XV. *Aberration Problems.—A Discussion concerning the Motion of the Ether near the Earth, and concerning the Connexion between Ether and Gross Matter; with some new Experiments.*

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1. THE nature of the connexion between ether and gross matter is one of the most striking physical problems which now appear ripe for solution, and as a preliminary to the undertaking of fresh experiments I propose to review the subject in order to realize our present position in connexion with it.

The subject may be attacked either optically or electrically. It first prominently presented itself historically in connexion with the earth's motion through space and the finite velocity of light; and it will be convenient to consider the matter first on this side, and to realize precisely what ought to be expected to happen on any simple hypothesis concerning the ether; working it out, however, in most cases with accuracy,* and by no means ignoring as negligible small quantities of the second order (hundred-millionths), which MICHELSON has practically shown to be nowadays within the limits of highly-refined observation.

Necessary Hypotheses or Postulates.

2. There are one or two hypotheses regarding the ether so elementally obvious that they may be regarded as almost axiomatic, such as the following :—

(i.) *In interstellar space the ether is free;* that is, its properties in no way depend on the existence or motion of gross matter. It may, therefore, be called at rest. Whether it is in absolute rest or not appears to be a question which can hardly be put into an intelligible shape. If it be moving relatively to itself, we have in those regions no obvious means of ascertaining the fact. But just as it is natural to assume that its properties in free space are uniform, so it is natural to assume that its motion there, whatever it is, is perfectly uniform, and it may be *defined* as absolute rest. When I speak of the ether anywhere as "stationary," I mean stationary with respect

* Whenever equations are approximate only, the symbol $\approx$ is used instead of the symbol $=$.

to interstellar or free ether. When I speak of the ether anywhere as "free," I mean that its properties are identical with the interstellar ether enormously distant from all gross matter. And this is the condition of ordinary space, except for the presence of meteoric particles, whose influence, if any, we at present legitimately ignore.

The only hypothesis which at first sight *appears* to assume infinitely distant ether to be affected by the motion of, say, the earth, is that of Sir GEORGE STOKES, in 1845, where an irrotational motion, zero with respect to the earth, was postulated for it. But he must have seen some way in which so impossible an assumption could be avoided; and the question how far any kind of irrotational motion can be conceived of as allowing rest at infinity, and yet no slip at the earth's surface, will be discussed later, § 31.

(ii.) *Inside material bodies the ether is modified.*—We learn this by direct experiment and observation.

For transparent bodies we learn it by optical experiments, which proves that light travels more slowly through their modified ether than it does in free ether; while at the same time there is no doubt but that the ether inter-penetrates them, because material substance itself is wholly incompetent to transmit anything possessing the properties and the speed of radiation.

In metallic bodies we find great opacity combined with anomalous dispersion and other complex effects. In them evidently the ether is intensely modified, if it exists at all.

I shall call the ether inside gross matter of any kind "modified ether," but as to the particular way it is modified I make no assumption. [Electrostatic experiments *suggest* that inside transparent bodies, something which may be called its "virtual elasticity" is diminished. Magnetic experiments suggest that inside several opaque substances it is loaded, so as to increase what may be called its "virtual density;" and there is a temptation to identify $4\pi/K$ with the one, and $4\pi\mu$ with the other, of these two ethereal constants. Further, electrokinetic experiments suggest that inside metallic conductors the ether has a virtual viscosity, whereby its motion through matter is resisted precisely as the first power of the velocity. But none of these doubtful hypotheses shall here be obtruded.]

Rate of Travel of "Modified" Ether.

3. Defined in this way it is quite obvious that "modified ether" travels at the same steady pace as its material encasement. For lift a lump of glass or of copper from one side of a table to the other, the modified ether which was in one place is now in another, and has necessarily accompanied the material body. If the modification of ether by matter requires time, there would be some lag during epochs of acceleration; but during steady velocity there would even so be no difference in speed between modified ether and matter, only a slight lag in space.

Ignoring this possible finite speed of affection of ether by matter, unless circumstances make us revert to it, the question faces us, what is meant by the travelling of the modified ether?

It is not a question easy to state without some looseness of language, but we may ask:

(*a*) Does it mean that the identical stuff inside the matter travels from one place to the other? If so, the free ether which it has displaced must stream back round the body in the same way as a material fluid would have to do.

(*b*) Or does it mean that no ether travels at all, that the mere presence of the matter causes the modification wherever it is, so that it is only the modification or affection which travels? If so, the ether abandoned by the matter becomes free *in situ,* while the ether encroached on by the matter becomes modified *in situ,* and there is no question as to its motion.

On hypothesis (*b*) the whole ether is fixed and imperturbable by the motion of matter. The portion enshrouded by matter at any instant has properties differing from those of free ether, but the modification is only connected with the matter causing it in the same sort of way as a shadow is connected with the object casting it.

Of the two hypotheses, there can be no question but that the second is the simpler and considered as a hypothesis is preferable, but we must enquire whether it is competent to sustain the weight of all known facts.

FRESNEL'S *Hypothesis.*

4. It is notorious that the hypothesis at present holding the field is not exactly either of these, but is some form of the bold and picturesque idea of FRESNEL; viz., that in addition to the free and undisturbed ether of space existing equally everywhere and flowing through the pores of gross matter, there is an extra quantity of bound ether fixed to the matter and travelling with it; this additional quantity being $(1 - 1/n^2)^{\text{th}}$ of the whole.

This idea of FRESNEL'S seems, at first sight, essentially to involve the condensation of ether by matter, so that its density inside bodies is n^2; for the fixed ether is superposed upon the normal ether of space. (Certainly the converse is true; viz., that extra ethereal density involves FRESNEL'S law, as will be shortly shown.)

Now the facts of gravitation, and many electrostatic experiments, suggest that the ether is practically incompressible; hence the notion of any actual increase of density inside gross matter is repugnant.

FRESNEL, however, himself pointed out, in a subsequently written postscript to his original letter to ARAGO* promulgating his famous hypothesis, that the extra density need not be taken too literally. (As this postscript seems rather to have been overlooked it may be worth while to quote it).

"*Note additionnelle à la lettre de* M. FRESNEL *à* M. ARAGO, *insérée dans le dernier Cahier des Annales.*

En calculant la réfraction de la lumière dans un prisme entraîné par le mouvement terrestre, j'ai supposé, pour simplifier les raisonnements, que la différence entre les vitesses de la lumière dans le prisme

* 'Ann de Chim. et de Phys.' (2), vol. 9, p. 56.

et dans l'éther environnant provenait uniquement d'une différence de densité, l'élasticité étant la même de part et d'autre ; mais il est très possible que les deux milieux diffèrent en élasticité comme en densité. On conçoit même que l'élasticité d'un corps solide peut varier avec le sens suivant lequel on le considère ; et c'est très probablement ce qui occasionne la double réfraction, comme l'a observé le Dr. YOUNG. Mais quelle que soit l'hypothèse que l'on fasse sur les causes du ralentissement de la marche de lumière dans les corps transparents, on peut toujours, pour résoudre le problème qui m'était proposé, substituer par la pensée, au milieu réel du prisme, un fluide élastique en équilibre de tension avec l'éther environnant, et d'une densité telle que la vitesse de la lumière soit précisément la même dans ce fluide et dans le prisme supposés en repos ; cette égalité devra subsister encore dans les deux milieux entraînés par le mouvement terrestre : or, telles sont les bases sur lesquelles repose mon calcul."—('Ann. de Chim. et de Phys.,' 1818, t. 9, p. 128 or 286.)

And Mr. GLAZEBROOK ('Phil. Mag.,' December, 1888) shows that in the interaction of ether and matter, a term depending on relative acceleration is sufficient to sustain the results achieved by FRESNEL's hypothesis. In other words, that a *virtual* density, or loading of the ether by matter, is quite enough without true condensation.

It is, however, still appropriate to speak of the extra ethereal density inside matter; meaning the coefficient of this acceleration term.

5. A plausible mode of exhibiting the naturalness of FRESNEL'S law is as follows :—

The constant which determines the speed of electromagnetic waves through any medium is μK ; by the differential equation to wave motion.

In a dense body the value of this constant is μ'K$'$.

Shift a lump of this body from one place to another. Its constant μ'K$'$ has been shifted in position too, but the ordinary space-value μK remains behind ; so the resultant shift of the property determining the velocity of light (the effective medium) is a fraction $(\mu'\text{K}' - \mu\text{K})/\mu'\text{K}'$, of the shift of the body.

So, if the lump moves with velocity u, the property of it concerned with the velocity of light shifts with velocity $(\mu'\text{K}' - \mu\text{K})/(\mu'\text{K}').u$; that is with speed $(1 - 1/n^2)\,u$.

And, as in all probability the velocity of wave motion *relative to its medium* is unaltered, this may be taken as the extra speed of the light caused by the motion of the matter.

6. It is here assumed that the medium simply carries the wave motion with it as air carries sound. It is not customary to doubt that wave motion must be affected by any motion of its medium in that simple manner. But a singular investigation by Professor J. J. THOMSON ('Phil. Mag.,' April, 1880) seems to show that on electromagnetic principles the speed of ether waves is affected with only *half* the velocity of the medium conveying them.

This extraordinary result is not at present positively contradicted by the FIZEAU experiment, even as repeated by MICHELSON, because the value of $1 - 1/n^2$ for water is not sufficiently different from $\frac{1}{2}$ to afford a certain criterion ; and water is the only substance for which a positive result has as yet been obtained. Certainly the negative result obtained for *air* by both FIZEAU and MICHELSON is in accord with FRESNEL's theory and not in accord with J. J. THOMSON's. But a definition of what is meant by "moving medium" seems necessary before we can adequately test the question whether electromagnetic waves in it move with it or lag behind.

I suppose that it must be desirable to examine substances other than water, especially those with a much higher refractive index. I hope to do this, though it may be noted that the value of n which would make FRESNEL's and THOMSON's theories *exactly* agree, is 1·4142, and that the available range of refractive indices of liquids and solids affords but a narrow margin for discrimination between the two hypotheses.

The balance of evidence is at present strongly in favour of FRESNEL's hypothesis, and I propose ordinarily to assume its truth. I cannot, indeed, understand the possibility of THOMSON's theory, though I detect no flaw in his work, for it seems to require a distinction between the case of source or receiver moving through a medium, and the case of medium flowing past source or receiver; that is, it seems to demand a knowledge of *absolute* velocity.

FRESNEL's *Law*.

7. The statement of FRESNEL's law can be thrown almost into the form of hypothesis (b), § 3, and at the same time its apparent licence of language about "free" and "bound" ether can be lessened, by supposing that the "modification" induced by the encroachment of matter on the ether is really a condensation, in the ratio $1 : n^2$; no motion in the ether other than what is necessarily involved in that act being postulated. On this method of statement the ether outside a moving body is absolutely stationary, but, as the body advances, ether is continually condensing in front, and, as it were, evaporating behind, while inside it is streaming through the body in its condensed condition at a pace such that what is equivalent to the normal quantity of ether in space may remain absolutely stationary. To this end its speed relatively to the body must be v/n^2, and accordingly its speed in space must be $v(1 - 1/n^2)$.

Thus, instead of saying that a portion of the ether is moving with the full velocity of the body while the rest is stationary, it is probably preferable to say that the whole internal ether is moving with a fraction of the velocity of the body.

One or other form of statement is absolutely involved in the Fresnellian idea of increased ethereal density, as may be rigorously shown (*vide* Lord RAYLEIGH, 'Nature,' March, 1892; *vide* also EISENLOHR), thus :—

Consider a slab moving forward flatways with velocity v, let its internal ethereal density be n^2, and let the external ether, of density 1, be stationary. Let the speed of the internal ether *through space* be xv, and consider that the amount of ether enclosed between two planes moving with the slab, one outside and one inside, must be constant; it follows at once that

$$v = n^2 (v - xv)$$

whence

$$x = 1 - \frac{1}{n^2}.$$

Now whatever may be the inner meaning of this statement concerning the velocity of the internal ether, it certainly agrees with, and is at once suggested by, the fact, thoroughly established by both negative and positive experiments, that light travels down a running stream of matter at a pace

$$\frac{V}{n} + v\left(1 - \frac{1}{n^2}\right).$$

The negative experiments supporting this are such as the achromatic prism experiment suggested and tried by ARAGO, repeated more elaborately by MAXWELL and by MASCART; the water-telescope observations suggested by BOSCOVICH, tried by AIRY and by HOEK; interference experiments of BABINET and of HOEK; and several other experiments by MASCART. The positive experiment establishing it is the very beautiful and well-known one of FIZEAU, now repeated and confirmed beyond the reach of any but quantitative cavil by MICHELSON.*

Whether any ether is moved by moving matter may still be an open question, but that the speed of light is affected in a fairly ascertained way by the motion of transparent matter through which it is passing, is certain.

8. But the specific motion of the internal ether is not the whole of FRESNEL's hypothesis; there is the fixity of the external ether to be verified too. And that has not yet been done. In fact, one important experiment, to be discussed later on, throws grave doubt upon it, at least for large moving bodies like the earth.

But unless the fixity of external ether be granted, our argument from density concerning the value of the internal velocity breaks down. Consider again two planes moving with a slab of matter, one inside and one outside the mass, and let the space motion of the outside ether at the position of outside plane be affected by the motion of the slab to the extent yv, then all we can say is that

$$v - yv = n^2(v - xv),$$

or,

$$x = 1 - \frac{1-y}{n^2},$$

wherefore it is possible for x and y to be unity together.

We may take it, however, that the quantitative accuracy of the FIZEAU experiment renders anything of this sort very unlikely, and that we are bound to suppose the ether immediately outside moving matter to be stationary, *i.e.*, to be completely unaffected by its motion, unless we are directly forced by facts to admit the contrary.

* 'Amer. J. Sci.,' vol. 31, p. 377.

The two parts of FRESNEL'S law, the motion of internal ether, and the fixity of external ether, can and ought to be verified separately. The FIZEAU experiment has verified the one. I propose to attempt the other. To this end I am passing a beam of light, split into two equal halves, very near a rapidly rotating disk (in fact between a pair of rotating disks clamped together), so that one half the light travels with the mechanical motion and the other half travels against it. The two half beams, after several journeys round and round, are united, with interference effects, and the observation consists in watching the system of bands for any shift caused by the motion. For description of this experiment see §§ 33–47 below.

Phenomena Resulting from Motion of Source, Receiver, or Medium.

9. The phenomena which can be appealed to as evidence of a state of motion, and which necessarily result from that motion if of a suitable kind, are four, viz.:—

(1) Changes or apparent changes in direction of ray, as observed by telescope with cross-wires; the change commonly called "aberration" proper.

(2) Changes or apparent changes in frequency of vibration, as observed by the pitch or colour appreciated by an observer, or by the shifted position of lines in a spectroscope; a change which may be referred to as the Doppler effect.

(3) Changes or apparent changes in the time taken over a fixed journey, as observed by the relative lag in phase between two portions of a split beam and the consequent shift of interference fringes when they are re-united.

(4) Changes or apparent changes in the intensity of radiation in different directions, as observed by the amount of energy received by a given area exposed normal to the rays at a given distance from a source, but having different aspects with respect to the line of motion.

Or, briefly summarizing them, the possible phenomena caused by motion are changes in direction, in period, in phase, and in amplitude.

Apparent Direction as Affected by Motion in General.

10. Consider the subject first from a corpuscular or projectile point of view, first ignoring the medium. A gun travelling broadside on must be aimed behind the object, and its shot will travel in a skew direction (keeping always straight in front of the muzzle, but not travelling along the axis of the gun) with a velocity compounded of the speed of projection and the speed of the gun. The apparent position of the source, as recognized through a hole in the target, will therefore be its true position at time of firing, but not its position at time of hit.

Whether we choose to call this an aberration or not is a matter of nomenclature merely.

If the gun is fixed, with the target moving across the line of fire, the gun must be aimed in front of the object. The shot will go straight along the barrel produced, but the hole in the target will indicate a gun in front of its true position; this error being aberration proper.

Fig. 1.

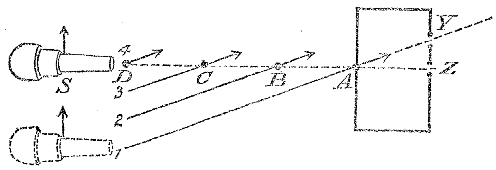


Diagram of shot fired from a moving cannon; piercing a target, at Y if stationary, at Z if moving at same pace as gun. ABCD is the locus of successive shots, but is not the line of fire.

If both gun and target are travelling at the same speed everything occurs as if they were at rest, unless a stagnant medium has to be taken into account. Relative motion of the medium causes windage, as is well known.

Since motion of the medium causes a shift of the line of fire, it may be expected to produce a miss, but this is not a true aberration, it only appears to be such because of the fire being limited to one line; suppose instead of a single gun a broadside of guns or a number of guns firing from a turret, then the effect of a cross-wind is, indeed, to displace all the shots, but not to prevent the target being hit by one which would otherwise have missed it, and the hole in the target will indicate the position of the gun really firing the shot.* Hence, even on a corpuscular theory, a wind across the line joining source and receiver, will not cause any effective aberration. Neither can a steady tail wind deliver a stream of bullets from a machine-gun more frequently than they are emitted.

If guns are fired from a revolving turret, the paths of the shot will not be radial, but will be skewed by an amount depending upon the peripheral velocity.

Watching the beams of a revolving lighthouse, tracking their way to a distance and brandished rapidly round, it is not at once quite evident whether the shape of those beams is not a spiral of enormous pitch (see below). We see, however, that on the corpuscular view the paths will be straight, though not radiating from the precise centre; for instance, the rays from the Sun, whose peripheral velocity is nearly 5000 miles an hour, would if regarded as projectiles, be inclined to their radius at an angle of $\frac{5}{186 \times 3600}$ radian, or about $1\frac{1}{2}$ seconds of arc; and the Sun's centre would be, apparently displaced through a fraction of this angle, equal to Sun's radius/Sun's distance; *i.e.*, through about the $\frac{1}{150}$th part of a second.

11. But now, proceeding to look at the matter from the point of view of waves, there are many differences; principally depending on the fact that there is no question of initial velocity of projection about a wave: it crawls through the medium,

* As these projectile examples are only used for illustration, I simplify matters artificially by omitting all curvatures of path. The subject of aberration in general is illustrated more fully in a Royal Institution Lecture, 'Proc. R. I.,' April 1, 1892; also reported in 'Nature,' vol. 46, p. 497.

self-propelled, at its own definite velocity. No aberrational effect can be produced by any cause which does not act on a wave-front for a finite time.

Fig. 2.

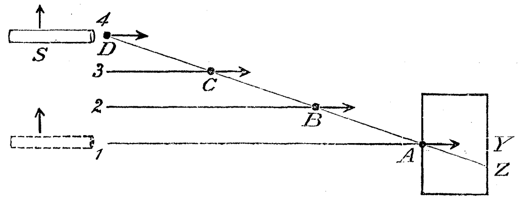


Diagram of disturbances or waves emitted without momentum from a moving source; leaving target or telescope, at Y if stationary, at Z if moving. The line ABCD is the locus of successive disturbances, but is not the ray or real path. The diagram may also be taken to represent the effect of a cross stream of medium, with source stationary.

