Consequences of Rotational Motion.

32. If the condition of a velocity potential is not satisfied, it follows from equations in §§ 23, 24, that a ray through an ether possessing rotational motion will not be straight.

An ether in the slightest degree carried along in the neighbourhood of moving matter must by all analogy be called viscous. Now a ray approaching a moving body in a viscous ether will in general be entering strata moving with increasing speed, and will accordingly be curved in the direction of the motion (§ 52). A negative or lagging real aberration would therefore occur, in addition to the simple positive apparent aberration caused by motion of the observer; and the direction actually perceived would be the resultant of the two. The motion of an observer is practically constant all over the earth, but the drift of ether strata would be different at different aspects to the earth's orbital motion. Hence the observed value of stellar aberration ought to vary with the time of day, and with the latitude of the observer.*

It becomes important to ascertain definitely whether the ether is viscous or not—whether moving matter can, in the smallest degree, drag or shear the ether in its neighbourhood. If it does, either the theory or the observations of astronomical aberration must be overhauled. But experiment is necessary to answer the question.

Details of Experiment to determine how much, if any, effect on the Velocity of Light is exerted by the Motion of Gross Matter near the Ray.

33. After considering the motion of belts, of fly-wheels, and of double plates rotating oppositely, as in the Holtz machine, I decided to try a pair of plates clamped together with a disk-shaped space between them, and to reflect a split beam of light several times round in this space, half the light in one direction, and half in the other, while the pair of plates were revolving at a high speed. MICHELSON'S device for obtaining two equal beams of light travelling in opposite directions round a contour, by means of a semi-transparent mirror, is plainly the most suitable for any case where the effect of motion is to be observed, and where great length of path is desirable.

Accordingly I sent to Mr. ADAM HILGER a strongly braced stout wooden frame, or hollow square, 38 inches in the side, internal measurement, and asked him to fit it—(1) with three plane mirrors, each 6 × 2 inches, supported in a specified fashion, and silvered on the front; (2) with a fourth mirror, 4 × 2 inches, supported rather differently, bevelled to 45° at two of its edges, and likewise fully silvered on the

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[* It has been pointed out also by Professor FITZGERALD that, if such stratified motion existed, the top of a tower or mountain should exhibit aberrational effects when viewed from below. This might be tested with greater accuracy than is possible in celestial observations.—July, 1893.]

front; (3) with telescope and collimator holders at 45° to the frame; and (4) with a holder for a thinly silvered piece of optically plane glass, 4 × 2 inches, at intersection of axes of telescope and collimator.

The drawings in Plate 31 sufficiently illustrate this part of the apparatus.

The telescope and collimator were a pair (1½-inch aperture 1 foot focal length) given to the Univ. Coll., Liverpool, by Mr. I. ROBERTS, F.R.S. They happened to have quartz lenses, which was unnecessary, but otherwise were well adapted for the purpose; the slit or aperture of the collimator having especially convenient motions. To the eye end of the telescope, in addition to its own cross-wire eyepiece of low power (which was useful for setting), I adapted an excellent micrometer by COOKE, belonging to a 4½-inch telescope, presented to the College by Mr. GEORGE ROGERSON. It has a pair of independent micrometer heads, each divided into 100 parts, moving respectively a vertical spider line and an X. It also has eyepieces of various powers: the one commonly used for the measurements here recorded being marked "200."

Fig. 7.

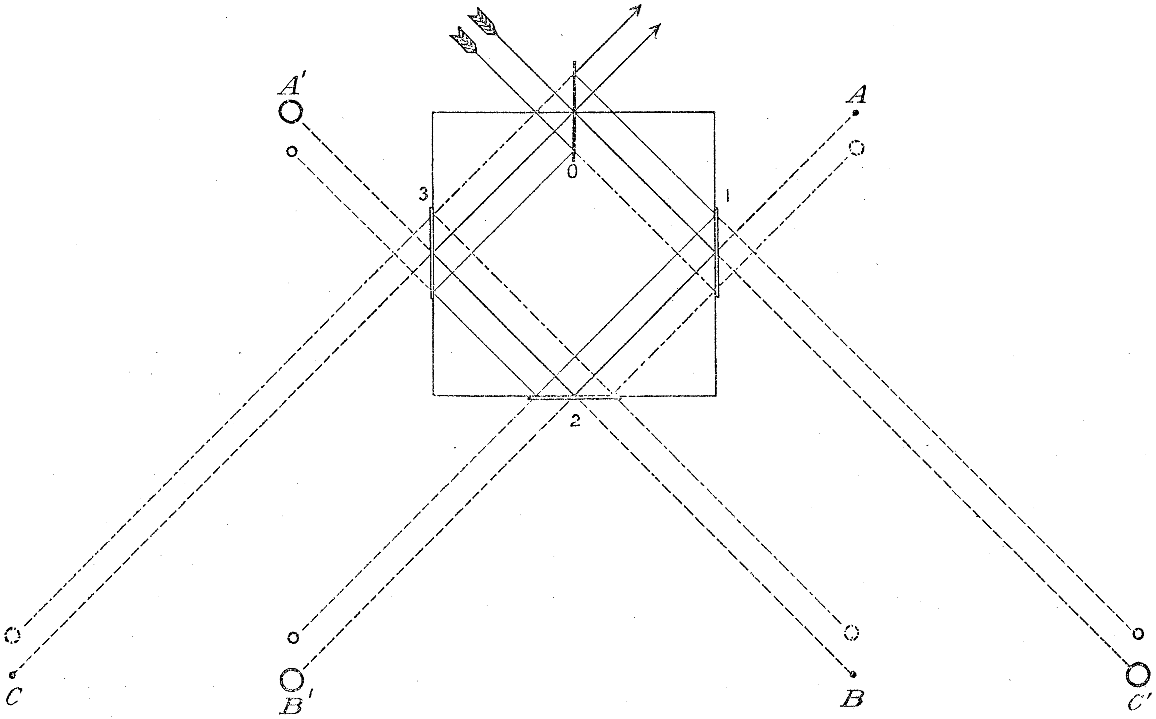

Diagram showing images in simplest case, with three mirrors, and beam going only once round optical square. The points to be imaged are the splitting points on semi-transparent mirror O. Two such points are shown, one imaged by a dot, the other by a ring. A represents the pair of images in first mirror for the transmitted ray; B the image of these in second mirror; C the image in mirror 3. Dashed letters mark the corresponding images for the reflected ray; and the final function of the semi-transparent mirror is to make C′ coincide with C so as to give interference.

Examining the path of a beam of light from collimator to telescope round the frame, as shown most simply in fig. 7, it appears that, by reason of there being an odd number of reflexions, the interfering rays do not travel identical paths in opposite directions, but only parallel paths.* They enter the square at one point of the semi-transparent mirror, and they leave it at another point, having meanwhile travelled side by side. I therefore designed a compensator, consisting of a plane piece of optical glass, with its faces not parallel but inclined at a very small angle; this was mounted in a round cell, and made capable of measured rotation in its own plane. By introducing this normally into the beam, and rotating it into the right position, it was supposed that accidental inequalities of path could be compensated; and also that the bands could be shifted by a measured amount. Hitherto, however, no use of the compensator has become necessary, and I have some doubts as to whether it would act in the way supposed, or whether it would not merely double the number of bands in some positions.

The object of the fourth or front mirror, shown in fig. 8, is to enable the light to go more than once round the frame. This mirror has to stand a little forward, in advance of the square defined by the planes of the other three, and the amount by which it stands forward regulates, at the same time, the width of the beam and the number of journeys it makes round the frame. Everything else can be permanently set.

If each beam is of breadth b, and travels n times round the frame; if the length of this fourth mirror is l, and the amount by which it stands forward out of the square is d; then it is easy to see, by fig. 8, that

$$l = (n - 1)\sqrt{2}\,.\,b, \qquad \text{and} \qquad b = \sqrt{2}\,.\,d;$$

also that the centre of the effective part of the semi-transparent mirror, *i.e.* the intersection of axes of telescope and collimator, is a fixed distance, viz. $\frac{1}{2}l$, behind the square.

Hence the only thing that requires re-adjustment in order to vary the number of times light goes round, is d, the setting forward of the front mirror.

The 45° bevel at the ends of this mirror is to enable the whole of its (silvered) face to be utilised, and to allow a beam which just misses it to graze past it unimpeded into the telescope. (See Plate 31).

With the front mirror 4 inches long, and the centre of semi-transparent mirror 2 inches back from the 38-inch square formed by the other three mirrors, the most frequent adjustment has been to set the front mirror 1 inch forward. A parallel beam incident on the centre of semi-transparent mirror, at 45°, must now go three times round the frame, rebounding three times from each of the three mirrors, and twice from the

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* This fact makes the bands more susceptible to some kinds of disturbance, *e.g.*, irregularities of temperature or density; more stable fringes can be obtained by using an even number of reflexions, *e.g.*, a triangle instead of a square (*cf.* 'Nature,' vol. 46, p. 500), but then the light will not go more than once round.

front mirror; and the width of the beam may be as much as 1·4 inch. The pair of plates between which the light has to go are, however, only 1 inch apart, so this limits the effective aperture in a direction perpendicular to the plane of reflexion. A narrower beam may be sent more times round, by setting back the fourth mirror the proper amount.

Fig. 8.

