5. The total mechanical force exerted by a volume of air while expanding indefinitely is proportional to its G temperature.

6. A given quantity of air while expanding, under a constant pressure, from one temperature to another, exerts a mechanical force equivalent to one-third the difference of temperature; and the quantity of heat required to change the temperature of air under a constant pressure, is four-thirds of that required to effect the same change of temperature with a constant volume.

The author concludes by observing that it is singular that these simple and, he considers, important deductions from MM. Gay-Lussac and Welter's experiments, have been overlooked by the eminent mathematicians who have elaborately discussed this subject. The artificial position of the zero-point on the ordinary scales of temperature may perhaps account for this by its tendency to confine our ideas. Dalton's and Gay-Lussac's law of expansion seems imperatively to have required that, in all computations having reference to gases and vapours, the temperature should have been reckoned from the zero of gaseous tension; yet it has not been so; and it is impossible to avoid the conclusion, that if it had been otherwise, if no other temperature but what we have had so often to refer to as the G temperature had been indicated in their analyses, we should have profited more by their labours, and been further advanced in the science of heat and elastic fluids.

LXXXI. *Intelligence and Miscellaneous Articles.*

ON THE HYPOTHESES RELATING TO THE LUMINOUS ÆTHER, AND AN EXPERIMENT WHICH APPEARS TO DEMONSTRATE THAT THE MOTION OF BODIES ALTERS THE VELOCITY WITH WHICH LIGHT PROPAGATES ITSELF IN THEIR INTERIOR. BY M. H. FIZEAU.

MANY hypotheses have been proposed to account for the phænomena of aberration in accordance with the doctrine of undulations. Fresnel in the first instance, and more recently Doppler, Stokes, Challis, and many others, have published memoirs on this important subject; but it does not seem that any of the theories proposed have received the entire assent of physicists. In fact, the want of any definite ideas as to the properties of the luminous æther and its relations to ponderable matter, has rendered it necessary to form hypotheses, and among those which have been proposed there are some which are more or less probable, but none which can be considered as proved.

These hypotheses may be reduced to three principal ones. They refer to the state in which the æther existing in the interior of transparent bodies may be considered to be.

This æther is either adherent, and as it were attached to the molecules of bodies, and consequently participates in the motions to which the bodies may be subjected;

Or the æther is free and independent, and is not influenced by the motion of the bodies;

Or, lastly, according to a third hypothesis, which includes both the former ones, only a portion of the æther is free, the other portion being attached to the molecules of bodies and participating in their motion.

This latter hypothesis was proposed by Fresnel, and constructed for the purpose of equally satisfying the phænomena of aberration, and a celebrated experiment of M. Arago, by which it has been proved that the motion of the earth has no influence upon the refraction which the light of the stars suffers in a prism.

We may determine the value which in each of these hypotheses it is necessary to attribute to the velocity of light in bodies when the bodies are supposed to be in motion.

If the æther is supposed to be wholly carried along with the body in motion, the velocity of light ought to be increased by the whole velocity of the body, the ray being supposed to have the same direction as the motion.

If the æther is supposed to be free and independent, the velocity of light ought not to be changed at all.

Lastly, if only one part of the æther is carried along, the velocity of light would be increased, but only by a fraction of the velocity of the body, and not, as in the first hypothesis, by the whole velocity. This consequence is not so obvious as the former, but Fresnel has shown that it may be supported by mechanical arguments of great probability.

Although the velocity of light is enormous comparatively to such as we are able to impart to bodies, we are at the present time in possession of means of observation of such extreme delicacy, that it seems to me to be possible to determine by a direct experiment what is the real influence of the motion of bodies upon the velocity of light.

We are indebted to M. Arago for a method based upon the phænomena of interference, which is capable of indicating the most minute variations in the indexes of refraction of bodies. The experiments of MM. Arago and Fresnel upon the difference between the refractions of dry and moist air, have proved the extraordinary sensibility of that means of observation.

It is by adopting the same principle, and joining the double tube of M. Arago to the conjugate telescopes which I employed for determining the absolute velocity of light, that I have been able to study directly in two mediums the effects of the motion of a body upon the light which traverses it.

I will now attempt to describe, without the aid of a diagram, what was the course of the light in the experiment. From the focus of a cylindrical lens the solar rays penetrated almost immediately into the first telescope by a lateral opening very near to its focus. A transparent mirror, the plane of which made an angle of 45° with the axis of the telescope, reflected the rays in the direction of the object-glass.