Hence waves emitted by a revolving source advance just as they would if it were stationary; any peculiarity on the surface, say a Sun spot, is depicted in a precisely radial direction, and there will be no displacement of the Sun's centre. So also with light from a flying star: the star will be seen in its position at time of emission, just as it is seen in the physical state corresponding to that instant, not to the instant of vision.

Fig. 3.

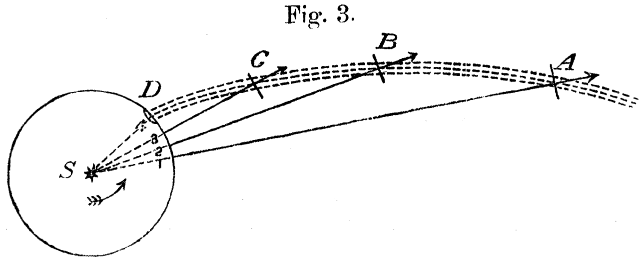


Diagram of parallel beams emitted from a revolving lighthouse. ABCD is the apparent beam, but AS, BS, &c., are the lines of vision or true rays.

As to a beam from a revolving lighthouse, it is not the path of a labelled disturbance, or true ray, which displays itself by illuminating dust particles, but it is the locus of successive disturbances sent out from a given moving point; so if the source has revolved through an angle θ while the light travels a distance r, $\theta = (\omega/V)\, r$, and their shape *is* a spiral of ARCHIMEDES as suspected above; though the direction of vision is not tangential to them, but is truly radial as already stated.

The analogy between rays of light and lines of force is fairly close, and just as it is convenient to say that a rotating source revolves its rays, so it is convenient to say that a rotating magnet revolves its lines of force. The induction phenomena obtained from a magnet spinning on its own axis are a sufficient justification of this statement.*

In an old note-book of date 1876, I find a suggestion for measuring the speed of magnetic propagation, by rotating a long bar magnet on its axis and observing its action on a distant magnetic needle;

* See also Mr. TOLVER PRESTON, 'Phil. Mag.,' February and March, 1885.

the idea being that with a finite speed of propagation the lines of force would lag, and thereby acquire a curvature out of the magnet's meridian; so that a distant needle instead of pointing straight at the magnet would be tangential to these lines, and would therefore be slightly deflected during the spin.

We now see, however, that no such aberrational effect is to be expected, except on a corpuscular view of magnetic propagation.

Concerning the effect of motion of other kinds, certain things are experimentally known; *e.g.*, motion of the receiver is known to cause aberration, however the fact be precisely accounted for; and motion of the medium alone is known not to cause aberration of any perceptible magnitude, else would terrestrial surveying operations be inaccurate. But no experimental data as yet obtained are evidence concerning small quantities of the second order, and it will be well to examine critically and geometrically the whole subject of wave motion from a moving point to a moving telescope through a uniformly moving medium, all the velocities being possibly different in magnitude and direction. So far as steady and uniform motion is concerned this may be considered the most general case.

Convenience of attributing Relative Motion to Medium.

12. Before considering separately the phenomena mentioned in § 9, it may be convenient to consider what it is which must be in motion in order to produce one or other of them. And, first, which of them a motion of the medium alone causes.

Nothing can be more certain than that relative motion is all we are concerned with, so that whether a source travels through a medium, or the medium drifts past the source, comes to precisely the same thing. Sometimes one mode of expression is convenient, sometimes the other. It may be most natural to contemplate the medium as stationary, and to throw all motion on source and receiver, but I find that it is often very simple and helpful to invert this order, and to think of the ether of space as drifting past the earth, or other body, supposed stationary.

We shall not invariably use this device, but whenever a number of things—source, mirrors, telescope, and observer—have to be thought of as moving all precisely alike through the ether, it is simpler to think of the ether as streaming past them.

Case of Fixed Source in Moving Medium.

13. Consider now a fixed point-source in a uniformly moving medium.

Spherical wave-fronts are thrown off and immediately begin to drift, so that their centres get displaced a distance, vt, while their radii enlarge by an amount, Vt; and the distance through space which a disturbance has by that time travelled in the direction θ will be compounded of these two distances, and will be inclined to the radius, or direction of travel if all were stationary, by an angle ϵ, which may be called the aberration angle. The velocity with which light journeys over the radius vector, r, is

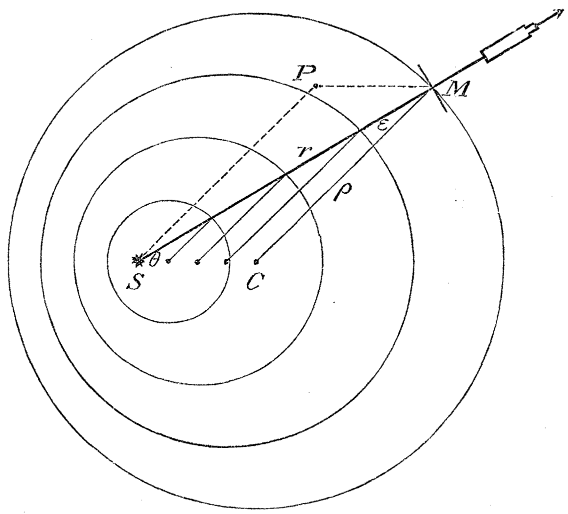
$$V \cos \epsilon + v \cos \theta, = V_1 \text{ say}:$$

the time of the journey being simply t, as before.

The angle ϵ is defined by the equally obvious geometrical relation

$$V \sin \epsilon - v \sin \theta = 0.$$

Fig. 4.



Successive waves emitted by a fixed source S into a drifting medium. The row of dots SC represent the respective wave-centres. The figure also represents waves in a stationary medium, emitted by a source moving from C to S.

Here is a picture of the source and successively-emitted and abandoned drifting wave-fronts. SM is the path of a labelled disturbance, and is to be considered as a ray; it is inclined at angle ϵ to the corresponding wave-normals.

SP is what would have been the light journey in the same time if the medium had been stationary; PM or SC represents the drift.

The result of the state of things exhibited in the diagram may or may not be appreciated by a spectator—that depends on what his own motion is,—but if he is moving simply with the medium, he perceives the following:—

(1) An aberration, ϵ, in any direction inclined at angle θ to the motion, such that

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

it being convenient to denote the ratio of velocities, v/V, by a single symbol α, and to call it the aberration constant. A telescope moving with the medium and placed with its object glass tangential to the advancing wave-fronts, will focus the image on its cross wires, and will be pointing not to the object, but to the centre of the wave it is receiving; its collimation axis coincides with a radius or wave-normal, not with a ray.

(2) A Doppler alteration of wave-length in every direction; as is obvious in the figure, from the distribution of drifted wave-fronts. It is positive on one side, and negative on the other side, of a certain direction, θ_0, such that the radius vector is equal to the radius, or

$$\cos\theta_0 = \frac{v}{2V} = \tfrac{1}{2}\alpha;$$

the aberration angle for this particular case of no Doppler effect being twice the complement of θ_0.

A spectator moving with the medium will perceive this change of wave-length as a change of pitch (or colour) of value

$$\log\frac{n}{n'} = \log\frac{\lambda'}{\lambda} = \log(\cos\epsilon + \alpha\cos\theta) \approx \alpha\cos\theta.$$

An observer travelling with the medium will not observe any modification in interference or diffraction effects, nor will he experience any change of intensity due to motion; for the waves will be brought him at the customary time periods, and be subject to the ordinary flux of energy, as if everything were stationary.

Case of moving Source in fixed Medium.

14. The same figure (fig. 4) serves to illustrate the common case of medium and observer stationary, and source alone moving.

But we must be careful to note that ϵ is only the aberration *angle*, and that whether it is to be called "aberration" or not depends on the meaning attached to that term. The source emits spherical waves in its successive positions, and leaves them to expand at their normal rate. The fixed telescope, pointing to centre of advancing wave, is therefore pointing to the source at the instant when it emitted that light; and, since it is thus seen in its true place at instant of emission, it is most natural to say that the aberration caused by moving source alone is *nil;* for that it may have moved by the time of vision, is obvious.

There is not much more to be said on this head, for the source after throwing off a wave may do what it likes, the light will convey information as to where and how it was at the time of emission. Phenomena depending on a *succession* of waves, *e.g.*, changes of pitch, are of course produced, see fig. 4.

The question arises whether the waves thrown off from a moving source are really spherical shells: whether the motion of the source does not affect its vibration? It is not easy to answer this thoroughly and accurately, but practically there can be no doubt that the emission of light cannot be affected by any feasible terrestrial motion;

for, in the time of one vibration, the earth, which is the quickest available vehicle, has only moved a distance of $\frac{1}{10000}$ of a wave-length; which is equivalent to a middle C fork sounding and creeping along at the rate of 15 inches an hour. No practical question as to imperfection in spherical form of wave from moving source is therefore likely to arise. See however § 19, for discussion of a question not of *shape* but of *intensity*.

There happen to be one or two interesting things connected with the reflexion of light from a moving source when there is some connexion established between the reflected ray and the subsequent position of the source, *e.g.*, as when a ray is reflected back upon itself, with the object of causing interference; these are specially dealt with in §§ 59, 60.

Case of Source and Receiver moving together through Stationary Medium; or, correlative case of Medium drifting past fixed Source and Receiver.

15. Consider a telescope fixed relatively to source, and medium drifting freely past both. The object-glass must be set skew to the wave front, but normal to the advancing ray or radius vector.

In fig. 4, SM is the axis of the telescope, and it points straight at the source. There is no resultant aberration, the object is seen in its true position.

It is also seen of its right colour, for the waves are carried to the receiver at their accustomed frequency: there is no Doppler effect. A steady wind alone is powerless to influence either direction or pitch.

But what about interference phenomena, depending on the *time* of a given journey? Manifestly a motion of the medium will be able to affect this, and may accordingly bring about the displacement of fringes representing hurry or lag of phase.

Consider a telescope fixed relatively to the source and placed so as to receive light along the radius vector r.

If the medium is stationary, the light journey is accomplished in the time

$$T = \frac{r}{V},$$

but if moving, the time of the journey is

$$T' = \frac{r}{V \cos \epsilon + v \cos \theta},$$

and so there is a hurrying up of phase

$$\frac{T}{T'} = \cos \epsilon + \alpha \cos \theta,$$

or

$$T - T' \approx \alpha T \cos \theta.$$

The wind therefore causes a positive or negative change of phase in every direction except that whose cosine is $\frac{1}{2}\, v/\text{V}$, the same direction as that already (§ 13) indicated as possessing a zero Doppler effect.

But the *observation* of the lag of phase thus caused by motion of the entire ethereal medium is not so easy as might appear, and, in fact, it has not yet been detected; for the simple reason that it is liable to affect both the interfering rays equally: as we now show.

Devices for Observing the Lag of Phase.

16. The possible ways in which change of phase, produced by a moving medium, may be looked for, are:—to split a beam of light into two halves, and then—

(1) Make the medium flow with one half beam and against the other.

This is successful, and is the FIZEAU experiment; but it entails control over the medium, and artificial motion of it; the terrestrial orbital motion cannot be utilized in this way.

(2) Send the two beams, not parallel, but round contours in two different planes; or, say one across the line of ether motion, and the other along.

This is MICHELSON's experiment; but it only attempts an effect whose magnitude is the second order of aberration magnitudes; because, before the beams can be brought together again to interfere, a reversal or complete circuit is necessary.

(3) Make the medium flow at different rates along the two beams: as for instance, by interposing a dense substance in one of them.

But, on FRESNEL's hypothesis, this ought to fail; because the free ether, which is the only ether in motion, is unaffected by the dense substance. The only way to move either more or less than the normal quantity of ether in any given space, is to move bodily a dense substance occupying that space. So long as that is stationary, with respect to source and receiver, motion of the whole produces no effect.

To prove that on FRESNEL's law, no dense substance can cause different interference effects when moving than it causes when stationary, we can proceed to calculate the virtual thickness of a slab immersed in an ether stream, or the time retardation it causes in a beam.

Interference Effects as modified by Ether Motion through Dense Stationary Bodies.

17. The calculation of the lag in phase caused by FRESNEL's ethereal motion is a very simple matter. A dense slab of thickness z, which would naturally be traversed with the velocity V/μ, is traversed with the velocity $(\text{V}/\mu)\cos\epsilon + (v/\mu^2)\cos\theta$; where v is the relative velocity of the ether in its neighbourhood; whence the time of journey through it is

$$\frac{\mu z}{V\left(\cos\epsilon + \frac{\alpha}{\mu}\cos\theta\right)}, \quad \text{instead of } \frac{\mu z}{V},$$

or the equivalent air thickness, instead of being $(\mu - 1)z$, is

$$\frac{\mu z}{\cos\epsilon + \frac{\alpha}{\mu}\cos\theta} - z = \left(\frac{\mu\cos\epsilon - \alpha\cos\theta}{1 - \left(\frac{\alpha}{\mu}\right)^2} - 1\right)z,$$

or, to the first order of minutiæ, $(\mu - 1)z - \alpha z\cos\theta$; θ being the angle between ray and ether drift inside the medium.

So the extra equivalent air layer *due to the motion* is approximately $\pm \alpha z\cos\theta$, a quantity independent of μ.

Hence, no plan for detecting this first-order effect of motion is in any way assisted by the use of dense stationary substances; their extra ether, being stationary, does not affect the lag caused by motion, except indeed in the second order of small quantities, as shown above.

Direct experiments made by HOEK,* and by MASCART, on the effect of introducing tubes of water into the path of half beams of light, are in entire accord with this negative conclusion.

Thus, then, we find that no general motion of the entire medium can be detected by changes in direction, or in frequency, or in phase; for on none of them has it any appreciable (*i.e.*, first-order) effect even when assisted by dense matter.

The remaining possible effect that may be looked for is a change of energy.

Effect of Motion on Intensity of Radiation in Different Directions.

18. At first sight it looks as if there ought to be an unequal distribution of energy round a source past which the medium is streaming. For when the waves are drifting along, their energy moves too, and it can thus be distributed unsymmetrically round the source.

The energy emitted per second, or the *power* of the radiation, is

$$P = 4\pi\rho^2 Vq,$$

where q is the energy per unit volume at distance ρ from the wave centre; supposing that radiating power is unaffected by the motion. So at a place r, θ, reckoning from source as origin, and line of drift as initial line (as in fig. 4), since $r = \rho\,(\cos\epsilon + \alpha\cos\theta)$,

* 'Archives Néerlandaises' (1869), vol. 4, p. 443, or 'Nature,' vol. 26, p. 500.

$$q = \frac{P(\cos\epsilon + \alpha\cos\theta)^2}{4\pi V r^2}$$
$$\approx q_0(1 + 2\alpha\cos\theta + \alpha^2\cos 2\theta - \tfrac{1}{2}\alpha^3\sin\theta\sin 2\theta),$$

q_0 being the energy at the same place when there was no drift.

So the energy received per second by a given small area A at that place, facing the source, *i.e.*, normal to the rays, is

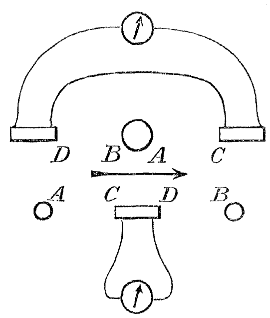
$$qV\,A\cos\epsilon = \frac{A\cos\epsilon}{4\pi r^2}\,P(\cos\epsilon + \alpha\cos\theta)^2.$$

The radiation at distance r from the source is, in fact, the same as what the radiation would be at distance ρ in a stationary medium; except for the small inclination ϵ.

So a pair of similar thermopiles, fore and aft, at equal distances from a source, will, on this hypothesis, receive unequal radiation; the difference being equal to $4\alpha\,(PA/4\pi r^2)$, or proportional to 4α.

FIZEAU suggested this method, but I am not aware of its having been tried yet.*

Fig. 5.



Thermopile experiment suggested by FIZEAU; in two alternative forms.

19. But it is a serious question whether the reasoning establishing the effect is quite sound. It is not unlikely that motion may affect the radiating power of a source. In fact, the theory of exchanges almost necessitates something of the kind, else the two faces of an enclosure would become unequal in temperature by reason of mere motion through the ethereal medium.†

Hence, if, as in fig. 5, we consider a pair of thermopiles with a hot body half-way

* The suggestion is quoted in a comprehensive, but to me not very intelligible, treatise on the whole subject of aberration: 'Astronomische Undulationstheorie,' by Professor Dr. KETTELER, of Bonn.

† BALFOUR STEWART ('Brit. Assoc. Report,' 1871, Sects. p. 45), argued that this inequality of temperature actually occurred; and, since motion thus afforded an available heat engine, he deduced an ethereal friction, dissipating energy. But, as Lord RAYLEIGH points out (in his Article on "Aberration," 'Nature,' March 1892), it is far more likely that motion should alter radiating and absorbing powers than that it should disturb equality of temperature.

between them, or a pair of equally hot bodies with a thermopile half-way between them, all subject to an ethereal drift in the direction of the arrow, we may assert that although the radiation from A is carried down stream in undue proportion towards C, the amount actually emitted in this direction is diminished in a compensatory manner, so that the resultant flux of energy remains unaffected by the motion.

It is not necessary to suppose that motion disturbs the equality which otherwise exists between radiating and absorbing powers. It is true that if a surface like A radiates less than when the medium is stationary, a surface like C facing the stream must radiate more; but then it may absorb more also. So that in all respects the balance may be undisturbed by the motion of the medium.

It is probable, therefore, that even by this intensity method, nothing more than the second order of aberration magnitude is effective for displaying a general drift of the medium as a whole.

At the same time it seems desirable that an experiment with thermopiles, like that suggested by FIZEAU, should be tried, in order to verify the above deductions from the theory of exchanges, combined with the supposed persistent uniformity of temperature of an enclosure whether at rest or in motion; for thereby the absence of friction or dissipation of energy by motion of solids through ether would be verified.

Case of only Receiver Moving.

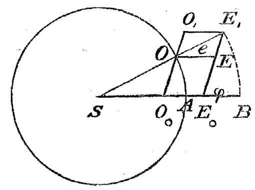
20. If the receiver be not fixed relatively to the medium, nor relatively to the source, but be moving on its own account, the effects due to this motion must be added to the preceding effects. First suppose both source and medium stationary.

The source S emits waves in spherical shells, whose radii are also rays. Any motion of the receiving telescope can be resolved tangentially and radially. Radial motion gives Doppler effect only; tangential motion gives aberration only—both of the commonplace type.

