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Experiments on the Absence of Mechanical Connexion between Ether and Matter

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VI. *Experiments on the Absence of Mechanical Connexion between Ether and Matter.*

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THE conclusion of the experimental part of a previously published memoir, on "Aberration Problems and the connexion between Ether and gross Matter," dated March, 1892, and published in the 'Phil. Trans.,' Series A, for 1893, p. 777, is as follows:—

"The velocity of light between two steel plates moving together in their own plane, an inch apart, is not increased or diminished by so much as $\frac{1}{200}$th part of their velocity."

Since that date, of March, 1892, a considerable number of further experiments have been made, tending to confirm and extend the above conclusion; and of these experiments it is the object of the present communication to give a brief account. The general plan of experimenting having been sufficiently indicated in the previous memoir, no more details will now be related beyond those necessary to make the record of use to a later student of the subject.† The figures on pp. 759, 761, 767 illustrated the apparatus used.

The chief conclusion of the theoretical part of the former paper (p. 752) is that no first-order effect of purely irrotational etherial motion can ever be optically detected; in other words, that as long as the motion of a medium is characterised everywhere by a single-valued‡ potential function, the course of all observable *rays* through it, however reflected and refracted they may be, is independent of the motion (no matter how the *waves* may be tilted), and the time of journey along any given path through any kind of material is likewise perfectly definite, and independent of the motion, except for experiments directed to the second order of aberration-magnitude.

Hence no attempt to disturb the ether by using a spoked wheel, or revolving bars

* Assisted by Mr. BENJAMIN DAVIES.

† It may be argued that the details of an experiment having a negative result should not be published; but to me it seems that their publication in that case is more essential than in any other, because on them alone can a judgment be made as to how far the problem has been attacked in a careful and responsible manner, and because an answer "no," *when really attained*, is just as definite and positive a reply to some questions as an answer "yes."

‡ The epithet "single-valued" should be explicitly prefixed to the words "potential function" in § 29, p. 752, of the memoir referred to, 'Phil. Trans.,' A, 1893.

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or paddles, would have a chance of success, unless there existed a trace of something akin to viscosity by which the medium could be got hold of, and as the previous arrangement of apparatus seemed as well calculated as any other to detect the existence of a trace of viscosity, whereby ether in the immediate neighbourhood of moving matter should sooner or later be more or less carried along by it, no fundamental change in the mode of experiment seemed necessary; only improvement in details, and some modifications, in order to secure a closer and a wider generalisation.

Hitherto the experiments had been conducted with a pair of hard steel disks like circular saws, clamped together on a vertical axis, at a distance apart of one inch.

These disks had been spun, at a speed not exceeding 1250 revolutions a minute in the most accurate experiments, and the effect of the motion on a bifurcated beam of light, whose two halves travelled in opposite directions several times round in the space between the disks, was observed. One half of the light travelled in the same sense as the motion, while the other half travelled in the opposite sense; the two half beams were made to interfere in the field of view of a micrometer eye-piece, and a shift of the central band of the system by so much as the hundredth part of the width of a band could be observed. In making the above careful estimate of the result, however, the safe course was taken of assuming that $\frac{1}{20}$th of a band shift was the minimum certainly detectable.

There were some modifications still to be made before accepting a definitely negative result of experiment.

1st: to steady the motion, so that quantitative readings could be taken without tremor at a much higher speed of rotation.

2nd: to continue the motion for some considerable time, and to narrow the light channel or watch the effect close to a disk.

3rd: to increase the mass of the revolving matter.

4th: to magnetise the revolving material.

5th: to electrify it.

The connexion looked for between ether and matter being something of the nature of viscosity, the space between the disks may be considered rather wide; though it is difficult to suppose that any motion generated at the surface of the disks in a substance possessing any of the properties of an ordinary fluid, should not spread into the nearly enclosed space between them. It may, however, be conceivably argued that this diffusion of motion might take considerable time, and hence the modification labelled No. 2 above was called for. The modification No. 3 is to meet the argument that, even though a viscous connexion between ether and matter were disproved, it did not follow that there was not another mode of connexion competent to transmit motion from one to the other, viz.: the unknown kind of connexion which is concerned in gravitation; and to display any effect on this, a large mass must be used.

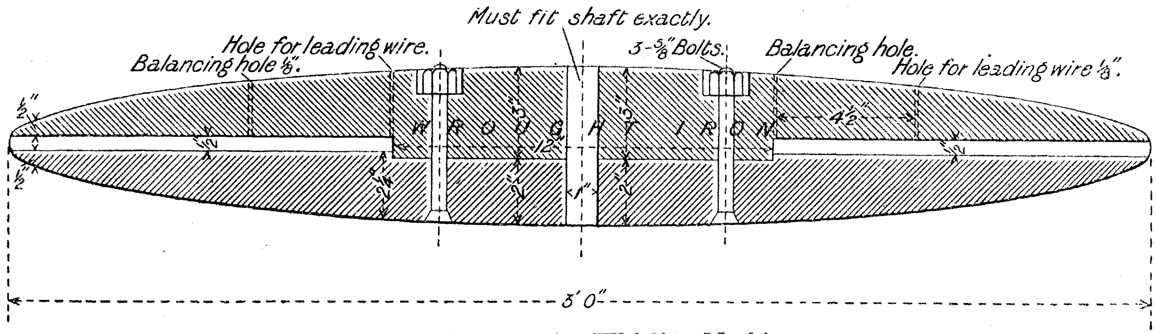
Probably the mass necessary to demonstrate any action of the gravitational kind would be impractically large, unless the earth itself could be used. Now, by staking out mirrors at the corners of a field, it is arithmetically quite possible to arrange for a perceptible shift of the bands due to the rotation of the earth, if it carries ether round with it; but it does not seem possible to experimentally observe that shift, unless some method could be devised of making the observer and his apparatus independent of the rotation.

