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XXXII. *On the Effect of the Motion of a Body upon the Velocity with which it is traversed by Light.* By M. H. Fizeau*.

MANY theories have been proposed with a view of accounting for the phænomenon of the aberration of light according to the undulatory theory. In the first instance Fresnel, and more recently Doppler, Stokes, Challis, and several others have published important researches on this subject; though none of the theories hitherto proposed appear to have received the complete approval of physicists. Of the several hypotheses which have been necessitated by the absence of any definite idea of the properties of luminiferous æther, and of its relations to ponderable matter, not one can be considered as established; they merely possess different degrees of probability.

On the whole these hypotheses may be reduced to the following three, having reference to the state in which the æther ought to be considered as existing in the interior of a transparent body. Either, *first*, the æther adheres or is fixed to the molecules of the body, and consequently shares all the motions of the body; or *secondly*, the æther is free and independent, and consequently is not carried with the body in its movements; or, *thirdly*, only a portion of the æther is free, the rest being fixed to the molecules of the body and, alone, sharing its movements.

The last hypothesis was proposed by Fresnel, in order at once

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to satisfy the conditions of the aberration of light and of a celebrated experiment of Arago's, which proved that the motion of the earth does not affect the value of the refraction suffered by the light of a star on passing through a prism. Although these two phænomena may be explained with admirable precision by means of this hypothesis, still it is far from being considered at present as an established truth, and the relations between æther and matter are still considered, by most, as unknown. The mechanical conception of Fresnel has been regarded by some as too extraordinary to be admitted without direct proofs; others consider that the observed phænomena may also be satisfied by one of the other hypotheses; and others, again, hold that certain consequences of the hypothesis in question are at variance with experiment.

The following considerations led me to attempt an experiment the result of which promised, I thought, to throw light on the question.

It will be observed that, according to the first hypothesis, the velocity with which light traverses a body must vary with the motion of that body. If the motions of the body and the ray are like-directed, the velocity of light ought to be increased by the whole velocity of the body.

If the æther be perfectly free, the velocity of light ought not to be altered by the motion of the body.

Lastly, if the body when moving only carries with it a portion of the æther, then the velocity of light ought to be increased by a fractional part of the velocity of the body and not by the whole velocity, as in the first case. This consequence is not as evident as the two preceding ones, though Fresnel has shown that it is supported by mechanical considerations of a very probable nature.

The question then resolves itself to that of determining with accuracy the effect of the motion of a body upon the velocity with which light traverses it.

It is true that the velocity with which light is propagated is so immensely superior to any we are able to impart to a body, that any change in the first velocity must in general be inappreciable. Nevertheless, by combining the most favourable circumstances, it appeared to be possible to submit to a decisive test at least two media, air and water, to which, on account of the mobility of their particles, a great velocity may be imparted.

We owe to Arago a method of observation, founded on the phænomena of interference, which is well suited to render evident the smallest variation in the index of refraction of a body, and hence also the least change in the velocity with which the body is traversed by light; for, as is well known, this velocity is in versely proportional to the refracting index. Arago and Fresnel have both shown the extraordinary sensitiveness of this method by several very delicate observations, such as that on the difference of refraction between dry and moist air.

A method of observation founded upon this principle appeared to me to be the only one capable of rendering evident any change of velocity due to motion. It consists in obtaining interference bands by means of two rays of light after their passage through two parallel tubes, through which air or water can be made to flow with great velocity in opposite directions. The especial object before me necessitated several new arrangements, which I proceed to indicate.

With respect to the intensity of light, formidable difficulties had necessarily to be encountered. The tubes, which were of glass and 5·3 millims. in diameter, had to be traversed by light along their centres, and not near their sides; the two slits, therefore, had to be placed much further apart than is ordinarily the case, on which account the light would, in the absence of a special contrivance, have been very feeble at the point where the interference bands are produced.

This inconvenience was made to disappear by placing a convergent lens behind the two slits; the bands were then observed at the point of concourse of the two rays, where the intensity of light was very considerable.

The length of the tubes being tolerably great, 1·487 metre, it was to be feared that some difference of temperature or pressure between the two tubes might give rise to a considerable displacement of the bands, and thus completely mask the displacement due to motion.

