment as an adherent of the relativity theory, I should contend that no such effect whatever could exist. Indeed, a rotation of the entire apparatus as a whole, the source of light included, should not give any shift at all from the standpoint of relativity. No effect would be expected were the earth and the apparatus at rest. According to Einstein, then, the same absence of an effect is to be expected for the moving earth. The full-period effect is thus in contradiction with the theory of relativity and of main importance. If then Mr. Miller has found some systematic effects whose existence cannot be denied, it is also important to know the cause for the full-period effect.

Let us discuss now the half-period effect. After having seen the different diagrams I think there can hardly exist any doubt that there is an actual displacement of the fringes with Mr. Miller's set-up. There arises, then, the question as to its possible cause. Mr. Miller himself has offered some suggestions which are very interesting. His conclusion is that the effect found corresponds to an absolute velocity of 10 km/sec. and for a definite sidereal time is the same throughout the year. It is certainly not connected with the orbital motion of the earth, but indicates a motion of the solar system relative to the stellar system of the same kind as found by Mr. Strömberg from a quite different point of view. The velocity of this motion is estimated to be at least 200 km/sec. For some reason or other, the full relative velocity between ether and earth

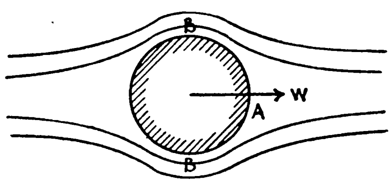

FIG. 19

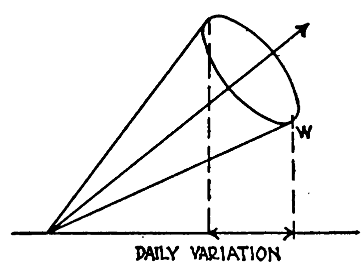

FIG. 20

does not come into action. Otherwise, one cannot account for the lack of an effect related to the orbital motion of the earth. There is, however, the following point to be mentioned. One could assume as Mr. Miller does that the entrainment is only partial, because the earth, for instance, is not completely impermeable to the ether. But then the following consideration would have to be taken into account. Suppose w to be the velocity of the earth relative to the ether (which is at rest at C). Then if the ether behaves as an ideal fluid, there will be a relative velocity in it at A with respect to B amounting to w/z. Miller accounts for the daily variation in amplitude of his effect, in a way which is immediately understood from Figure 20. According to the consideration given above, however, w could not be considered as a vector of constant length but would itself vary during one day. This would of course make the interpretation of the drift more complicated.

As to the average displacement of the azimuth to the west (50°), it seems to be very hard to explain. Fortunately, however, it also changes about this position periodically with sidereal time. Otherwise, one could hardly escape the suspicion that the whole effect might be due to some cause in the laboratory.

Now some words about Piccard's experiment: I saw the fringes in his laboratory, and they were extremely nice indeed. As a matter of fact, Piccard considers his experiments only preliminary ones, which he hopes still to improve considerably. He worked the first time under very unfavorable conditions, as the night of his first ascent was unusually warm. I might mention, just for general interest, that such observations as Piccard's are very exhausting. Those of Mr. Miller are, too, of course. Piccard told me that he did not notice any physiological effect in the turning balloon due to centrifugal force; but motions in the vertical direction, nodding the head, for instance, were very painful, because of the effect of Corioli's force.

PROFESSOR A. A. MICHELSON: There are one or two questions which I should like to ask. Did Mr. Miller put his results together with the intention of finding an orbital effect (effect due to the motion of the earth in its orbit around the sun)?

PROFESSOR D. C. MILLER: Certainly. It was for this purpose that the observations were made at four epochs, approximately at intervals of three months; so that the direction of the orbital component of motion changes about 90° from epoch to epoch. The observations for each epoch have been reduced to determine the actual resultant motion for that epoch. The apex of the motion indicated by all the observations is near the pole of the ecliptic, and hence the orbital motion would manifest itself in a change in the position of this apex from epoch to epoch; that is, it would produce a sort of annual aberration in the apex. A comparison of the results for the four epochs fails to show conclusive evidence of this effect. I hope, however, that when several sets of observations for each epoch are available, the effect of the orbital motion may be evident. The positive effect actually obtained corresponds to a relative motion of the earth and ether of about 10 km/sec., with a probable error of $\frac{1}{2}$ km/sec. It follows that the effect of the orbital motion on the observed resultant velocity must therefore be less than $\frac{1}{2}$ km/sec.