It is to be observed, that since a motion of the disks relatively to the observer and the light causes no effect, the ether being stationary, it follows that a motion of the light and observer would produce an effect, since they would be moving relatively to the ether. Hence if, instead of spinning only the disks, the whole apparatus, lantern, optical frame, telescope, observer and all were mounted on a turn-table and caused to rotate, a reversible shift of the bands should be seen. It would not matter in the least whether the disks were revolving or not, and they might just as well be absent. The effect would be of an aberrational kind, the opposite light beams being accelerated and retarded by the motion appropriately. In an actual experiment of this kind, centrifugal force would give some trouble by introducing strains, and rapid rotation would be uncomfortable for the observer; but really rapid rotation should be unnecessary to show the effect. My present optical apparatus mounted on a turn-table revolving 4 times a minute should show something, viz.: $\frac{1}{100}$th band shift each way. A certain amount of discomfort during the accelerative stages of any speed could hardly be avoided, and even during steady motion there would be some inconvenience; for instance, at 30 revolutions a minute the observer's weight, at a metre and a half from the centre, would be half as much again, and would be inclined at 45° to the vertical. This, however, might be tolerated.

If the ether is stationary near the earth, that is, if it be neither carried round nor along by that body, then a single interference square, 1 kilometre in the side, would show a shift of rather more than one band width, due to the earth's rotation in these latitudes; see p. 772, 'Phil. Trans.,' 1893. But as the effect depends on the area of the square, a size of frame capable of mechanical inversion is altogether too small; there may, however, be some indirect ingenious way of virtually accomplishing a reversal of rotation—something for instance based on an interchange of source and eye—and if so, it would constitute the easiest plan of examining into the question of terrestrial ether drift.

If matter conceivably drags the ether with it in proportion to its mass, an ordinary lump of matter can hardly be expected to cope with the heart and to shift it in opposition to that body; nevertheless, since nothing is known on the subject one way or the other, it was thought well to give a more massive body a chance, by rotating a solid piece of iron about three-quarters of a ton in weight, and with a much narrower groove or channel cut in it for the passage of the light. It was easy to arrange at the same time for the magnetisation of this piece of iron when desired, so as to be able to attack the question above, numbered 4, without additional expense. Accordingly, I ordered from Messrs. MATHER and PLATT an oblate spheroid of best Swedish iron, a yard in diameter and half a foot thick, with a deep channel or groove half an inch wide cut into its rim to a depth of one foot all round. It was not found practicable to make the iron all in one piece, and accordingly it was constructed of two pieces bolted together, and its section is shown in fig. 1.

Fig. 1.



Oblate Spheroid for Whirling Machine.

The bottom of the groove was wound with wire to a depth of $4\frac{1}{2}$ inches, the wire used being No. 20 B.W.G. double silk-covered copper; and of it 14 lbs. 10 ozs. was wound on, in 94 layers of 9 convolutions per layer, the central iron core being 1 foot thick. The ends of the wire come out through small holes drilled for the purpose, with balancing holes drilled at equal opposite radii so as to leave the centre of gravity undisturbed, and the wire was then tightly bound with tape and steel to resist centrifugal force.

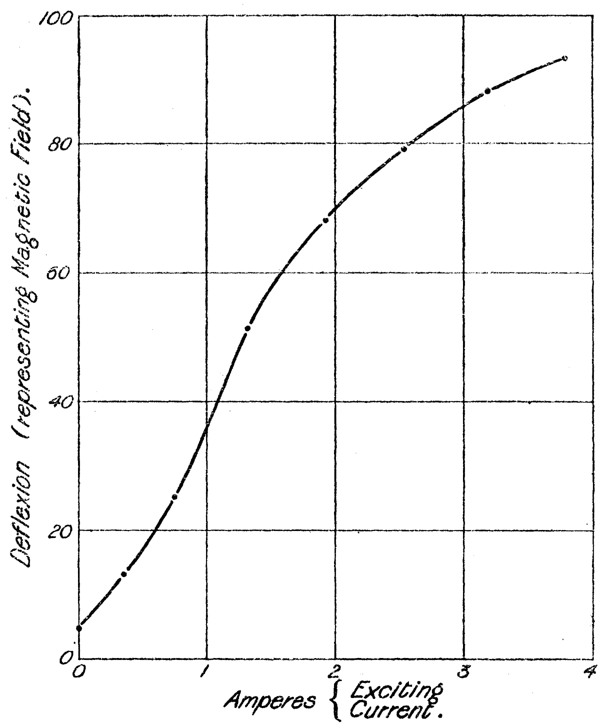
The free ends of the covered wire were clamped to the surface of the disk and led to a set of insulated brass rings on the upper part of the axle, so that an electric current either steady or commutated could conveniently be supplied whenever desired.

The resistance of the wire coil was measured by one of my students as 29·9 ohms, and its insulation resistance was just short of 2 megohms. The length of the wire is about 1 kilometre or two-thirds of a mile.

The magnetising current was usually supplied from the town mains, at 110 volts nominal, which gave a current of 3·8 amperes through the coil of 846 turns, and accordingly developed a magneto-motive force of 4000 cgs.

