COMMENTS ON MORALES' PAPER ON FRESNEL'S PARTIAL CONVECTION OF LIGHT

by: The Editor.

Morales has presented this Journal with a most important paper. It rewards us as Editor for the tremendous effort put into the publication of an issue. Again and again we have ourself sounded the drums of war against this particular subject of De Fresnel's partial drag and by this time the readership will probably be finding it to be a drag, indeed. There has been discouragingly little response to the call to arms and Dissidence goes on accepting the fallacy despite all: partial drag is accepted by the textbooks and therefore has to be correct and no criticism by any other Dissident is worthwhile paying any attention to. We are glad to find at least one ally on our side, after all these years.

Morales begins with a discussion of Hoek's experiment. Despite the key importance of that experiment, a description or discussion of it is scarcely to be found in the standard textbooks of the literature. Even Kantor, well versed in most of the experiments relative to the propagation of light, fails to refer to it in his book. In fact, when we went over to the local university library to locate the exact reference to the original source and description of the experiment, after searching through over 200 volumes that ought to contain such a reference, we did not find a single one in the whole lot. The major, great, world encyclopedias do not say a word about it nor do they even mention Hoek as among the celebrities of science. Born, from whom Morales got the description that is given in Morales' paper, fails to state where the original paper appears and it took a whole half day of library searching to locate it. It is referenced now as [1] below, but we still have not been able to obtain the original as of the time of writing since the university library does not have the back issues of either periodical in which it appears. It has been requested on library interloan and if it comes in before press time we shall certainly reprint the paper here to make it more readily available to the interested readers of this journal, in this issue if possible, in the next if it does not come in in time.

In the meantime, we are compelled to rely on Born's description, something we are reluctant to do at any time - that is, accept a secondary interpretation - and we are especially reluctant to have to do so through the eyes and intellect of any relativist. Those people see things in the weirdest and most illogical light, having little intellect or intelligence left after the brainwashing they have been subjected to while acquiring competence, so-called, in relativity. Seeing something through another's eyes is bad even despite that and an example of it is Minneart's comment on this particular experiment:

> **Hoek also investigated optical phenomena in moving bodies (1861-1869). Fresnel had suggested, and Fizeau had found through experimentation (1851), that in an object moving with a velocity v the "ether" is carried along with a velocity**
>
> $$vk = v\left(1 - \frac{1}{n^2}\right),$$
>
> **where n is the index of refraction. By a modified setup Hoek (1868) reduced this experiment to a zero-method and confirmed the value of the convection coefficient k with a much higher precision (1.3 per-cent). In the theory of relativity this result is explained, without any "ether," by simple application of the velocity-addition theorem.**

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The two interpretations of the experiment, the classical and the relativistic, are at variance in principle with one another and, for that matter, they are quite irreconcilable with one another. They both ignore the obvious: namely, that the light waves are moving with the medium which transmits them. This is precisely what Morales points out to us and we agree with him.

The strength of Morales' paper is not this quite obvious conclusion, but it is his demonstration that this property is invariant with respect to any velocity or orientation of direction of the motion of the transmitting substances in a global frame. That fact does not differ from the conclusion of the M & M experiment performed on a rotating sandstone block. This latter, much more famous experiment, is actually nothing but an encore to Hoek's. As Morales points out, as we have repeatedly before him, it is not possible to go on crediting De Fresnel's partial drag hypothesis. It is erroneous, and so be an end to it. Hoek's experiment negates it. The only alternative that is left and that can be considered reasonably is that both air and water, and presumably all other transmitting chemical substances, carry the electromagnetic waves traversing them concomitantly with themselves on a local basis.

After Hoek, whose experiment antedates Fizeau's, what has Fizeau's experiment with light in a moving column of H_2O to say? It purported to demonstrate three things:

1) that the moving H_2O - that is to say, moving now relative to a laboratory frame - does alter the velocity of a light wave traversing it and does convect it;
2) that, quantitatively, this change in velocity is governed by De Fresnel's formula

$$c_1 = c + (1 - 1/n^2)v$$

3) that for $n \approx 1$, there is no such convection within measurable limits.

