A DISCUSSION OF NEDVĚD'S PAPER 'THE LIGHT FROM BINARY STARS' AND OF EDWARD'S CRITIQUE OF THE SAME

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1. **Introduction.** Nedvěd's paper **The Light From Binary Stars** was published in the January 1988 issue of this **Journal** [V. 6, #4, pp. 3355-9]. His topic was of such fundamental scientific significance that a discussion of it was called for even among the regular contributors to this publication. We welcome at all times the criticisms from readers outside this group if they are worthwhile critiques of the papers appearing here, but discourage the same thing from within it, as likely to lead to internecine infighting with bare knuckles. We do not mind bare knuckles - rather enjoy them in despite of a bloody nose or so - but in such instance the outcome is likely to be prejudicial to this publication on which too much effort is expended to allow any harm to come to it. However, in this case an exception was made as attention needed to be given to what Nedvěd had to say.

The importance attached to his effort is that it is a direct challenge to DeSitter's argument that dealt the death blow, presumably, to the emission, corpuscular and ballistic forms of the theory of light. In recent times, the common principle behind these theories has come to be referred to as the Ritzian principle, though there was nothing basically novel in what Ritz himself advanced, as it had been introduced already in variant forms by Euler, even Newton, De la Place, Poisson, Biot and probably a dozen other lesser scientific figures several centuries ago. The principle referred to is that light propagates at velocity c relative to the source of its emission, so that if that source is moving at velocity ±v towards or away from a receiver, the velocity of the emission is c ± v relative to the receiver. It is most important to emphasize that this property is supposed to hold everywhere along the path which the light ray pursues except for some refractive corrections according to Snell's law when it enters some medium other than a vacuum. For instance, the ballistic theory of light which presumes light to be made up of massed, small particles, agrees with the c ± v principle and it was Euler who demonstrated that it formed a consistent theory; which, of course, it does, for the principles of classical mechanics have merely been carried over into it. There are numerous variants on this ballistic theory which frequently drop the massed particle supposition and replace it with other things. Nedvěd's own theory of light propagation is one of them.

There are many significant objections to the c ± v principle, of which DeSitter's argument is only one. Another relates to the behaviour of light at a moving reflector, for if it follows a purely mechanical principle of massed ballistae, then the emanation must rebound from the moving reflecting surface acquiring an enhanced velocity. This it apparently does not do as experiment has indicated. This has been the principal reason for the many variants of the ballistic theory which either drop the massed property of the emanation or go over to a wave theory in an aether, or do something else. A second major objection is that the light from moving laboratory sources has been experimentally tested for the presumed change in velocity and found not to obey the enhanced velocity condition. A third objection is that the velocity of light from stars, which according to Doppler effects are presumably moving at significantly great velocities towards and away from the Earth, has been measured and does not obey the c ± v principle. The fourth important objection is DeSitter's argument to which Nedvěd's paper is a challenge. There are still other objections involving a variety of laboratory experiments that have been done. There is one outstanding experiment in favour of the principle: i.e., Kantor's, and to a partial degree that of Babcock & Bergman.

Edwards in his critique mentions that he only knows of Waldron who supports the c ± v principle - in addition to Nedvěd, of course. There are several living proponents of the principle, most notably Kantor, whose book, **Relativistic Propagation Of Light** is entirely devoted to a justification of it, about as strong a position for it being developed therein as can be assumed. Others are Tedenstig and Hobson, both contributors to this periodical; there are probably still more with whom the reviewer is presently unacquainted.

If we may introduce here a remark relative to the reviewer's own views and how they relate to Nedvěd's, our transmission theory does accept the c ± v principle but only in the immediate vicinity of the light source; it presumes that during transmission from source to receiver the emanation undergoes an alteration in velocity so that it is altered to c relative to the transmitting medium (up to Snell's law) that it encounters en route. We are in partial agreement, therefore, with Nedvěd, but only in a local sense and not in the global sense that he, Kantor, Waldron, Ritz, Tedenstig, Hobson and the ancients accept. We add, as well, it agrees with the ballistic theory at moving reflectors but only to the extent that the emanation has been brought to velocity c in the reflector's own frame already, before it impinges on it, and that after rebound it returns again to velocity -c in the same frame, thereafter being corrected once again to a velocity of c relative to the transmitting medium.