The lines of force so generated streamed across the half inch gap from the one half of the oblate spheroid to the other, being rather more plentiful in the deep parts of the channel. But the course of the beam of light only partially penetrated into the most intense region, and its mean track was situated about 4·6 inches from the periphery; so at this place I asked a student to measure the magnetic field excited by various strengths of current (by the common method of suddenly snatching out a small exploring coil and comparing the galvanometer throw with that caused by an earth inductor in the same circuit thrown over two right angles). There was a certain amount of permanent magnetism, and the result is indicated in the following table and plotted in a curve (fig. 2). The field intensity was not very different at different depths in the channel; it varied from 1730 cgs. near the rim to 1830 near the winding, when 110 volts were applied.

Fig. 2.



INTENSITY of field in the channel of the oblate spheroid (with decreasing current), at a depth of 4·6 inches from the outside.

Exciting current in amperes . .	0·0	0·34	0·67	1·32	1·92	2·52	3·18	3·80
Field in cgs. lines per sq. centim.	92	255	490	980	1310	1530	1670	1800

The maximum current was commonly used for purposes of excitation; and sometimes for a short time this current was doubled, by the application of 220 volts.

By the 8th June, 1892, this iron mass, weighing 14 cwt., had been mounted on the vertical shaft of the whirling machine, in place of the steel disks, and a spin was taken, with the optical frame in good action, and one half of the beam of light going three times round in the channel of the iron.

The shaft, however, was rather too weak to carry the weight, and exhibited a tendency to bend, which prevented the attainment of high speed. Moreover, the step bearing on which the shaft rested (as described in the previous paper, the shaft was supported on a hard steel pivot resting on another steel surface inside an oil chamber) had insufficient area; the intensity of pressure was too great for lubrication, and accordingly the surfaces ground together and got hot, causing the oil vessel to smoke vigorously. Considerable power was, therefore, needed to drive it, even at 300 revolutions a minute, but at this speed some observations were made. The bands were so clear that $\frac{1}{100}$th of a band shift could have been seen, but there was not a trace of shift when the mass was spinning, either with or without its magnetising current. The 110 volts of the town main were switched on and off and reversed many times, both when the mass was stationary and when it was revolving five times a second, but there was no effect.

Before proceeding to greater speeds it was obvious that the greater part of the weight of the iron mass must be taken off the pivot and dynamo-axle, and must be supported in some other way. A safety collar or guard attached to a frame above the wooden clutch, as shown in Plate 32, 'Phil. Trans.,' A, 1893, suggested the use of ball-bearings resting on this collar, which hitherto had been an inactive safety-guard, but was quite strong enough to support the weight required.

Accordingly we had this arrangement made, all the weight of the spheroid now rested on the guard collar by means of ball-bearings, and the steel pivot had nothing but the dynamo armature to support, this axle also being quite relieved from strain. The old wooden friction clutch was now of course useless, and it was replaced by gripping brass collars on the ends of the joining shafts, just below the ball-bearings, the power being transmitted by a pair of tangential stout copper wires, one on each side, looped round screw heads on the brass collars, so as to transmit a driving couple of considerable magnitude; but if by any accident the force transmitted was too great, the wires could snap and permit independent movement of the mass. Parenthetically it may be here stated that the wires broke several times during the course of the series of experiments, showing that the precaution was very necessary, and that a rigid connexion between the axles would have been dangerous.

During these alterations, other experiments, to be presently recorded, were in progress, and it was not till May, 1893, that careful optical spins were again conducted with the iron spheroid.

At this date the fringes were sometimes used after the light had been four or five times round, but usually the superior brightness and definition of the three-times-round beam was preferred. With a driving current of from 30 to 40 amperes and a voltage of about 80, the speed of 1000 a minute was readily maintained in the heavy mass by aid of the ball-bearings.

At first, however, a new disturbing phenomenon was observed: on beginning a spin the bands began to tremble and became partially obscure. This was not from shaking, nor did it seem likely to be due to wind reaching the semi-transparent plate, because the speed was quite slow. Screens glazed with microscope cover-glass were nevertheless provided, and next day another attempt was made. The flickering of the bands was just the same as before at low speeds, although there was no shaking, but as the speed increased they became clear, and at 800 revolutions were quite sharp. At 1000 revolutions per minute a careful set of observations was made, with the magnetising current applied to the spheroid,—on, off, reverse, off,—many times. But there was no effect whatever on the bands. Then we slackened speed and repeated the magnetisation and reversals down to stoppage, but not a trace of shift. The flickering and blurring of the bands, already spoken of, which still occurred at low speeds, and especially at increasing speeds, was not a serious trouble. It of course prevented exact observation while it occurred, but it was a purely temporary disturbance and did not cause the slightest permanent shift. As soon as the bands were clear again their position was absolutely unchanged. Nevertheless it was desirable to detect and remove the cause of the disturbance. Accordingly air was blown into the channel from foot bellows, but unless the wire coil inside had been recently used and imperceptibly warmed by the current the air jet made very little difference, though if there was the slightest inequality of temperature it caused a slight flicker. But a whiff of coal gas, the merest trace, distorted the bands with agony—sent them waving through ellipses and contortions into invisibility, allowing them to re-appear as the gas diffused away. They were manifestly extremely sensitive to fluctuating density, and hence we traced the previous flicker to hot air from the carbon rheostat which regulated the driving current. It seemed to get drawn into the channel sometimes at low speeds, but at high speeds was blown clear away. Starting and stopping the iron by *hand* did not cause the bands to flicker; they only flickered when the motor was used. It was plainly a heat convection effect. Hence we arranged that the carbon rheostat should be far away, and even the slight heat of the motor itself was screened and diverted off by a suitable platform or tray of wood and cardboard arranged above the motor.