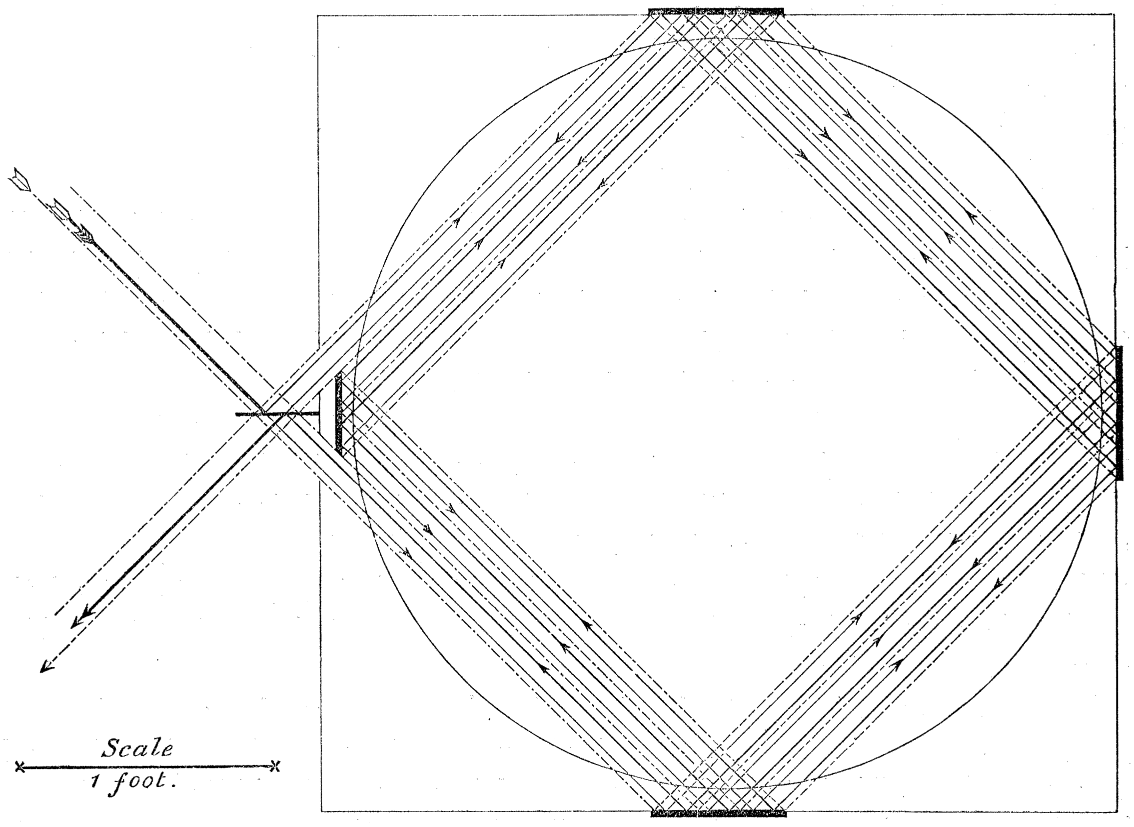

Diagram showing arrangement of mirrors for the usual case of beam going three times round. Interfering rays travel parallel but not identical paths, and re-unite at a point different from that at which they split. The greatest possible size of beam, with a 4-inch fourth mirror, is represented. The centre of the semi-transparent glass, *i.e.*, the intersection of telescope and collimator axes, has to be 2 inches behind square formed by the three mirror faces. The fourth or extra mirror has to be 1 inch in front of the same square.

Whirling Machine.

34. For the pair of plates I use a couple of steel circular-saw disks, one yard in diameter, of best hammered steel, and bevelled down somewhat from centre to edges, braced up at centre with wrought-iron cheeks and bolts. The plates were specially made by SEEBOHM and DIECKSTAHL, of Sheffield, and are stated to be able to stand sixty-seven tons to the square inch.

At this strength they would fly to pieces at 8800 revolutions per minute, supposing them simply cylindrical and unbraced at the centre hole.* The bevelling and the clamping ought to afford margin enough to run them up to 6000 (though the makers recommend no speed over 4000); but hitherto I have not spun them at more than 3000 revolutions to the minute, and even at this speed, I now have a screen or sentry-box of double boiler plate (consisting of a small iron boiler cut in halves longitudinally and one half fitted inside the other) for the observer, whose eye is in the plane of the disks, to look through.

The use of steel disks is sufficiently justified by the high speed they will stand, but it may be also held that iron is the most probable great constituent of the earth; and further, that as there are so many other ways of experimenting on transparent matter, opaque matter is appropriate in this experiment.

Since steadiness of rotation was very essential, I arranged to rotate the disks horizontally on a vertical shaft balanced on a steel point in an oil vessel, and with a slightly flexible or elastic bearing near the top, so as to get the whole to sleep like a tee-totum; and in order to avoid any lateral strain, as of driving belt, to drive electrically by a dynamo armature on the axle itself (fig. 9).

Messrs. MATHER and PLATT were good enough to undertake this part of the work, using their smallest size Manchester dynamo as motor, with its axle set up on end, the armature being wound with less wire than usual, and being extra strengthened against centrifugal force by steel wire. The ordinary bearings of the dynamo remain, with oiling wicks inserted, and hence there is a little nicety required to get all three bearings in a precise line. It is also needful for the shaft to be vertical, to avoid any attempt at gyroscopic precession.

Fearing lest some damage to the disks might occur from sudden application of too great moment to the armature, especially by reason of some accidental jamb or other sudden stoppage, I imitated a device employed in some milk-whirling machines, and introduced a wooden cup or friction coupling between the top of the dynamo shaft and the bottom of the disk spindle. The cup being made of hard wood grips the rounded end of the disk spindle, and thus applies to it sufficient rotating moment, but permits slip in preference to violent acceleration. This plan is, I think, on the whole advisable, and is certainly a safeguard. It may seem to spoil the tee-totum idea, but the dynamo axle, which is supported at each end, and constrained to rotate about rather a long axis, is to be regarded as a driving machine only; the "top" begins from the wood cup upwards. The brass collar of the upper bearing is let into an india-rubber cylindrical socket, so as to afford some very slight play; and just above the wooden cup is a safety collar or loose guide not touching the shaft.

My assistant, Mr. B. DAVIES, had some trouble in getting and keeping the two shafts accurately aligned, especially since any wear of the wooden cup tends to throw

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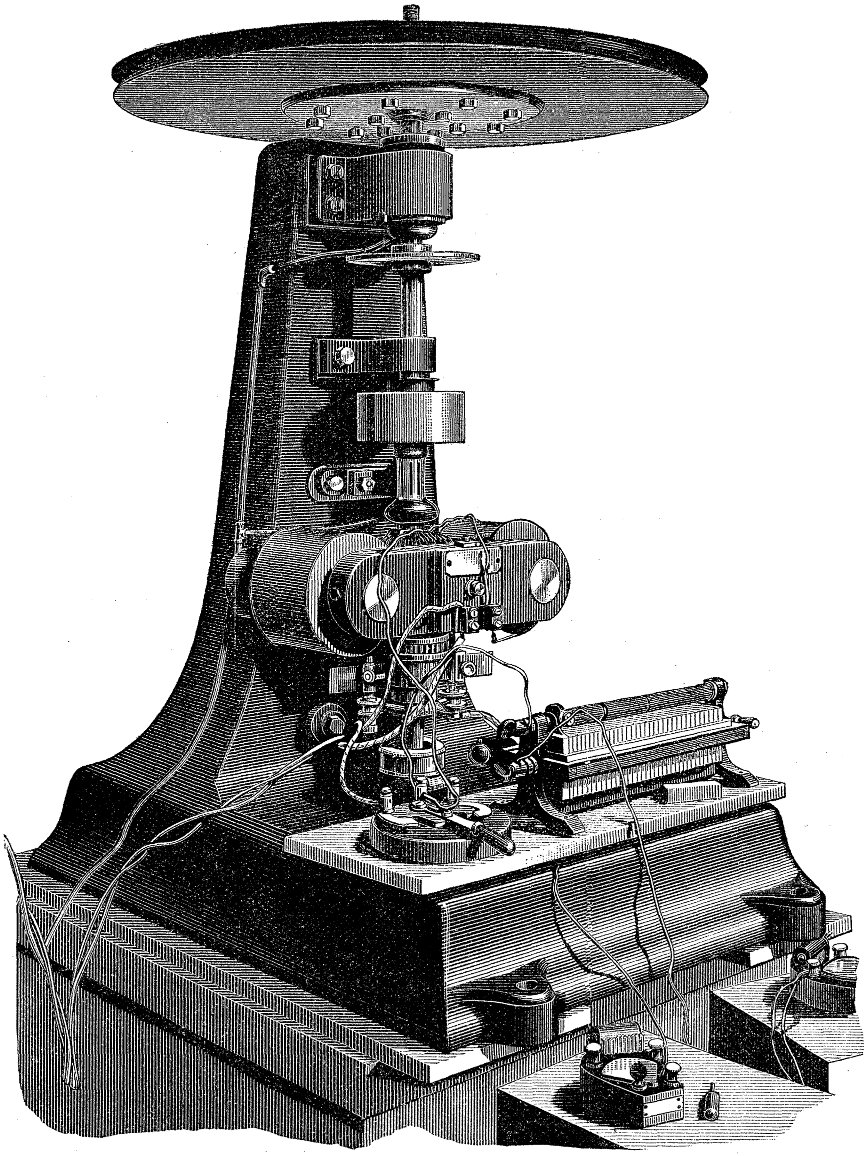
* The connexion between tenacity and maximum peripheral speed for a ring is $T = \rho v^2$; while, for a uniform disk with a small hole in it, EWING adapts GROSSMANN to show that it is $T = \frac{1}{4}(3 + \mu)\rho v^2$ where ρ is density of material, and μ is POISSON's ratio. See 'Nature,' vol. 43, pp. 462, 514, 534.

them out again. Also the bearings of the armature are, at present, hardly tight enough. Difficulties such as these have hitherto prevented the whirling machine from being quite satisfactory. At about 800 revolutions a minute a tremor begins. At high speeds it steadies itself, but at specific speeds the tremor re-occurs, and at some of the highest speeds is rather alarming. At others, however, the tee-totum action steadies the spin, and at these only could observations be made.