MICHELSON: What is the probable error for this $\frac{1}{2}$ km/sec.?

MILLER: This $\frac{1}{2}$ km/sec. is itself the probable error in the measurement of the magnitude of the effect, as determined from the calculations. Since no effect has yet been detected which can positively be attributed to the orbital motion one can only say that such an effect, if present, must be less than $\frac{1}{2}$ km/sec.

MICHELSON: Excuse me if I insist on this point. This estimation of the probable error is based on an interpretation of the experiments which does not intend to find the effect of the motion of the earth at all. Can you not find the probable error in discussing the observations from the point of view of finding the orbital motion?

MILLER: I have not calculated the error from such a point of view.

MICHELSON: It would, however, be possible to do so. I should really like to see such calculations carried out.

Had I known earlier of the beautiful and ingenious apparatus of Mr. Kennedy, I probably should not have undertaken my experiments now going on in the same form. In any case, the problem in question must be investigated further. Even a more precise repetition of experiments with older devices already used will be of great value for the reliability of the results. We have now to find out definitely what actually is the truth, without going at it with any prejudice.

I am happy in respect to Mr. Kennedy's experiment that I had the idea of this device, too. I also had intended to use the photometric comparison of the field produced by light which is reflected from a divided mirror, the two half-surfaces being at a distance of a fraction of a wave-length. But it did not occur to me that the separation could be made so nicely by sputtering. I intended to take the layer off by acids. The apparatus is indeed so beautiful that I should like to work with a similar device, in case Mr. Kennedy has no objections.

As far as Mr. Piccard's remarks are concerned (see Lorentz) I must say that every beginner thinks himself lucky if he is able to observe a shift of 1/20 of a fringe. It should be mentioned, however, that with some practice shifts of 1/100 can be measured, and that in very favorable cases even a shift of 1/1000 of a fringe may be observed. For this purpose the fringes must be extremely black. We are sufficiently advanced with our new apparatus to show such fringes [the apparatus was on view in the laboratory]. The main thing, of course, is to eliminate all stray light, which comes especially from the silver-coated plate. The ordinary plate gives rise to reflections at both surfaces. I now get rid of scattered light by this simple device illustrated in Figure 21, consisting of two prisms, with a half-silvered surface where they are in contact, oriented so that the incident light is not quite perpendicular to the face of the first prism. Very black fringes may be obtained by this combination of prisms. There are still some difficulties as to the separating surface which I hope to overcome, however, very shortly. Probably the precision will reach 1/1000 of a fringe.

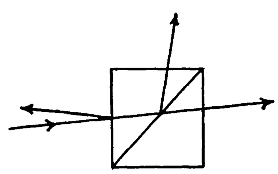

FIG. 21

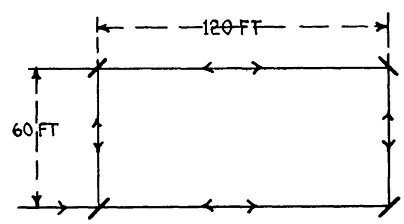

FIG. 22

I should like to make some remarks on the interpretation of Mr. Miller's experiments. It seems to me to be very hard to explain them. Indeed, why should the ether be dragged along by the earth to the extent of 19/20 and not some other fraction? If this really occurs, then we must suppose that there will be a great difference between the drag on the surface of the earth and a thousand miles above it. There the drag would probably be zero. Assuming then, for illustration, some kind of an exponential decrease of drag with altitude we should expect a large difference between the shift at sea-level and on Mount Wilson. In this case another arrangement of apparatus could be used to observe the effect. Two rays of light could be sent around a vertically mounted rectangle (Fig. 22). A shift of several hundred fringes might then be expected. No shift, however, seems to exist, according to experiments made in the Ryerson Laboratory.