This difficulty was avoided by causing the two rays to return towards the tubes by means of a telescope carrying a mirror at its focus. In this manner each ray is obliged to traverse the two tubes successively, so that the two rays having travelled over exactly the same path, but in opposite directions, any effect due to difference of pressure or temperature must necessarily be eliminated by compensation. By means of various tests I assured myself that this compensation was complete, and that whatever change in the temperature or density of the medium might be produced in a single tube, the bands would preserve exactly the same position. According to this arrangement, the bands had to be observed at the point of departure itself of the rays; solar light was admitted laterally, and was directed towards the tubes by means of reflexion from a transparent mirror; after their double journey through the tubes, the rays returned and traversed the mirror before reaching the place of interference, where the bands were observed by means of a graduated eye-piece.

The double journey performed by the rays had also the advantage of increasing the probable effect of motion; for this effect must be the same as if the tubes had double the length and were only traversed once.

This arrangement also permitted the employment of a very simple method for rendering the bands broader than they would otherwise have been in consequence of the great distance (9 millims.) between the slits. This method consisted in placing a very thick plate of glass before one of the slits, and inclining the same in such a manner that, by the effect of refraction, the two slits had the appearance of being very close to each other: in this manner the bands become as broad as they would be if the two slits were, in reality, as near each other as they appear to be; and instead of the intensity of light being sensibly diminished by this expedient, it may, in fact, be greatly augmented by giving greater breadth to the source of light. By causing the inclination of the glass to vary, the breadth of the bands may be varied at pleasure, and thus the magnitude most convenient for precisely observing their displacement may be readily given to them.

I proceed to describe the disposition of the tubes, and the apparatus destined to put the water in motion.

The two tubes, placed side by side, were closed at each extremity by a single glass plate, fixed with gum-lac in a position exactly perpendicular to their common direction. Near each extremity was a branch tube, forming a rounded elbow, which established a communication with a broader tube reaching to the bottom of a flask; there were thus four flasks communicating with the four extremities of the tubes.

Into one flask, which we will suppose to be full of water, compressed air, borrowed from a reservoir furnished with an air-pump, was introduced through a communicating tube. Under the influence of this pressure the water rose from the flask into the tube, which it then traversed in order to enter the flask at the opposite end. The latter could also receive compressed air, and then the liquid returned into the first flask after traversing the tube in an opposite direction. In this manner a current of water was obtained whose velocity exceeded 7 metres per second. A similar current, but in an opposite direction, was produced at the same time in the other tube.

Within the observer's reach were two cocks fixed to the reservoir of air; on opening either, currents, opposite in direction, were established in both tubes; on opening the other cock the currents in each tube were simultaneously reversed.

The capacity of the reservoir, containing air at a pressure of about two atmospheres, amounted to 15 litres (half a cubic foot), that of each flask to about 2 litres; the latter were divided into equal volumes, and the velocity of the water was deduced from the section of the tubes, and from the time of efflux of half a litre.

The apparatus above described was only employed for the experiments with water in motion: with some modifications it might also be used for air; but my experiments on moving air had been previously made with a slightly different apparatus, of which more hereafter, and the results had been found quite conclusive. I had already proved that *the motion of air produces no appreciable displacement of the bands.* But I shall return to this result and give further details.

For water there is an evident displacement. *The bands are displaced towards the right when the water recedes from the observer in the tube at his right, and approaches him in the tube on his left.*

The displacement of the bands is towards the left when the direction of the current in each tube is opposite to that just defined.

During the motion of the water the bands remain well defined, and move parallel to themselves, without the least disorder, through a space apparently proportional to the velocity of the water. With a velocity of 2 metres per second even, the displacement is perceptible; for velocities between 4 and 7 metres it is perfectly measureable.

In one experiment, where a band occupied five divisions of the micrometer, the displacement amounted to 1·2 divisions towards the right and 1·2 divisions towards the left, the velocity of the water being 7·059 metres per second. The sum of the two displacements, therefore, was equal to 2·4 divisions, or nearly half the breadth of a band.

In anticipation of a probable objection, I ought to state that the system of the two tubes and four flasks, in which the motion of the water took place, was quite isolated from the other parts of the apparatus: this precaution was taken in order to prevent the pressure and shock of the water from producing any accidental flexion in parts of the apparatus whose motion might influence the position of the bands. I assured myself, however, that no such influence was exerted, by intentionally imparting motions to the system of the two tubes.