Now I repeated the observations over and over again, with all sorts of changes, and never found either motion or magnetisation of the heavy iron mass cause the slightest real shift of the bands at the speed of 1000.

The channel being narrow, the plates themselves were visible in the eye-piece, and the bands could be seen reflected in them. Also diffraction or interference phenomena could be seen where the bands terminated on the iron (LLOYD's bands due to oblique reflexion) at one or other of the plates. A frequent appearance of the bands under these circumstances is depicted in fig. 3, next page.

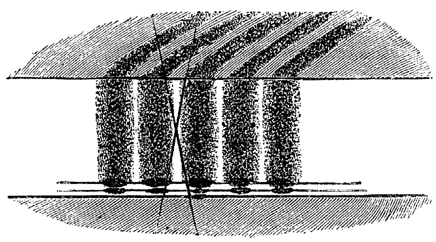
These reflected bands, and also the horizontal boundary stripes with the swellings of the bands on them, were also watched, the cross wires being shifted and set upon one feature after another, but in no case was the slightest shift seen on magnetisation; though certainly the test was not so delicate as with the free-air bands, because the plane of the channel-boundaries was not absolutely steady as the plates revolved.

The bands observed were often so broad that the distance between them was comparable with the half-inch channel-width, and sometimes the cross of the hyperbola-system was used as the feature upon which the cross wires were set, so that an exceedingly small fraction of a band-width could have been observed.

At 1000 revolutions a minute, the ball-bearings began to get hot, and some attention had to be paid to them to get the number of steel shot and the lubrication as perfect as possible. The iron mass took a long time to slow down from full speed, and after being left to itself for twenty minutes or half an hour was still moving. Sometimes it was stopped more quickly by a brake, to see if acceleration had any effect, but none was seen.

On the 8th May, 1893, we had a good spin at 1200 a minute with the spheroid,

Fig. 3.



Appearance of the interference bands as seen in the half-inch channel of the oblate spheroid. The bands happened to be reflected in the upper iron surface, and to show subordinate interference stripes in the lower iron surface.

magnetised and reversed, etc., looking as carefully as possible at the bands reflected in the iron and at every part of the bands, but no change of the minutest kind was visible.

On the 9th May, we had a similar spin in the reverse direction, conditions fairly satisfactory, and results definitely negative.

Sometimes an alternating or commutated current was supplied to the coil, but its self-inductance and time-constant were so great that little power could be thus developed. Anyhow, no shift of the bands was seen.

Experiments at Higher Speeds.

During the next few weeks, the iron spheroid was replaced by the old steel disks, and great pains were bestowed on getting these accurately balanced, so that a high speed could be reached without tremor.

By June, the ether machine could be driven at speeds above 3000 a minute, the power used being 50 amperes and 100 volts.

But at these higher speeds there were many difficulties. The blast was, of course, excessively strong, and it was necessary to carefully screen it from the mirrors and frame by boxing the plates up in the wooden drum before described; moreover, higher speeds could be attained with the air thus boxed up. But the air got very hot, and this spoiled the fringes, so that at high speeds they were often invisible. Without the drum the fringes remained visible, but the blast caused a shift often of as much as two bands. This shift came back on stopping, and sometimes rather more than came back, ultimately settling down as if slow strains were working themselves out. The drum was now replaced without floor or roof, and with only very narrow slits for the light to get through. The light was often got four times round. A smaller shift still remained, and there was nothing for it but to glaze the slits, and broaden the drum above and below, so that no trace of air blast could reach the frame, at the same time that there was plenty of ventilation to keep the air quite cool.

It need hardly be said that the presence of so many glass surfaces in the course of the beam increased the difficulty of getting the fringes distinct for the three-times-round path, for each half of the beam had to undergo not only 11 reflexions as usual, before returning to the semi-transparent plate, but also 24 transmissions through panes of glass, *i.e.*, 48 transmissions through a glass-air surface at 45°. The intensity of the beam is thereby greatly enfeebled, and the glass has to be of excellent optical quality and free from strain if good definition is to be got. Ultimately, by selecting from a number of glass plates supplied by Mr. HILGER, the patience of Mr. DAVIES overcame the difficulties, and fringes were got of sufficiently satisfactory quality with the beam three times round; a Brockie-Pell arc light imaged upon the aperture of the collimator, and kept finally steady by hand, being used as the source. It was found that a great width of beam was difficult to use, probably for a reason subsequently to be mentioned (varying air density due to centrifugal force), and a diaphragm was commonly used over the object glass of the collimator.

Under these conditions a set of observations were made, with the speed up to 2,800 a minute, first in one direction, then the other, and then the first way again.

In each case the bands remained visible at the highest speed, though at certain intermediate speeds, especially about 1000 and 1700, a slight tremor smudged them.

The shift observed now was moderately small but quite distinct, and was estimated with the micrometer at $\frac{1}{16}$th band. It repeated itself each time without regard to the direction of spin, and disappeared, though not instantly, when the disks stopped. It seemed probably due to some obscure residual effect of the blast, perhaps on the cover glasses of the drum. The shift was irreversible, and of reversible shift there was none.

At these higher speeds it would naturally be thought that the true theoretical effect due to whirling air $(\mu^2 - 1)$ should be observable; but if its amount be reckoned it will be found to be less than $\frac{1}{200}$th band, and therefore not detectable for certain under the above conditions.