If the telescope were stationary, its object-glass must be tangential to the wave front, but directly it moves it must encounter the wave front obliquely, with the same obliquity ϵ as if it were stationary and the medium drifting (fig. 4), and the eyepiece will then be brought to the light at the right instant. Revolution of a radial telescope about the source would effect this in the simplest way, without introducing any Doppler effect or change in focal length.

Consider a telescope O_0E_0 pointing straight at a source S (fig. 6), and at the instant a given luminous disturbance starts from S, let the telescope begin moving in a direction ϕ with a velocity u. Let it thus reach the position OE by the time the light has got as far as O, *i.e.*, to the spherical wave front indicated in the diagram. Then it follows that by the time the telescope has reached the position O_1E_1 the light will have reached E_1, too, and will accordingly have passed along the collimating axis by reason of the combined motions.

Fig. 6.



A telescope receiving light from S and moving from OE to O_1E_1 while light traverses OE_1.

A stationary telescope receiving the same ray at the same instant would have had to occupy the position OE_1, and would have looked straight at the object (with a slightly greater focal length). Hence the angle O_1E_1O or OSO_0 is the angle of aberration, the amount by which the object appears to be displaced in the direction of motion. A telescope which had been revolving round the source, instead of being translated, would have gone from AB to OE_1 in the time, and have rotated through this same angle. Call it e; it is such that

$$\frac{\sin e}{\sin \phi} = \frac{EE_1}{OE_1} = \frac{u}{V} = \beta, \text{ say,}$$

the medium, remember, being stationary.

The focal length of the moving telescope differs from that necessary for a fixed one; being OE instead of OE_1, or

$$f' = f(\cos e - \beta \cos \phi);$$

but this is best regarded as part of the Doppler effect, since its principal term represents radial motion. With a non-achromatic lens the change of refrangibility due to motion tends to compensate* this effect. But whereas the change of refrangibility is produced equally by motion of source or motion of receiver, this change of focal length seems to be caused only by motion of receiver. It is a shortening of focus as a telescope recedes from the light. I suppose it is too small to observe, else it would seem able to discriminate motion of earth from motion of star, and give absolute motion of telescope through the ether.

A terrestrial source (*e.g.*, a sodium flame) might be used, and a perfectly achromatic lens; but surely no focussing could be delicate enough to discriminate such sort of difference as exists between the two sodium emissions?

The way in which motion of receiver to or from source causes an apparent change of frequency, *i.e.*, a real change in the frequency with which waves are *received*, is too well known and simple to be more than mentioned. Its amount in any direction is

$$\log \frac{n}{n'} = \log (\cos e + \beta \cos \phi) \approx \beta \cos \phi,$$

where $\beta = u/V$, $\sin e = \beta \sin \phi$, and u is the velocity of the telescope at angle ϕ with the ray.

* This was originally written "exaggerate."

Summary.

21. Collecting these statements together, we may summarize them thus :—

Source alone moving produces—

- A real and apparent change of colour ;
- A real but not apparent error in direction ;
- No lag of phase, except that appropriate to altered wave-length ;
- A change of intensity corresponding to different wave-length.

Medium alone moving, or
Source and receiver moving together } gives—

- No change of colour ;
- No change of direction ;
- A real lag of phase, but undetectable without control over the medium ;
- A change of intensity corresponding to different virtual distance, but probably compensated by change of radiating power.

Receiver alone moving gives—

- An apparent change of colour ;
- An apparent change of direction ;
- No change of phase, except that appropriate to extra virtual speed of light ;
- A change of intensity corresponding to different virtual velocity of light.

Thus the interference effect and the Doppler effect do not occur together. Motion of the medium produces one ; motion of source or of receiver produces the other.

Aberration of direction and of pitch occur simultaneously, but are complementary to each other, since one depends on motion across the line of sight, the other on motion along it. One varies as the sine, the other as the cosine, of the inclination. Further discussion of the Doppler effect is deferred to §§ 53–58.

22. It is noteworthy that not one of the methods is able to establish the existence or non-existence of a general ethereal drift near the earth ; for, as shown above, *uniform motion of the entire medium produces no observable first-order effect of any kind.* It plainly becomes the more necessary to attend minutely to possible second-order effects.

In a paper in the 'Archives Néerlandaises,' vol. 21, Professor H. A. LORENTZ discusses, with much power, the whole subject of ether movement ; the idea of the following method of treatment is derived from that paper.

Definition of Ray.

23. In § 13 we defined a ray as the path of a labelled disturbance,* for it is that which enables an eye to fix direction, it is that which determines the line of collimation of a telescope. Now in order that a disturbance from A may reach B, it is necessary that adjacent elements of a wave front at A shall arrive at B in the same phase; hence the path by which a disturbance travels must satisfy this condition from point to point, viz., that disturbances arriving at any point from a preceding point of a ray agree in phase. This condition will be satisfied if the time of journey down a ray and down all infinitesimally differing paths is the same.

The equation to a ray is therefore contained in the statement that the time taken by light to traverse it is a minimum; or

$$\int_A^B \frac{ds}{V} = \text{minimum.}$$

If the medium, instead of being stationary, is drifting with the velocity v, at angle θ to the ray, we must substitute for V the modified velocity $V \cos \epsilon + v \cos \theta$, and so the function that has to be a minimum in order to give the path of a ray in a moving medium is

$$\int_A^B \frac{ds}{V(\cos \epsilon + \alpha \cos \theta)} = \int_A^B \frac{V \cos \epsilon - v \cos \theta}{V^2 (1 - \alpha^2)} \, ds = \text{minimum.}$$

Path of Ray, and Time of Journey, through an Irrotationally Moving Medium.

24. Writing a velocity-potential ϕ in the above equation to a ray, that is putting

$$v \cos \theta = \frac{\partial \phi}{\partial s},$$

and ignoring possible variations in the minute correction factor $1 - \alpha^2$, between the points A and B, it becomes

$$\text{Time of journey} = \int_A^B \frac{\cos \epsilon}{1 - \alpha^2} \cdot \frac{ds}{V} - \frac{\phi_B - \phi_A}{V^2 (1 - \alpha^2)} = \text{minimum.}$$

Now the second term depends only on end points, and therefore has no effect on path. The first term contains only the second power of aberration magnitude; and hence it has much the same value as if everything were stationary. A ray that was

* [It has been objected that a bit of wave-front cannot be labelled, because of diffraction effects. This seems to me only a practical difficulty, and a more practical definition based upon preserved phase-connexion follows a few lines later in the text; but the meaning conveyed by the convenient phrase "labelled disturbance" can equally well and I think unobjectionably be expressed by calling a ray the path of a definite, or identical, portion of energy—the direction of energy-flux.—July, 1893.]

straight, will remain straight in spite of motion; whatever shape it had, that it will retain. Only cos ϵ, and variations in α^2, can produce any effect on path, and effects so produced must be very small, since the value of cos ϵ is $\sqrt{(1 - \alpha^2 \sin^2 \theta)}$. A second-order effect on direction may therefore be produced by irrotational motion, but not a first-order effect. A similar statement applies to the time of journey round any closed periphery.

MICHELSON'S *Experiment.*

25. We conclude, therefore, that general ethereal drift does not affect either the path of a ray or the time of its journey round a complete contour, to any important extent. But that taking second-order quantities into account, the time of going to and fro in any direction inclined at angle θ to a constant drift is, from the above expression, § 24,

$$\frac{2\,\mathrm{T} \cos \epsilon}{1 - \alpha^2} = \frac{\sqrt{(1 - \alpha^2 \sin^2 \theta)}}{1 - \alpha^2} \times 2\mathrm{T},$$

where 2T is the ordinary time of the double journey.

Hence, by this means, interference effects due to drift would seem to be possible, since the time depends subordinately on the inclination of ray to drift (*cf.* §§ 59–62)

The above expression applies to MICHELSON'S* remarkable experiment of sending a split beam to and fro, half along and half across the line of earth motion; and is, in fact, the theory of it. There ought to be an effect due to the difference between $\theta = 0$ and $\theta = 90°$, but he does not observe any. Hence, either something else happens, or the ether near the earth is dragged with it, so as not to stream through our instruments. When α is constant I see no way out of this conclusion, except hypothetical disturbance at reflexion of some minute kind, one of the mirrors being normal and the other tangential to the drift; but I perceive no adequate reason for this suggestion (see § 60). It is true that if the earth is carrying the ether with it, α will *not* be constant, at different distances from its surface; but, then, the plane of MICHELSON'S experiment was horizontal.

If the ether is dragged along near moving matter it behaves like a viscous fluid, and a velocity-potential must (save by some exceptional theory, § 31) be abandoned; but, as this would involve the curvature of rays striking the earth and much complication, it seems a pity to abandon it until compelled by direct experimental evidence to recognize ethereal viscosity.

The experiment of MICHELSON'S raises a strong presumption in favour of such viscosity, nevertheless his negative result is conceivably explicable in other ways; one of which has been ingeniously suggested by Professor FITZGERALD, viz., that the cohesion force between molecules, and, therefore, the size of bodies, may be a

* 'Phil. Mag.,' Dec., 1887.

function of their direction of motion through the ether; and accordingly that the length and breadth of MICHELSON'S stone supporting block were differently affected, in what happened to be, either accidentally or for some unknown reason, a compensatory manner.

26. There is already one experiment, which I have never seen criticised either way, tending in a sense precisely contrary to MICHELSON'S. FIZEAU* observed the polarization produced by a pile of plates, and considered that he had proved that the azimuth of the plane of polarization varied with the direction of orbital motion of the Earth, and hence that the ether was streaming past them. If so, polarization by reflexion is the only phenomenon known which is capable of showing a first-order effect of the general ethereal drift. The experiment seems to me extremely difficult, but to be well worthy of repetition by other observers. [I believe that Lord RAYLEIGH'S objection to the experiment as performed by FIZEAU is that the effect was unseen until an illegitimate or unsafe magnifying device was employed.]

Meanwhile I shall hope to examine the question of ether motion near moving matter in a simpler fashion (§ 33).

Assuming for the present that the ether is not disturbed in a viscous manner by the motion of gross matter through it, we can make the following assertions :—

General Statements Concerning Aberration.

27. A ray is straight whatever the motion of the medium, unless there are eddies, and accordingly no irrotational currents of ether can divert a ray. But, if the observer is moving, the apparent ray will not be the true ray, and accordingly the line of vision will not be the true direction of object.

In a stationary ether, wave-normal and ray coincide, but the line of vision of a moving observer slants across both (§ 20).

In a moving ether, wave-normal and ray enclose an angle, and line of vision depends upon motion of observer. If the observer is stationary his line of vision is the ray; if he moves at the same rate as the ether his line of vision is the wave-normal (§ 13).

The line of vision, in fact, always depends on the motion of the observer, not at all on the motion of the ether so long as it has a velocity-potential. Hence nothing can be simpler than the theory of aberration if this condition is satisfied.

A similar but more general condition (to be obtained in the next section) suffices to secure the straightness of a ray whatever happens, or more generally that whatever the path of a ray may be by reason of reflexion or refraction in a stationary ether, the same it shall be in a moving one; and readily accounts for the absence of all effect on direction due to the general relative drift of the medium, whether in the

* 'Ann. de Chim. et de Phys.,' 1859, vol. 57, p. 129.

presence of dense matter (water-filled telescopes) or otherwise (*cf.* 'Nature,' vol. 46, p. 498).

However matter affects or loads the ether inside it, it cannot on this theory be said to hold it still, or carry it with it. The general ether stream must remain unaffected, not only near, but inside matter, if rays are to retain precisely the same course as if it were relatively stationary.

But it must be understood that the ethereal motion here contemplated is the *general drift of the entire medium,* or its correlative the uniform motion of all the matter concerned. There is nothing to be said against aberration effect being producible or modifiable by motion of *parts* of the medium, as, for instance, by sliding one portion of the ether past another portion, as by the artificial motion of slabs and other partitioned-off regions. These matters are to some extent mixed up with the law of refraction, which we consider later, but the general ideas concerning them have been already given. Artificial motion of matter may readily alter both the time of journey and the path of a ray (*cf.* §§ 7 and 52).

Effect of placing Ordinary Matter in the path of a ray in a Drifting Medium. FRESNEL'S *Law a special case of a universal Potential-function.*

28. Inside a transparent body light travels at a speed V/μ; and the ether, which outside drifts at velocity v making an angle θ with the ray, inside may be drifting with velocity v' and angle θ'.

Hence the equation to a ray inside such matter is

$$T' = \int \frac{ds}{(V/\mu)\cos\epsilon' + v'\cos\theta'} = \text{min.}, \quad \text{where } \frac{\sin\epsilon'}{\sin\theta'} = \frac{v'}{V/\mu} = \alpha'.$$

This may be written

$$T' = \int \frac{\cos\epsilon'\, ds}{V/\mu\,(1-\alpha'^2)} - \int \frac{v'\cos\theta'\, ds}{V^2/\mu^2\,(1-\alpha'^2)};$$

the second term alone involves the first power of the motion, and assuming that $\mu^2 v' \cos\theta' = d\phi'/ds$, and treating α'^2 as a quantity too small for its possible variations to need attention, the expression becomes

$$T' = \mu T \frac{\cos\epsilon'}{1-\alpha'^2} - \frac{\phi'_B - \phi'_A}{V^2(1-\alpha'^2)},$$

T being the time of travel through the same space when empty. Now, if the time of journey and course of ray, however they be affected by the dense body, are not to be more affected by reason of ethereal drift through it than if it were so much empty space, it is necessary* that the difference of potential between two points A and B should be the same whether the space between is filled with dense matter or not (or, say, whether the ray-path is taken through or outside a portion of dense medium); in other words (calling ϕ the outside and ϕ' the inside potential-function), in order to secure that T′ shall not differ from μT by anything depending on the first power of motion, it is necessary that $\phi'_B - \phi'_A$ shall equal $\phi_B - \phi_A$, *i.e.*, that the potential inside and outside matter shall be the same up to a constant, or that $\mu^2 v' \cos\theta' = v\cos\theta$; which for the case of drift along a ray is precisely FRESNEL'S hypothesis.

Another way of putting the matter is to say that to the first power of drift velocity

$$T' = \mu T - \int(\mu^2 v' \cos\theta - v\cos\theta)\ ds/V^2,$$

and that the second or disturbing term must vanish.

29. Hence FRESNEL'S hypothesis as to the behaviour of ether inside matter is equivalent to the assumption that a potential-function, $\int \mu^2 v \cos\theta\, ds$, exists throughout all transparent space, so far as motion of ether alone is concerned.

Given that condition, no first-order interference effect due to drift can be obtained from stationary matter by sending rays round any kind of closed contour, nor can the path of a ray be altered by ethereal drift through any stationary matter.

As soon as matter is locally moved, however, its motion may readily produce an effect, for it has no potential conditions to satisfy; it may easily be moved in a closed contour. Suppose it moves with velocity u, always with the light, the relative drift of ether thereby caused in it must, as above, be u/μ^2, and so it may be said to virtually carry the ether inside it forward with velocity $u - u/\mu^2$; for that is the amount by which it affects the time of journey of a ray. This does not mean that it carries with it any ether of space; in fact, it definitely means that it does *not* appreciably disturb the ether of space (*cf.* § 3, *b*).

The equation to a ray in *moving* matter, subject to an independent ether drift, is

$$\int \frac{ds}{V/\mu \cos\epsilon + v/\mu^2 \cos\theta + u[1 - (1/\mu^2)]\cos\phi} = \text{const.}$$

30. It is noteworthy that almost all the observations which have been made with negative results as to the effect of the Earth's orbital motion on the ether are equally consistent with complete connexion and complete independence between ether and

* [The argument has here been slightly expanded since the MS. was sent in to meet a suggestion of inadequacy made by Dr. SCHUSTER, to whom I am also indebted for an objection to the term "velocity-potential" at first applied to this function ϕ'. As Professor FITZGERALD has observed, it is more general than a velocity-potential, though it reduces to that when the medium is homogeneous, or when $\mu = 1$. The text has been altered accordingly.—July, 1893.]

matter. If there is complete connexion, the ether near the earth is relatively stagnant, and negative results are natural. If there is complete independence, the ether is either absolutely stationary or has a velocity-potential, and the negative results are thereby explained.

Ordinary astronomical aberration, and all other phenomena concerned with vision through strata high above the earth, so far as they have been accurately observed, are consistent with complete independence, but not with a viscous drag.

On the other hand, the negative result of Mr. MICHELSON'S attempt to detect a second-order effect appears only to be consistent with relative stagnation.

A doubtful positive result, supposed to be obtained by FIZEAU (§ 26), on a change in the azimuth of the plane of polarization effected by transmission through oblique plates, would, if established, support relative motion between earth and ether.

31. Is it possible for a sphere to move through a fluid without disturbing it rotationally and propagating rotary motion into space?

It is not possible for an ordinary solid moving through an ordinary fluid. Diffusion of motion, or viscosity, is bound to occur.

It is possible for a vortex ring or assemblage of vortex rings, because at their surface there is no slip. It is possible also if the sphere be a solidified portion of the fluid, which condenses in front and evaporates behind (as already mentioned).

Professor STOKES seems to say, that though not possible to retain a velocity-potential with any *viscosity*, yet with some kind of *rigidity* it may be possible, because deviations from irrotational motion go off into space with the speed of light. If so, the earth might possibly carry some ether with it, and yet a ray be straight.

I do not see any way in which it can abstain from rotationally disturbing the fluid if at the same time it has to carry some with it. Neither, I think, do Mr. HICKS or Mr. LARMOR, to whom I wrote.

Lord KELVIN, however ('Papers,' vol. iii., p. 436), has invented an "ether," or kinematically rigid incompressible ideal substance, which satisfies electromagnetic equations and magnetic boundary conditions, whose equations of motion are like those of an elastic solid, and which yet permits locomotion of smooth solids filling vesicular hollows in it, and which in general "takes precisely the same motion for any given motion of the boundary as does a frictionless incompressible liquid in the same space showing the same boundary."

The experiment now to be described proves, I think, that by the motion of ordinary masses of matter the ether is appreciably undisturbed, and raises a presumption in favour of the earth's motion being equally impotent.

The one thing in the way of the simple doctrine of an ether undisturbed by motion is MICHELSON'S experiment, viz., the absence of a second-order effect due to terrestrial movement through free ether. This experiment may have to be explained away; perhaps as suggested above (end of § 25).

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

[* 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 ($1\frac{1}{2}$-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 $4\frac{1}{2}$-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.