After establishing the existence of the phænomenon of displacement, I endeavoured to estimate its magnitude with all possible exactitude. To avoid all possible sources of error, I varied the magnification of the bands, the velocity of the water, and even the nature of the divisions of the micrometer, so as to be unable to predict the magnitude of the displacements before measuring them. For in measuring small quantities, where our own power of estimating has to play a great part, the influence of any preconception is always to be feared; I think, however, that the result I have obtained is altogether free from this cause of error.

For the most part the observations were made with a velocity of 7·059 metres per second; in a certain number the velocity was 5·515 metres, and in others 3·7 metres. The magnitudes observed have been all reduced to the maximum velocity 7·059 metres, and referred to the breadth of a band as unity.

	Displacements of the bands for a mean velocity of water equal to 7·059 metres per second.	Differences between the observed displacements and their mean value.
	0·200	−0·030
	0·220	−0·010
	0·240	+0·010
	0·167	−0·063
	0·171	−0·059
	0·225	−0·005
	0·247	+0·017
	0·225	−0·005
	0·214	−0·016
	0·230	0·000
	0·224	−0·006
	0·247	+0·017
	0·224	−0·006
	0·307	+0·077
	0·307	+0·077
	0·256	+0·026
	0·240	+0·010
	0·240	+0·010
	0·189	−0·041
Sum	4·373	
Mean	0·23016	

By doubling the mean value we have 0·46, nearly half the breadth of a band, which represents the magnitude of the displacement produced by reversing the direction of the current in each tube.

To show the deviations on each side, the differences between the several observed displacements and the mean value of all have been inserted in the Table. It will be seen that, in general, they represent a very small fraction of the breadth of a band; the greatest deviation does not exceed one-thirteenth of the breadth of a band.

These differences are due to a difficulty which could not be overcome; the displacement remained at its maximum but for a very short period, so that the observations had to be made very rapidly. Had it been possible to maintain the velocity of the current of water constant for a greater length of time, the measurements would have been more precise; but this did not appear to be possible without considerably altering the apparatus, and such alterations would have retarded the prosecution of my research until the season was no longer favourable for experiments requiring solar light.

I proceed to compare the observed displacement with those which would result from the first and third hypotheses before alluded to. As to the second hypothesis, it may be at once rejected; for the very existence of displacements produced by the motion of water is incompatible with the supposition of an æther perfectly free and independent of the motion of bodies.

In order to calculate the displacement of the bands under the supposition that the æther is united to the molecules of bodies in such a manner as to partake of their movements, let

v be the velocity of light in a vacuum,

v' the velocity of light in water when at rest,

u the velocity of the water supposed to be moving in a direction parallel to that of the light. It follows that

$v'+u$ is the velocity of light when the ray and the water move in the same direction, and

$v'-u$ when they move in opposite directions.

If Δ be the required retardation and E the length of the column of water traversed by each ray, we have, according to the principles proved in the theory of the interference of light,

$$\Delta = \mathrm{E}\left(\frac{v}{v'-u} - \frac{v}{v'+u}\right),$$

or

$$\Delta = 2\mathrm{E}\frac{u}{v}\cdot\frac{v^2}{v'^2-u^2}.$$

Since u is only the thirty-three millionth part of v, this expression may, without appreciable error, be reduced to

$$\Delta = 2\mathrm{E}\frac{u}{v}\cdot\frac{v^2}{v'^2}.$$

If $m=\frac{v}{v'}$ be the index of refraction of water, we have the approximate formula

$$\Delta = 2\mathrm{E}\frac{u}{v}m^2.$$

Since each ray traverses the tubes twice, the length E is double the real length of the tubes. Calling the latter $\mathrm{L}=1{\cdot}4875$ metre, the preceding formula becomes

$$\Delta = 4L\frac{u}{v}m^2;$$

and the numerical calculation being performed, we find

$$\Delta = 0{\cdot}0002418 \text{ millim.}$$

Such is the difference of path which, under the present hypothesis, ought to exist between the two rays.

Strictly speaking, this number has reference to a vacuum, and ought to be divided by the index of refraction for air; but this index differs so little from unity, that, for the sake of simplicity, the correction, which would not alter the last figure by a unit, may be neglected.