To conclude, I might still mention some advantages of the new apparatus: (1) The fringes are very black. (2) The frame will be built of invar so as to make it very insensitive to changes of temperature. (3) Photographic registration will be used so as to make continuous readings possible. Thus recorded, the observations would be preserved and could be discussed later on, independently of the observer. These are the three points which represent a considerable improvement in comparison with the earlier apparatus.

It may be of some interest to mention that originally another apparatus was planned; this, however, was abandoned and the present interferometer adopted. The arms were to be 100 m long. The apparatus could not be turned, but the moving earth would have brought it into different positions relative to the ether. We now intend to try this, and the experiment is in preparation at Chicago.

LORENTZ: In regard to the theoretical details raised by Dr. Michelson, I offer the following remarks: If the ether moves freely through matter, no such difficulties arise, as far as entrainment is concerned. If, on the other hand, we should be obliged by the facts to introduce a substantial ether again, it would, of course, be a very difficult problem to find out what its properties are. What would happen, for instance, in case matter should turn out to be only partly permeable for the ether, nobody can tell. For this reason the question about the ratio 19/20 could not well be raised before the properties of the ether were better known. We can even leave open the possibility that the motion of the ether may be irrotational. In this case the ether drift would of course have a component normal to the surface of the earth, and it would be rather large. This might very well be the case, and the effect mentioned by Dr. Michelson would be null. The relative velocity of the ether drift might increase with increasing distance above the surface of the earth, and still have no rotation. This, for instance, is the case in Planck's modification of Stokes's theory. A further possibility would be a compressible ether. This would remove even the necessity of having an irrotational motion of the ether. But it is sufficient for the present moment to point out that a motion of the ether with rot $w=0$ would be sufficient to give a quantitative explanation of aberration phenomena and of Michelson's result. I tell you all this only to show how numerous the different possibilities for the theory are, if we are compelled by new experiments to go back to the notion of a substantial ether.

QUESTION TO DR. KENNEDY: Your apparatus is so sensitive that it would detect a change in the optical path equal to $\delta l = 2 \cdot 10^{-3} \lambda$.

Now this is not the sensitivity which you calculated theoretically. I should like to ask how you found this sensitivity. I do not ask this for myself, because I know how you have done it, but for the sake of the audience, because I think the method you applied is so very beautiful. Then I suggest also that you tell us if you could detect the orbital effect on the assumption of a drag of 19/20.

DR. R. J. KENNEDY: Answering first the second question, I think that the effect due to the orbital motion of the earth should be observable with my apparatus.

As to the first question, I thought that the method of determining δl was rather crude. A weight of 5–6 kg on the slab on which the apparatus was mounted produced a shift of one fringe. I determined the minimum weight (about 10 g.) which produced an effect just observable. The ratio of the two weights gives, then, $\delta l/\lambda$.

I might explain also that I got rid of the surplus scattered light, using a different method from that suggested by Professor Michelson for his new device. I used polarized light, impinging under the proper angle on the glass plate (Brewster's angle) so that no light at all was reflected. [See description in Kennedy's report. MICHELSON exclaims: "Very nice indeed."] The method I used is not my own invention. It has been suggested somewhere in *Comptes rendus* (1911), if I remember rightly.

The shift of azimuth (50° to the west) in Miller's experiment seems to indicate that some spurious effect is present, dependent only on the position of the apparatus relative to the meridian, which shifts the azimuth of the whole effect to the west. The result must then be considered as a superposition of spurious effect and ether drift. This explanation would probably require a magnitude for the effect due to ether drift smaller than anything that could have been observed with the devices used. It might also explain, as I think, the difference between the results obtained by Mr. Miller and myself.

Piccard's experiment does not seem to be of great value. As far as I can make out, he worked just at a time of the day when hardly any effect was to be expected.

LORENTZ: I do not think that Kennedy's last remark is quite right. Piccard really ascended at the time when the constellation of Hercules rose above the horizon.