The only effect distinctly due to heat in the above experiments was a flicker of the bands at the lowest speeds, just before stopping. It was due to the gentle warmth of the motor, an air current rising towards the disk and mirrors when the blast was insufficient to drive it away. But it never did the least harm, and could only be seen just before stopping, or sometimes a second or two after stopping. When it was over, the bands were absolutely in the old place; its effect had been to wave them about slowly and slightly.

In all these experiments the brass collar coupling, with the copper wire force-transmitters, was used to connect the two axles, instead of the old friction clutch, which was insufficient and not so dependable.

Possible Time Effect.

On June 6 we kept the disks spinning for three hours at 1900 revolutions, to see if any shift developed with time. The result on this particular occasion was an apparent shift followed by a blur and invisibility of the fringes. They did not recover on stopping, but could be brought into visibility by moving some of the screws. This was evidently a bad experiment, and the apparatus was overhauled and steadied up.

Took another spin next day, at a speed of 2400 revolutions, for three hours, and not the slightest shift developed itself in this time.

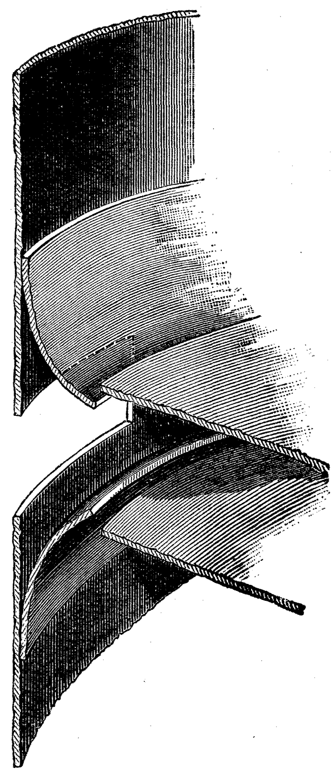
By June 23, the step bearing at the bottom of the axle shaft was replaced by a new one, and other mechanical conditions were improved. The machine now ran up to 2400 without a tremor, and a current of 30 to 35 amperes was sufficient to do the driving. An observation was made with the drum in, with glazed windows, no top or bottom (as before), the light going three times round, and the speed being kept at 2100 for two hours. At the first instant there was a shift of $\frac{1}{20}$th band, but it did not increase, nor did it recover on stopping. Went on with a reverse spin, also at 2100, under the same conditions, and saw not a trace of shift at going or stopping, or during long spin—only the usual flicker as the speed got very slow. The bands were distinct all the time. A good experiment. We conclude that time has nothing to do with the matter.

Attempt to Observe the Air Effect.

From June to November, 1893, continual attempts were made (except during a month's vacation), by careful and repeated setting of the micrometer wires on the bands, by taking the average of a set of readings at each speed and plotting them, to get some dependable record of the true air effect, free from disturbing causes. This labour was undertaken not only because it was thought of interest to observe this hitherto unobserved small quantity, but also because its detection would emphasise the truly negative character of the ether effect.

Taking μ for air as 1·00029, the $k = 1 - \frac{1}{\mu^2} = \cdot 00058\ \theta$, and the fraction of a band shift observed being x at an angular speed ω, and with n light-journeys round the optical square of side a, we should have $kn\omega a^2 = 4 \times 10^5 x$ (see p. 772, *loc. cit.*); wherefore, writing $\omega = 2\pi N$, and considering that the observable x is $\frac{1}{100}$th of a band, it follows nN must be 300, in order to show the air effect, that is, the speed must rise to 6000 revolutions a minute with a light-journey of three times round. This speed would lead impractically near to the bursting-strength of materials; but it was hoped that, by taking the average of a series of settings, $\frac{1}{200}$th of a band could be safely observed, and thus the effect of the air-spin detected. And if everything had gone well, I think this might have been done, but the difficulties met with caused a careful examination of the brick pillars and foundations beneath the floor, with the result of discovering that the brick pillars, by which the optical frame was ultimately supported on its gallows support (fig. 11, p. 767, 'Phil. Trans.,' 1893), were not so entirely independent of the whirling machine's stone altar as they ought to have been. During the vacation bricklayers and carpenters were accordingly called in to re-set the warped pillars beneath the floor, and to clear away all joists and everything that could be suspected of in any way helping to transmit vibration. The result of this work was a beautiful steadiness and visibility at high speeds; but still the bands showed what we call a concertina action, that is, a slight moving of the lateral bands in or out from the central one, as the speed varied. There would be no strong objection to such motion, if the precise symmetry of the central band could be assured, but, as this assurance was not forthcoming, the central band sometimes shared in this motion; and even if it did not, its steadiness was suspicious, because perhaps it *ought* to have slightly moved.

Fig. 4.



The residual trouble appeared possibly due to an obscure influence on the glass windows of the drum, possibly a slight warping due to warmth or air pressure, and an attempt was made to dispense with the window panes and to screen from the blast by another method. Accordingly a fresh drum was made of brass, with a pair of deflecting rims or flanges, so placed as to catch the air whirled off the disks all round, and deflect it out of the way upwards and downwards, the light passing on through a chink or slit in the brass drum to the region protected by these flanges (fig. 4).

Some residual draught did however manage to reach the mirrors, and, although they were strongly supported, it seemed to flutter them even if unable regularly to tilt them. It was then attempted to lessen the freedom of air supply to the axis of rotation, by wooden circular boards, fitting the axle loosely, and nearly as large as the drum, thus greatly interfering with the supply of air. As soon however as the ventilation was thus interfered with the air got distinctly hot, which was a worse evil.