The above quantity being divided by the length of an undulation, will give the displacement of the bands in terms of the breadth of one of them. In fact, for a difference of path amounting to 1, 2, ... m undulations, the system of bands suffer a displacement equal to the breadth of 1, 2, ... m bands.

For the ray E the length of an undulation is $\lambda = 0{\cdot}000526$, and the rays about it appear to preserve the greatest intensity after the light has traversed a rather considerable thickness of water. Selecting this ray, then, we find for the displacement the value

$$\frac{\Delta}{\lambda} = 0{\cdot}4597.$$

Had, therefore, the æther participated fully in the motion of the water, in accordance with the hypothesis under consideration, a displacement of 0·46 of a band would have been observed in the foregoing experiments. But the mean of our observations gave only 0·23; and on examining the greatest particular values, it will be found that none approached the number 0·46. I may even remark that the latter number ought to be still greater, in consequence of a small error committed in the determination of the velocity of the water; an error whose tendency is known, although, as will soon be seen, it was impossible to correct it perfectly.

I conclude, then, that this hypothesis does not agree with experiment. We shall next see that, on the contrary, the third, or Fresnel's hypothesis, leads to a value of the displacement which differs very little from the result of observation.

We know that the ordinary phænomena of refraction are due to the fact that light is propagated with less velocity in the interior of a body than in a vacuum. Fresnel supposes that this change of velocity occurs because the density of the æther within a body is greater than that in a vacuum. Now for two media whose elasticity is the same, and which differ only in their densities, the squares of the velocities of propagation are inversely proportional to these densities; that is,

$$\frac{D'}{D}=\frac{v^2}{v'^2},$$

D and D′ being the densities of the æther in a vacuum and in the body, and v, v' the corresponding velocities. From the above we easily deduce the relations

$$D'=D\frac{v^2}{v'^2}, \quad D'-D=D\frac{v^2-v'^2}{v'^2},$$

the latter of which gives the excess of density of the interior æther.

It is assumed that when the body is put in motion, only a part of the interior æther is carried along with it, and that this part is that which causes the excess in the density of the interior over that of the surrounding æther; so that the density of this moveable part is $D'-D$. The other part which remains at rest during the body's motion has the density D.

The question now arises, With what velocity will the waves be propagated in a medium thus constituted of an immoveable and a moveable part, when for the sake of simplicity we suppose the body to be moving in the direction of the propagation of the waves?

Fresnel considers that the velocity with which the waves are propagated then becomes increased by the velocity of the centre of gravity of the stationary and moving portions of æther. Now u being the velocity of the body,

$$\frac{D'-D}{D'}u$$

will be the velocity of the centre of gravity of the system in question, and according to the last formula this expression is equal to

$$\frac{v^2-v'^2}{v^2}u.$$

Such, then, is the quantity by which the velocity of light will be augmented; and since v' is the velocity when the body is at rest,

$$v'+\frac{v^2-v'^2}{v^2}u \text{ and } v'-\frac{v^2-v'^2}{v^2}u$$

will be the respective velocities when the body moves with and against the light.

By means of these expressions the corresponding displacement of the bands in our experiment may be calculated in exactly the same manner as before. For the difference of path we have the value

$$\Delta = \mathrm{E}\left\{\frac{v}{v' - \frac{v^2 - v'^2}{v^2} u} - \frac{v}{v' + \frac{v^2 - v'^2}{v^2} u}\right\},$$

which by reduction and transformation becomes

$$\Delta = 2\mathrm{E}\frac{u}{v}\left\{\frac{v^2 - v'^2}{v'^2 - u^2\left(\frac{v^2 - v'^2}{v^2}\right)^2}\right\}.$$

Taking into consideration the smallness of u with respect to $v'\left(\frac{u}{v'} = \frac{1}{33000000}\right)$, and the circumstance that the coefficient of u^2 differs little from unity, the term in u^2 may, without appreciable error, be neglected, and the above expression considerably simplified. In fact, if m be the index of refraction, and $\mathrm{L} = \frac{1}{2}\mathrm{E}$ the length of each tube, we have approximately

$$\Delta = 4\mathrm{L}\frac{u}{v}(m^2 - 1),$$

whence by numerical calculation we deduce

$$\Delta = 0{\cdot}00010634 \text{ millim.}$$

On dividing this difference of path by the length λ of an undulation, the magnitude of the displacement becomes

$$\frac{\Delta}{\lambda} = 0{\cdot}2022,$$

the observed value being 0·23.