Seemingly, we have a contradiction here; on the one hand, Hoek proves the convection is total; on the other, Fizeau proves it is partial and that it becomes vanishingly small as $n \to 1$.

Nature does not accept arbitrariness and we, as logical scientists should not either. Statement 1) can scarcely be disputed after the experimental determination has been made. Not only does the Fizeau experiment itself support it, but there are those of Zeeman and the Michelson-Morley redo of Fizeau's experiment. Hoek's experiment supports it when we change the static earthbound frame in which it was performed into either an aether frame or a celestial frame or a mechanically inertial frame with respect to which the Earth is in motion. Thus, we are forced to understand and believe that moving transparent matter does entrain light. There are no two ways about it.

The question is not whether it does or does not - that is settled - but it is to what extent. What is the quantitative amount of the convection? Is it total, or is it partial? We have seen that Hoek's experiment shows it to be total, as does the M & M experiment on the sandstone block. On the other hand, again, the Fizeau experiment, the Michelson-Morley redo and the Zeeman experiment indicate the convection to be partial under the circumstances of those experiments. This is a contradiction which we refuse to believe Nature permits to occur; neither will we permit the relativists to warp mathematics with their illogical rationalizations such as the Lorentz transformation so as to get around the difficulty. Something is wrong.

What may it be? The key to the puzzle was furnished by Galileo long since. He noted that the pitch of a sound made inside a cabin aboard ship is different to that made on the deck, when the ship is in motion. This is due to the movement of the boat through the encompassing air mass in the latter case; while in the former, the sound waves are propagated through a medium that is contained in the cabin and that moves with it. There is a difference in the experimental conditions. There is also

a difference in the Hoek and the M & M class of experiment, between those and the Fizeau, its redo and the Zeeman class. The classes are not the same and the same consequence may not be expected, therefore.

The difference lies in the fact that in the former class, the transmitting medium is moving at the same velocity as the source and receiver in the laboratory cabin. On the other hand, in the second class of experiment, the transmitting medium is moving relative to the source and receiver over a portion, and only a portion, of the light path. Here it is necessary to consider all parts of the path, most particularly those in the air between the source and the moving segment of the medium, and also, in the Fizeau experiment, the portion of the water column that is at rest against the viewing ports at the ends of the tubes. In other words, in the partial convection cases, the medium undergoes an acceleration from rest relative to fixed source and receiver, somewhere along the light path, to a velocity different from zero relative to the same rest frame.

There is an absolute world of difference in mechanics between the properties of matter that is in uniform motion, and those that are pertinent to matter in accelerated, non-uniform motion. For one thing, forces arise in the latter while there are none involved with the former. There is no theory, known to this writer aryway, that deals with the behaviour of light waves in an accelerated transparent medium. But here the theories pertinent to a medium in uniform, unaccelerated motion have been applied cavalierly to a medium that is in non-uniform, accelerated motion. The transmitting medium is necessarily accelerated in the so-called open experiments - the case of sound on the deck, the Fizeau, redo and Zeeman class of experiment. These experiments are, however, analysed while ignoring the acceleration of the media, as if those portions of the light path were insignificant to the result. We remark that it is precisely this very class of experiments that is producing the anomalies that cannot be reconciled with any reasonable theory and that are forcing the warping of reason to get around the difficulties they present when they are being incompletely analysed in this way.

Not only does one not know how light behaves when transiting an accelerated medium, but there is no worthwhile theory to explain it. In the meantime Fizeau, Michelson and Morley, as well as Zeeman, go on pretending that it must behave as if it all did not matter. It is only the closed class of experiment that can be considered reliable, until we have some reasonable theory to cover the accelerated case; and, more important still, have an accurate evaluation of the distribution of the accelerations in the medium between source and receiver, up to and including the segment of the light path that is in relative motion between the two. That information is totally lacking from the open experiments; and, moeover, we need an appropriate and correct analysis to go with it all. All that is lacking from Fizeau's, Zeeman's and Michelson and Morley's works. Statements are simply uttered by these experimenters and assumptions are made by them without the least backup for them from the theory of fluid dynamics. When this writer has gone in himself and attempted to make good the deficiency with some sort of requisite justification based on classical theories of fluids, he has found that the experiments were so poorly designed to begin with by persons unacquainted with the extreme difficulties of a theoretical analysis, that it is all but impossible to make any such analysis. None was attempted by Fizeau, M & M, nor by Zeeman. We have attempted it using modern computational techniques and have found in the liquid cases, that the flows are turbulent with vortical backflows occurring. This occurs in just the very instances of the anomalous conclusion of a supposed partial drag. We can rely only on the closed set of experiments such as Hoek's and there there is no difficulty that presents itself.