Fig. 9.



Whirling machine for ether experiment, with pair of steel disks, 1 yard in diameter. From a photograph taken during preliminary tests, before it was bolted down to stone pier. Full voltage is always supplied to the field magnets, variable resistance is in armature circuit only. The brass tube conducts away surplus oil. Drawings of the machine are given in Plate 32.

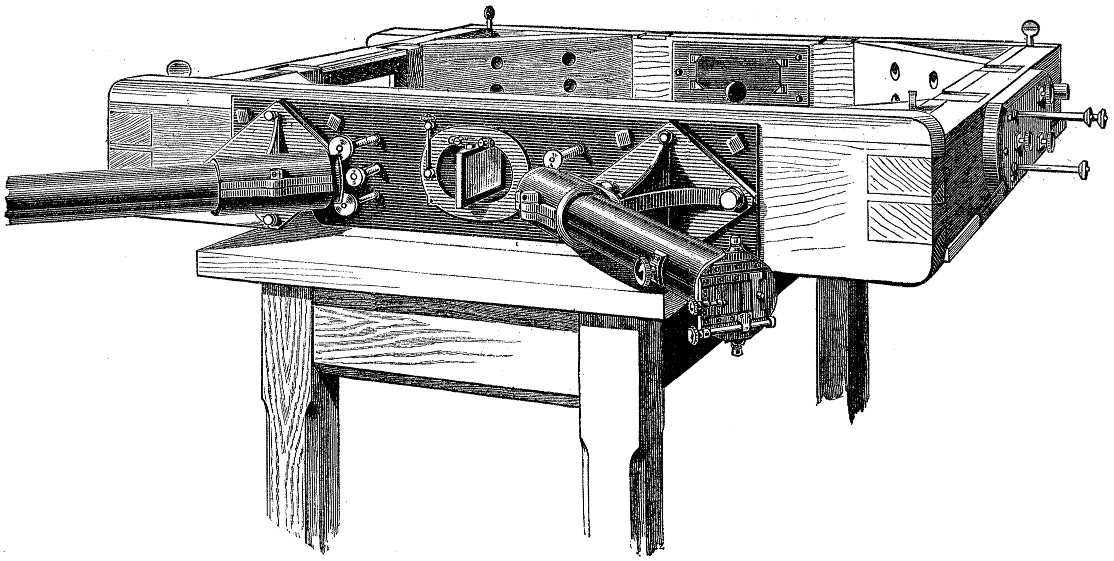
A perforated steel disk, pierced with circles of 3, 5, 9, 15, and 25 holes, and fixed on the spindle, gives the speed quite accurately by the musical note from an air jet through it, or even a card held against it. But the particular octave was occasionally checked by a wheel speed-counter; and recently one of the mercury speed-indicators of Messrs. NAPIER BROS. has been fitted on, and works very well for approximate readings.

The current is supplied usually from about twenty small storage cells, which I happened to have. The regulation is effected by one of ROUSSEAU'S very convenient carbon rheostats introduced into the armature circuit, the whole voltage of the battery being always supplied to the field magnet.

When the whirling machine arrived it was mounted on a stone pier in the middle of my laboratory, a pier built up from the sandstone rock beneath, and it was bolted down to timbers embracing the pier. The disks were then put on, a pulley gearing from the ceiling being convenient for raising and lowering them; and after some preliminary work the battery was applied. The field magnet took only 4 or 5 ampères from the 20 cells with gas-engine going; 10 ampères began to drive the machine slowly, and 30 ampères gave a speed of 800 revolutions up to 1400 revolutions a minute, though the last figure was not reached before the disks were encased. The volts actually on the terminals were from 20 to 30. The bearings, however, were not at this time quite easy, and less power now suffices. Thus the numerous spins at 1250 revolutions per minute taken during March, 1892, required, to maintain full speed, 385 watts, viz., 27·5 volts and 14 ampères.

35. The optical parts were the first to arrive (about June, 1891), and were the subject of much preliminary experiment. After a few alterations, such as planing down the base of the telescope-holders to a better angle, there was no particular difficulty in getting the light of an oxyhydrogen lantern to go three times round the frame and then enter the telescope. A dark room and strong light were useful for making this adjustment, for the course of the ray could then be tracked without difficulty; but after the adjustments were made, a paraffin lamp could be substituted for the lantern without too much enfeebling the image. On now inserting in its place the semi-transparent mirror, a second image made its appearance. Removing the lens of the collimator, the two images of the "slit," or square aperture usually employed, were seen very small and separate. By adjusting the semi-transparent mirror, which only moved the reflected image, the two were made to coincide. The collimating lens was then re-inserted, and the telescope focussed for infinity. Bands at once appeared. They were usually slant, but when best defined became horizontal. In these early trials vertical bands were only got with difficulty; they seemed to prefer being horizontal. Shaking the whole table on which the frame rested did not hurt them, but pressing gently on the wooden frame distorted them. It was easier to get them with light which had gone only once or twice round the frame, but there was no real difficulty with three times round. Introducing the wedge compensator and turning it

round caused them to shift. Introduction of ordinary sheet glass into the beam distorted them till they were like the water-mark on cloth.



View of the optical frame, supporting the mirrors, telescope, and collimator: detached from its position round the steel disks, where it is shown in fig. 11.

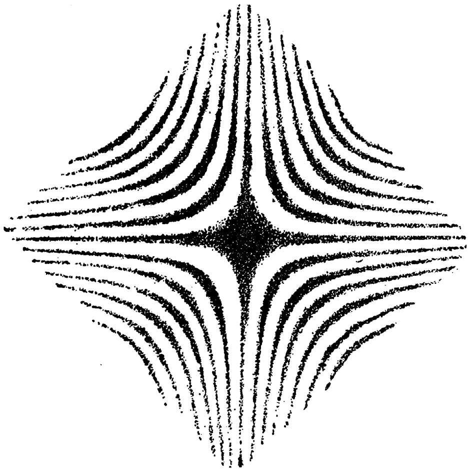
Later experience makes it absurdly easy to get the bands and to arrange that they shall be vertical, well defined, of any convenient width, and with the centre white band symmetrical among the coloured ones. Tilting the mirrors, pressing on the frame, or touching the semi-transparent plate, makes the coloured fringes move with a concertina-like motion towards or from the middle band, but the middle band is not easily shifted by anything. Altering the angle between the mirrors widens or narrows the bands, and when they get very wide a double system of hyperbolæ usually makes its appearance.

I conjecture that the horizontal bands first seen were the right or left branches of fig. 10, and that the widening or narrowing of the vertical bands may be expressed as an up or down motion of the figure.

Sometimes by pushing in the eye-piece of the telescope another set of bands could be seen horizontal, in exchange for the vertical bands which had, as it were, gone out of focus. These horizontal bands were more tremulous than the others and tilted readily. They occurred in the proper focal plane for infinity (assuming the mirrors to be accurately plane), and are probably what I saw first. The vertical bands do not become visible till the eye-piece is pulled several inches out. The probable meaning of this double set is that the mirrors have a very slight cylindrical curvature, the generating line of the cylinder being vertical. A pair of opposite mirrors, though very nearly parallel, will intersect in a line, and the bands will be parallel to this line. If this line is horizontal the bands will be horizontal, and for this direction the mirrors seem accurately plane. But for the vertical line of intersection the *length* of the mirrors is effective, and this seems to be slightly curved, so as to throw the focal plane of the vertical bands further out.

This does not account for the hyperbolæ. But with sodium light, systems of hyperbolæ, and also ellipses, can be seen in Brewster or Jamin plates, with different angles of setting (see LUMMER, 'Wiedemann's Annalen,' vol. 24, p. 417), and I expect the theory is somewhat similar.

Fig. 10.



One frequent appearance of the bands.* (But light and dark should be interchanged in the figure.)

36. The following observations were made recently as to the effect of various movements on each set of fringes :—

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[*] Cassini ovals are just as easy to get as these. The bands are always curves of the fourth degree and are, of course, sections of surfaces of constant retardation. The virtual sources may be taken as in fig. 7, viz., the nearly coincident images, C and C'.