The drum was supported separately on long wooden girders, so that no part of it was in immediate contact with the optical frame. The girders at first rested on the same gallows as supported the frame (as shown in the figure, page 767, just referred to), but this was ultimately found to be bad, because of a torque received from the whirling air and transmitted to these piers, which conveyed some trace of it to the frame. So while the frame was still supported on its independent piers, and the whirling machine was still clamped to its massive stone altar on the rock, the drum which received and screened the blast was now separately supported by special uprights from the floor (on which people did not walk during an observation), and this, on the whole, was an improvement. Any torque effect, however minute, being of a reversible character, was peculiarly dangerous, for it might easily have been mistaken for a result of the kind that was being looked for.

This memoir shall be abbreviated by the omission of all the careful sets of readings taken during this period, a record which occupies seventy pages of the laboratory note book; for it must be admitted that, although representing a good deal of work, they fail ultimately to show the air effect; and this probably for the reason that any effect of that magnitude would be certainly masked by the residual slight disturbing causes present.

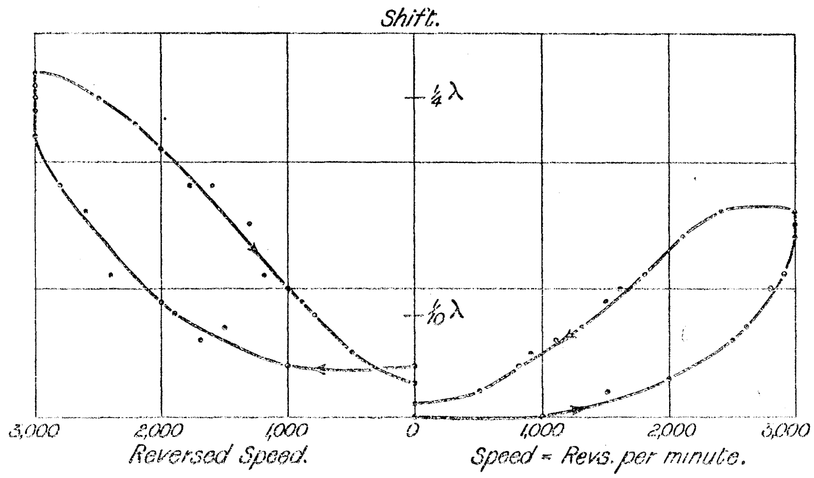
The only thing I will record is a plotting of one of the larger spurious shifts (obtained before the foundation was inspected and altered) to illustrate its typical lagging character. The dots in this case represent individual readings, not averages of setting, and they incidentally show the kind of setting which is possible at high speeds through all the cover-glasses, with the light three times round, and when the steadiness was by no means perfect. The process was as follows :—

The micrometer wires were set, the single vertical wire in the middle of the middle band, and the X wire on the yellow of the first band to the left; or else *vice versâ*. Both wires were read, at gradually increasing, and then at decreasing speeds, and the results plotted on the right-hand side of the two diagrams (figs. 5 and 6), so as to show (a) the shift of the middle band due to strains and slight communicated tremors, (b) the change in the scale of wave-length due to concertina action. Then the brushes of the dynamo were reversed, and another spin taken in the opposite direction, and then the readings taken which are plotted on the left-hand side of the two diagrams. The total maximum shift was about $\frac{1}{4}$th of a band on this occasion.

The following *averages* of a set of readings taken in July, 1893, may also be quoted :—

With disks stationary, the middle band read . .	94·4	each division being	$\frac{1}{61}$th	of a wave-length.
Disks revolving 3000 a minute, the middle band read	94·4	,,	$\frac{1}{59}$th	,,
Disks stationary again, the middle band read .	95·6	,,	$\frac{1}{67}$th	,,
Disks revolving 3000 a minute in the opposite direction, the middle band read	95·5	,,	$\frac{1}{77}$th	,,
Disks stationary again, the middle band read	92·3	,,	$\frac{1}{60}$th	,,

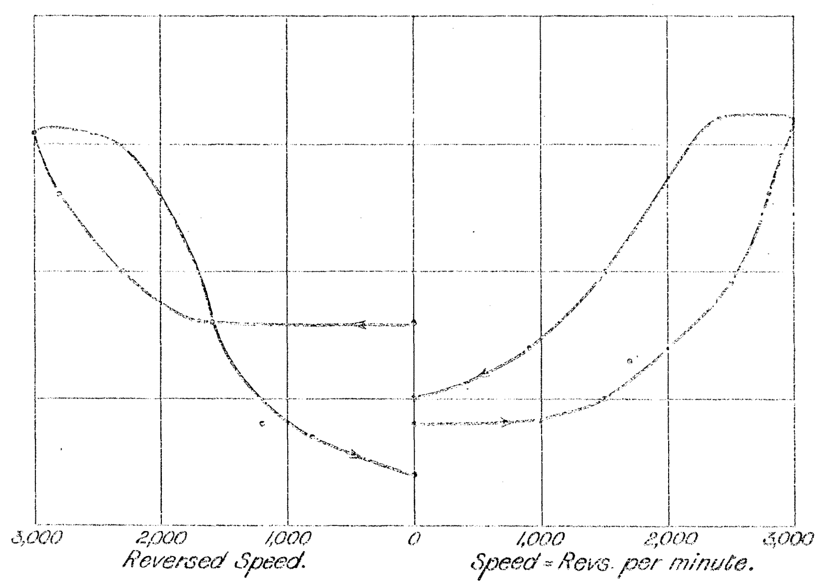
Fig. 5.



Details of one of the larger spurious shifts of the middle band, as observed before the brick pier supports of the optical frame had been properly overhauled.