These values are almost identical; and what is more, the difference between observation and calculation may be accounted for with great probability by the presence of the before-mentioned error in estimating the velocity of the water. I proceed to show that the tendency of this error may be assigned, and that analogy permits us to assume that its effect must be very small.

The velocity of the water in each tube was calculated by dividing the volume of water which issued per second from one of the flasks by the sectional area of the tube. But by this method it is only the *mean* velocity of the water which is determined; in other words, that which would exist provided the several threads of liquid at the centre and near the sides of the tube moved with equal rapidity. It is evident, however, that this cannot be the case; for the resistance opposed by the sides of the tube, acting in a more immediate manner on the neighbouring threads of liquid, tends to diminish their velocity more than it does that of the threads nearer the centre of the tube. The velocity of the water in the centre of the tubes, therefore, must be greater than that of the water near the sides, and consequently also greater than the mean of both velocities.

Now the slits placed before each tube to admit the rays whose interference was observed, were situated in the middle of the circular ends of the tubes; so that the rays necessarily traversed the central zones, where the velocity of the water exceeded the mean velocity*.

The law followed by these variations of velocity in the motion of water through tubes not having been determined, it was not possible to introduce the necessary corrections. Nevertheless analogy indicates that the error resulting therefrom cannot be considerable. In fact, this law has been determined in the case of water moving through open canals, where the same cause produces a similar effect; the velocity in the middle of the canal and near the surface of the water is there also greater than the mean velocity. It has been found that, for values of the mean velocity included between 1 and 5 metres per second, the maximum velocity is obtained by multiplying this mean velocity by a certain coefficient which varies from 1·23 to 1·11. Analogy therefore permits us to assume that in our case the correction to be introduced would be of the same order of magnitude.

Now on multiplying u by 1·1, 1·15, and 1·2, and calculating the corresponding values of the displacement of the bands, we find in place of 0·20 the values 0·22, 0·23, 0·24 respectively; whence it will be seen that in all probability the correction would tend to cause still greater agreement between the observed and the calculated results. We may presume, then, that the small difference which exists between the two values depends upon a small error in estimating the real velocity of the water; which error cannot be rectified in a satisfactory manner, in consequence of the absence of sufficiently accurate data.

Thus the displacement of the bands caused by the motion of water, as well as the magnitude of this displacement, may be explained in a satisfactory manner by means of the theory of Fresnel.

It was before observed that the motion of air causes no perceptible displacement of the bands produced by the interference of two rays which have traversed the moving air in opposite directions. This fact was established by means of an apparatus which I will briefly describe.

A pair of bellows, loaded with weights and worked by a lever, impelled air forcibly through two parallel copper tubes whose extremities were closed by glass plates. The diameter of each

* Each slit was a rectangle 3 millims. by 1·5, and its surface was equal to one-fifth that of the tube.

tube was 1 centimetre, and its effective length 1·495 metre; the direction of the motion in one tube was opposite to that in the other, and the pressure under which this motion took place was measured by a manometer placed at the entrance of the tubes; it could be raised to 3 centimetres of mercury.

The velocity of the air was deduced from the pressure and from the dimensions of the tubes, according to the known laws of the efflux of gases. The value thus found was checked by means of the known volume of the bellows, and the number of strokes necessary to produce a practically constant pressure at the entrance of the tubes. A velocity of 25 metres per second could easily be imparted to the air; occasionally greater velocities were reached, but their values remained uncertain.

In no experiment could a perceptible displacement of the bands be produced: they always occupied the same positions, no matter whether the air remained at rest, or moved with a velocity equal or even superior to 25 metres per second.

When this experiment was made, the possibility of doubling, by means of a reflecting telescope, the value of the displacement, and at the same time of completely compensating any effects due to accidental differences of temperature or pressure in the two tubes, had not suggested itself; but I employed a sure method of distinguishing between the effects due to motion, and those resulting from accidental circumstances.

This method consisted in making two successive observations, by causing the rays to traverse the apparatus in opposite directions. For this purpose the source of light was placed at the point where the central band had previously been, when the new bands formed themselves where the source of light had previously been placed.