Operation.	Effect on vertical bands.	Effect on horizontal bands.
Pressing lightly and intermittently downwards on corner of large wooden frame.	Concertina action; no shift of middle band.	Either rotation of whole set about a point, or shift up and down of whole set, or mere blurring.
Turning slightly one of the supporting screws of front or "fourth" mirror.	Concertina action and tilting of all bands, except the middle one; no shift of middle one	Shifting of entire set, and disturbance of bands.
Vertical screw supporting semi-transparent plate turned so as to tilt its plane about a horizontal axis, the axle being two inches to one side of the plate.	No concertina action, but a rapid blurring of the bands.	Concertina action, and tilting of all bands but the middle one; no shift of middle one.
Horizontal screws supporting semi-transparent plate turned so as to tilt it about a vertical axis, the axle being near one end of the plate.	Concertina action; no shift of middle band.	No concertina action, but a rapid blurring of the bands.

The effects of vertical and horizontal screws were thus complementary on the two sets, as was natural.

37. In order that the dark bands shall be really dark, a nice adjustment of the thickness of silver film on the semi-transparent plate is necessary. It can only be hit on by a sort of chance, for when once taken out of the silvering bath it is useless to put it back again if not sufficiently done.

The equality of transmission and reflexion at 45° is readily *tested* by two gas flames a yard or two apart, with the plate at the corner of a right-angled isosceles triangle at whose acute angles are the flames. On looking into the plate, one flame and the image of the other are seen side by side, and ought to be of equal brightness.

The film, however, by gradual tarnishing gradually becomes more transparent, so it is best to slightly overdo the plate, and let it age till right. Or an overdone film may be thinned down with potassic cyanide if wanted quickly. The unpermanence of these plates is a little troublesome; I should prefer to deposit a thin platinum film by "electrical evaporation," after the manner of Mr. CROOKES.

Certainly the bands can be *seen* when the images are very unequal, but they are on a background of spurious or non-interfering illumination, and for *measurement* it is desirable to get the bands exceedingly sharp.

The unsilvered side of the semi-transparent plate, of course, reflects some light, and gives another image. With a short course for the beam this useless image comes into the field, but it need not be superposed on the other unless the plate happens to be of very uniform thickness; and, if superposed, it can be got rid of by using a Nicol. With a very long course for the beam a Nicol is useless, for all the light seems polarized after so many reflexions; but fortunately, although the plate should be of good quality and uniformly thick, it is extremely unlikely for anything like superposition to occur after so long a journey, and often the useless image is not even in the field—never with a high power.

I must say that the satisfactory behaviour of the optical arrangements is due to the skill of Mr. ADAM HILGER in working glass to true planes. It is a difficult matter, for he says they are apt to change after being taken off the tool. He has several times tried to improve on the first set of four mirrors he sent me, but without success.

Those now sent usually have radii of curvature from three to eight hundred metres, and are not at all satisfactory, though their curvature is too small to detect with a spherometer.* Judging by their behaviour the original set must be very good. I expect they are of superior, or older glass. They are, of course, mounted so as not to strain them in the least.

38. To support the optical frame over the whirling machine, with the plane of the light between its two disks, a substantial wooden structure was erected, from brick piers coming up through the floor, entirely independent of any support from the whirling table or its stone pier. To this the frame was fixed, and it was supplied with a lid and floor, to box in the disks and make them easier to drive. The lid had a domed cavity for the top of the spindle; the floor had a hole edged with thin india-rubber sheeting to permit the spindle bearing to pass through air-tight without transmitting vibrations.

In order that the semi-transparent plate might not be affected by the blast from the whirling plates, a couple of optical glass windows were inserted to screen it completely. I feared lest the blast would have some effect upon the mirrors themselves, but they were substantially backed by thick brass plates bearing steadily against three accurate screws in a strong frame (see Plate 31), and I hoped it might not.

39. On the 21st of July, 1891, a first complete spin was taken. The bands being vertical, the cross wires were set on one of the dark ones, and the speed increased until a shift of three bands might have occurred. The shift actually observed was $1\frac{1}{2}$ band, and they recovered their old position very fairly when the motion ceased. Strongly suspecting this shift to be spurious, I had the brushes of the dynamo reversed, and later in the same day was able to take a reversed spin. The shift was approximately the same in amount and in the same direction. The centrifugal force of the blast evidently did affect the mirrors. Pressing their supporting plates by hand, a similar shift could be got: the screws did not hold them with absolute firmness, and it seemed as if the end held by only one screw yielded more than the end held by two, so as to produce a minute tilt.

To see if the pressure of the blast distorted the frame as a whole, or only tilted the mirrors, the box was made air-tight, like an organ chest, and air was pumped into it

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* I measure it by focussing a telescope on the image they give of an object at a considerable known distance.

by foot bellows: but this made very little difference to the bands, though the pressure was 6 centims. of water. A water gauge was also used to measure the centrifugal force of the blast: it was about 3 centims. of water, but was not easy to measure satisfactorily. Evidently the blast acted mainly on the mirrors. I contemplated a vacuous chamber, but shrank from some obvious difficulties, besides realizing that the residual air must give trouble anyhow. I decided to risk interposing transparent matter in the beam.

So some plate-glass protectors were cut and framed, one in front of each of the three mirrors, the fourth at present being left still exposed, partly because it seemed less likely to be affected by the wind, and partly because of the very narrow space available between it and the disks.

Seeing that each half-beam of light, in going three times round, has to go through each piece of plate-glass twice at every reflexion, or eighteen transmissions altogether, the intensity of the emergent light was feeble, and the bands were distorted by imperfections in the glass. Still they could be got clear, though curled, and the cross wires could be set on them. On 24th and 25th of July, spins at 1380 revolutions a minute were taken, and no shift so great as $\frac{1}{10}$th of a band could be detected; whereas, with the light only going twice round, there might have been a shift of $2\frac{1}{4}$ bands if the ether had been carried full tilt. Hence it would appear that the ether was not carried round with the disks by so much as $\frac{1}{20}$th of their speed.

The alignment of the shafts and other mechanical details were now attended to, so as to make possible higher speeds of rotation. Fresh arrangements for holding and adjusting the semi-transparent plate (the setting of which is a delicate matter) were made, and the micrometer was more firmly fitted by a much longer tube into the telescope. All sorts of steadyings were attended to. A pair of wires were laid across to the Walker Engineering Laboratory, where Professor HELE SHAW had a large Crompton dynamo; and so in December, 1891, my assistant, Mr. DAVIES, got the speed up to 2800 revolutions per minute.

43 amperes and 75 volts on the terminals (4·3 H.P.) gave 2500 revolutions a minute with lid of box off. With lid on, it went up to 2800 revolutions, but now the air in the box got quite hot, and a new difficulty arose from oil. It crawled up in an imperceptible film, notwithstanding the traps arranged for it, and, flying from the under surface of the lower disk, bespattered the mirrors and spoilt them.

More elaborate oil-catchers, to spray the oil off before it got to the disks, were therefore arranged, and the disks were kept whirling many days to get rid of all traces of the oil that had already soaked between the steel and the wrought-iron cheeks. Having cured the oil difficulty, fresh and improved cover glasses for the mirrors were got from Mr. HILGER, one extra large and extra thin one for the front mirror included, and these were carefully framed and placed in position. The light was then with difficulty, and considerable skill on the part of DAVIES, got three times round through all the glasses, and, during Christmas, 1891, spins were taken, but the highest speeds shook too much to give good observations. At 1800 revolutions, a shift of half a band occurred. Reversing the rotation, the shift was appreciably the same. It was probably due to pressure of the blast on the frame itself.

Hence, ordered a light wooden circular drum to be put inside the frame to catch the air pressure, and also to keep hot air from the mirrors if possible.

DAVIES took out the old wooden socket from the axle, and made a new boxwood one to connect the two shafts. We also now got the large boiler-plate protector for the eye end of the observing telescope to protrude through. It just shows at the back of fig. 11.

Resilvered the mirrors, and got the optical arrangements more perfect. Also ordered a much heavier mass of metal to whirl in subsequent experiments. For the present, however, we go on with the steel disks.

40. During February, 1892, the various preparations were made. The drum was a very satisfactory mahogany structure, octagonal outside and circular inside, with long slit windows glazed with optical glass to catch the blast. The drum was fitted inside the frame, with $\frac{1}{8}$th-inch clear space all round between the two. It was secured to the floor of the frame and joined with the lid so as to be fairly air-tight.

Still observed a shift, often of about $\frac{3}{4}$ths band. At speeds above 2000 the bands usually disappeared, from shaking. Removed the lid of the frame for the air to escape, and the shift was smaller. It was specially noticeable that it lagged. It took a little time to reach its maximum, and when the disks stopped the bands continued to recover for some time afterwards. It was just as if a solid had experienced a strain and sub-permanent set. All the effects were irreversible. Gradually made the drum more air-tight and supported it by wooden bars from above. A shift of $\frac{1}{4}$th band was still got at a speed of 1000, with the light three times round. Strains in the drum still seemed to be transmitted somehow to the frame.