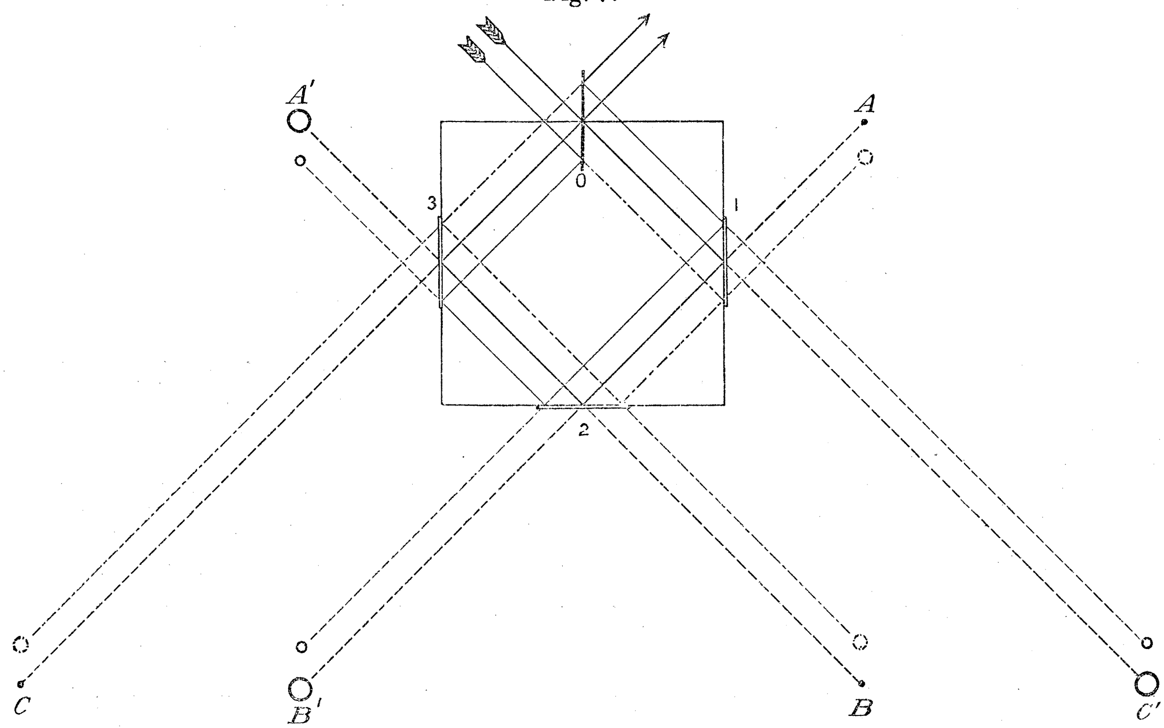


Diagram showing images in simplest case, with three mirrors, and beam going only once round optical square. The points to be imaged are the splitting points on semi-transparent mirror O. Two such points are shown, one imaged by a dot, the other by a ring. A represents the pair of images in first mirror 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

* 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.

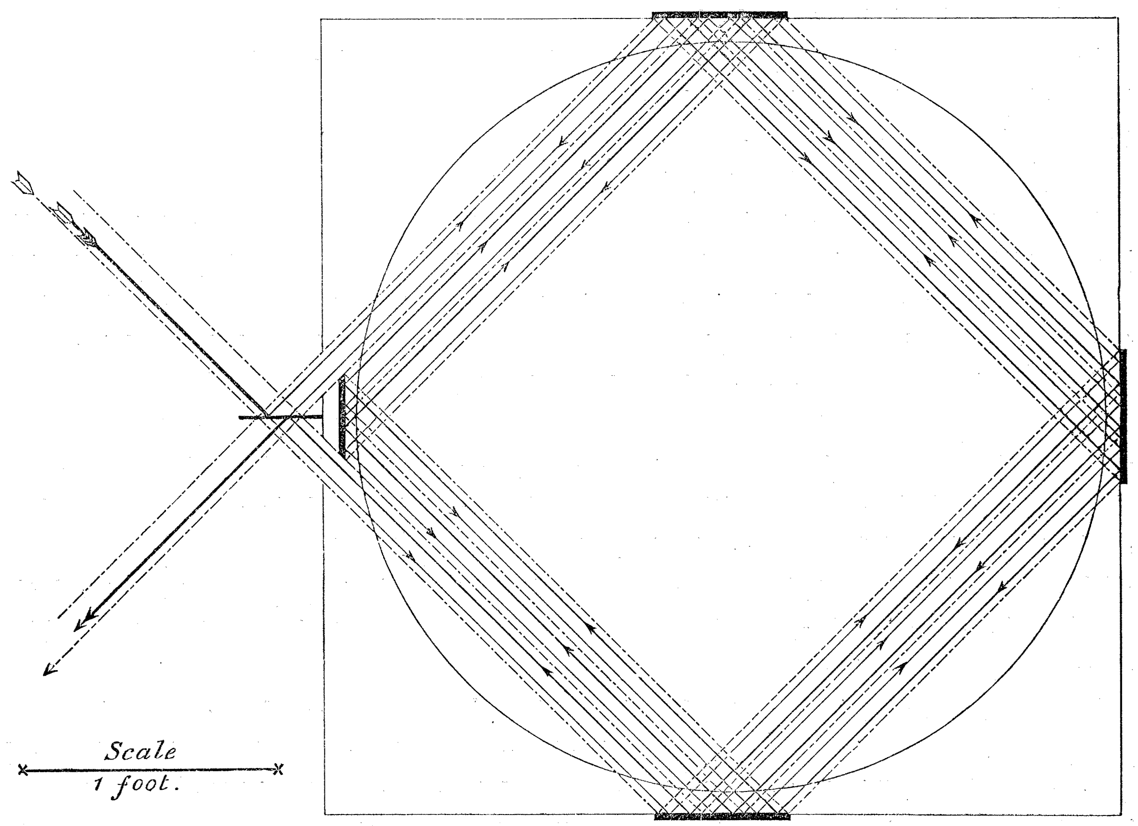


Diagram showing arrangement of mirrors for the usual case of beam going three times round. Interfering rays travel parallel but not identical paths, and re-unite at a point different from that at which they split. The greatest possible size of beam, with a 4-inch fourth mirror, is represented. The centre 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.

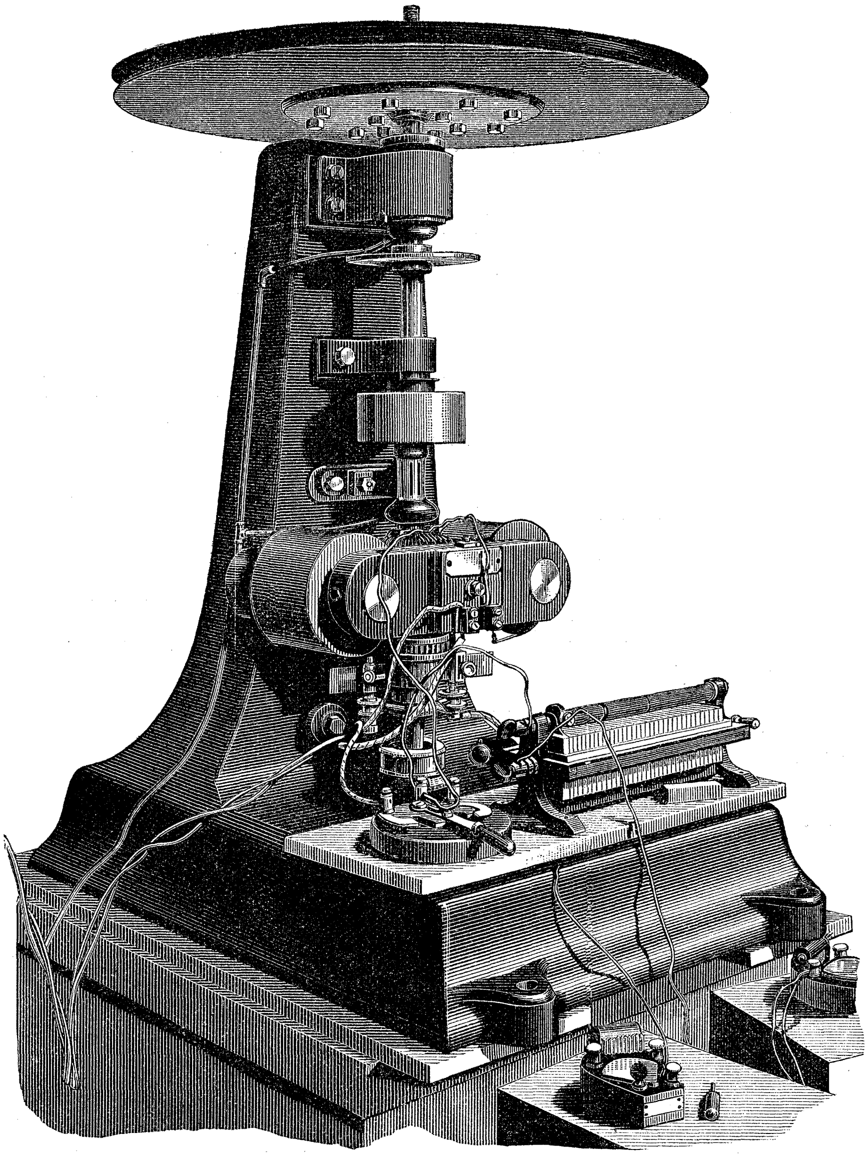
Fearing lest some damage to the disks might occur from sudden application of too great moment to the armature, especially by reason of some accidental jamb or other sudden stoppage, I imitated a device employed in some milk-whirling machines, and introduced a wooden cup or friction coupling between the 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

* 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.

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.

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.

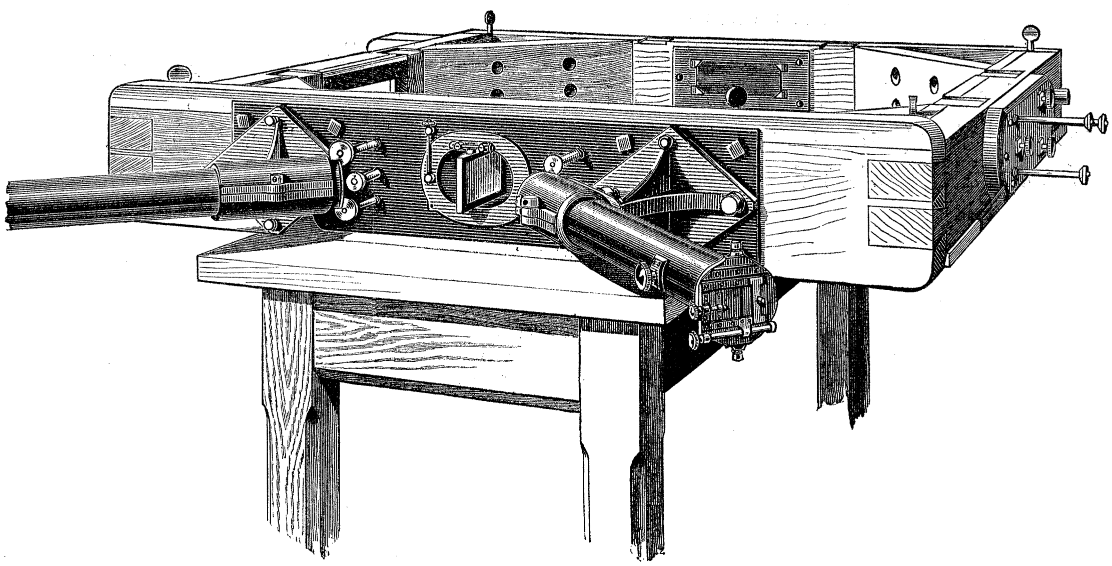
A perforated steel disk, pierced with circles of 3, 5, 9, 15, and 25 holes, and fixed on the spindle, gives the speed quite accurately by the musical note from an air jet through it, or even a card held against it. But the particular octave was occasionally checked by a wheel speed-counter; and recently 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.

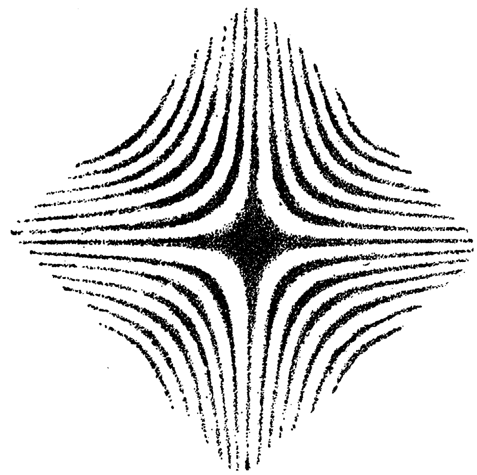
Later experience makes it absurdly easy to get the bands and to arrange that they shall be vertical, well defined, of any convenient width, and with the centre white band symmetrical among the coloured ones. Tilting the mirrors, pressing on the frame, or touching the semi-transparent plate, makes the 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 :—

* 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

* 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

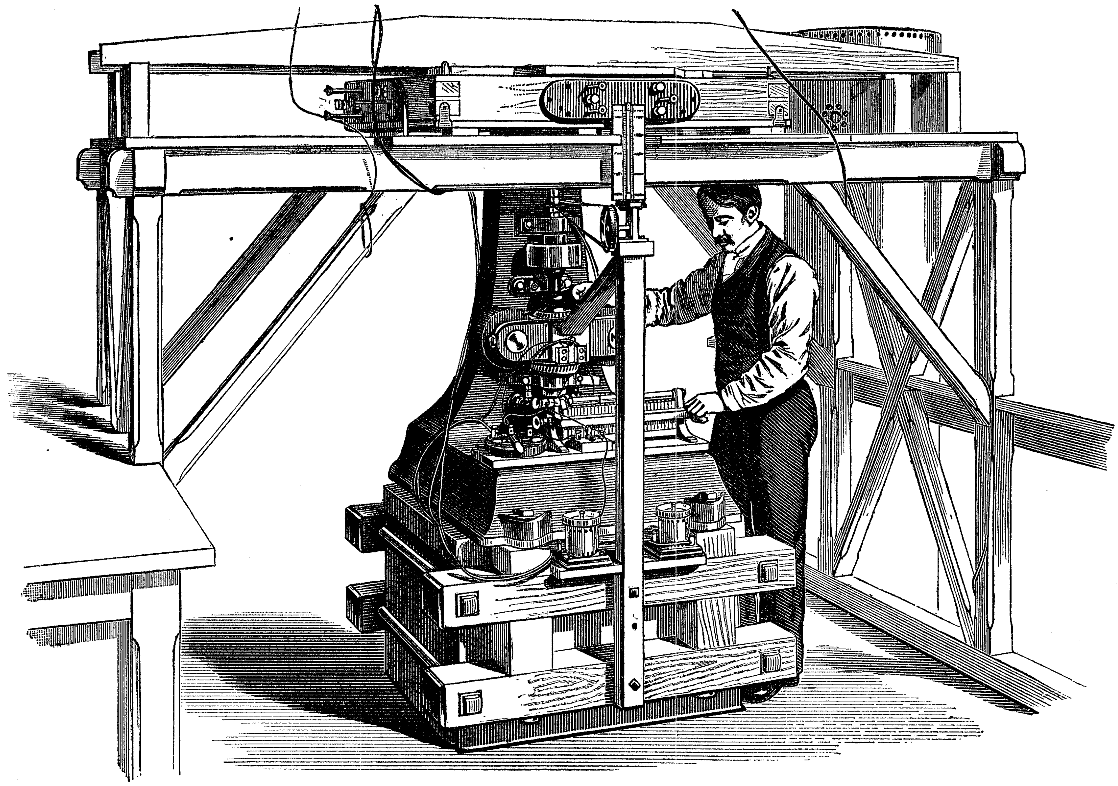
* Quite recently (March, 1892) I have taken a series of spins without the drum, and with no cover glasses on except one over the front mirror, something being there necessary to protect from wind the semi-transparent plate, which is of course extremely sensitive.

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

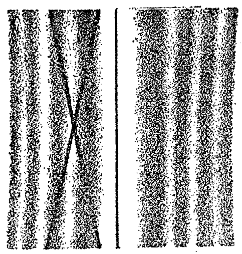
the frame and tilt the mirrors about a horizontal axis; this kind of tilt being well calculated to shift the vertical bands.

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.

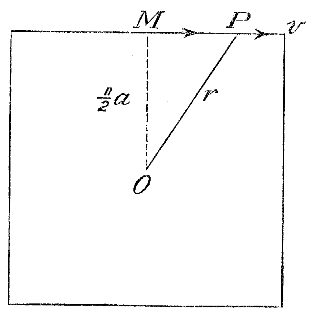
Another way of testing the order of the image in use is to tilt the front mirror so as to broaden the bands by a measured amount, and then to imitate or compensate this by tilting one of the other mirrors. The angle of tilt necessary with one of these will be $n/(n+1)$ of the tilt of the front one.

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

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

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

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

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

$$\frac{4 \times 10^5}{na} \sqrt{\frac{7}{9 \times 10^9}} = \frac{11 \text{ centims.}}{na}.$$

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	$38\frac{1}{2}$	28
Spinning (at 1260) . . .	32	$32\frac{1}{2}$
Rest again	37	26
Spinning again	30	$32\frac{1}{2}$
Rest	38	25
Spinning	$32\frac{1}{2}$	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	$65\frac{1}{2}$	
		$4\frac{1}{2}$ to right.
1260 revolutions	$63\frac{1}{2}$	
		$6\frac{1}{2}$ return.
Stationary	62	
		$6\frac{1}{2}$ to right.
1260 revolutions	$61\frac{1}{2}$	
		$7\frac{1}{2}$ return.
Stationary	62	
		7 to right.
1260 revolutions	$63\frac{1}{2}$	
		5 return.
Stationary	70	
	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	8 to right
1250 revolutions	$63\frac{1}{2}$	7 return
Stationary	$63\frac{1}{2}$	$8\frac{1}{2}$,,
1250 revolutions	63	7 ,,
Stationary	$64\frac{1}{2}$	$8\frac{1}{2}$,,
1250 revolutions	$61\frac{1}{2}$	$7\frac{1}{2}$,,
Stationary	63	
	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.

	X-wire. (Set and reset in yellow of first band.)	Vertical wire. (Set and reset in centre of middle band.)
Disks stationary	$81\frac{1}{2}$	20
	$80\frac{1}{4}$	20
	84	21
	91	21
	83	22
	90	$20\frac{1}{2}$
	85	..
	Average 85	Average $20\frac{1}{2}$

The width of this average band was $208\frac{1}{2}$ 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.

	X-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.

* [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).

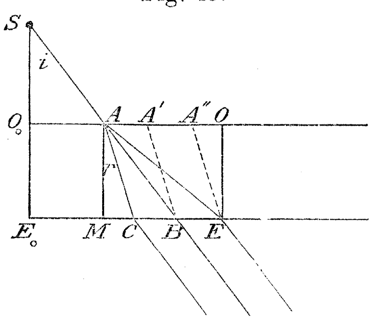
In practice, when the telescope is being carried along by the earth at angle ϕ to the light, it is customary to assume the medium stationary, in which case $\alpha = 0$ and the first term alone exists; or we may assume that the ether is more or less carried along by the earth, in which case $\theta = \phi$, 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 ·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.