The direction of the motion of the air in the tubes remaining the same in both cases, it is easy to see that the accidental effects would in both observations give rise to a displacement towards the same tube, whilst the displacement due solely to motion would first be on the side of one tube and then on the side of the other. In this manner a displacement due to motion would have been detected with certainty, even if it had been accompanied by an accidental displacement due, for instance, to some defect of symmetry in the diameters or orifices of the tubes, whence would result an unequal resistance to the passage of air, and consequently a difference of density.

But the symmetry given to the apparatus was so perfect that no sensible difference of density existed in the two tubes during the motion of the air. The double observation was consequently unnecessary; nevertheless it was made for the sake of greater security, and in order to be sure that the sought displacement was not accidentally compensated by a difference of density, which, though small, might be sufficient totally to mask such displacement.

Notwithstanding these precautions, however, no displacement of the bands occurred in consequence of the motion of the air; and according to an estimate I have made, a displacement equal to one-tenth of the breadth of a band would have been detected had it occurred.

The calculations with respect to this experiment are as follows. Under the hypothesis that the air, when moving, carries with it all the æther, we have

$$\Delta = 2\mathrm{L}\frac{u}{v}m^2 = 0{\cdot}0002413 \text{ millim.},$$

m^2 being equal to 1·000567 at the temperature 10° C.

This experiment having been made in air, the maximum illumination was due to the yellow rays; and this maximum determined the breadth of the bands. Hence the value of λ corresponding to the ray D being taken, we have

$$\frac{\Delta}{\lambda} = 0{\cdot}4103.$$

Now so great a displacement could certainly not have escaped observation, especially since it might have been doubled by reversing the current.

The following would be the results of the calculation according to the hypothesis of Fresnel:—

$$\Delta = 2\mathrm{L}\frac{u}{v}(m^2 - 1) = 0{\cdot}0000001367,$$

$$\frac{\Delta}{\lambda} = 0{\cdot}0002325.$$

Now a displacement equal to $\frac{1}{5000}$th of the breadth of a band could not be observed; it might, in fact, be a hundred times greater and still escape observation. Thus the apparent immobility of the bands in the experiment made with moving air may be explained by the theory of Fresnel, according to which the displacement in question, although not absolutely zero, is so small as to escape observation.

After having established this negative fact, and seeking, by means of the several hypotheses respecting æther, to explain it as well as the phænomenon of aberration and the experiment of Arago, it appeared to me to be necessary to admit, with Fresnel, that the motion of bodies changes the velocity with which light traverses them, but that this change of velocity varies according to the energy with which the traversed medium refracts light; so that the change is great for highly refracting bodies, but small for feebly refracting ones such as air.

I was thus led to anticipate a sensible displacement of the bands by means of the motion of water, since its index of refraction greatly exceeds that of air.

It is true that an experiment of Babinet's, mentioned in the ninth volume of the *Comptes Rendus*, appeared to be in contradiction to the hypothesis of a change in the velocity of light in accordance with the law of Fresnel. But on considering the conditions of that experiment, I detected the existence of a cause of compensation whose influence would render the effect due to motion insensible. This cause proceeds from the reflexion which the light suffers in the experiment in question. It may, in fact, be demonstrated that if a certain difference of path exists between two rays, that difference becomes altered when these rays suffer reflexion from a moving mirror. Now on calculating separately the two effects (of reflexion) in the experiment of Babinet, their magnitudes will be found to be equal and opposite in sign.

This explanation rendered the hypothesis of a change of velocity still more probable, and induced me to undertake the experiment with water, as being the most suitable one for deciding the question with certainty.

The success of this experiment must, I think, lead to the adoption of the hypothesis of Fresnel, or at least to that of the law discovered by him, which expresses the relation between the change of velocity and the motion of the body; for although the fact of this law being found to be true constitutes a strong argument in favour of the hypothesis of which it is a mere consequence, yet to many the conception of Fresnel will doubtless still appear both extraordinary and, in some respects, improbable; and before it can be accepted as the expression of the real state of things, additional proofs will be demanded from the physicist, as well as a thorough examination of the subject from the mathematician.

