

point whatever in this circle are constantly parallel to each other; in other words, the several parts of the circle F H G are moving absolutely in the same direction. The same thing may be proved by joining B B, any two positions of the point B, and also I I, the corresponding positions of the point I; then these lines, since they join equal and parallel lines towards the same parts, are themselves equal and parallel; that is to say, the chords of the equal arcs described by the points B and I in the same time, are not only equal, but also parallel, and this is the case, however great or however small these arcs may be; but when these arcs are infinitely small, these chords coincide with their respective arcs—from which it follows, that the circumference of the circle of orbital motion passing through I, has, *at the point I*, precisely the same direction that the circumference of the circle A B D E passing through the centre B has *at the point B*; which is, in effect, the same conclusion as that arrived at by the first method. There may, at first sight, be an apparent absurdity in this, inasmuch as the two circles of orbital motion *cut* each other; but a little reflection will show that this absurdity is only apparent.

At the time my first article was sent to the editor of the Journal, I had seen no attempt at a regular and rigid demonstration of the new and interesting experiment. Subsequently, however, I have met with several articles in which the idea of the “*cone of latitude*,” as J. R. Young appropriately terms it, is more or less fully developed. Though indebted to no one for the employment of this means of simplifying the problem, it appears that the idea of the tangential cone had suggested itself to one or more other individuals before it had occurred to me.

Before leaving the subject, it may be well to observe that the denominator of the fraction in the formulas given on page 42 of last number, varies slightly with the latitude of the place—that there given (77787) being calculated for the latitude of 45° . A better form of the general expression for the value of y is the following, which is true for any latitude, s being the length (in feet or in the same denomination as l) of a pendulum beating seconds at the given place, viz:

$$y = \frac{r \sin. L \cos. a}{43082} \times \sqrt{\frac{l}{s}}$$

Philadelphia, 7th mo., 9th, 1851.

On the Velocity of Light.—Experimental Proof of the Theory of Undulation. By JOHN TYNDALL, Ph. D., Marburg.*

1. *Méthode générale pour mesurer la vitesse de la lumière dans l'air et les milieux transparents*, by M. L. Foucault. *Comptes Rendus*, May 6, 1850.
2. *Sur la Vitesse comparative de la lumière dans l'air et dans l'eau*, by MM. H. Fizeau and L. Breguet. *Ibid.* June 17, 1850.

The state of Arago's sight has recently induced him to resign the carrying out of his proposition of submitting the two rival theories of light to experimental decision to physicists gifted with younger eyes. It is well known, that, according to the theory of Newton—the so called *emission theory*—the velocity of light in passing from a rarer medium into

* From the London, Edinburgh, and Dublin Philosophical Magazine, for June, 1851.

a denser is *increased*. For example, the index of refraction in passing from air to water is $\frac{4}{3}$; according to the emission theory, the velocity of light in air is to its velocity in water as 3 is to 4. Opposed to this stands the theory of undulation, proposed by Huyghens, and supported by Euler, Young, and Fresnel. According to this theory, the velocity of light in passing from a rarer to a denser medium is *diminished*; in the case of air and water, for instance, the above ratio is reversed; the velocity of light in air is to its velocity in water, in the ratio of 4 : 3. The genius of Fresnel has won for the latter theory almost universal recognition; a direct proof was however wanting, and this urged Arago* to the hardy thought of submitting the question to an experimental test.