The dots represent individual settings and readings of the micrometer cross wire set on the middle band during a pair of spins in opposite directions, while the speed was first increasing and then decreasing.

Fig. 6.



Corresponding concertina action (on same scale), or change in the breadth of the bands during the above spurious shift. Dots represent differences between the readings of a micrometer wire set on the yellow of the 1st order and the readings of the cross wire set on the middle band. The zero of the vertical scale is far below, 7 squares below 0. At that distance this figure may be placed above fig. 5.

(To realize that such readings fail to show the air effect it may be simplest to write down ideal readings that *would* show it exactly, viz. :—

94·4	each division being $\frac{1}{60}$th λ	
94·1	,,	,,
94·4	,,	,,
94·7	,,	,,
94·4	,,	,,

if they had been obtained.

Or the following would do equally well, though less obviously :—

94·4
94·6
94·4
95·2
94·4

where there is an irreversible shift of half a division ($\frac{1}{120}\lambda$) superposed upon the reversible effect.)

After the overhauling of the foundations, in September, the bands were beautifully distinct, and there was no tremor. There was now no clearly perceptible shift of the middle band, but still there was a concertina action, shown by a broadening of the bands during spin, thus altering the scale of wave-length. The following extract will serve to illustrate this :—

8th September.

With the Disks at rest, the middle band read	7	while the yellow of the first band read	85	divisions.
With the Disks spinning 3000 revs., the middle band read	7	,, ,,	107	,,
With the Disks at rest, the middle band read	7	,, ,,	85	,,
With the motion reversed 3000 revs., the middle band read	7	,, ,,	102	,,
With the Disks at rest, the middle band read	7	,, ,,	85	

Here the constancy of the number 7 means that no change could be seen, but the readings are not averages, nor was the wire reset, and nothing less than a whole division shift would have been observed. The air effect would require $\frac{1}{2}$ a division shift. The existence of the concertina effect was held to render useless an attempt to take a serious set of averages ; and by no means could it be wholly got rid of.

Ultimately we decided to do away with the optical frame so close to the disks altogether, and to arrange the mirrors at a distance, out of the blast, on brackets fixed to opposite walls of the room, sending the light round a large oblong instead of a square, and letting two sides of this oblong pass through the channel between the disks. (The arrangement of this experiment is shown in fig. 8.) Meanwhile, we dismantled the machine and sent the disks back to MATHER & PLATT to be fitted with a third one for electrification. (26th Oct. 1893.)

If there were good reason to push the experiment still further (and for the present I see no such good reason), I should be disposed to attempt placing the disks in an air-tight chamber, kept exhausted by a mechanical oil pump, so as to do away with the greatest part of the troublesome air phenomena.

A possible reason for the concertina effect, and for the slight residual irreversible shift sometimes observed, suggests itself in the gradation of density in the air between the disks, due to centrifugal force. To estimate its magnitude under any circumstances, we may consider the equilibrium of an element dm of air at radius r and write :—

$$r\omega^2 \, dm = \frac{dp}{dr} \, dr \, . \, r d\theta,$$

or

$$\rho r \omega^2 \, dr = dp = k d\rho,$$

whence the density at any radius is

$$\rho = \rho_0 e^{-\frac{\omega^2}{2k}(a^2 - r^2)}.$$

Hence, for disks a yard in diameter making 3000 revolutions a minute, the density at centre is about $\frac{8}{9}$ths of that at circumference; and the change of density per centimetre breadth of beam, at a radius of 1 foot, is

$$\frac{dp}{dr} = \cdot 425 \times 10^{-5};$$

which, if $\mu - 1$ be taken as proportional to ρ, gives $d\mu$ about equal to $\cdot 23 d\rho$; or say 10^{-6} as the difference of refractive index, on either side of a beam of light 1 centimetre broad, in the region of the mean light path. This is equivalent to the effect of a difference of temperature, in the air on either side of the beam, of a $\frac{1}{5}$th of a degree centigrade.

The gradation of density could therefore cause a distinct effect if the beam of light had an odd number of paths between the disks ; but since there are in our case an odd number of reflexions, and therefore an even number of paths, with the beam laterally inverted at each reflexion, the effects must very nearly compensate each other.

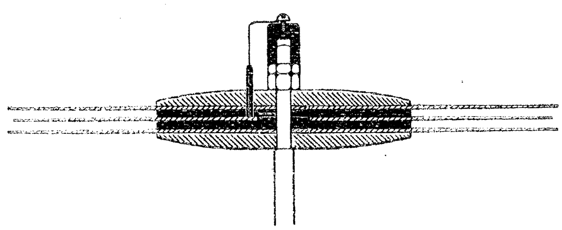
If by reason of some want of symmetry there was on the whole a centimetre length of path uncompensated by a laterally inverted portion elsewhere, the corresponding retardation due to gradation of density would be a millionth of a centimetre, causing an irreversible shift of $\frac{1}{50}$th of a band. This cause may therefore account for part of the residual irreversible shift observable at high speeds, when all kinds of mechanical and thermal disturbance have been apparently eliminated.

The question of Electrification.