On leaving the object-glass, the rays having become parallel among themselves, encountered a double chink, each opening of which corresponded to the mouth of one of the tubes. A very narrow bundle of rays thus penetrated into each tube, and traversed its entire length, $1^{m}\cdot487$.

The two bundles, always parallel to each other, reached the object-glass of the second telescope, were then refracted, and by the effect of the refraction reunited at its focus. There they encountered the reflecting plane of a mirror perpendicular to the axis of the telescope, and underwent a reflexion back again towards the object-glass; but by the effect of this reflexion the rays had changed their route in such a way that that which was to the right before, was to the left after the reflexion, and *vice versâ*. After having again passed the object-glass, and been thus rendered parallel to each other, they penetrated a second time into the tubes; but as they were inverted, those which had passed through one tube in going passed through the other on returning. After their second transit through the tubes, the two bundles again passed the double chinks, re-entered the first telescope, and lastly intersected at its focus in passing across the transparent mirror. There they formed the fringes of interference, which were observed by a glass carrying a graduated scale at its focus.

It was necessary that the fringes should be very large in order to be able to measure the small fractions of the width of a fringe. I have found that that result is obtained, and a great intensity of light maintained, by placing before one of the chinks a thick mirror which is inclined in such a way as to see the two chinks by the effect of refraction, as if they were nearer to each other than they really are. It is in this way possible to give various dimensions to the fringes, and to choose that which is the most convenient for observation. The double transit of the light was for the purpose of augmenting the distance traversed in the medium in motion, and further to compensate entirely any accidental difference of temperature or pressure between the two tubes, from which might result a displacement of the fringes, which would be mingled with the displacement which the motion alone would have produced; and thus have rendered the observation of it uncertain.

It is, in fact, easy to see that in this arrangement all the points situated in the path of one ray are equally in the path of the other; so that any alteration of the density in any point whatever of the transit acts in the same manner upon the two rays, and cannot consequently have any influence upon the position of the fringes. The compensation may be satisfactorily shown to be complete by placing a thick mirror before one of the two chinks, or as well by filling only one of the tubes with water, the other being full of air. Neither of these two experiments gives rise to the least alteration in the position of the fringes.

With regard to the motion, it is seen, on the contrary, that the two rays are subject to opposite influences.

If it is supposed that in the tube situated to the right the water runs towards the observer, that of the two rays which comes from the right will have traversed the tube in the direction of the motion, while the ray coming from the left will have passed in a direction contrary to that of the motion.

By making water move in the two tubes at the same time and in contrary directions in each, it will be seen that the effects should be added. This double current having been produced, the direction may be again reversed simultaneously in the two tubes, and the effect would again be double.

All the movements of the water were produced in a very simple manner, each tube being connected by two conduits situated near their extremities, with two reservoirs of glass, in which a pressure is alternately exercised by means of compressed air. By means of this pressure the water passes from one reservoir to the other by traversing the tube, the two extremities of which are closed by the mirrors. The interior diameter of the tubes was $5^{mm}\cdot3$, their length $1^{m}\cdot487$. They were of glass.

The pressure under which the flowing of the water took place might have exceeded two atmospheres. The velocity was calculated by dividing the volume of water running in *one second* by the area of the section of the tube. I ought to mention, in order to prevent an objection which might be made, that great care was taken to obviate the effects of the accidental motions which the pressure or the shock of the water might produce. Therefore the two tubes, and the reservoirs in which the motion of the water was made, were sustained by supports independent of the other parts of the apparatus, and especially of the two lunettes; it was therefore only the two tubes which could suffer any accidental movement; but both theory and practice have proved that the motion or flexions of the tubes alone were without influence upon the position of the fringes. The following are the results obtained.

When the water is set in motion the fringes are displaced, and according as the water moves in the one direction or the other, the displacement takes place towards the right or the left.

The fringes are displaced towards the right when the water is running from the observer in the tube situated to his right, and towards the observer in the tube situated to his left.

The fringes are displaced towards the left when the direction of the current in each tube takes place in a direction opposed to that which has just been described.

With a velocity of the water equal to $2^{m\cdot}$ a second, the displacement is already very sensible; with a velocity of 4 to 7 metres it is perfectly measurable.

After having demonstrated the existence of the phænomenon, I endeavoured to determine its numerical value with all the exactitude which it was possible to attain.

By calling that the simple displacement which was produced when the water at rest in the commencement was set in motion, and that the double displacement which was produced when the motion was changed to a contrary one, it was found that the average deduced from nineteen observations sufficiently concurring, was 0·23 for the simple displacement, which gives 0·46 for the double displacement, the width of a fringe being taken as unity. The velocity of the water was 7·069 metres a second.