The rotating mirror of Mr. Wheatstone was proposed as the instrumental agent for carrying out this idea. If we conceive a ray of light to enter a dark room through a hole in a window-shutter, and to fall upon the plane surface of a reflecting mirror set perpendicular to the direction of the light, the latter will be sent back along the path by which it entered. If the reflecting surface be oblique to the direction of the light, the latter will be reflected in some other direction; supposing a second reflecting mirror to be set perpendicular to this latter direction, the light will be reflected from this in the direction of the perpendicular, will again strike the other mirror, and be finally sent back by the latter through the aperture by which it entered. In this case the ray suffers two reflexions from the intermediate mirror; and if it be true that light requires time in passing from one point to another, these two reflexions *cannot occur contemporaneously*. A certain portion of time, however small, will be required for the journey to and fro from one mirror to the other. Supposing, for instance, the mirrors to be placed six feet apart; the light proceeding from the aperture is received upon the first mirror and reflected by it on to the other; from this it is reflected back again, and thus accomplishes a journey of twelve feet between its two reflexions by the first mirror. To this journey, as has been said, time is necessary. If the aperture and the two mirrors be perfectly motionless, the path of the light in coming, will coincide with its path in returning; but if, while on its route *between the two mirrors*, we conceive the position of the first mirror to be changed, that, for instance, it has become more inclined to the direction of the ray, the latter will not be reflected in the line of its approach, but will be thrown against the window shutter more or less to the side of the aperture. This change in the position of the mirror during the almost infinitesimal portion of time occupied by the light on its twelve feet journey, is accomplished by imparting to the mirror a high angular velocity, say a thousand revolutions in a second. We here find ourselves in possession of a means of comparing the velocity of light in air with its velocity in water. When the mirror rotates, the ray sent back does not strike upon the aperture, but more or less

* It ought to be mentioned, that the same subject had occupied the attention of Sir John Herschel and Mr. Wheatstone some years before it was mooted by Arago; and a proposition was actually made by the former to send a bar of light through a tube of water or alcohol a mile in length, and thus determine the influence of this medium. This idea, though not so practical, is the same in principle as that of Arago. The latter, however, was not aware that any such proposition had ever been made.

to the side of it. The less time occupied by the light in performing its double journey between the two mirrors, the less ought this divergence to be, and *vice versa*. Hence, if the Newtonian theory be true, the introduction of a column of water six feet long ought to bring the reflected image of the aperture *nearer* to the aperture itself; and if the undulation theory be true, the introduction of such a column ought to make the divergence *greater*. These speculations have been recently submitted to the test of experiment, and the result has pronounced in favor of the theory of undulation.

Of course, such experiments, though easily described and simple enough in principle, demand considerable delicacy of manipulation. The divergence spoken of is in reality exceedingly small. In order to observe it, M. Foucault has made use of a square aperture furnished with a number of vertical bars of fine platinum wire; eleven of these fitted in the space of one millimetre, and between each two there was a small space through which the light entered. The image given by this was a small field furrowed with alternate black and white stripes. The light after entering through this aperture fell upon a lens, by which it was converged, but before it came to a focus on the opposite side it fell upon the rotating mirror; it was thence cast upon a concave mirror placed about six feet distant, which reflected it back again. By a peculiar artifice, M. Foucault was enabled to compare with great nicety the divergence of the black and white stripes in the image from the platinum wires and their intervening spaces. "I have already proved," says M. Foucault, "by two successive operations, *that the deviation of the image after the journey of the light through air is less than after its journey through water*. I have also made another confirmatory experiment, which consists in observing an image formed partly by light which has passed through air, and partly by light which has passed through water. For small velocities, the stripes of this mixed image were apparently continuations of each other. *But by the acceleration of the motion the image is transported, and the stripes are broken at the point of junction of the air image with the water image. The stripes of the latter take the advance in the sense of the general deviation. Further, on taking into account the length of water and of air traversed, the deviations are found to be proportional to the indices of refraction.* These results indicate a velocity of the light *which is less in water than in air*, and, according to the views of M. Arago, fully establish the theory of undulation."

The following interesting article on the same subject is from MM. Fizeau and Breguet.

We have realized with great exactitude, the experiment described in our note presented to the Academy during its session of the 6th of May last; an experiment which we felt called upon to make, although M. Foucault in the same session had read an extended paper upon this subject, in which he announces that he has already obtained decisive results.

We have thought that, for the solution of a capital question like the present, the proofs could not be too much multiplied, and that experiments made under different circumstances could not but contribute to render our knowledge of an important fact more certain.

We have applied ourselves to the solution of the question as proposed by M. Arago in 1848; that is to say, How can the two opposite theories regarding the nature of light be submitted to a definite test? We have adopted such measures as are calculated to exhibit in a striking manner the differences of the phenomena as deduced from the one or the other theory.