Made a saw-cut all round the floor of the frame, so as to leave the floor on the drum instead of on the frame; also supplied a lid to the drum, and supported it by this lid from long wooden girders, as shown in fig. 11, keeping it and its supports everywhere out of contact with the frame or its supports, except the upright posts from the tables.* Studied the bands more particularly and observed the double hyperbolæ system. Found also the great advantage of the middle white

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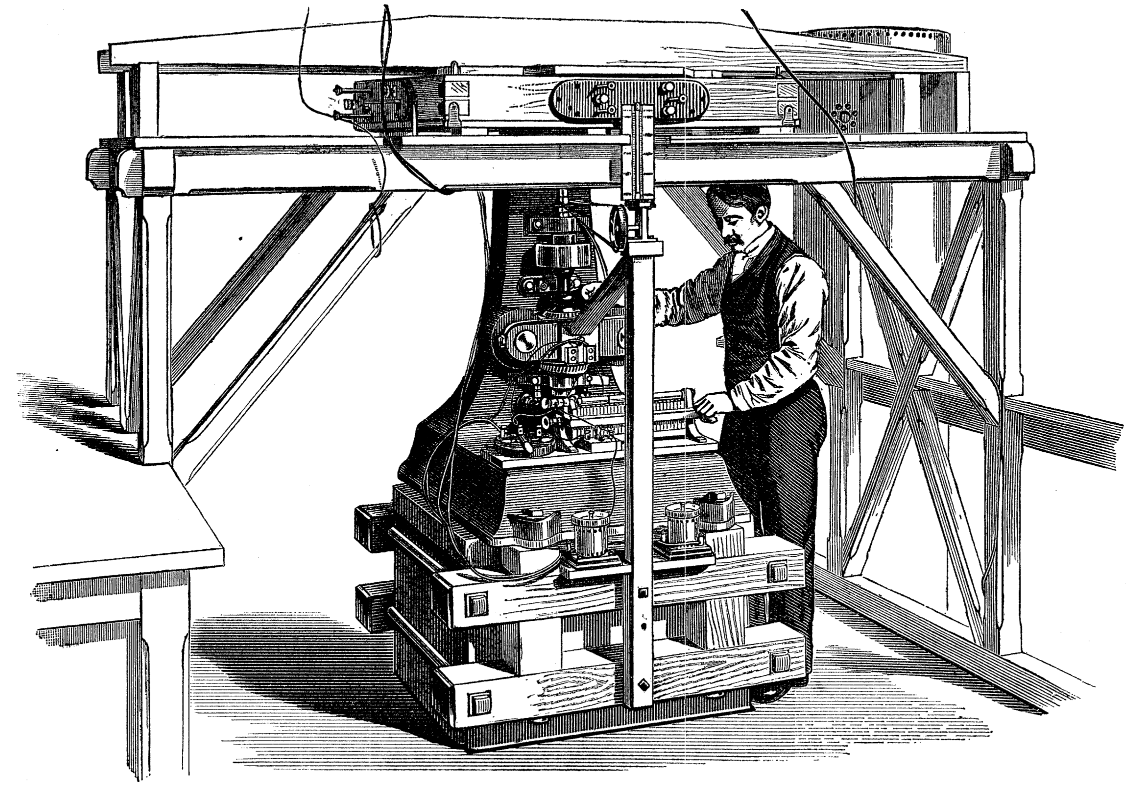
* Quite recently (March, 1892) I have taken a series of spins without the drum, and with no cover glasses on except one over the front mirror, something being there necessary to protect from wind the semi-transparent plate, which is of course extremely sensitive.

By this time, as described above, the bottom of the square frame had been removed, being attached to the drum instead, and we now found that this floor must have been the cause of most of the trouble. For without it the blast produced no effect on any of the bands, and neither shift or concertina action occurred, up to a speed of 800. No motion of the middle band could be detected, the light going twice round.

It follows that the blast did not take effect on the mirrors, nor directly on the frame, but that by exhausting the air near the middle of the box it must have sucked and bent the floor sufficiently to strain band; all others being readily moved by trivial strains. Always now set the vertical spider line in the centre of the middle white band and watched for its shift.

To see if the irreversible and lagging (very slight) shift still observed could be due to the action of hot air on the glass windows, I arranged to throw into the drum air heated by passing through a metal pipe in a gas flame. The hot air-stream flickered the side bands about, but did not alter the middle one.

Fig. 11.



General view of the whirling machine, and independent support of optical apparatus, in action. The speed indicator is seen on top of front post, ammeter and voltmeter on framework. The long upper girders support the glazed drum which encloses the disks and secures the mirrors from the blast. Telescope, &c., are at back, not seen; boiler-plate screen for observer just visible. Everything independent of floor, and no contact between anything on central altar with anything on gallows framework.

Quantitative Experiments.

41. I now began a series of actual readings, the plan adopted being to set the vertical line of the micrometer in the middle of the white band, and the χ of the micrometer on some definite colour of the first bright band on the left: the yellow

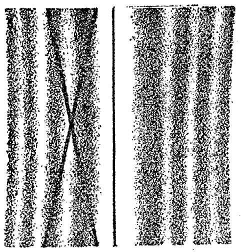
the frame and tilt the mirrors about a horizontal axis; this kind of tilt being well calculated to shift the vertical bands.

I tried also the horizontal set of bands without any floor to the frame (both with and without the drum), and perceived no shift in the middle one of these either, due to the motion of the disks.

being usually selected. Then read their positions, and started a spin. When spinning at a given speed, I reset the wires and read them afresh. Then stopped the motion, reset them, and read once more.

Lastly, shifted the vertical wire till it coincided with the X, and thus obtained the interval between them, or the width of one band corresponding to yellow light.

Fig. 12.



Appearance of the bands with the two micrometer wires set in position.

This process was quite satisfactory, but it is not worth while to quote all the readings; for it was noteworthy that up to 800 revolutions no perceptible shift of the middle band had occurred, but at that speed the first tremor set in, and although there was supposed to be no contact anywhere between machine and frame, yet it was transmitted somehow, through the earth at any rate, and sometimes produced an effect obviously spurious. Often the first spin of a set had the effect of shaking things into place, and subsequent spins were better. This was the case, for instance, on March 16, when the following readings were taken.

March 16. Bands very clear and sharp. The first spin was spoiled by a tremor which set in at 800 revolutions and fogged the bands while it lasted; when they re-appeared they had been shaken aside and broadened. No shift had occurred up to 800. It was afterwards found that the semi-transparent plate was not held quite tightly enough, and that it was affected by tremor with just the above effect. I give only the results of this first spin; not the details.

First Spin.

The width of a band was	when originally at rest	91 divisions.
	when in motion at 1250 revs. . .	170 ,,
	when at rest again	144 ,,

The middle band . . .	shifted .	37 divisions, to the right.
	returned .	22 ,,

This was very bad, but went on with another attempt.

Second Spin.—Direction of motion as before, viz., such as to assist the reflected half-beam.

This time the tremors which still occurred at about 800, 1000, and 1150 revolutions did not seem to produce much effect.

The following were the readings; each wire read on its own micrometer head.

	X-wire, set on yellow of first band to left.	Vertical wire, set in middle of middle band.
At rest	7	60
At 800, no shift of middle band		
At 1000, barely any		
At 1260 revs., set again and read	11·5	68
Stopped. Set again and read	11·5	64

To carry the vertical wire from its position 64 into coincidence with the X, required a motion of 154 divisions (viz., about one revolution and a half of its micrometer screw).

The experiment was satisfactory. Its result was

Width of a band of yellow light	at first	146 divisions.
	when moving . . .	158 ,,
	at last	154 ,,
Displacement of middle band. .	shift to right . . .	8 ,,
	return on stopping .	4 ,,

The highest estimate of this shift is, therefore, $\frac{8}{158}$ths, or say $\frac{1}{20}$th of a band.

The lowest estimate is $\frac{4}{154}$ths, or say $\frac{1}{38}$th of a band.

Then reversed the brushes and immediately took another satisfactory observation.

Third Spin.—Direction reversed, that is, so as to help the transmitted beam.

	X-wire set on yellow of 1st band to left.	Vertical wire set on centre of middle white band.
At rest.	9	69
No shift at 800 or 900. Tremor at 1000		
Set at 1250 and read	15·5	78
Stopped and set again	14	73

To carry the vertical wire from 73 up to the X at 14 needed 165 divisions. The result of this was

Width of band of yellow light	at first	156	divisions.
	when moving . .	$171\frac{1}{2}$	,,
	at last	165	,,
Displacement of middle band .	shift to right . .	9	,,
	return on stopping	5	,,

The highest estimate of this shift is $\frac{9}{172}$nds or say $\frac{1}{19}$th of a band.

The lowest estimate of this shift is $\frac{5}{165}$ths or say $\frac{1}{33}$rd of a band.

And these figures are within the limit of error certainly, the same as those given by the previous experiment with the spin reversed.

Taking this experiment alone, therefore, one may say that reversing the rotation of the disks, from 21 revolutions a second one way, to 21 the other way, does not affect the virtual path of the light between them by so much as the

$$\tfrac{1}{33} - \tfrac{1}{38} = \tfrac{1}{200}\text{th of a wave length,}$$

or even by so much as the

$$\tfrac{1}{19} - \tfrac{1}{20} = \tfrac{1}{400}\text{th of a wave length.}$$

Tested to see how many times the light was on this occasion going round the frame. Found that it was going only twice. Hence the length of path of each half beam was $2 \times 4 \times 2$ feet, since its path was approximately the periphery of a square two feet in the side. The whole path of the two beams is therefore 32 feet. A shift of $\frac{1}{200}$th of a wave length in this length of path means a fraction

$$\frac{\frac{1}{200} \times 6 \times 10^{-5}}{32 \times 30} = 3 \times 10^{-10}.$$

The perpendicular distance of the average light path from the axis of rotation was 1 foot, hence the effective speed of the disks either way was $2\pi \times 21$ feet per second, or say 260 feet a second altogether.