The Fizeau experiment and the M & M redo of it are supposed to have verified the De Fresnel conjecture

$$c_1 = c + (1 - 1/n^2)v$$

when $n \approx 1$, as in the case of air when no convective drag is supposed to occur at all and the light waves propagate, supposedly, relative to the nebulous aether-sea-at-rest. This relates to property 3) mentioned above. This was done by both the experimenters simply blowing air down the tubes at a high velocity. They then failed to observe any change in the fringe displacements in any way comparable to what occurred when water flowed through them with a much greater value of refractive index, $n = 1.33$. Again, there is no aerodynamical analysis supporting the conclusion arrived at and the assumption was made that the air flow would be simple and rectilinear. This is very highly unlikely to be the actual case and a helical or compartmentized flow is the much more reasonable probability, if we allow ourselves the luxury of degenerating to guesswork. It would have been instructive had the experimenters employed some liquid of $n \approx 1$ in place of air, but they did not do so.

Aerodynamics is a most difficult and uncertain subject for mathematical description. The open type of experiment is thus a quantitatively unreliable means to prove De Fresnel's conjecture in respect to 3). The Hoek experiment might be readily adapted to demonstrate that the convection in air is total; indeed, it requires hardly any adaptation at all. We actually need only to reexamine it in a somewhat different light. Suppose that we imagine there were a second container W_2 of material, which might be air but we prefer it to be some liquid with $n_2 \approx 1$, in the path of the second ray, figure 1. Now, if the drag be partial, as De Fresnel states, as the two containers are oriented into the aether stream there is no convection to speak of by the matter in W_2 but there would be by the H_2O in W_1. On reversing the orientation to the aether stream, the same condition prevails with respect to W_2, while the convection by the water-filled W_1 would be reversed. Thus, a fringe movement with reversal thus is predicted by De Fresnel. But is not all this just what failed to occur when Hoek did the experiment? All that is lacking is the imaginary container for the air, W_2, and it really plays no part in the experiment.

The experiment would be more direct if the arrangement of source and interferometer were as in figure 2. Here we would not encounter any reversal of light path in respect to the aether.

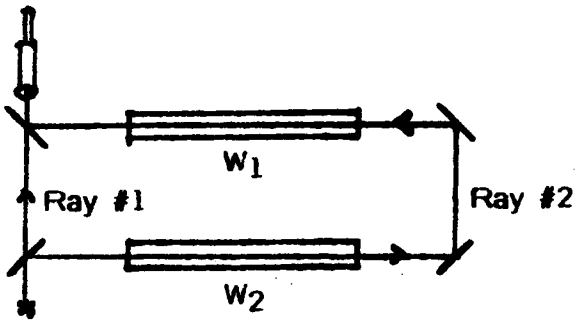

Figure 1.

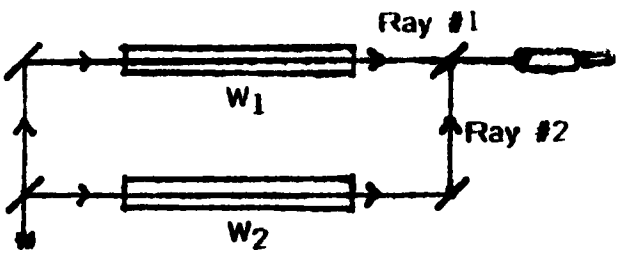

Figure 2.