As remarked in our preceding communication, the observation was made simultaneously on two bundles of light; the one having traversed the air, the other a column of water.

For each of these bundles the path was as follows:—A telescope was so disposed that its object glass was very near the rotating mirror; a little rectangular prism was placed in the focus of the telescope, in such a position that the solar rays falling upon it from a convenient lateral opening near the eye-glass, were totally reflected towards the object-glass.

Beyond the rotating mirror, and at a distance which for the ray that passed through water amounted to two metres, there was a fixed reflector designed to send back the light to the rotating mirror by a normal reflexion.

The focal distance of the telescope was such that the image of the little prism placed at its focus formed itself distinctly upon the fixed reflector just mentioned. After having been reflected from it, the light returned to the rotating mirror, was sent on through the telescope, and on passing the focus formed an image which exactly covered the prism.

By the rotation of the mirror we give birth to a number of images which succeed each other very rapidly, and the super-position of which produces the sensation of a permanent image.

When the rotation became sufficiently rapid, the permanent image was pushed forward in the direction of rotation, this deviation being the result of the angular motion of the mirror during the time occupied by the light in passing twice over the space which separated it from the fixed mirror.

A second similar fixed mirror was placed beside the former: it permitted us to make the experiment with air and water simultaneously.

If the lengths traversed had been equal for both media, the times occupied in passing them would be in the ratio of 4 : 3 or of 3 : 4, according to the one or the other theory, and the deviations produced by the rotation of the mirror would have been in the same ratio.

Instead of equal lengths, we have adopted equivalent lengths; that is to say, lengths traversed by the light in equal times. These lengths are very different, according as they are calculated from the one or the other theory. The length for water being 1, the equivalent length for air would be $\frac{3}{4}$ by the theory of emission, and $\frac{4}{3}$ by the theory of undulation.

If the experiment be made by adopting the length $\frac{3}{4}$ for air, that of water being 1, according to the theory of emission, the times occupied by the two bundles of light in passing over these spaces will be equal,

and consequently the deviations will be equal. By the other theory, on the contrary, the times occupied by the light in passing through both media, will be very different; these times will be for water and for air in the ratio of 16 to 9, and the deviations will be in the same ratio.

To coincide with the one or the other theory, it will therefore be sufficient to prove, either that the deviations are equal, or that one is nearly double the other.

If the equivalent lengths calculated from the theory of undulation be taken, the results will be similar, but inverse.

According to the theory of emission, the deviations will be in the ratio of 16:9; according to the other theory, they will be equal.

We have made these two experiments, and the results obtained are very exact. The phenomena observed are altogether in accordance with the theory of undulation, and in manifest opposition to the theory of emission.

In the first arrangement the deviation is greater for water than for air; it is nearly double. The difference is sensible with a velocity of 400 or 500 revolutions per second, with a velocity of 1500 revolutions it becomes quite evident.

In the second arrangement the deviation is the same for air and water; and whatever be the velocity of the mirror, there is no sensible difference between the two deviations.

These experiments have been made in the meridian room of the observatory; the column of water was 2 metres long, and was contained in a crystal tube closed at the ends with glass. This length is more convenient than that which we at first employed, namely, 3 metres. The light is less weakened, and, after its double passage, retains an intensity which may be estimated at double of that which was obtained with the tube of three metres.

The deviations were observed at a distance of 1.50 millim. from the rotating mirror.

On the Law of the Compressibility of Water at different Temperatures.

*By W. J. MACQUORN RANKINE, F. R. S. E. &c.**

Having lately had occasion, in the course of some theoretical researches on the velocity of sound in liquids, to endeavor to represent the experiments of M. Grassi on the compressibilities of distilled water at different temperatures (*Comptes Rendus*, xix.) by an empirical formula, I was much struck on unexpectedly finding that they followed sensibly this law:—

The compressibility of water is inversely proportional to the density multiplied by the temperature, as measured from the absolute zero of a perfect gas-thermometer, viz: a point 274°·6 below the ordinary zero of the Centigrade scale, and 462°·28 below that of Fahrenheit's scale.

* From the London, Edinburgh, and Dublin Philosophical Magazine, June, 1851.