Compared with the speed of light this gives a fraction

$$\frac{260 \times 30}{3 \times 10^{10}} = 2 \,.\, 6 \times 10^{-7}.$$

Comparing these two fractions we may conclude that the meaning of the above experiment is that the ether is not carried forward by the spinning disks with so much as $\frac{1}{800}$th of their velocity.

Modes of Testing Number of Light Journeys.

42. There are many ways of making sure how many times round the frame the light is going. One is to look into the telescope with a low-power eye-piece, or none at all, before the semi-transparent plate is inserted and to give a wide opening to the collimator, often also removing its lens. Three or more images can then be seen in different parts of the large field, and it is easy to see which is the one near the centre. On now tipping the front mirror to and fro, the image which has been only once round (if visible) remains unaffected; the one which has been twice round moves; the three-times-round one moves twice as much; and so on.

By altering adjustments and passing the successive images in review, it is not difficult thus to work up to the high numbers. But on the specification of these it is well to have some check. The best check can be got, with everything in position and bands visible, by passing an opaque strip slowly in front of one of the mirrors and observing the eclipses at the telescope. If the semi-transparent plate is inserted in its holder, these eclipses occur in pairs, with shadows moving oppositely; if not, they occur singly. (A very narrow strip passed not quite close to the mirror may show each single shadow double. Too broad a strip will, of course, merge a pair of shadows into one.)

Three times round naturally gives two pair of eclipses on the front mirror, and three pair on each of the others.

Without the cover-glasses the light can be got many times round, but, when they are on, the faintness of the light which gets through all the surfaces makes it unwise to aim at more excursions, because the definition and visibility of the bands suffers in undue proportion. In fact, the superior sharpness of the twice-round bands perhaps more than compensates for the advantage of the half-as-long-again path belonging to the three-times-round set.

The cover-glasses are, of course, not set absolutely vertical, else the enormous number of reflexions from their surfaces would confuse everything.

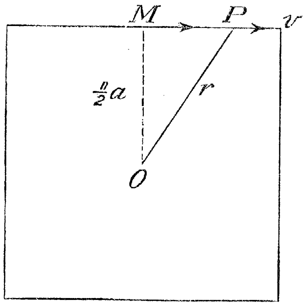
Another way of testing the order of the image in use is to tilt the front mirror so as to broaden the bands by a measured amount, and then to imitate or compensate this by tilting one of the other mirrors. The angle of tilt necessary with one of these will be $n/(n+1)$ of the tilt of the front one.

The three screws against which they each press all have fifty turns to the inch, and hence it is possible to apply this test whenever the eclipse method happens to be inconvenient. For instance, $\frac{1}{10}$th of a revolution of the single screw of the front mirror broadened the bands till one occupied nearly the whole field (with the high-power eye-piece that happened to be on). This meant $\frac{1}{500}$th of an inch advance of one end of a plane of 10 inches base, or an angular tilt of $\frac{1}{5000}$ radian, or 40 seconds of arc. Tilting the back mirror about 20″ gave the same effect, showing that there were twice as many reflexions on it as on the front one, and that the light was therefore going twice round.

General Arithmetic of a Shift.

43. It may be convenient for easy future reference to write down the meaning of any observed reversible shift under given circumstances.

An odd number of reflexions must be used if the light is to be sent many times round, hence triangles and pentagons are excluded. An even number of reflexions has the advantage that it makes the paths of the two half-beams *identical* and not merely parallel (*cf.* fig. 8) : but it does not seem readily feasible to get the light round more than once with an even number of reflexions. The square or hexagon are therefore the natural figures for the path of light. Take a square, whose side is a, as the mean path of the light. Then its perpendicular distance from the centre of rotation is $\frac{1}{2}a$; and it is the perpendicular distance which is important, for, since the velocity of light at any point P has to be resolved perpendicular to the radius vector, we get precisely the same tangential component everywhere as exists at the point M.



Let the disks revolve with angular velocity ω, and let the shift of the middle band be x band-widths of a particular wave-length λ. Then, if the light goes n times round each way, with velocity v,

$$\frac{x\lambda}{8na} = \frac{\frac{1}{2}ak\omega}{v}$$

where k is the fraction of the velocity of matter which is imparted to the ether between the disks, the quantity to be determined by observation of x.

Thus

$$\frac{k}{x} = \frac{\lambda v}{4n\omega a^2} = \frac{4 \times 10^5}{n\omega a^2} .$$

The limit of speed of a given material (see § 34) is given by something like

$$\mathrm{T} = \rho v^2 = \rho \left(\omega \frac{a}{\sqrt{2}} \right)^2 = \tfrac{1}{2}\, \rho\omega^2 a^2,$$

hence the limiting value of k/x, observable by this method, is

$$\frac{4 \times 10^5}{na} \sqrt{\left(\frac{\rho}{2\mathrm{T}}\right)} .$$

Taking 30-ton steel as the material, this gives as the smallest observable value of k/x,

$$\frac{4\times10^5}{na}\sqrt{\frac{7}{9\times10^9}}=\frac{11\text{ centims.}}{na}.$$

It is fairly easy to make sure that x is not greater than $\frac{1}{100}$th. In my apparatus a is 2 feet or 60 centims., and the light may go three times round with the cover-glasses on; hence the limiting determination of ether-drag that can be made with it thus arranged is $\frac{1}{1800}$th, unless the setting of the micrometer wires can be relied on more closely than to the $\frac{1}{100}$th of a band, or unless the steel will safely stand more than 30 tons to the square inch.

(Evidently the larger the square the better, and a large enough square might show even the earth's rotation effect, only it is difficult to see how to imitate the effect of stopping and reversing the rotation, at least with the unwieldy size of frame necessary.)

Testing for Cause of Slight Irreversible Shift.

44. The following experiments tend to show (and do distinctly show in my opinion) that such shift as is observed is independent of the width of the bands, and therefore is an absolute shift caused by shake or strain; very likely by a strain caused by a shake, for its effect often dies out slowly.

Spins on 18th March. Light going three times round.

First spin, in direction to help transmitted beam.

Micrometer wires set and moved as before (fig. 12), but only the results of the readings quoted.

Width of yellow band	at rest	114	divisions.
	while revolving 1200 times a minute	159	,,
	at rest again	184	,,

Shift of middle band .	shift to the right	6	,,
	return on stopping	5	,,

Found that the front mirror was too near the drum, probably touching it, hence the continuous widening of the band. Moved it back a bit and tightened up the nuts of the semi-transparent plate.

Started again with much narrower bands, and continued spinning and stopping alternately without intermission, all in the same direction.

It may be worth while to quote the actual readings of this set. The wires were set at each stoppage, and likewise during each spin while the speed was kept at 1260 revolutions per minute.

Second spin.

	X-wire.	Vertical wire.
Rest	38½	28
Spinning (at 1260) . . .	32	32½
Rest again	37	26
Spinning again	30	32½
Rest	38	25
Spinning	32½	32
Rest	44	27

To move the vertical wire into coincidence with the X (or *vice versâ*), needed 54 divisions of either micrometer head.

The result of this set may be thus tabulated

	Width of a yellow band.	Shift of the middle band.
Stationary	65½	
1260 revolutions	63½	4½ to right.
Stationary	62	6½ return.
1260 revolutions	61½	6½ to right.
Stationary	62	7½ return.
1260 revolutions	63½	7 to right.
Stationary	70	5 return.
	Average 64	Average 6

Practically, therefore, the absolute shift of the middle band was just the same as in the previous experiment, though the width of the bands was now only about one-third what it then was.

Now reversed the brushes and went on.

Third spin. Reversed. Direction to help reflected beam. Results :—

	Width of yellow band.	Shift of middle band.
Stationary	65	
1250 revolutions	$63\frac{1}{2}$	8 to right
Stationary	$63\frac{1}{2}$	7 return
1250 revolutions	63	$8\frac{1}{2}$ "
Stationary	$64\frac{1}{2}$	7 "
1250 revolutions	$61\frac{1}{2}$	$8\frac{1}{2}$ "
Stationary	63	$7\frac{1}{2}$ "
	Average 63·4	Average 7·7

The shift was a trifle greater than before, but so was the tremor. Another shift taken same day gave a shift of 9 and a return of 12. The effect of the tremor seemed increasing.

45. Since such shift as is observed is apparently independent of the width of the bands, it is manifestly well to reduce its apparent significance by having the bands very broad. It might be doubtful how far accuracy of setting could be accomplished with the spider line in the midst of a very broad band. To test this the following observations were made.

Accuracy of Setting of Micrometer Wire.

The bands were broadened, by tilting the back mirror a little, until it took two revolutions of a micrometer head to carry a wire from one to the next. The light was going three times round, and the cover glasses were on. The vertical wire was then carefully set in the centre of the middle band (it is always easy to tell the middle band, even without the colour of the others, by their concertina-like motion to and from it when the corner of the frame is pressed) and the X was set as near as possible on the yellow of the first band to the left. The position of this colour was not so well defined as when the light only went twice round, especially when the disks were spinning; neither was the middle band quite so clear then as when they were stationary. It is the setting of the vertical wire in the middle band that is really important. Readings of both micrometers being taken, the wires were displaced at random and then re-set and re-read. This was done several times. The following are the results of successive re-settings on the same bands.