KENNEDY: Piccard ascended twice. Once, when the sidereal time was right, his observations were spoiled by temperature effects. His errors were thirty times greater than the effect he was looking for. The second time he got rid of his errors, but there was no effect to be expected at the sidereal time chosen for this observation.

MILLER: I agree with Hedrick that the theory of the instrument used for the experiments should be thoroughly studied. The theory of Lorentz is exact; but it is general, and does not take into account the special conditions of the apparatus used. What actually happens to the fringes is dependent on the adjustment of the mirrors. When I became interested in the experiment in 1900, there existed no really adequate theory of the instrument. A theoretical study of the apparatus was then undertaken by W. M. Hicks, which was published in the *Philosophical Magazine* for January, 1902. We [Miller and Morley] thought it necessary to take up the question again, as Hicks had suggested that there was an additional term in the expression for the effect which had not previously been considered. This term represents an effect of appreciable magnitude, which is periodic in each full turn of the interferometer, while the ether-drift effect is periodic in each half-turn. In the *Philosophical Magazine* for May, 1905, we gave a review of the theory, showing that Hicks's calculations did not affect the conclusions previously drawn. The full-period effect is actually present in the experiments of 1887, as well as in all those that have followed. In *Comptes rendus*, **168,** 837, 1919, Righi began a series of articles, setting forth the theory in detail. He thought that our conclusions were not justified by the theory. It seems to me that Righi's theory is correct in the abstract; but it does not deal with the actual things happening in the interferometer, as Hicks's theory does. The question needs still further investigation, as suggested by Professor Hedrick. Hicks's theory takes into account the fact that in practice the image c (Fig. 23) of mirror a with regard to a is slightly oblique to mirror b. This is necessarily true when straight-line fringes of finite width are obtained. Righi's calculations are based on the assumption that b and c are exactly parallel, which would produce fringes of infinite width; thus, his criticism does not apply to the actual case. When b and c are oblique to each other, an actual ether drift will produce the additional effect predicted by Hicks, which is periodic in a full turn of the apparatus. Hicks has calculated its magnitude, showing that it depends on the angle between *b* and *c*. The effect increases with increasing angle and decreasing width of fringes. As the effect we are looking for (ether drift) must be periodic in each half-turn, we are justified in eliminating the full-period effect. This is done by plotting the single observations, turn by turn of the interferometer; these curves are analyzed by the mechanical harmonic analyzer, and the second harmonic (half-turn effect) is taken as representing the ether drift. If there is an ether-drift effect, the full-turn effect is necessarily produced, according to Hicks, and its presence may be taken as further evidence of the ether drift. The magnitude and phase of the full-period effect is variable, because it depends upon the adjustment of the mirrors as well as the ether drift. [Slides were shown representing the full-period effect.] It is evident that the magnitude is very different for different sets of observations. The half-period effect, on the other hand, is characterized by a constant magnitude. The full-period effect is small when the width of the fringes is such that five of them cover the mirror (10 cm in diameter). Under other conditions, however, it may be very large. The full-period effect is not new, but has always been present in all the experiments. It is present in Professor Michelson's original observations.

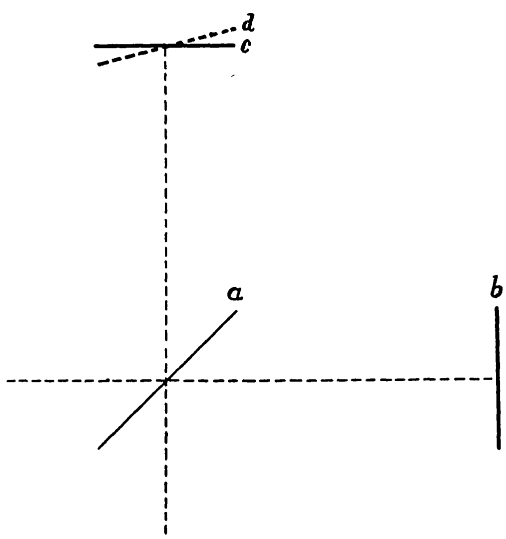

FIG. 23

KENNEDY: Are the effects the same in case you use a concrete frame instead of an iron frame?