Although it must now be taken that such masses of matter as we had been dealing with are incompetent to disturb the ether in a rotational manner (for, as has been emphasised in the previous memoir, irrotational motion of a single-valued kind could not be detected by interference or any other optical experiments, since such motion in no way affects either the path or the speed of a ray), and although further it has now been shown that the conveyance of a magnetic field by moving matter confers no power of gripping the ether, yet it was thought possible that electrification might do it; because an oscillatory charge certainly radiates wave motion into the ether. And although radiation is not any known kind of mechanical disturbance, but is concerned with the ether's electrical properties, and need not necessarily involve anything analogous to etherial viscosity even in the neighbourhood of matter, yet it was thought possible that electric charge, being as it were the connexion between ether and matter, might confer upon a material body some power of gripping and rotationally carrying forward the ether in a quasi-viscous manner. At any rate, whatever reason could be urged for or against such a connexion, it was desirable to bring it to the test of experiment and superpose an electric field upon the moving disks.

The natural plan for electrifying the disks would seem to be to make one of the disks positive and the other negative, but after consideration it was found impracticable to insulate the existing disks satisfactorily; and a third disk half way between the other two was contemplated. This might possibly have been stationary and independently supported, but some preliminary experiments with a plate thus held showed that in the draught of air it developed some warmth and interfered with the

Fig. 7.

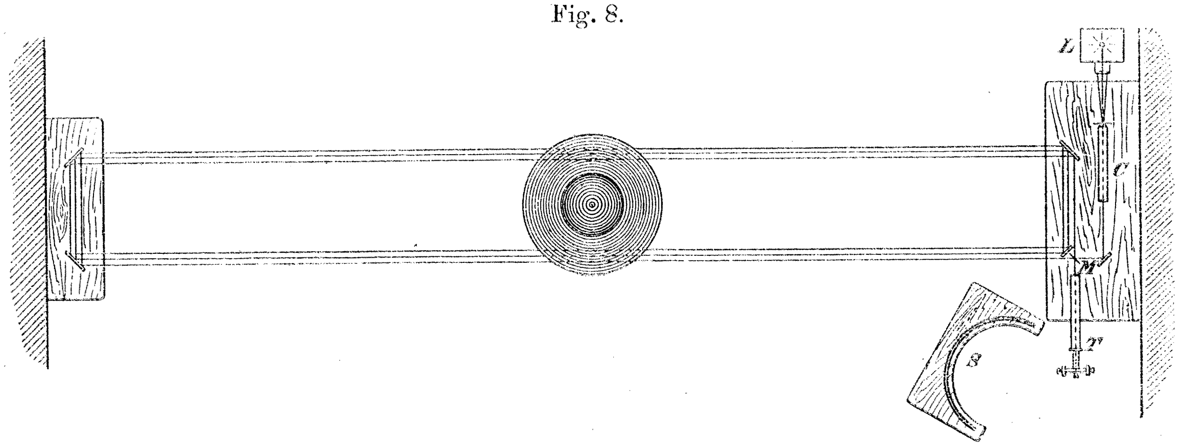


Arrangement of the insulated steel disk between the other two, showing the mode of electrical connexion, with an axial stud touching a Voss machine terminal.

fringes; so a rotating disk, clamped in insulating washers between the other two, was decided on, and made as shown in fig. 7. The new disk was made an inch smaller in diameter than the others, but otherwise it was just like them. Connexions were arranged through insulating holes for the supply of electricity near the axle while spinning, and with a Voss or Wimshurst machine a constant succession of sparks could be maintained from the middle insulated disk to the earthed outer ones. These sparks were about half an inch long, and were sharp and clear: they usually occurred from its rounded edge, but sometimes from its flat surface, which on account of the bevel was slightly nearer the other plates than the edge was. It may be taken that the difference of potential concerned was not far short of 40,000 volts., and that the electric tension was about as much as common air can stand.

The interference bands could now be seen bisected by the middle disk, and usually either the upper or the lower half was used for an observation, the positions of the bands close to one of the plates being specially watched, especially at and before each spark, while the disks were revolving at 2800 a minute and the light going three times round.

Fig. 8.



One of the latest arrangements of the optical parts, on the opposite walls of a room, so as to be undisturbed by the force or heat of the blast from the disks revolving in the middle of the room. L is the electric lamp; C, the collimator; T, the double micrometer telescope; and S the double boiler-plate screen to protect the observer; M is the semi-transparent plate, and the light is indicated going three times round a rectangle, with part of its course between the disks. The whole is drawn to scale, the diameter of disks being 3 feet, or nearly 1 metre.

The experiments were chiefly done in February, 1894, and the bands were broad and clear. There was a trace of irreversible shift when the disks were spinning, but its amount was quite independent of the direction of rotation, and there was not the slightest difference whether the plates were electrified or not.

The path of light in this set of experiments was the long oblong with two of its sides between the disks as already briefly mentioned. The mirrors were supported on opposite walls of the room, and a diagram of the arrangement is annexed (fig. 8), the light being sent three or more times round the oblong.

With this plan, *alternate* light paths go between the disks, and accordingly the density-gradient-effect, if any, is uncompensated. A very narrow beam was used, however, and the effect is demonstrably small, though it probably accounts for the irreversible shift observed.

There was not a wink when the sparks occurred, whatever the speed. The test for electrification effect is easy, because the charge can be switched on and off while in a state of steady spin, and the slightest difference would be observed. There is certainly no perceptible effect.

When the disks were slowed down and nearly stopped, there was the old temporary flickering of the bands, but this had nothing to do with electrification, it was merely irregular warmth in the air.

We tried also a couple of Leyden jars with their outer coats connected to the disks, so as to get strong "B" sparks between them (see 'Proc. Roy. Soc.,' 1892, vol. 50, pp. 4 and 18), but still there was no effect.

Without further delay I conclude that neither an electric nor a magnetic transverse field confers viscosity upon the ether, nor enables moving matter to grip and move it rotationally.