* '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 $\mathrm{O_0E_0}$ 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

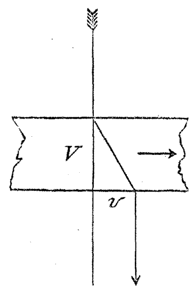
$$\mu^2 (u - v_0) = u$$

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

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

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

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

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 $u = 2Vs/\mu z = (4 \times 10^{10} \times 10^{-4})/10 = 4 \times 10^5$ 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 $z = 2Vs/\mu u = (4 \times 10^{10} \times 10^{-4})/15000 = 4000/15 = 3$ 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 $1/T$, so that

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

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

$$s\sin\theta' = V(T - t) = (V - v)T.$$

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 $V/(V + v)$; so that

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

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

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

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

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

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

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

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

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

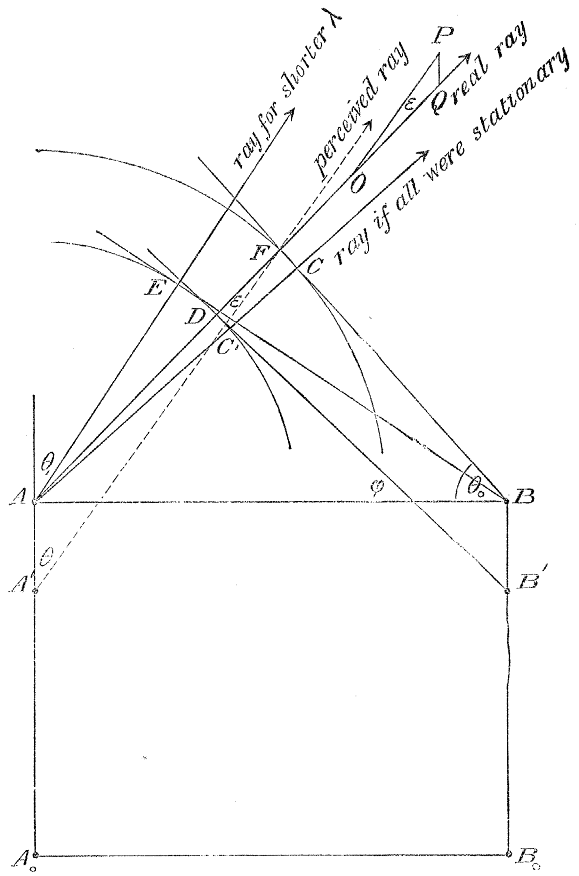
and so determine w the unknown speed of the ether past the solar system.

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.

Effect of Motion on Diffraction Grating.

56. To avoid any confusion about motion relatively to source, and the alteration of wave-length thus caused, it will be best to abandon our usual convenient plan of letting the ether move, and attend explicitly to the motion of the grating with its telescope and observer; all else being stationary.

Fig. 14.



Details of Doppler effect with moving grating, AB, and telescope OP.

Consider a plane wave, A_0B_0, advancing through a stationary medium with ordinary velocity, V, towards a stationary grating. Let $A_0A = AC = \lambda_0$ be an ordinary wave-length, while $AB = s$ is the width of one complete element of the grating; then BC is a wave-front, and AC is the ray, inclined to the normal to grating at angle $CBA = \theta_0$.

Now let the grating advance with velocity v to meet the wave a distance $BB' = AA' = CC'$ in one period; the disturbance B_0 will only have to go as far as B′, and the disturbance A only as far as C′; so drawing a tangent B′D to the sphere of radius AC′, we get the wave-front appropriate to moving grating; AD is the ray, inclined to the normal to grating at angle $DB'A' = \phi$.

Now θ_0 and ϕ are very nearly equal; showing that diffraction really does depend on wave-length simply, in spite of motion of grating, so far as minutiæ of the first order are concerned.

But then this direction, ϕ, will not be the direction appreciated by the observer; for the motion of his telescope will cause ordinary aberration, since his motion is partially across the diffracted rays.

Not to confuse the figure, I indicate the telescope OP further along the ray. While the light is travelling along it its eyepiece will have time to move to Q, such that

$$\frac{\mathrm{PQ}}{\mathrm{OQ}} = \frac{v}{\mathrm{V}} = \frac{\mathrm{AA'}}{\mathrm{AF}} = \alpha.$$

Hence, A′F is parallel to the axis of the telescope which receives the ray, and may be called the apparent or perceived ray. The angle at which it is inclined to the grating-normal may be called θ.

Now θ is less than θ_0, and is very nearly the same as if wave-length had been really shortened to AD, instead of AC.

Draw BE a tangent to the AD circle, and we get the wave-front appropriate to this shortened wave and a stationary grating; while AE is the ray belonging thereto, the inclination of which to the grating-normal we may call θ_1. [$\sin\theta_1 = (1-\alpha)\sin\theta_0$.]

Now, plainly, AE and A′F are very nearly parallel, but not quite; there is a second-order difference between θ and θ_1, which may be readily calculated.

Perhaps the simplest way of displaying the result is to introduce the aberration angle POQ $= \epsilon$ (such that $\sin\epsilon = \alpha\sin\theta$) and to write

$$\sin\theta_1 = \sin\phi - \alpha\cos\phi\sin\theta_0;$$

whereas

$$\sin\theta = \sin\phi\cos\epsilon - \alpha\cos\phi\sin\theta.$$

(Or one might write $\cot\theta = \cot\phi + \alpha\,\mathrm{cosec}\,\phi$.)

The difference between the apparent ray and the shortened wave-length ray is approximately

$$(\theta_1 - \theta)\cos\theta = \sin\phi\,(1-\cos\epsilon) - \alpha^2\cos\phi\sin\theta_0,$$

or

$$\theta - \theta_1 = \alpha^2\tan\theta\,(\cos\phi - \tfrac{1}{2}\sin\theta\sin\phi),$$

and is probably quite too small to be detected.

The point of the whole thing is that a grating has *the same real effect whether moving or stationary*, but that the motion of the observing telescope causes an *aberration* which necessitates very nearly the same alteration of its direction as if the waves were really shortened in simple proportion to the motion. The Doppler effect caused by motion of observer is, therefore, essentially a case of common aberration.

Now, as there is no hypothesis or difficulty whatever about the aberrational effect of a moving telescope, all that has been said of a grating applies, at least broadly, to a prism.

Effect of Motion on the Dispersion of a Prism.

57. The deviating power of a prism depends on its relative refractive index with respect to the surrounding medium; hence, in this sense, its deviation is certainly affected by the length of the waves with which it is supplied.

Its dispersive power, however, is not a superficial, but a deep-seated, phenomenon, depending on its internal structure; and, since no variation of outside medium can affect *internal* wave-length, the dispersive power of a prism may be assumed constant for given waves. It follows that the dispersion caused by a given prism, immersed in different media, is simply proportional to the mean deviation in each case for given kind of light.

But what about the effect of motion?

If only we can assume that the prism interferes with the ether as little as the grating has been supposed to do, then all that has been said of the grating remains true of the prism. If we supposed the prism to modify the free ether inside it, we should have to modify this statement. On the hypothesis of FRESNEL, however, the free ether is not supposed to be affected; and experiments directed to test the matter, by ascertaining the effect of prism chasing a source at the same speed, have resulted in finding this effect zero, in accordance with the above statement. Hence it must be allowed that a Doppler effect observed by a prism depends *really* on wave-length, but *apparently* on frequency, just as is the case with a grating.

It must be noticed that the observation of a Doppler effect by a prism depends entirely on dispersion; *i.e.*, on waves of different length being affected differently. But prisms can be constructed whose dispersion is corrected and neutralized. Such achromatic prisms, if perfectly achromatic, will treat waves of all sizes alike; and, accordingly, the shortening of the waves from a moving source will not produce any effect. Achromatic prisms will behave to terrestrial and to extra-terrestrial sources, *i.e.*, to relatively stationary and relatively moving sources, in the same way.

ARAGO used an achromatic prism on a star when he showed that refractive index was unaffected by motion of the earth.* In criticising ARAGO's experiment adversely, MASCART forgets this, and thinks he ought to have perceived a Doppler effect. MASCART used a terrestrial source and an ordinary dispersive prism, when he experienced the same negative result. MAXWELL sent light both ways through his prism,

* BABINET, 'Comptes Rendus,' vol. 9, p. 774 (1839). ARAGO, 'Ann. de Chim. et de Phys.' (3), vol. 37, p. 180 (1853). MAXWELL, 'Phil. Trans.' (1868), vol. 158, p. 532; also 'Ency. Brit.' article "Ether." HOEK, 'Archives Néerlandaises' (1869), vol. 4, p. 443. MASCART, 'Annales de l'Ecole Normale,' vols. 1 and 3 (1872 and 1874); Professor MASCART here describes a large number of negative experiments which he has made as to the effect of motion on most of the phenomena of optics.

and therefore neutralized all refraction, except what was entirely caused by motion, when he proved that this latter was *nil.*

BABINET, HOEK, and MASCART, all tried a modified form of the same experiment in an interferential manner, and likewise got a negative result.

58. If we wish to follow out the ether motion through a prism into greater detail we can say:— Let the prism advance with velocity v to meet the waves, and let the ether in it be carried forward with velocity kv, then the virtual velocity of the light towards the prism is $V + v$, and inside the prism is

$$\frac{V + v}{\mu} - kv;$$

hence, on ordinary notions of refraction the new index will be

$$\mu' = \frac{(V + v)\mu}{(V + v) - k\mu v};$$

or

$$\frac{\mu}{\mu'} = 1 - k\mu\alpha,$$

where

$$\alpha = v/(V + v) = d\lambda/\lambda,$$

or

$$d\mu = k\mu^2\alpha,$$

which, on FRESNEL's hypothesis, equals $(\mu^2 - 1)\alpha$.

This seems to give a sort of theory of dispersion for the case:—

$$\frac{d\mu}{\mu^2 - 1} = \frac{d\lambda}{\lambda},$$

or

$$\frac{\mu - 1}{\mu + 1} = A\lambda^2,$$

or

$$\mu = \frac{c^2 + \lambda^2}{c^2 - \lambda^2}.$$

Interference Effects with Rays at Different Angles to Ether Drift. Effects of Normal Reflexion. Further Discussion of the Theory of Mr. MICHELSON's *Experiment.*

59. The experiment of MICHELSON, already referred to in § 25, has to do with the effect of a plane mirror sending a ray straight back upon itself. Consider the aspect of the mirror necessary to do this; first, for the case of a moving source in a stationary medium (fig. 15).

Let S_1 be initial position of the source, throwing off a wave-front to M, and itself moving to S, so that

$$S_1S = vT_1; \text{ and } S_1M = r_1 = VT_1.$$

Let SM now be reflected at M so as to travel to S_2, and reach it the same time as source, then

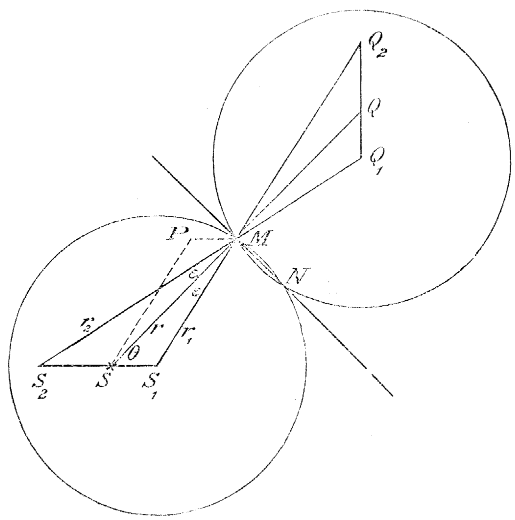
$$SS_2 = vT_2; \text{ and } MS_2 = r_2 = VT_2.$$

Hence

$$\frac{S_1S}{SS_2} = \frac{T_1}{T_2} = \frac{r_1}{r_2},$$

wherefore SM, bisects the angle S_1MS_2, or the angles of incidence and reflexion are precisely equal, and the required mirror is normal to SM, but is not tangential to the wave-front.

Fig. 15.



Details of normal reflexion with moving source or moving medium.

Let Q_1, Q, Q_2 be the images in this mirror of S_1, S, S_2, and with centre Q_1 construct a circle of radius QM cutting the wave-front in N; MN is the mirror able to throw a light ray back on the moving source.

A stationary observing telescope will observe the source along Q_2M; one moving with the source will observe it along QM, that is, in its true direction at moment of vision. The colour will change by the amount log r_1/r, as already said; and, as for any possible interference effect, the fringes will shift by an amount depending on

$$\frac{T_1 + T_2}{2T} = \frac{r_1 + r_2}{2r} = \frac{\cos \epsilon}{1 - \alpha^2} = \frac{\sqrt{(1 - \alpha^2 \sin^2 \theta)}}{1 - \alpha^2},$$

which gives very approximately

$$T_1 + T_2 - 2T \approx \alpha^2 T (1 + \cos^2 \theta).$$

There is, therefore, always a lag of phase caused by the motion, which cannot be made negative, or even zero, but which is a minimum when the motion is across the line of light, and a maximum when along it; being, indeed, twice as great for motion along as it is for motion across. Supplementary angles give the same effect.

One may express the fact by saying that the virtual distance the light has to go is S_1Q_2, or S_2Q_1, instead of SQ.

Case when the Mirror Moves too.

60. It is observed that in this investigation the mirror has been supposed stationary with respect to the medium, it is therefore possible, if the mirror is moving at the same rate as the source, *e.g.*, if they were both fixed and the medium streaming past both, that the circumstances may be a little different; because since the whole of the wave-front does not strike quite simultaneously, there may be time for some effect to occur during its period of contact, short though of course it is. Not even for normal incidence is the time of impact of a finite portion of a wave infinitesimal; for even when the source is infinitely distant (or when a collimating lens is used) it has to be remembered that the waves are not normal to the rays in a moving medium, and that, accordingly, when the incidence of the ray is normal and the medium movement not normal, the wave is inclined to the surface, a minute, but possibly important, angle.

But in MICHELSON'S arrangement the ray is not exactly normal on the tangentially moving mirror, but is inclined so that the mirror is precisely parallel to the wave-front; and so the time of contact is nothing on either mirror.

The statement of theory, therefore, proceeds as follows, without apparent error.

Let S be and remain the position of the source, and let a mirror MN (fig. 15) be arranged normal to SM, so as to send a ray back upon itself, when everything is stationary, in time 2T.

It is required to find if any tilt must be given to the mirror to send the ray back upon itself when the medium is moving, and whether a different time will be taken in the journey.

While light travels from S to M the wave-front's centre has drifted to S_1, and, accordingly, it strikes the mirror obliquely and is reflected as if coming from Q_1; hence it would travel towards S_2 but for the drift. The drift will carry it precisely back to S if $SS_2 = vT_2$, T_2 being the time of the return journey; just as $SS_1 = vT_1$, when T_1 is the time of the outward journey.

Hence, no tilt whatever is required by the mirror, but it reflects the light back upon itself just as when the medium was stationary, and the distance really travelled is exactly the same as it was, viz., $2r$. The velocity of the light is, however, different on the out and in journeys, for

$$V_1 = V \cos \epsilon - v \cos \theta,$$
$$V_2 = V \cos \epsilon + v \cos \theta,$$

so

$$T_1 + T_2 = \frac{r}{V_1} + \frac{r}{V_2} = \frac{2T \cos \epsilon}{1 - \alpha^2} = 2T \frac{\sqrt{(1 - \alpha^2 \sin^2 \theta)}}{1 - \alpha^2},$$

as before (§ 59).

61. In the actual experiment, as performed for instance by MICHELSON, it is natural to use a collimator and plane waves, and since his null result is very surprising and remarkable, it may be as well to examine whether the introduction of the lens pro-

duces any disturbance. At first I thought the lens and glass slabs used by him might possibly be the cause of his failure to get any result, because the ray across the motion travels through the glass obliquely, while along the motion it travels normally. But a little consideration shows that both along and across the motion the effect of the glass would be to increase the lag in a simply proportional manner to the previous lag. And calculation gives as the time of the journey, when a total thickness z of glass is interposed, using FRESNEL'S theory that the speed of the ether inside the glass is $1/\mu^2$ of what it is outside,

$$T_1 = \frac{r - z}{V \cos \epsilon + v \cos \theta} + \frac{z}{\frac{V}{\mu} \cos \epsilon + \frac{v}{\mu^2} \cos \theta},$$

and T_2 = corresponding expression with v negative.

So

$$T_1 + T_2 = \frac{2(r - z) \cos \epsilon}{V(1 - \alpha^2)} + \frac{2\mu z \cos \epsilon}{V\left(1 - \frac{\alpha^2}{\mu^2}\right)} = \frac{2T \cos \epsilon}{1 - \alpha^2} + \frac{2\mu z \cos \epsilon}{1 - \alpha^2}\left(\frac{1}{1 - \frac{\alpha^2}{\mu^2}} - \frac{1}{1 - \alpha^2}\right)$$

where

$$T = \frac{r + (\mu - 1) z}{V};$$

wherefore the effect of introducing glass is to increase the lag, but not quite so much as by the equivalent of the extra distance thus virtually added, the second term in the above expression being negative; but the diminution is independent of direction, except when fourth powers of aberration magnitude are attended to. Neglecting these, the effect of the glass is merely to cause, in addition to the lag naturally to be expected, an extra term, independent of direction, of this value:

$$-\frac{2\mu z}{V}\left(1 - \frac{1}{\mu^2}\right)\alpha^2.$$

MICHELSON'S *Interference Experiment in a Different Medium.*

62. Indeed, the simplest plan would be to consider the effect of immersing the whole arrangement in a different medium. It is merely to change the light velocity V to V/μ, and its mechanical velocity v to v/μ^2 the ethereal velocity inside it. Consequently α becomes α/μ.

The aberration angle ϵ changes to ϵ', such that

$$\sin \epsilon' = \frac{\alpha}{\mu} \sin \theta,$$

and the lag

$$\frac{2r \cos \epsilon}{V(1 - \alpha^2)} \quad \text{becomes} \quad \frac{2\mu r \cos \epsilon'}{V(1 - \alpha^2/\mu^2)},$$

approximately μ times as great as before. But although this is the case, *the extra lag caused by motion* is not so great inside the medium as it was *in vacuo*, for

$$\frac{2\mu r \cos \epsilon'}{V\left(1 - \frac{\alpha^2}{\mu^2}\right)} - \frac{2r \cos \epsilon}{V(1 - \alpha^2)} = \frac{2r \cos \epsilon}{V(1 - \alpha^2)} \left(\frac{\mu \cos \epsilon' (1 - \alpha^2)}{\left(1 - \frac{\alpha^2}{\mu^2}\right) \cos \epsilon} - 1 \right),$$

or, approximately,

$$= \frac{2(\mu - 1) r \cos \epsilon}{V(1 - \alpha^2)} - \frac{2\mu r}{V} \left(1 - \frac{1}{\mu^2}\right) \alpha^2.$$

[The conclusion here is that whatever may be the effect of a dense medium it is independent of θ, and therefore can have nothing to say to MICHELSON's experiment, which entirely depends on a difference between what can be observed with $\theta = 0$ and $\theta = 90°$.—July, 1893.]