	χ-wire. (Set and reset in yellow of first band.)	Vertical wire. (Set and reset in centre of middle band.)
Disks stationary	81½	20
	80¼	20
	84	21
	91	21
	83	22
	90	20½
	85	..
	Average 85	Average 20½

The width of this average band was 208½ divisions.

Hence when stationary the error of setting of the vertical wire *each time* did not attain $\frac{1}{100}$th of a band; and the probable error of the average of a series of settings is very small.

Same operation continued, with disks spinning.

	χ-wire.	Vertical wire.
Disks revolving 1260 times a minute. (Bands now not so clear. There was too much tremor)	85	26
	90	30
	95	32
	89	34
	89	26
	90	26
	85	27
Average	89	29

The width of this average band was 221 divisions.

Here the error of individual settings of the important wire amounts to $\frac{1}{40}$th of a band in one case, and the probable error is, say, $\frac{1}{70}$th. The error in the average of a series, however, would be less than $\frac{1}{100}$th, even under the above disadvantageous circumstances.

(The average shift caused by this spin was 8·5 divisions, or, say, $\frac{8\cdot5}{215} = \frac{1}{25}$th of a band.)

The effect of the tremor of the whirling machine on the optical part was becoming too conspicuous, and accordingly all the saw cuts and separations between drum and frame had to be examined. A slight contact was discovered.

46. On the 19th March a rather hasty pair of spins were taken, with the following results:—

First, with direction helping the reflected beam.

	X-wire.	Vertical wire.	
At rest	13	78	($\lambda = 169$)
1220 revolutions . . .	5	80	($\lambda = 163$)
At rest	5	81	($\lambda = 164$)

Reversed the brushes, and took a spin in the opposite direction.

Second, direction helping the transmitted beam.

	X-wire.	Vertical wire.	
At rest	13	81	($\lambda = 172$)
1220 revolutions . . .	7	85	($\lambda = 170$)
At rest	—	—	

Something happened that prevented the last readings from being taken. Notice that the yellow band seemed to have slowly returned to its old position in the interval between the two spins. These slow recoveries are frequent. Here the wave-length, or width of band, corresponded to 169 divisions at first, and the first shift was only two divisions; but the shift was spurious, for instead of returning, it went on, or at least stood still. The small shift (4 divisions) in the same direction observed on reversal was also no doubt spurious. I feel sure that I have never observed a genuine reversible shift of the middle band due to rotation.

Now the light was going three times round the frame, and if the ether had been carried round full speed with the disk there would have been a shift of $3\frac{1}{4}$ bands, or 550 divisions at each rotation, or over 1000 divisions in all, whereas not more than two divisions at the outside were seen, and they might readily be spurious.

In saying they were spurious, I mean not that a shift so small as this could not be observed, but that there was amply sufficient tremor to account for it

The entire absence of perceptible shift at about 800 revolutions, before any important tremor has occurred, is to me really the most conclusive fact; and I feel confident that either the ether between the disks is quite unaffected by their motion, or, if affected at all, that it is by something less than the thousandth part. At the same time, so far as rigorous proof is concerned, I should prefer to assert that 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.

Air Effect.

47. Of course, there must be an effect due to the air which is whirled with the disks. The index of refraction for air is 1·00029 for yellow light, and so $1 - 1/\mu^2 = {\cdot}00058$. The effect dependent on air amounts, therefore, to $\frac{1}{2000}$th of the speed of the disks, and is smaller than any shift which at present I have been able to observe.

I should like to push the method far enough to detect the air effect.*

A great number of other experiments suggest themselves. It may be objected that the disks were too far apart, or that insufficient time was given for the viscosity of the ether to assert itself, or that the disks had inadequate mass. This last objection is, perhaps, important, and I am proceeding to cope with it, and incidentally with the others, in some new experiments. A positive result could no doubt be obtained by rotating frame and observer instead of the disks.

The apparatus used in the above research was constructed by means of generous aid volunteered by a private friend, to whom I hereby express my grateful thanks.

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* [The effect of centrifugal force on the density of the air between the disks, and the influence of varying density, will be discussed with other matters in a future communication.—July, 1893.]

MORE DETAILED DISCUSSION OF ALLIED PROBLEMS.

Effect of a different Entire Medium upon Aberration.

48. If, instead of air or vacuum, the whole medium contemplated in fig. 4 is changed, the velocity of light is reduced from V to V/μ; wherefore the aberration will change too, unless the telescopic velocity be suitably reduced, or unless the medium is constrained to move in some compensatory manner. If the new medium is just as stationary as the old, and only the receiving telescope, or line of vision, moves, then the aberration angle will become μ times as great as before.

Minute Influence of Motion of Entire Medium on Aberration.

49. But if the medium, instead of being stationary, is drifting in some direction θ, with velocity v, then, perhaps, its motion may have some effect on the aberration. For, though a drifting medium cannot by itself cause aberration, yet it may modify it when otherwise produced. And this we shall find true in the second order of minutiæ. For, in the drifting medium, the rays differ from the wave-normals by the angle ϵ, such that

$$\sin \epsilon = \frac{v}{V} \sin \theta = \alpha \sin \theta,$$

and the velocity of light is

$$V_1 = V \cos \epsilon + v \cos \theta = V (\cos \epsilon + \alpha \cos \theta).$$

Hence an aberration caused by motion of telescope at speed u and angle ϕ, which would naturally be

$$\sin e = \frac{u}{V} \sin \phi = \beta \sin \phi$$

becomes

$$\sin e_1 = \frac{u}{V_1} \sin \phi = \sin e \left(\frac{\cos \epsilon - \alpha \cos \theta}{1 - \alpha^2}\right)$$
$$= \beta \sin \phi - \alpha\beta \sin \phi \cos \theta + \text{higher powers.}$$

The conditions most favourable for observing the second term are when the telescope moves across, and the ether moves along, the ray.

Unless the ethereal velocity near the earth were very great, much greater than the earth's orbital velocity, it would be hopeless to look for this term, as it would require the fixing of a star's position to the five-hundredth of a second, which must be considered quite impossible. The effect is connected with the slight alteration of focal length of the telescope (the difference between sin e and tan e), and may be regarded as a secondary sort of Doppler effect (§ 20).

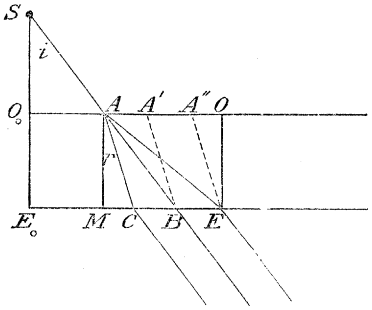
In practice, when the telescope is being carried along by the earth at angle ϕ to the light, it is customary to assume the medium stationary, in which case $\alpha = 0$ and the first term alone exists; or we may assume that the ether is more or less carried along by the earth, in which case $\theta = \phi$, and the second term attains its maximum value, $\frac{1}{2}\alpha\beta$, when the angle is 45°. But even if α is as great as β, the most favourable case, unless the ether has a proper motion of its own, this only means a discrepancy in a star's absolute position of $\cdot 001''$; so it is hopeless to look for a motion of the medium this way.

50. The experiment of filling a telescope tube with water as suggested by BOSCOVICH, and tried by AIRY, and more exactly by HOEK,[*] aims at a motion quite different from that above contemplated. It aims at moving a portion only of the ether in a partitioned-off region of space. It is easy to show that on FRESNEL'S theory no different aberrational effect can be thus observed than is observed with an air telescope; in fact, FRESNEL himself, in his original letter to ARAGO (§ 4) contemplated this experiment, and predicted its negative result, but it may be instructive to enter on its consideration in a geometrical fashion.

Effect of Motion combined with Change of a Portion of the Medium.

So long as the whole medium is changed or moved, we have seen what effect there is on aberration. Motion has no effect on it, it drifts the wave-normals but cannot affect the rays; increased μ without motion increases it. But if the ray has to pass through a bounding surface, and if the change or motion occurs only on one side of that surface, then circumstances are different.

Fig. 13.



If, for instance, the source S sends a ray SA, which would have gone to B, a change of medium may carry it to C, while a drift of the medium, carrying A to A′ and B to E, will slant the ray along AE (fig. 13).

It is, therefore, just possible for a drift to neutralize a refraction, and to let a ray enter a dense medium without bending. To this end the drift must equal CB or AA′, and the ray will then be straight.

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[*] 'Archives Néerlandaises' (1868), vol. 3 p. 180.

Now,

$$\frac{\mathrm{CB}}{\mathrm{AC}} = \frac{\sin(i-r)}{\cos i} = \tan i \cos r - \sin r = \frac{v_1}{\mathrm{V}/\mu},$$

where v_1 is the necessary velocity of drift; so

$$v_1 = \frac{\mathrm{V}}{\mu} \sin i \left(\frac{\cos r}{\cos i} - \frac{1}{\mu}\right).$$

The bending will be usual or unusual according as v_1 is less or greater than this. Ordinarily, of course it is far less.