MILLER: Yes, they are essentially the same. The concrete instrument showed smaller temperature effects than did the one with the steel frame, but its mechanical strength was also less. I have always used (as did Kennedy) the method of shifting the fringes by putting weights on the end of the frame; to produce a shift of one fringe, approximately 325 g was necessary. This is less than the corresponding weight in Dr. Kennedy's apparatus, because the arms of the frame are longer in my apparatus than in his. I should like to mention again that my experiments have been carried out under a great variety of conditions. My assistant moved around the apparatus to see if his position affected the distribution of temperature or the stability or level of the instrument. The light was placed in different positions, both inside and outside the house. At Mount Wilson, the instrument has been mounted in two different buildings, differently oriented. The effect has persisted throughout. After considering all the possible sources of error, there always remained a positive effect.

PROFESSOR E. R. HEDRICK: Mathematically speaking there cannot be any question as to the correctness of the computations which Professor Lorentz has presented to us. The result for the second-order terms seems beyond question. It is conceivable, however, that there is introduced an error when the path of the beam of light is changed by the motion of the apparatus into a new one. The instrument might not be always in the ideal position assumed in the calculations.

I should like to call your attention to a second point. We start from a certain number of assumptions. Now our aim in mathematics is always to reduce the necessary number of assumptions to a minimum. We make use in this special case of the two principles of Huyghens and Fermat. Can we trust them to terms of the third order? We do not know. Might not a combination of third-order effects eventually affect the magnitude of the second-order effect? Anyhow, if we could reduce the number of physical principles involved in our calculations to a single one, it would be very desirable. That is what Righi and also I have attempted to do.[1]

LORENTZ: I should like to defend my theory. Hedrick says we should try to reduce the number of our assumptions. Now the two

[1] It should be stated clearly that the operations of differentiation and integration, freely used in these discussions, cannot be trusted to the extent that is often assumed. The derivative of an approximation to a true formula is not necessarily an approximation to the derivative of the true formula. It is true also that the integrals to successive approximations to a true formula are not necessarily successive approximations to the integral of the true formula, unless the successive approximations are *uniform*. These conditions cannot be said to hold in such fine approximations as those of the Michelson experiment. Therefore it has seemed to us, and it still seems to us, to be necessary to proceed by direct calculations from definitely stated assumptions, rather than through an intermediate proof (e.g., Fermat's principle) that is thus questionable.

principles of Huyghens and Fermat are not independent. The second may be deduced from the first. It is easy to prove that this is true. There is then no question of having two assumptions.

HEDRICK: Is this really generally true?

LORENTZ: Yes; the relation between Huyghens' principle and Fermat's principle is absolutely general. I might repeat more precisely some of the features of the reasoning I gave yesterday.

Suppose P (Fig. 5) to be a luminous point. (The difficulties might of course begin here, if we were obliged to state exactly what we mean by this.) Suppose, further, that rot $w=0$, which is Fresnel's idea. Making use of Fresnel's coefficient and entrainment, we find the influence of a motion of the apparatus on effects of the first order to be the same for each of the paths l_1 and l_2.

There is still one point to be considered which I did not mention before. If we take into account effects of the second order, the path of the rays will be changed by the motion of the apparatus, so that we should have to use in one moment l and in the next l'. Still I think that for the effects under consideration it does not make any difference which one we take. [HEDRICK remarks: "Yes, that is all right."] It can easily be seen that the difference between l and l' produces only an effect of the fourth order. We are thus justified in using the path existing without motion of the ether.