The Laws of Reflexion and Refraction as modified by Motion.

63. It is necessary now to enter on the somewhat thorny question as to the effect of motion upon the laws of reflexion and refraction. FRESNEL by considering some special cases satisfied himself that no discrepancy need be expected on his version of the undulatory theory; and Sir GEORGE STOKES, examining the question in a more general manner in 1846, proved that, at least as far as the first order of minutiæ, the laws were obeyed in spite of any relative motion between mirror and medium (motion of source has obviously nothing to do with it, unless it affects the shape of the incident wave). And the long continued use of artificial horizons by astronomers shows that there has been no practical doubt on the subject, at least as far as reflexion is concerned.

But these statements do not by any means exhaust the subject; the law of reflexion is *not* precisely obeyed in a moving medium, and recently MICHELSON has proposed to utilize the theoretical error (which has never yet been practically realized) as a fresh method of attacking the problem of the relative motion of the ether and the earth.

I propose, therefore, to enter upon it, and I must confess that though the results are easily stated, they have given me much trouble to be sure of, and I have found a good many mare's nests by the way.

The reasoning for reflexion and refraction is much the same, and I attend more pronouncedly to reflexion because without assuming FRESNEL's theory as to the motion of ether inside dense matter we have no guide to what shall happen in refraction;

and although the theory has been to a certain extent, and with fairly high accuracy, verified, yet it can hardly be yet said to have a secure rational basis.

In a drifting medium we must draw a clear distinction between waves and rays; the laws obeyed by one need not be obeyed by the other, for they are inclined to each other, and may become differently inclined after reflexion or refraction.

Now it is pretty plain that if motion is to have any effect upon these aberration angles, the rays must be differently inclined to the direction of drift; and on the other hand, if motion is to affect the reflexion of waves, that it must act during the period of contact of a wave with the reflecting surface; so that if a wave comes down plumb it will rebound as it comes, because its time of contact is then infinitesimal and no finite motion could cause any disturbance. But even in this case of normal incidence the law of reflexion need not be obeyed for rays, for they are not normal to the waves, and will be differently inclined to the direction of drift, unless indeed the latter be either normal or tangential.

64. The following are statements which I will afterwards justify :—

(1) The planes of incidence and reflexion are always the same.

(2) The angles of incidence and reflexion, as measured between rays and normal to surface, in general differ.

(3) If the mirror is stationary and medium moving, they differ by a quantity depending principally on the square of aberration magnitude, *i.e.*, by one part in a hundred million, and a stationary telescope would be able to observe the effect, if it were delicate enough.

(4) If the medium is stationary and mirror moving, the angles differ by a quantity depending principally on the first power of aberration magnitude, *i.e.*, by one part in ten thousand, but a telescope moving with the mirror will not be able to observe this large effect; for the apparent (or commonplace) aberration caused by the motion of the receiver will obliterate the odd powers and leave only the even powers of the aberration, so that the observed effect should be the same as in case 3.

(5) As regards the angles which the reflected and incident waves make with the surface, *i.e.*, as to the obedience to the law of reflexion shown by *waves* instead of by *rays*, in case 3 the angles differ by an amount depending on the first order of aberration, but in case 4 they only differ by the square of this quantity.

(6) At grazing incidence the ordinary laws are accurately obeyed by the rays as observed, and at normal incidence the error is a maximum.

(7) The ordinary laws are obeyed whenever the direction of motion is tangential or normal to the mirror.

(8) In general the shape of the incident waves is not precisely preserved after reflexion, so that, when spherical waves impinge on a mirror in a moving medium, the reflected waves from a plane mirror diverge from a sort of caustic instead of from a point, and the position of the image varies (but almost infinitesimally) with the position of the observer. In other words, such a mirror acts to a parallel beam as if

slightly tilted, to a divergent beam as if slightly curved. But either effect, as observable in the result, is almost hopelessly small.

(9) Similar statements are true for refraction.

65. In considering a plane mirror in a drifting medium it is very tempting to image the direction of drift of successive wave centres (fig. 4); in which case everything will be symmetrical, and the law of reflexion will be obeyed altogether, by both waves and rays, in the simplest possible manner. But a little thought shows that this is illegitimate, for it would make the reflected waves assisted in their progress by the reflected drift just as much as the incident waves are assisted; whereas they are really travelling in the teeth of the wind, their progress being impeded and their wave-length shortened just as much as the incident waves are helped and lengthened (or of course *vice versâ*). Plainly the drift is not reflected, but must be supposed to act on the waves emitted by the image exactly as it acts on the waves emitted by the source.

Another tempting thing to do is to start a system of waves from source and its ordinary image simultaneously, both subject to precisely the same drift velocity, one being the incident, the other the reflected system. But applying this, and taking a pair of waves intersecting at any one point of the mirror, it will be found they have not travelled the same distance to get there, nor have they taken the same time, and the drift of their centres has been different. Moreover, they do not intersect at a second point of the same wave, and, in fact, the system behind the mirror is not in any sense the image of the front set.

The really essential thing is that the phase of the reflected wave shall be identical with that of its incident exciter at the point of contact with the mirror, and accordingly that the time of virtual journey from any point to be considered as an image is to be equal to the time of journey from the corresponding point of the source. Nothing less direct or more geometrical than this seems satisfactory, so it had better be applied in its usual Huyghenian baldness. At the same time a little caution is necessary in using HUYGHENS' construction in a moving medium, for the centre of the elementary waves does not remain at the point of incidence, but drifts away, as in fig. 4, and the construction has to remember this, or it will go wrong.

Laws of Reflexion and Refraction in a Drifting Medium.

66. Since the direction of drift need not be in the plane of incidence, it will be convenient to resolve it into two components, respectively in and perpendicular to that plane, and consider them separately.

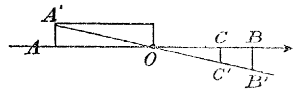
Component of Drift Perpendicular to Plane of Incidence.

The perpendicular component is very easily disposed of, as was shown by Sir GEORGE STOKES.* For looking down the normal to the mirror we shall see the

* 'Math. and Phys. Papers,' vol. 1, p. 144.

incident and reflected rays AO and OB, incident at the point O, affected by the drift in such a way that their direction is A′O and OB′ respectively; but they are still both in a plane normal to the surface, and they still make equal angles with the surface.

Fig. 16.



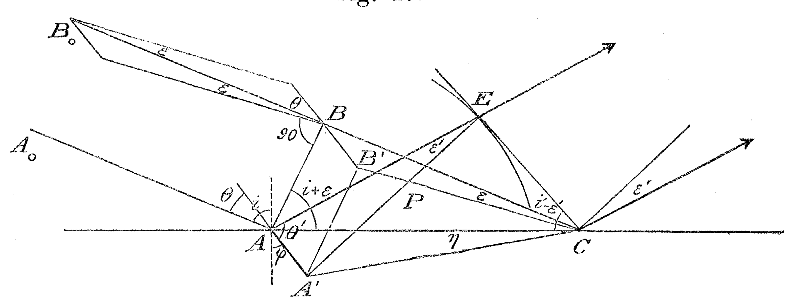
And as regards refraction, an equal length of the refracted ray will be foreshortened into $OC = OB/\mu$; and the drift inside the medium being, according to FRESNEL'S theory, BB'/μ^2, will in the longer time available carry C to C′, such that OC′B′ remains a straight line; accordingly, the plane of the refracted ray is still the plane of incidence. Hence the perpendicular component of the drift is wholly ineffective, and we have only to consider the component in the plane of incidence; call it v.

Component of Drift in Plane of Incidence.

Consider a plane wave A_0B_0 travelling, with an aberration angle ϵ between its normal and its direction of advance caused by a drift in direction AA′, towards a plane mirror, at a pace compounded of the speed of light and of the drift, viz., $V \cos \epsilon + v \cos \theta$.

Let incident and reflected rays make angles i and i' respectively with the mirror-normal, and θ and θ' with the line of drift, so that if ϕ is the inclination of the drift direction to the mirror-normal, $\theta = i - \phi$, $\pi - \theta' = i' + \phi$.

Fig. 17.



The incident and reflected waves will be inclined to the surface by the angles $i + \epsilon$, $i' - \epsilon'$, respectively.

When the wave AB strikes the mirror, A becomes a source of radiation, and B travels on to C, with a velocity compounded of BB′ and B′C. By the time B arrives at C the radiation from A will have spread a distance equal to B′C through the medium, and the centre of a wave with this radius will have had time to drift with the moving medium to A′, a distance equal to BB′; hence, drawing the semicircle A′B′EC, and choosing on it a point E, such that A′E = B′C, we have the direction of the reflected ray, viz., AE, and of the reflected wave CE.

67. It is easy to see that the triangle PA′C is isosceles, and accordingly that the angle A′CA is equal to half the difference of the inclinations of incident and reflected waves to the mirror surface ; *i.e.*, calling this angle η,

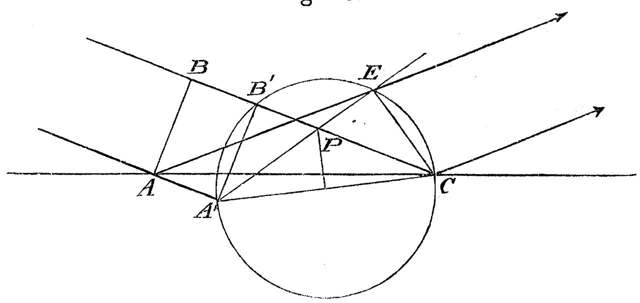
$$2\eta = (i + \epsilon) - (i' - \epsilon'),$$

or

$$\eta = \frac{\epsilon + \epsilon'}{2} + \frac{i - i'}{2},$$

hence the *wave* is reflected precisely as if the mirror were rotated through the angle η and there were no drift ; the angle of virtual rotation being very approximately the mean of the aberration angles.

Fig. 18.



The first approximation to its value is

$$\eta = \alpha \sin i \cos \phi ;$$

it practically vanishes, therefore, for normal incidence and for tangential drift.

Further, as regards the change of width of the beam or distance between the rays, it is apparent that measured along the wave-surface it is the same, because EC = A′B′ = AB ; so measured perpendicularly it changes in the ratio of $\cos \epsilon : \cos \epsilon'$ before and after reflexion.

68. It is not to be supposed that the *ray* is reflected after this manner ; and, in fact, we shall find that the error of ray-reflexion, or difference between angle of incidence and reflexion, $i - i'$, is exceedingly small.

To determine this difference, and the whole circumstances of the problem, we write down the following equations, obvious from figure 17 :

$$\frac{\sin \epsilon}{\sin \theta} = \frac{\mathrm{BB'}}{\mathrm{B'C}} = \frac{v}{\mathrm{V}} = \alpha = \frac{\mathrm{AA'}}{\mathrm{A'E}} = \frac{\sin \epsilon'}{\sin \theta'},$$

$$\theta = i - \phi, \qquad \theta' = \pi - (i' + \phi).$$

Also, for the time of journey of the wave from position AB to EC,

$$t = \frac{\mathrm{BB'}}{v} = \frac{\mathrm{B'C}}{\mathrm{V}} = \frac{\mathrm{BC}}{\mathrm{V}\cos\epsilon + v\cos\theta} = \frac{\mathrm{AE}}{\mathrm{V}\cos\epsilon' + v\cos\theta'} = \frac{\mathrm{A'E}}{\mathrm{V}} = \frac{\mathrm{AA'}}{v}.$$

Lastly

$$\frac{BC}{AC} = \frac{\sin(i + \epsilon)}{\cos \epsilon}; \qquad \frac{AE}{AC} = \frac{\sin(i' - \epsilon')}{\cos \epsilon'}.$$

These solve the problem, and they may be conveniently worked on the following lines—

$$\sin \epsilon = \alpha \sin (i - \phi); \qquad \sin \epsilon' = \alpha \sin (i' + \phi),$$

$$\frac{\sin i' - \cos i' \tan \epsilon'}{\sin i + \cos i \tan \epsilon} = \frac{AE}{BC} = \frac{V \cos \epsilon' + v \cos \theta'}{V \cos \epsilon + v \cos \theta} = \frac{\cos \epsilon' - \alpha \cos (i' + \phi)}{\cos \epsilon + \alpha \cos (i - \phi)} = \frac{\cos \epsilon - \alpha \cos (i - \phi)}{\cos \epsilon' + \alpha \cos (i' + \phi)},$$

the last equality being added for convenience, and being true because

$$\cos^2 \epsilon - \alpha^2 \cos^2 \theta = 1 - \alpha^2.$$

Therefore, exactly,

$$\sin i \cos \epsilon - \tfrac{1}{2} \alpha^2 \cos i \sin 2 (i - \phi) \sec \epsilon = \sin i' \cos \epsilon' - \tfrac{1}{2} \alpha^2 \cos i' \sin {}^2 (i' + \phi) \sec \epsilon',$$

whence, expanding $\cos \epsilon$ and neglecting α^4,

$$\sin i - \sin i' = \alpha^2 \cos^3 i \sin 2\phi,$$

or

$$i - i' = \alpha^2 \cos^2 i \sin 2\phi.$$

The discrepancy between the angles of incidence and reflexion (which I call for brevity the error of reflexion) is therefore exactly expressible in even powers of aberration magnitude, and no part of it reverses with the reversal of the ray. It vanishes for grazing incidence, and is a maximum for normal incidence (at which I am somewhat surprised). It vanishes both for tangential and for normal drift, being a maximum when the medium drifts at 45° to the mirror.

The maximum possible value of the error of reflexion is α^2, or 10^{-8} of a radian, or $0''{\cdot}00205$, or $\frac{1}{500}$th of a second of arc; an amount which, although equivalent to an error of only 15 inches in the circumference of the earth, it is perhaps possible to detect; especially if, as Mr. MICHELSON suggests, it be increased by multiple reflexion. Indeed, it strikes me as perhaps the simplest way of examining into the motion of the ether near the earth.

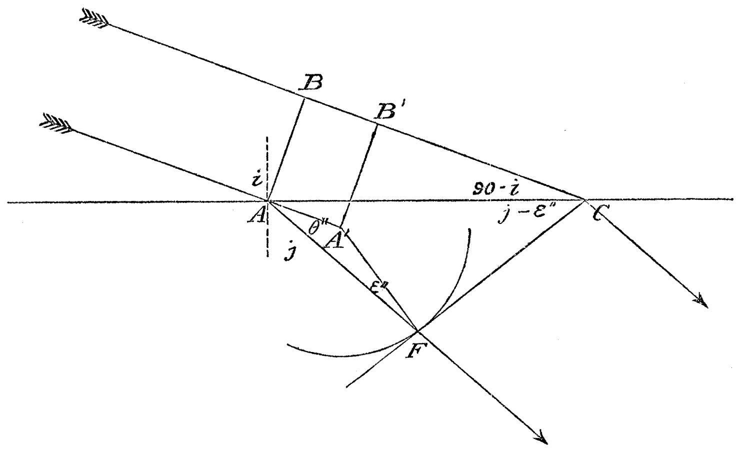
Refraction in a Drifting Medium.

69. The reasoning for refraction is precisely of the same kind, and there needs nothing more but to write down the equations, putting V/μ everywhere instead of V, v/μ^2 instead of v, and consequently α/μ instead of α.

It is best thus to assume FRESNEL'S theory, and leave observation to point out any deviation from it that may be existent.

A separate figure may save confusion; and though the general case is easily drawn (like fig. 17) a special case serves better for illustration, and I depict the case of drift along incident ray.

Fig. 19.



θ'' and ϵ'' are the angles between refracted ray and the drift direction and wave-normal respectively; the angle of refraction, defined as usual, may be called j; so that

$$\theta'' = \phi - j,$$

$$\sin \epsilon'' = \frac{\alpha}{\mu} \sin \theta'',$$

$$t = \frac{\mathrm{AA'}}{v/\mu^2} = \frac{\mathrm{A'F}}{\mathrm{V}/\mu} = \frac{\mathrm{AF}}{\mathrm{V}/\mu \cos \epsilon'' + v/\mu^2 \cos \theta''},$$

$$\frac{\mathrm{AF}}{\mathrm{AC}} = \frac{\sin (j - \epsilon'')}{\cos \epsilon''}.$$

These are the equations to be used in conjunction with the previous set, and so it follows that

$$\frac{\sin j - \cos j \tan \epsilon''}{\sin i + \cos i \tan \epsilon} = \frac{\mathrm{AF}}{\mathrm{BC}} = \frac{1}{\mu} \frac{\cos \epsilon'' + \alpha/\mu \cos \theta''}{\cos \epsilon + \alpha \cos \theta} = \frac{1}{\mu} \frac{\cos \epsilon - \alpha \cos (i - \phi)}{\cos \epsilon'' - \alpha/\mu \cos (j - \phi)}.$$

Wherefore

$$\sin i \cos \epsilon - \alpha \sin \phi - \alpha \tan \epsilon \cos i \cos (i - \phi)$$
$$= \mu \left\{ \sin j \cos \epsilon'' - \frac{\alpha}{\mu} \sin \phi + \frac{\alpha}{\mu} \tan \epsilon'' \cos j \cos (j - \phi) \right\}.$$

Or

$$\sin i \cos \epsilon - \mu \sin j \cos \epsilon'' = \tfrac{1}{2} \alpha^2 \left\{ \cos i \sin 2 (i - \phi) - \frac{1}{\mu} \cos j \sin 2 (j - \phi) \right\}.$$

Which shows that the difference between $\sin i$ and $\mu \sin j$, or the error of refraction, is likewise of the second order of aberration magnitude, *i.e.*, ordinarily speaking, nil;

its value being easily obtainable if ever wanted. The displacement of Fraunhofer lines due to the Sun's rotation is a small thing to detect with a prism spectroscope, but this effect of motion on terrestrial sources, if it is ever to be seen, is 660 times smaller.

70. It may, perhaps, be well to check over our results by the less geometrical method employed by Sir GEORGE STOKES, viz., by expressing the fact that the intersection of the three waves (incident, reflected, and refracted) with the mirror is a joint intersection, and runs along the mirror at a pace which can easily be written down (viz., AC/t); for the wave advances through the medium at a speed V, and the medium helps it along with a component of its drift velocity $v \cos(\theta + \epsilon)$; so the total speed of the joint wave intersection as it runs along the mirror is

$$\frac{V + v \cos(\theta + \epsilon)}{\sin(i + \epsilon)},$$

which it is easy to see is precisely the same as what we should have obtained by attending to rays and to the figure, viz.,

$$\frac{AC}{t} = \frac{V \cos \epsilon + v \cos \theta}{\sin(i + \epsilon)/\cos \epsilon}.$$

So the equations for reflection and refraction can be written down at once, thus:

$$\frac{V + v \cos(i - \phi + \epsilon)}{\sin(i + \epsilon)} = \frac{V - v \cos(i' + \phi - \epsilon')}{\sin(i' - \epsilon')} = \frac{V/\mu - v/\mu^2 \cos(j - \phi - \epsilon'')}{\sin(j - \epsilon'')},$$

together with the values of the aberration angles, obtained, say, by resolving the wave and drift velocities perpendicular to the ray, or resultant direction of advance, and expressing the fact that they must neutralize each other;

$$\frac{v}{V} = \frac{\sin \epsilon}{\sin(i - \phi)} = \frac{\sin \epsilon'}{\sin(i' + \phi)} = \frac{\mu \sin \epsilon''}{\sin(j - \phi)}.$$

These two sets of equations contain the entire solution, and of course μ may be written μ_2/μ_1 if it is a question of passage from one medium to another instead of from vacuum to a medium; the V and v then expressing speeds in first medium.