In §3, Morales indulges in a gedanken experiment. Of the many deliterious things Einstein did for science, the gedanken experiment is the most pernicious. By definition, a gedanken experiment is one that can be performed effortlessly, always yields uniquivocal results, that are of incontestable certainty and which invariably support the gedanken experimenter's preformed, bigoted opinion. Starting with the granddaddy of all gedanken experiments, the train and embankment gedanken experiment, they usually involve some confusing bit of sophistry. Like the ancient paradoxes of Zeno, they are hard to disentangle. If, instead of presenting the outcome of such experimentation as factual - more sophistry - the gedanken experimenter would just present his consequences, so-called, to us as provisional upon an actual performance of a veritable experiment similar to what he descibes to us, we would go along with him for the sake of argument. Were we to insist on that, in the train and embankment experiment, for example, the vanity of the hogwash would become conspicuous.

Morales' intended experiment is a well conceived one and might yield the conclusion he says would be the outcome of it. But, is his experiment not, in the long run, prescisely the experiment which Marinov, Wesley and Silvertooth have actually performed, without the outcome being what Morales says it is? The outcome of the performed experiment is disputed but we are supposed to accept the outcome of the hypothetical experiment instead.

It may be said of the proposed experiment that it is one of great simplicity, much less encumbered by apparati than were those of Marinov et al., in the same sense that Hoek's experiment is vastly superior to the M & M experiment in design and therefore more reliable. If Morales performed his experiment it might not really end all dispute of the fretted question of the aether-sea-at-rest for wranglers will go on, but it should in the mind of the more intelligent. However, the gedanken experiment is not performed, so the stated outcome of it is postulatory.

Out of his formula (6) Morales has drawn the false implication that $0 = V$ 0. The actual implication is that $V = 0$, in which case it is readily seen the equation balances. This implies a local carrier for light rather than a cosmically based one.

Section 4 of this paper is mathematically unsound. We have pointed this out to Morales when sending him the proof, but we have left the section in nonetheless because it is a challenge in concept to dispersion as a velocity change with the different wavelengths. This challenge may have merit but not on the basis of Morales' discussion of it. His fallacy is readily seen, for simply put $V = 0$, then the time difference between the arrival of the red ray and that of the yellow ray is inherent in the expression

$$\frac{1}{c_1} - \frac{1}{c_2}$$

and nothing has been proven as to whether this is zero or not.

The writer knows of no direct experiment ever made to show whether red light takes longer to traverse a medium than yellow light. The inference is always drawn from a difference in the angles of refraction of the two and the presumed validity of the Huyghens theory that light velocity and the angle of refraction have a direct relation through Snell's law. If Huyghens' theory is set aside and some other wave mechanics rather than that the wave be an envelope of wavelets were to replace it, then the angle of refraction and the refractive index (velocity of the wave) may not be directly related as they are according to the Huyghens' theory.

Section 6 of the paper is also fallaciously reasoned, as based on what is no different from the Huyghens' wavelet-envelope principle, Morales comes out with an entirely different conclusion to Huyghens'. We have gone over the Huyghens argument well enough to know that his conclusion is the correct one. More on this in another critique.

As was pointed out at the outset, the paper is a valuable one to be seriously considered, after setting aside some parts of it that are erroneous.

References

[1] Hoek, Martinus: **Détermination de la Vitesse avec laquelle est Entrainé un Rayon Lumineux Traversant un Milieu en Mouvement**, Verslagen der Koniglijke Akademie van Wetenschappen te Amsterdam, 2nd Ser., V. 2. pp. 189-94 (1868); & V. 3, pp. 306-13 (1869). A slightly abridged text of this is found in Astronomische Nachrichten, V. 70, pp. 193-8 (1867) & V. 73, pp. 193-200 (1869). There seems to be a related paper written with Oudemans, A. C.: **Recherches sur la Quantité d' Ether Contenue dans les Liquides**, Recherches Astronomiques de l' Observatoire d' Utrecht, V. 2, pp. 1-71 (1864).

[2] Minnaert, M. G. J.: Biographical article on Hoek in the Dictionary of Scientific Biography, V. 6, p. 453, Scribner (1970).