Of course the value of the light-path l must be exact to the second order. For those cases in which we are concerned with the propagation in ether only, this value follows from the expression for v (velocity of light in the moving system):

$$\frac{1}{v}=\frac{1}{c}\left[1-\frac{w}{c}\cos\vartheta+\frac{w^2}{c^2}\left(\cos^2\vartheta+\tfrac{1}{2}\sin^2\vartheta\right)\right].$$

[See expression (3) in Report II.] But the question arises, and this is what I wanted to add, what will be the form of the equation when we deal with light passing through the moving glass plates? In this case w^2/c^2 would be replaced by $k^2\,w^2/c^2$, where $1-k=(n^2-1)/n^2$ is Fresnel's coefficient. Now this value for k might not be quite rigorous in this connection. The expression $wkdt$ due to the entrainment by matter might be doubted if terms of the second order are to be considered. This indeed might necessitate a change of magnitude for these second-order effects. It is to be remarked, however, that the distances through which the light travels in glass in Michelson's experiment are comparatively so small, and that practically they cannot give rise to any difficulty at all. For all these reasons I think that the theory which I presented is general, and, at the same time of exact applicability to the actual instrument. In any case, I intend to study all the recent work such as Mr. Hedrick's.

DR. G. STRÖMBERG: It is often said that the sun's motion "in space" is 20 km/sec. toward the point $\alpha = 270°$, $\delta = +30°$. This expression is quite inadequate and means that the sun's motion referred to the brighter stars is of this magnitude and direction. Referred to distant objects, this velocity is much greater. The sun's velocity relative to the system of globular star clusters is about 300 km/sec. in the direction $\alpha = 320°$, $\delta = +65°$, and relative to the spiral nebulae it may be even larger, although in about the same direction.

As the bigger reference frame is, presumably, the more fundamental, the higher velocity may also be of more fundamental nature.

And this is just what has been found to be the case. The sun's motion as referred to different classes of objects in our neighborhood is quite different, and the general rule has been established that the higher the internal velocity dispersion in a group, the larger is the sun's motion relative to this group. Practically all celestial objects can be arranged in a sequence with increasing velocity dispersion, and moving with different velocity along a certain axis. This sequence terminates with the globular clusters, and a quadratic relationship exists between group motion along a certain axis and the velocity dispersion along the same axis. This phenomenon can, at least formally, be explained as the effect of a velocity restriction in a fundamental reference frame in which the globular star clusters are statistically at rest.

Recent studies of the velocities of giant M stars have completely confirmed this hypothesis. In fact, it has been found possible to represent the velocity distribution along this fundamental axis in a much more satisfactory way by one disposable constant, in addition to this fundamental velocity vector, than by four arbitrary constants, as in the prevalent methods.

In stellar motions we have to introduce a fundamental velocity vector of 300 km/sec. in the direction mentioned in order to secure order and regularity. This implies the existence of a "fundamental" reference frame, or "medium," or "ether," whatever we prefer to call it. The introduction of such a conception has been of great value in the study of stellar motions.

PROFESSOR H. BATEMAN: The Michelson-Morley experiment may be regarded as a test of the laws of reflection by a moving mirror. For the general case in which the source of light is moving relative to the earth, the question resolves itself into two: (1) Is the image of a moving point source of light a single moving point source of light as in the classical electromagnetic theory? (2) Are the space-time co-ordinates of a point source and its image connected by the relations

$$x'=x-\frac{2c^2}{c^2-u^2}(x-ut) \qquad t'=t-\frac{2u}{c^2-u^2}(x-ut) \,,$$

$$y'=y \qquad z'=z \qquad (u=\text{velocity of mirror}) \,,$$

of the classical electromagnetic theory and the theory of relativity?

On the assumption that the first question is to be answered in the affirmative, various modifications of the equations connecting the space-time co-ordinates of a point source and its image might be tried on the arrangements of mirrors in the Michelson-Morley experiment. The interference fringes may in each case be regarded as the fringes produced by light coming directly from certain image sources and traveling in accordance with certain assumed laws of propagation which are also under test. The general problem is still more complicated by the contraction of the apparatus. The first question of the sharpness of the image of a point source which is moving relative to the mirror is difficult to settle experimentally on account of the lack of point sources of light moving at a high speed and at some distance from the earth. The velocity of a shooting star may be forty-five miles a second, but this is probably too small for the production of lack of sharpness in the image.

Director Adams closed the conference, thanking all the speakers for their contributions.

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April 1928