71. In Sir GEORGE AIRY'S* beautifully performed and described experiment of the value of the coefficient of aberration measured by a zenith sector full of water, there should, we see, on FRESNEL'S theory, have been a slight discrepancy, but one wholly too small to observe with the various inaccuracies inseparable from star-light. If it is to be detected it must be with light from a terrestrially fixed source. The obser-

* 'Phil. Mag.,' iv., vol. 43, p. 310.

vations of HOEK and others, performed with terrestrial light, aimed only at disproving KLINKERFUES' notion that an error proportional to the first power was to be expected, and did not aim at the immense delicacy needed to observe α^2.

Wave-length as altered by Reflexion.

72. Since the laws of reflexion are so closely obeyed, an image in a mirror will practically appear just the same whether the medium is stationary or not, and, accordingly, the image may be treated as the virtual source for all questions relating to wave-length and Doppler effect, and the waves coming from that image will in general be affected by the drift otherwise than are the waves coming from the source, because the direction is different.

For instance, sunshine strikes the earth perpendicularly to its motion, but reflected sunshine may coincide with the direction of motion, and, in that case, will have to travel against (or with) the ether wind precisely as if it came from a terrestrial source, and its wave-length will be affected as already reckoned; in other words, thinking of a mirror moving with the orbital motion of the earth only, considered as circular, the image of the Sun moves as if attached to the mirror (not at twice the rate), and, accordingly, reflected sunshine behaves as regards wave-length precisely as if it were coming from a terrestrial source. [More generally (*i.e.*, including eccentricity of orbit and aberration) reflected light *as seen by an observer moving with the mirror* appears in every respect like direct light.]

For irregular reflexion, *e.g.*, from white paper or from the Moon or a planet, these things can be treated as being themselves the sources.

Change of Phase caused by Reflexion in a Moving Medium.

73. Now consider the phase as affected by reflexion.

Consider the two parallel rays A and B, in fig. 17, distant b from each other, and let B lag initially by an amount $b \tan \epsilon$ behind A (§ 67), then, after reflexion, the distance apart has changed so that $b/\cos \epsilon = b'/\cos \epsilon' = c$ say, and the lag is $b' \tan \epsilon'$.

Hence the gain of lag by reflexion, $b' \tan \epsilon' - b \tan \epsilon$

$$= c(\sin \epsilon' - \sin \epsilon),$$

$$= -2\alpha c \cos \frac{i+i'}{2} \sin\left(\phi - \frac{i-i'}{2}\right),$$

which, very approximately,

$$= -2\alpha c \cos i \sin \phi.$$

For normal incidence and tangential drift it has its maximum value, $2\alpha b$.

Now whatever the initial lag may be, and it may be arbitrary, the final lag will differ from it by this same amount; and if the rays, instead of being parallel, are coincident in path, then no difference in phase is caused by reflexion.

Change of Energy at Reflexion from Moving Mirror

74. When reflexion takes place from a moving (receding) mirror, there is some work done on the mirror by reason of the intrinsic pressure of light. Calling the energy per unit volume $e + e'$, the energy of the incident light per second is $\mathrm{V}e$, and of the reflected $\mathrm{V}e'$, the pressure is $e + e'$, and the work done per second $(e + e')\,v$.

So

$$\mathrm{V}e - \mathrm{V}e' = (e + e')\,v,$$

or

$$e\,(1 - \alpha) = e'\,(1 + \alpha).$$

Or consider the mirror fixed and medium moving with source away from it, the speed of incident light is $\mathrm{V} - v$, of the reflected is $\mathrm{V} + v$, but no work is done; so

$$(\mathrm{V} - v)\,e = (\mathrm{V} + v)\,e'.$$

Wherefore, on either mode of consideration, the energy of the reflected light is from a receding mirror less, from an advancing mirror greater, than that of the incident light, in the ratio

$$\frac{e'}{e} = \frac{1 - \alpha}{1 + \alpha}.$$

Possible Effect of Light Pressure in Astronomy.

75. Light energy per unit volume on Mount Whitney, as determined by LANGLEY, amounted to 67 microbarads, or, say, in outside space, three quarters of 10^{-4} ergs per cubic centimetre; giving a pressure of the same number of dynes per sq. centim.

This pressure on the Moon is withdrawn during eclipses, but, although equal to the ordinary weight of 10,000 tons or so, it is too small to make sensible perturbations, as it could only push the Moon $\frac{1}{40}$th inch in a fortnight.

On a small body, however, it may become comparable with gravitation.*

On a small-enough dust particle, such as may be in tails of comets, the light pressure and gravitative attraction of the Sun might balance. I make the size about 1 micron diameter for a sphere of the density of water, at any distance. Anything smaller than this would be repelled, and would get up an excessive velocity in time.

[* I find that FITZGERALD made a communication years ago to the Royal Dublin Society on this subject.]

Direction of Motion of a place on the Earth.

76. Of all the motions to which the earth is subject its orbital motion is the largest, and is the most important for aberrational effects; but two others must not be overlooked, since they may introduce secular variations into the amount of those effects, viz., the diurnal rotation and the motion of the system through space.

The speed of the motion of the system is only approximately known, but it is estimated at 10·9 miles a second, or 1·75 million C.G.S., and its direction is completely specified by stating a point among the fixed stars.

The speed of the diurnal rotation is $\frac{\text{equatorial circumference}}{1 \text{ sidereal day}} \times \cos \text{latitude}$, or very small compared with that of light, and its direction is simply from west to east. It causes a variation in the observed total aberration, amounting to nearly 2 per cent. at the equator.

Both these motions are steady.

The orbital motion is not quite constant in speed, and goes through the whole plane cycle of directions, but its average value may be stated as $\frac{1}{10,000}$ that of light, and its direction is sufficiently expressed for practical purposes by saying that it is in the plane of the ecliptic, and at right angles to the Sun's direction. For instance, a half-moon is roughly in the line of the earth's orbital motion. We are moving as if going away from an increasing half-moon or towards a decreasing half-moon. Another way of putting the matter, is that at midnight the annual and diurnal motions approximately agree in direction, at midday they are opposed. At the epoch of the solstices the agreement is good, *i.e.*, the orbital motion at a solstice is from east to west at noon, from west to east at midnight; and at no time of the year is the error of this statement of very great practical import, for even at the equinoxes 91·7 per cent. of the motion is in the direction stated.

A clock might easily be made to point out the direction of orbital motion. By starlight it is never difficult to realize it, for there are usually planets enough to make the ecliptic manifest, and there is no difficulty in estimating whereabouts the Sun is. Hold a twenty-four hour watch in the plane of the ecliptic with its noon line pointing west, and its hour hand will constantly indicate the direction of the earth's orbital motion. The only difficulty is knowing where the plane of ecliptic is. Consider a terrestrial globe with its axis tilted $23\frac{1}{2}°$, and rotating by internal mechanism once in twenty-four hours. The plane of ecliptic is horizontal, and the direction of motion will be given by a pointer revolving once a year in a horizontal plane, or, more simply, by the appropriate radius of a horizontal card with 365 days of the year written round its circumference. With that alone, however, it would be a little puzzling to compare this slowly changing direction with the position of any given locality on the rotating earth. The whole might be turned by hand till the required locality came to the top, with the axis in the meridian, and then the pointer would agree with the direction of motion; or, more simply, there need be no globe at all, but simply a polar axis revolving once a day opposite to the earth, and carrying with it a dial with the names of the months and days recorded round its circumference, set on the axis at an obliquity of $23\frac{1}{2}°$, and adjusted once for all to coincide with the ecliptic. The date on the card will then point out the line of motion. The clock, if kept to G.M.T., would never give the motion more erroneously than a small correction analogous to the equation of time. By giving the card one step forward every 29th of February, it could be kept right until the whole thing wanted that $\frac{1}{260}$th part of a rotation per century about a vertical axis which precession demands. [My assistant, Mr. E. E. ROBINSON, has connected a clock through a HOOKE'S joint with a pointer which moves so as very fairly to indicate the direction of orbital motion at any instant.]

Electrical methods of detecting Motion through Ether.

77. It might perhaps appear possible that electrical methods may succeed in showing a first-order effect of terrestrial motion, since charged bodies in motion repel each other with modified force.

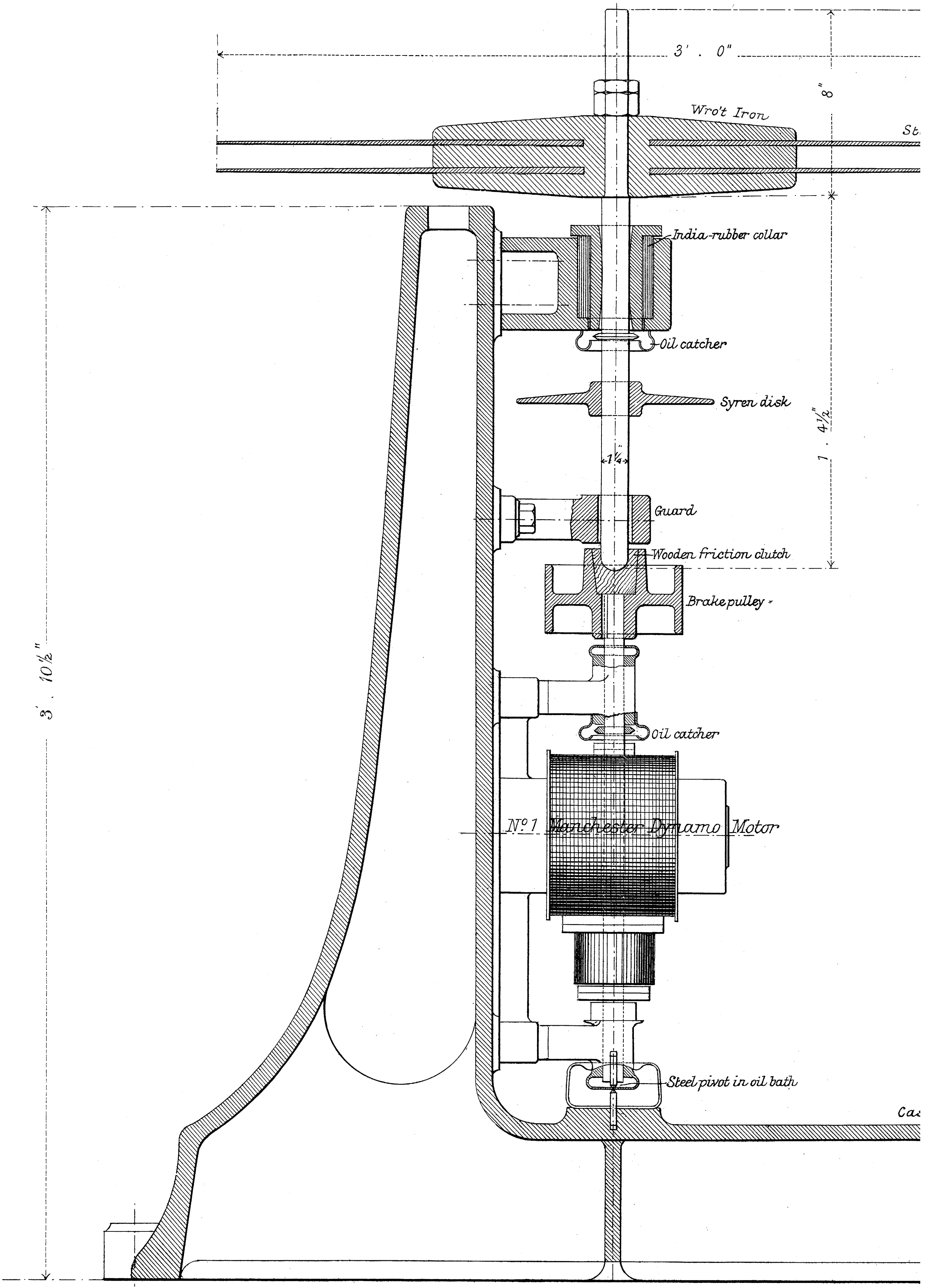
It is not possible to control or vary it except by combining the above several kinds of movement, and FITZGERALD has suggested a plan of observing whatever effect may be caused by the alternate agreement and disagreement between the earth's orbital motion and the solar system's proper motion: say by measuring the attraction of charged parallel plates at intervals of six months.

But, inasmuch as the force between charged bodies is independent of the direction of their motion, or (otherwise) because the electrical attraction between parallel moving charges depends on the product of their velocities, it must be the second-order of aberration magnitude that is really involved.

DESCRIPTION OF PLATES 31 AND 32.

Plate 31. Details of optical frame, showing the mode of supporting the mirrors, both the silvered and the semi-transparent.

Plate 32. Details of whirling machine, showing the pair of steel disks, 1 yard in diameter, driven by an electric motor.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Steel pivot in oil bath
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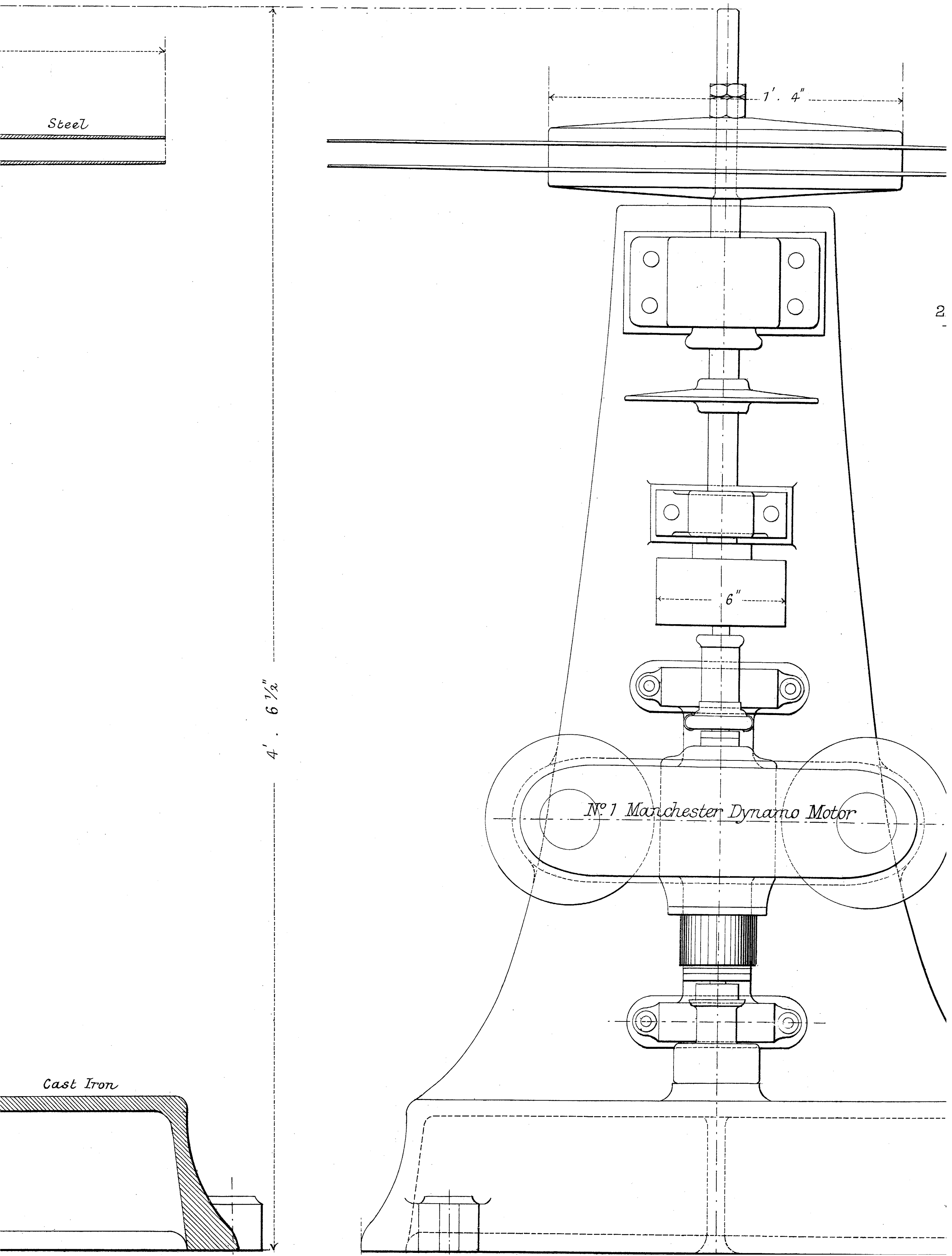
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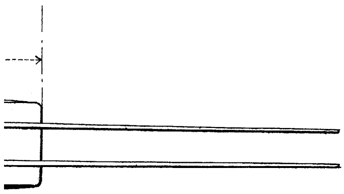
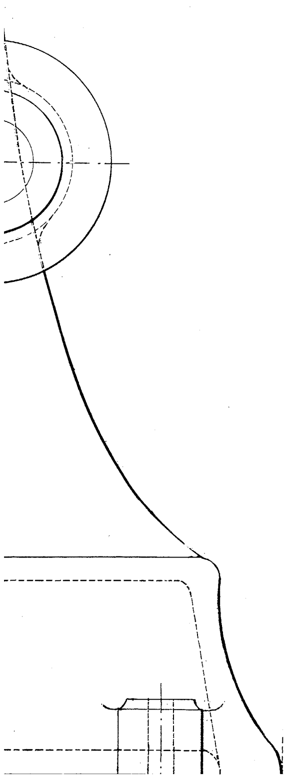
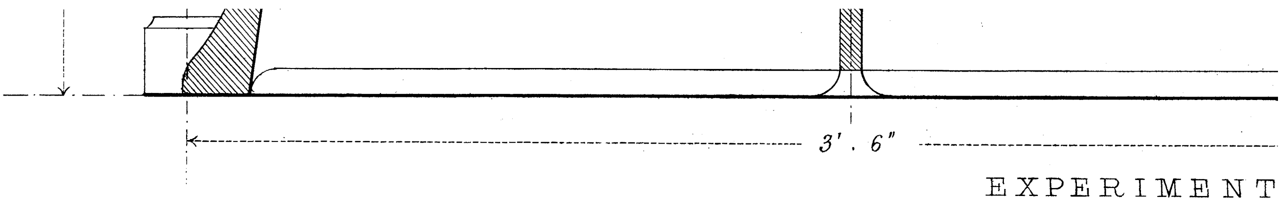
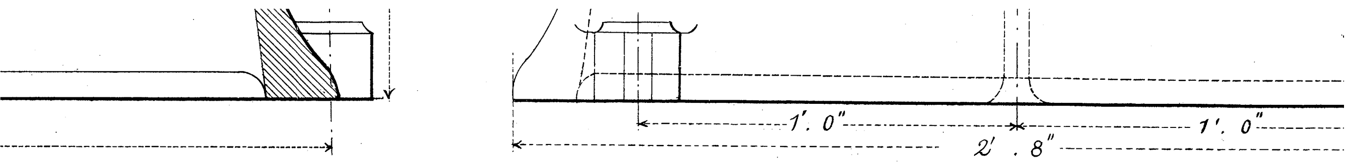


Scale
2 in. = 1 Foot

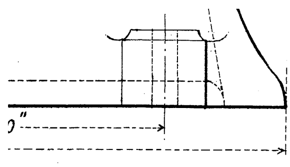




EXPERIMENT



ENTAL WHIRLING MACHINE.