Shortly before the publication of the above interesting memoir in the *Annales de Chimie*, M. Fizeau presented to the Academy a second memoir, containing the results of his experiments on the effect of the motion of a transparent solid body, such as glass, upon the velocity with which it is traversed by light. The *Comptes Rendus* of November 14th, 1859, contains a brief extract from this memoir; and from it we gather the principal results of his experiments, and the principles upon which the same were based.

The method of experiment which was employed in the foregoing researches on air and water being no longer applicable, recourse was had to the following property of light established by the researches of Malus, Biot, and Brewster. When a ray of polarized light traverses a plate of glass, inclined towards its direction, the plane of polarization of the transmitted ray is in general inclined towards that of the incident ray. The magnitude of the rotation of the plane of polarization which is thus caused by the two refractions at the two surfaces of the plate of glass depends, *first*, upon the angle of incidence; *secondly*, upon the azimuth of the primitive plane of polarization with reference to the plane of incidence; and *thirdly*, upon the index of refraction of the glass forming the plate.

The angle of incidence and the azimuth of the primitive plane of polarization remaining the same, the rotation of this plane increases with the index of refraction of the glass plate. Now since this index is inversely proportional to the velocity with which waves of light are propagated through the glass, it follows that the magnitude of the rotation of the plane of polarization increases when the velocity with which light traverses the glass plate diminishes. The determination of any change in this velocity is, therefore, reduced to that of the corresponding change in the rotation of the plane of polarization.

In the first place it was deemed necessary to determine the change in the rotation which any given increase or decrease of the index of refraction could produce. By direct and comparative measurements of these indices and rotations, in the cases of flint and ordinary glass, it was found that when the index was increased by a small fraction, the rotation increased by a fraction $4\frac{1}{2}$ times greater than the first.

The question next arises what change, according to the hypothesis of Fresnel, ought to be produced in the velocity of light when it traverses glass in a state of motion? The answer is based upon the following data.

The greatest velocity at our command is unquestionably that of the earth in its orbit. At noon, during the period of the solstices, for instance, the direction of this motion is horizontal and from east to west; from this it follows that when a plate of glass receives a ray of light coming from the west, it ought to be considered as really moving to meet the ray with the immense velocity of 31,000 metres per second. When, on the contrary, the incident ray comes from the east, the glass plate must be considered as moving with this velocity in the same direction as that of the propagation of the waves of light, by which latter it is in reality overtaken.

Now, according to the theory of Fresnel, the difference between the velocities of the light in these two extreme cases would be sufficient to produce a change in the rotation of the plane of polarization equal to $\frac{1}{1500}$th part of the magnitude of that rotation.

In order to test this result by experiment, a series of glass plates were interposed in the path of a polarized beam of parallel rays of light. The primitive plane of polarization was determined by a divided circle, and the rotation which this plane underwent by the action of the plates was measured by means of a second graduated circle fixed to a convenient analyser. The instrument could, moreover, be fixed in any direction so as to study the influence of all terrestrial motions upon the phænomena.

In order to make the two necessary observations conveniently and rapidly, two mirrors were previously fixed on the east and on the west of the instrument, and upon each, alternately, a beam of solar light was thrown by means of a heliostat, and thence reflected towards the instrument.

The greatest difficulties were encountered in the annealing of the glass plates of the series; and as perfectly homogeneous plates could not be obtained, it was necessary to employ various compensating expedients, all which will be found described in the memoir itself.

The conclusions to which M. Fizeau was led by means of more than 2000 observations are thus stated :—

1. The rotation of the plane of polarization produced by a series of inclined glass plates is always greater when the light which traverses them comes from the west than when it comes from the east; the observation being made about noon.

2. This excess of rotation is decidedly at a maximum at or about noon during the solstices. Before and after this hour it is less, and at about 4 o'clock is scarcely perceptible.

3. The numerical values deduced from the numerous series of observations present notable differences, the cause of which may be guessed, though it cannot yet be determined with certitude.

4. The influence of the earth's annual motion, as determined by calculation on the hypothesis of Fresnel, leads to values of the above excess of rotation which agree tolerably well with the majority of the values deduced from observation.

5. Theory, as well as experiment, therefore, lead us to conclude that the azimuth of the plane of polarization of a refracted ray is really influenced by the motion of the refracting medium, and that the motion of the earth in space exerts an influence of this kind upon the rotation of the plane of polarization produced by a series of inclined glass plates.