51. But now suppose the obliquity i has been caused by the aberration necessary to bring a ray to a telescope (moving with velocity u) which except for motion would be looking straight at source; for instance, the telescope whose position was $\mathrm{O}_0\mathrm{E}_0$ when the light started from S and has moved to AM when light has reached A; in other words, let i be an *aberration angle;* then

$$\frac{\mathrm{MB}}{\mathrm{AB}} = \frac{u}{\mathrm{V}} = \sin i.$$

But, when a dense medium is inserted in the telescope, or say between the two planes, the time required for the shorter light journey, AC, is longer, and the telescope may get carried as far as OE, where

$$\frac{\mathrm{ME}}{\mathrm{AC}} = \frac{u}{\mathrm{V}/\mu}.$$

To bring the ray to the eye-piece at the right moment, C must drift to E, and the Huyghenian centre A to A″, in the same time. So if v_0 is the ethereal velocity able to undo the effect of the dense medium, and to leave the aberration what it was,

$$\frac{\mathrm{CE}}{\mathrm{AC}} = \frac{v_0}{\mathrm{V}/\mu}.$$

Hence

$$\frac{\mathrm{MC}}{\mathrm{AC}} = \frac{u - v_0}{\mathrm{V}/\mu} = \sin r = \frac{\sin i}{\mu} = \frac{u}{\mu\mathrm{V}}.$$

Therefore

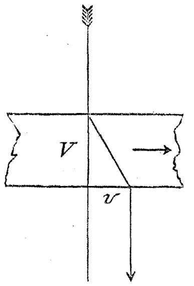
$$\mu^2 (u - v_0) = u$$

or

$$v_0 = \left(1 - \frac{1}{\mu^2}\right) u.$$

If this condition is satisfied, the observed aberration caused by any motion of a telescope will be independent of the medium inside it, although the course of a ray through it is really altered [viz., from AB to AE. The aberration angle in vacuo is MAB; in the medium is A″EO. The diminished velocity of light is exactly compensated by a virtually diminished speed of telescope with respect to the ether inside it; and so a steadily moving telescope sighted on a star can remain parallel to itself, with whatever medium it may be filled.—July, 1893.]

52 But, by moving a portion of medium *relatively to the observer*, say by spinning



a glass disk, and looking through it axially near its circumference, where its velocity is u, we are looking through a moving stratum of thickness z, and a parallel shift may in that case be experienced.

The angle is

$$\frac{v}{\mathrm{V}/\mu} = \frac{(1 - 1/\mu^2)\,u}{\mathrm{V}/\mu}\,,\ \text{or for glass about } \frac{\mu u}{2\mathrm{V}}\,;$$

and the shift is z times this angle, viz.,

$$s = \frac{\mu z u}{2\mathrm{V}}.$$

To give a shift of 1 micron = a thousandth of a millimetre = 10^{-4} centims., with a thickness $z = 10$ centims., would require a velocity $u = 2\mathrm{V}s/\mu z = (4 \times 10^{10} \times 10^{-4})/10 = 4 \times 10^5$ centims. per second = 4 kiloms. per second. My machine, at 3000 revolutions a minute, 50 a second, gives a peripheral speed of $50 \times 3 = 150$ metres per second, so the thickness of glass needed to give a shift of 1 micron is $z = 2\mathrm{V}s/\mu u = (4 \times 10^{10} \times 10^{-4})/15000 = 4000/15 = 3$ metres. To-and-fro reflexion may be used to diminish the required thickness.

More detailed discussion of Doppler Effects.

53. There is rather a nice point to be considered in connexion with change (2), § 9, viz., what the pitch, as perceived, really depends on. The coarse statement of examination candidates that it depends on wave-length, or on the frequency of vibration of the source, is of course not true; it depends on the frequency of disturbance reaching the receiver. This fact is suggested by listening or looking through a different medium, wherein the wave-length is quite different; though, indeed, it must be admitted that the medium in ear or eye cannot be changed. It is proved (for the case of sound at

least) by travelling towards the source, when the observed rise of pitch *must* be caused by increased frequency of arrival, the wave-length remaining unaltered.

But when we consider the effect observed in a spectroscope, there might possibly be a difference according as its essential part was a grating or a prism.

For it may be argued that a grating, consisting as it does of a set of apertures of fixed width, *must* deviate and disperse in proportion to wave-length; and hence that if a grating be supplied with crowded waves, either by holding it to an approaching source, or by immersing it in a denser medium, or in a medium flowing from it towards source, it must act as if coarser relatively to the waves, and so deviate and disperse them less.

But although this is a simple and plausible statement it is only half the truth. We had better examine the problem particularly (§ 56), for it is a curious mixture of Doppler effect and aberration, but at present it will suffice to say:

If θ is the deviation caused by a given grating for a given fixed source of frequency $1/\mathrm{T}$, so that

$$\frac{\mathrm{B}}{\mathrm{N}}\sin\theta = \mathrm{VT},$$

then if the source be approaching at the rate v, the time-interval between successive like phases is diminished in the proportion $(\mathrm{V} - v)/\mathrm{V}$; and accordingly

$$s\sin\theta' = \mathrm{V}(\mathrm{T} - t) = (\mathrm{V} - v)\,\mathrm{T}.$$

If it be the grating that is advancing towards a fixed source, the time interval between the arrival of like phases is likewise diminished, but in the ratio $\mathrm{V}/(\mathrm{V} + v)$; so that

$$s\sin\theta'' = \frac{\mathrm{V}^2}{\mathrm{V} + v}\,\mathrm{T}.$$

It is noteworthy that between θ' and θ'' there is a minute difference of the second order of aberration magnitude;

$$\theta'' - \theta' = \frac{v^2}{\mathrm{V}^2}\tan\theta.$$

If the grating be plunged into a different medium, the velocity of advance is changed, and

$$s\sin\theta''' = \frac{\mathrm{V}}{\mu}\,\mathrm{T}.$$

Lastly, if both source and grating are stationary, but the medium flowing from one to the other, or (what is the same thing) if source and grating are moving at the same pace, chasing each other through a stationary medium, the velocity and the wave-length are affected together, and

$$s\sin\theta'''' = (\mathrm{V} + v)\,\mathrm{T}' = (\mathrm{V} + v)\,\frac{\mathrm{V}}{\mathrm{V} + v}\,\mathrm{T} = s\sin\theta.$$

No Doppler effect, therefore, is produced by a stream of medium flowing past source and receiver if relatively fixed, *i.e.*, if they be moving together through a stationary medium.

We may in short summarise thus :—

> Source approaching shortens waves,
> Receiver approaching alters virtual velocity,
> Medium flowing alters wave-length and velocity together, in an exactly compensatory manner.

Steady Motion of Medium cannot cause any Doppler Effect.

54. Before abandoning the present consideration of the Doppler effect, let us distinctly assure ourselves of the important fact that no steady motion of the medium can change the pitch even infinitesimally, unless source and receiver are moving relatively to each other. Let source recede with velocity v, then the wave-lengths approaching us at their ordinary velocity V are longer than usual,

$$\lambda' = \frac{V + v}{V} \lambda.$$

Let receiver approach with same velocity, then it sweeps up per second a number of waves

$$n' = \frac{V + v}{\lambda'} = \frac{V}{\lambda} = n.$$

So, without relative motion of source and receiver, there is no Doppler effect, however small.

But the easiest way of assuring ourselves of the impotency of a steady wind on pitch, is to remember that such a wind cannot bring waves at a greater frequency than they are emitted from the source. Gusts will cause wailing, but a *steady* wind has no effect on pitch.

This is true also on a corpuscular theory, though for a slightly different reason. For, consider a machine-gun receding and firing at regular time intervals, it will be seen that, while the distance between the bullets is the same as if it were stationary, the speed with which they travel is $V - v$; and, if a target is chasing the gun at the same pace v, the number caught in a second will be

$$n' = \frac{(V - v) + v}{\lambda} = n.$$

Effect of Moving Medium on Doppler Effect.

55. But finally examine if wind has any effect on pitch when source and receiver are *not* moving at the same pace, *i.e.*, when a Doppler effect certainly exists. Let source recede with speed u, receiver advance with speed v, and let the medium flow from source towards receiver at speed w (or in any direction at speed $w \sec \theta$; the argument is the same):—

Then speed of wave is

$$V + w.$$

Length of each is

$$\lambda' = \frac{V + w + u}{V} \lambda = \frac{V + w + u}{n}.$$

The number caught per second is

$$n' = \frac{V + w + v}{\lambda'} = \frac{V + w + v}{V + w + u} n,$$

or

$$\frac{dn}{n} = \frac{u - v}{V + w + v}.$$

The medium velocity *does*, therefore, enter into the expression for the ordinary Doppler effect, though in a very subordinate manner, by affecting the velocity of light. It cannot *cause* the effect, but it can *modify* it when otherwise produced.

The simplest plan of detecting this effect of a moving medium, would be by some direct observation of the velocity of light itself; either simultaneously from stars in two opposite directions, or in a given direction at six months' interval. No terrestrial object must be used as source, because it would be moving at practically the same rate as receiver. Hence, for a six-months' experiment, the Jupiter's satellites method of determining the velocity of light would seem the best plan; and, if observations could be exact enough, one could thus get

$$\frac{V - (w + v)}{V + (w + v)},$$

and so determine w the unknown speed of the ether past the solar system.

A simultaneous aberrational method, such as comparing the aberration of two stars 180° apart, will not work well; for if they be at 90° to the apex of earth's motion there will be no discrepancy, while if one be toward that apex and the other away from it there will be no aberration.