This result was afterwards compared with those which have been deduced by calculation from the different hypotheses relative to the æther.

According to the supposition that the æther is entirely free and independent of the motion of bodies, the displacement ought to be null.

According to the hypothesis which considers the æther united to the molecules of matter in such a way as to participate in its motions, calculation gives for the double displacement the value 0·92. Experiment gave a number only half as great, or 0·46.

According to the hypothesis by which the æther is partially carried along, the hypothesis of Fresnel, calculation gives 0·40, that is to say, a number very near to that which was found by experiment; and the difference between the two values would very probably be still less if it had been possible to introduce into the calculation of the velocity of the water a correction which had to be neglected from the want of sufficiently precise data, and which refers to the unequal velocity of the different threads of fluid; by estimating the value of that correction in the most probable manner, it is seen that it tends to augment a little the theoretical value and to approach the value of the observed result.

An experiment similar to that which I have just described had been made previously with air in motion, and I have demonstrated that the motion of the air does not produce any sensible displacement in the fringes. In the circumstances in which that experiment was made, and with a velocity of 25 metres a second, which was that of the motion of the air, it is found that according to the hypothesis by which the æther is considered to be carried along with the bodies, the double displacement ought to be 0·82.

According to the hypothesis of Fresnel, the same displacement ought to be only 0·000465, that is to say, entirely imperceptible. Thus the apparent immobility of the fringe in the experiment made with air in motion is completely in accordance with the theory of Fresnel. It was after having demonstrated this negative fact, and while seeking for an explanation by the different hypotheses relating to the æther in such a way as to satisfy at the same time the phænomena of aberration and the experiment of M. Arago, that it appeared to me to be necessary to admit with Fresnel that the motion of a body occasions an alteration in the velocity of light, and that this alteration of velocity is greater or less for different mediums, according to the energy with which those mediums refract light, so that it is considerable in bodies which are strongly refractive and very feeble in those which refract but little, as the air. It follows from this, that if the fringes are not displaced when light traverses air in motion, there should, on the contrary, be a sensible displacement when the experiment is made with water, the index of refraction of which is very much greater than that of air.

An experiment of M. Babinet, mentioned in the ninth volume of the *Comptes Rendus*, seems to be opposed to the hypothesis of an alteration of velocity in conformity with the law of Fresnel. But on considering the circumstances of that experiment, I have remarked a cause of compensation which must render the effect of the motion imperceptible. This cause consists in the reflexion which the light undergoes in that experiment; in fact it may be demonstrated, that when two rays have a certain difference of course, that difference is changed by the effect of the reflexion upon a mirror in motion. On calculating separately the two effects in the experiment of M. Babinet, it is found that they have values sensibly equal with contrary signs.

This explanation renders still more probable the hypothesis of an alteration of velocity, and an experiment made with water in motion appears to me completely appropriate to decide the question with certainty.

The success of the experiment seems to me to render the adoption of Fresnel's hypothesis necessary, or at least the law which he found for the expression of the alteration of the velocity of light by the effect of motion of a body; for although that law being found true may be a very strong proof in favour of the hypothesis of which it is only a consequence, perhaps the conception of Fresnel may appear so extraordinary, and in some respects so difficult, to admit, that other proofs and a profound examination on the part of geometricians will still be necessary before adopting it as an expression of the real facts of the case.—*Comptes Rendus*, Sept. 29, 1851.

ON THE FORMATION OF ANHYDROUS CRYSTALLIZED ALUM. BY THE PRINCE OF SALM-HORSTMAR.

Alumina, obtained by precipitating ammonia-alum by ammonia and heating the precipitate to redness, was fused with four times its weight of bisulphate of potash; on treatment of the fused mass with water, six-sided tables which did not doubly refract light were left, and on analysis were found to consist of anhydrous alum.—*Journ. für Prakt. Chem.* vol. lii. p. 319.

ON THE COMPOSITION OF THE GASES EVOLVED ON THE PRODUCTION OF COKE FROM COAL. BY M. EBELMEN.

The question might arise, whether in the formation of coke from coal in a furnace, the air which enters the furnace gives up its oxygen to the matters which are evolved in the gaseous state, or to the solid carbon; and again, whether the oxygen forms carbonic oxide or carbonic acid. Ebelmen examined the composition of the gases of the coke-ovens at Seraing, and found that more than two-thirds of the hydrogen of the coal is burned, the remainder existing in the evolved gaseous mixture. The quantity of carbonic acid is three times that of the carbonic oxide.—*Comptes Rendus*, vol. xxxii. p. 92.