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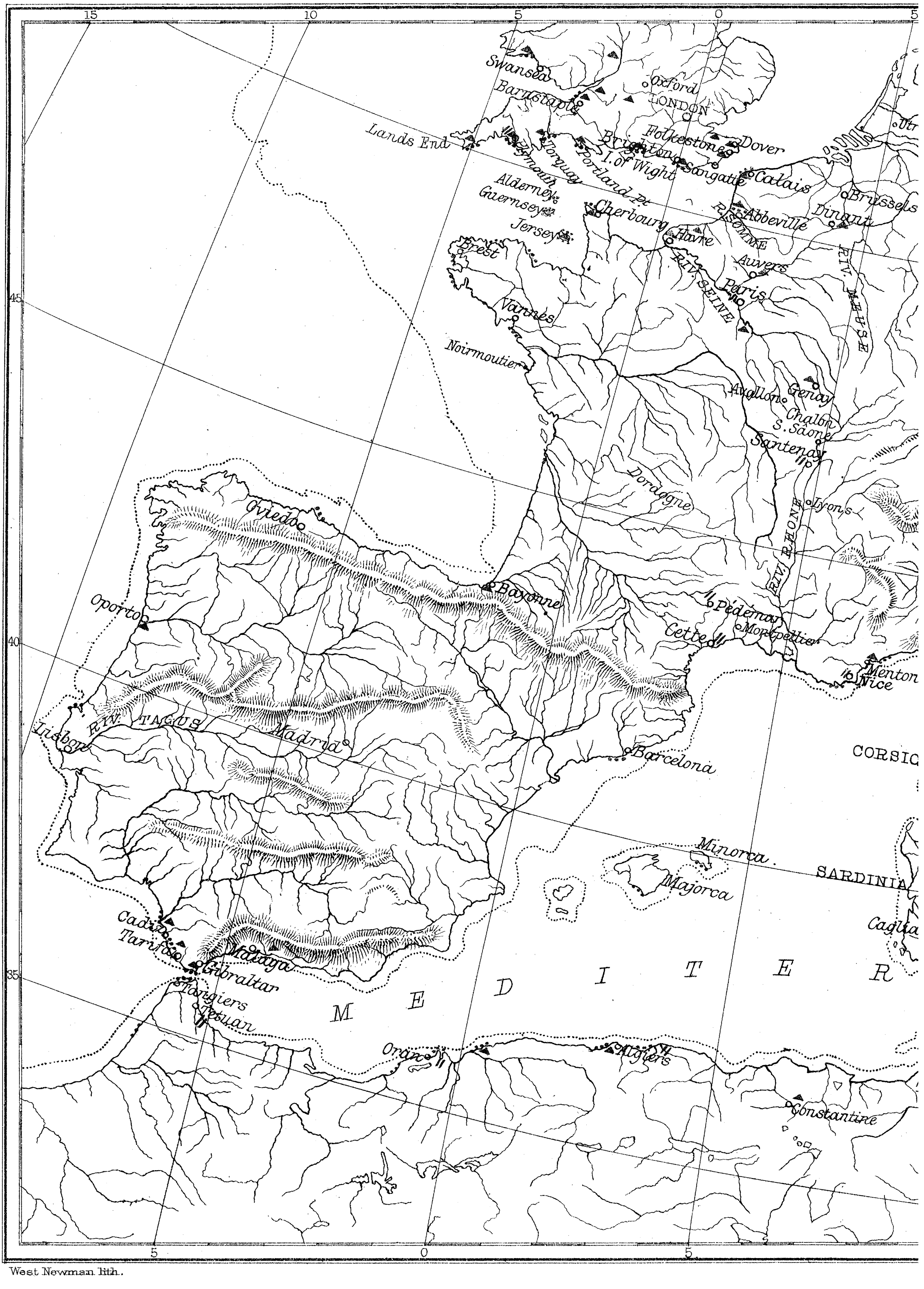

West Newman lith.

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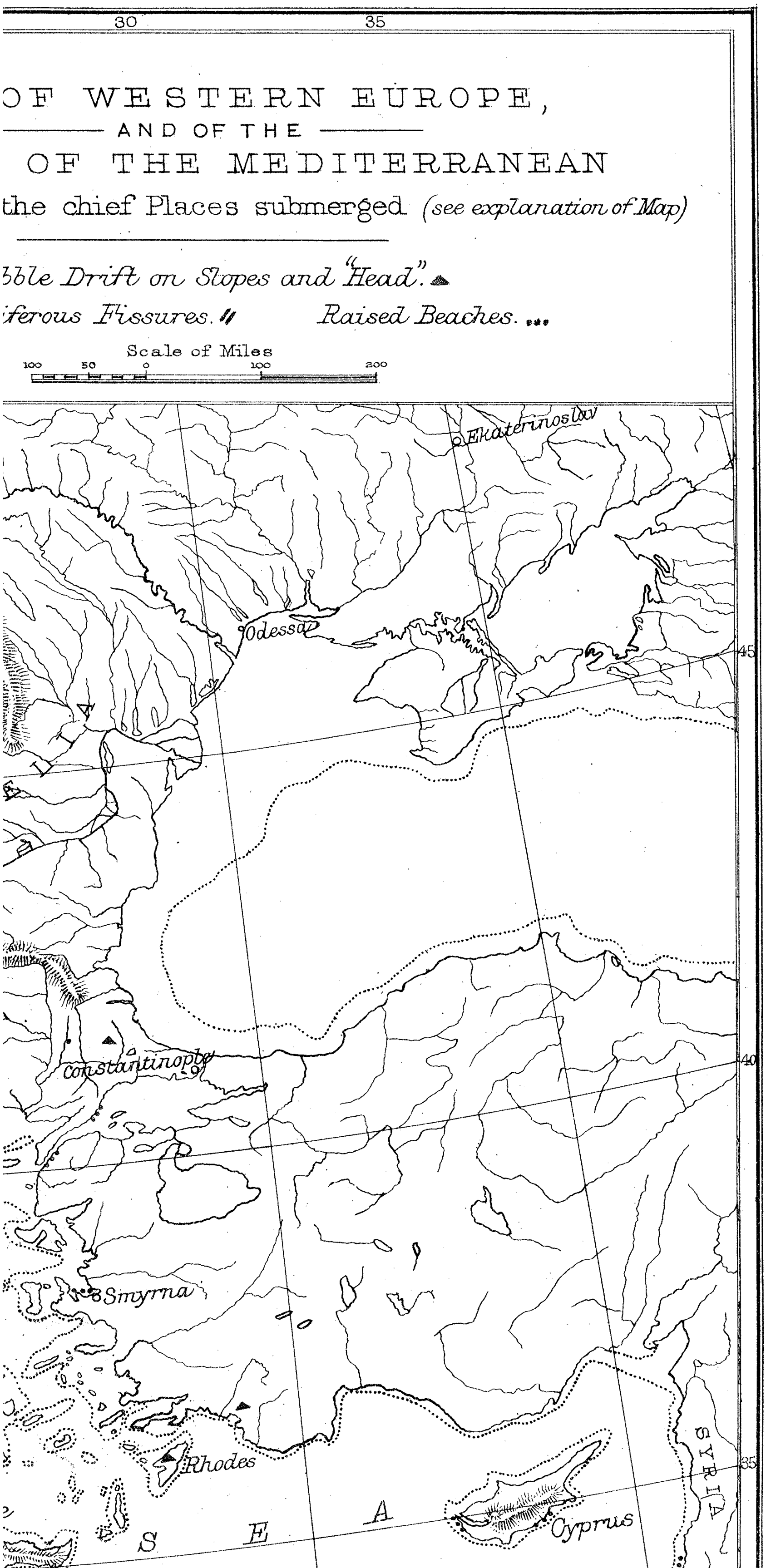
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West Newman lith.

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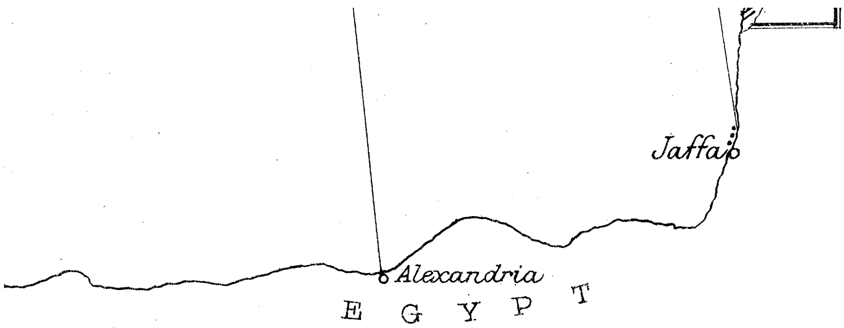
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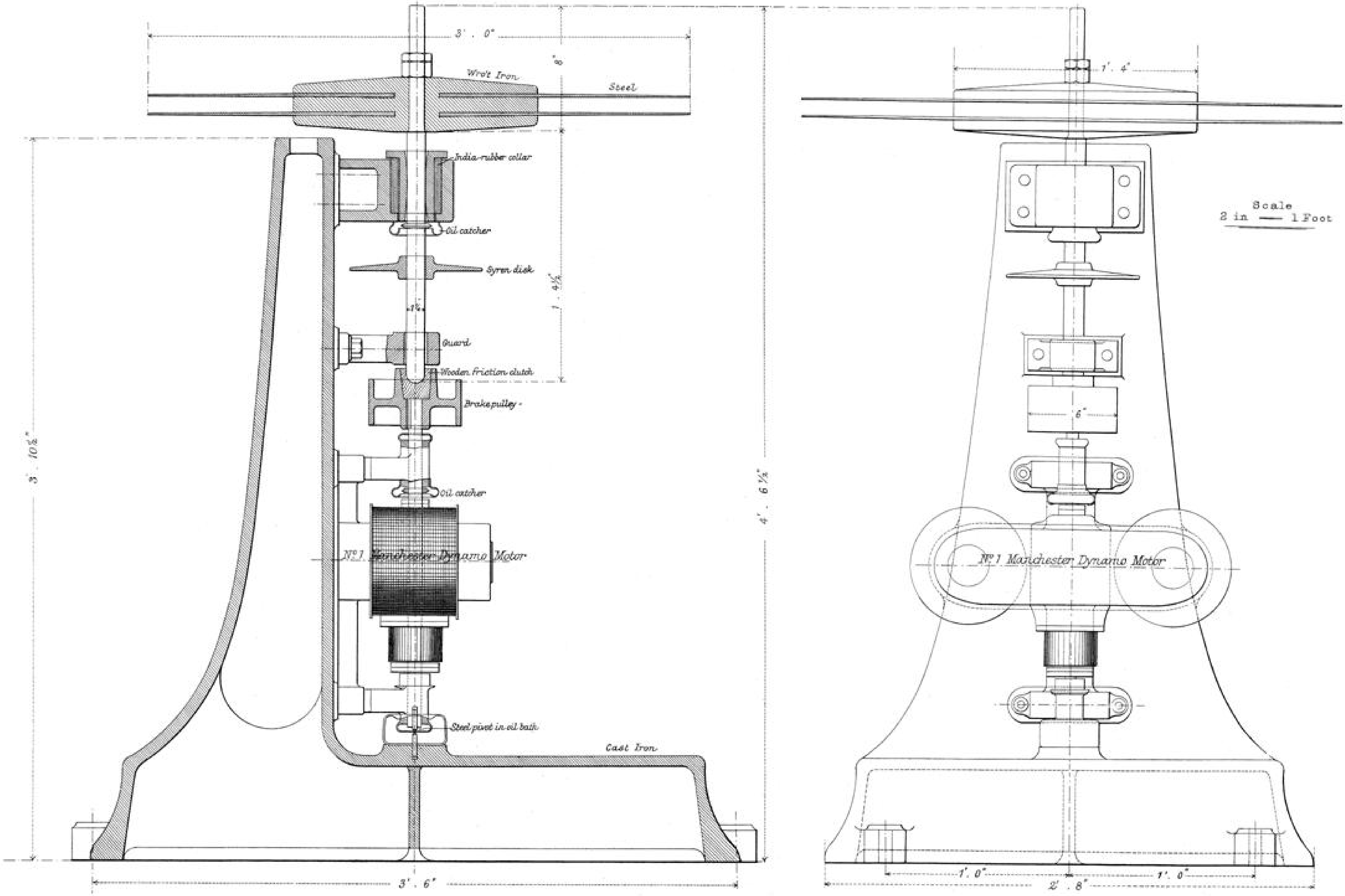

EXPERIMENTAL WHIRLING MACHINE.

West, Newman lith.

West, Newman lith.

West Newman lith.

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ROUMELIA
RIV. DANUBE
Genoa
Spezzia
Pisa
Leghorn
Mentone
Nice
Bastia
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Naples
Capri
SARDINIA
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MEDITERRANEAN
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OF WESTERN EUROPE,
AND OF THE
OF THE MEDITERRANEAN
the chief Places submerged (see explanation of Map)
bble Drift on Slopes and "Head".
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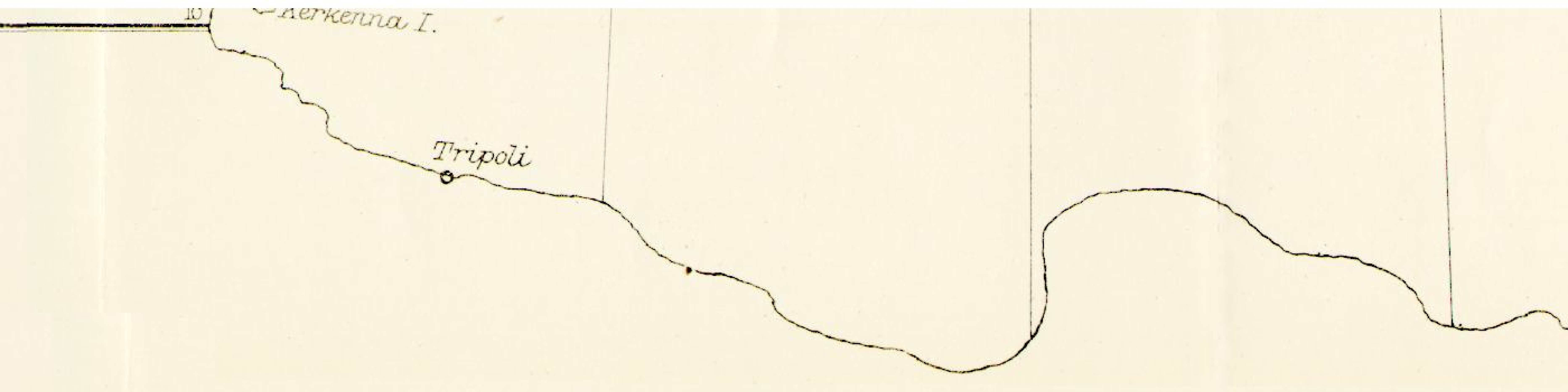

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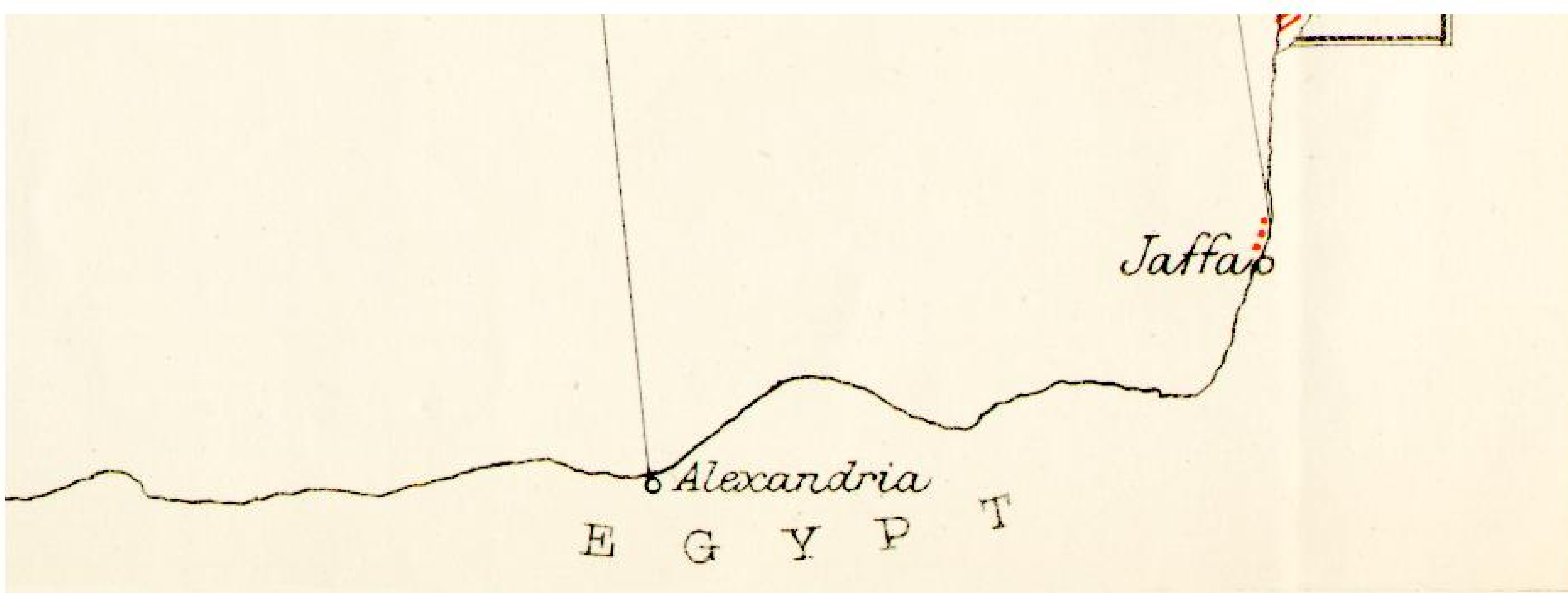
Kerkenna I.
Tripoli


Jaffa
Alexandria
EGYPT