After opening Nedvěd's paper to general discussion, we waited for it, but there was none. Then we asked most of the senior contributors to study and evaluate the paper because of its fundamental significance as a possible rebuttal to De Sitter's counter-example to the c ± v principle. The response was quite disappointing; there were only two replies, one of which was too flimsy to merit publication. The remaining dissidents either wished to avoid controversy, were disinterested, or disagreed fundamentally with c ± v, having rejected it to the point of intolerance and contempt, unwilling to listen to any rebuttal reasonably; or they found they could not comprehend Nedvěd's exposition. So few of the contributors to this magazine take the necessary pains to make their ideas clear to their audience and when, as editor, we expostulate, we are told that we scold. When they discover that no one gives them the attention they feel they deserve, they are disappointed. No one cares to have to wade painstakingly through a paper to try and fathom a meaning or fill in undefined symbolism, never sure he has understood what was intended. We are acutely aware of this defect but can do nothing to control it, as the dissidents are, by and large, a headstrong group unwilling to listen to good advice.

2. Edward's Criticism. Though supremely grateful to Edwards, another journal editor familiar with our problems, for his cooperation, that does not let him off scot free and, as editor, we proceed to moderate the discussion in all fairness to Nedvěd, we hope, as well as in a search for the truth here.

Edwards begins by asserting that a quotation from Whittaker demolishes Nedvěd's position, that quotation depending heavily on the notions of relativity theory. In the first place, the theory of relativity has been so frequently shown to involve so many fallacies of logic, fact and self-contradiction, as well as experimental evidence, that it has more holes in it than a Swiss cheese. It is an asset to a theory, any theory, even one that the Moon is made of green cheese, that it would not agree with relativity; for to do so would be to say that it agrees with something that is false and therefore that it is itself fallacious.

Next, we are unimpressed by proof based on citation of the opinion of eminent authority. After all is said and done, is Whittaker a scientist of sufficient repute that we should defer to his opinion, merely as opinion? We actually know of only one contribution that Whittaker has made to science and that has been questioned already in these pages. What Whittaker is, is an eminent and exceptional bibliographer, a compiler of the works of others, a walking encyclopedia and a very learned man and a top level scientific historian. These occupations do not leave anyone the time to be creative as well. Were we to ask Whittaker what journal a certain paper appeared in or when, or even for the content of that paper, we would bow in deference to his opinion in preference to that of someone less well informed. But when it comes to his scientific opinion as compared to those of Newton, Euler, De la Place, Biot or Ritz, who have all espoused the principle in question, favoured by Nedvěd, Whittaker is a nobody. The same is true for most other textbook authorities, who are, in fact, very low on the rungs of the scientific ladder. Then, is this lesser authority an unbiased one, as he ought to be in his learned opinion? We submit that the very passage cited reveals a man taken in by the theories in popular vogue in his time, unlikely to think otherwise than as the Establishment dictates he should. Throughout his excellent History there is one similar passage after another where relativism is supported, to the very point where one acquires the impression of its author that he is playing patsy with the in-group of science so as to preserve his own repute with it. We can find scarcely a single reference or citation throughout the whole volume to the counter evidence offered to relativism. Whittaker was certainly far from unbiased. Even were he not, Truth is no respecter of persons and even Newton has made some boners that a child might correct.

Is it correct to presume that Nedvěd's theories are overthrown because Ritz's may have been? That is guilt by association. What is the distinction between Nedvěd's and Ritz's principles? If there is some, then, perhaps, Whittaker's remarks do not apply equally to Nedvěd's theories as they do to Ritz's. If there be no distinction, then it would be proper, if undiplomatic, to point this out and then apply Whittaker's remarks; at least one needs to show that the two theories are identical in the area to which those remarks do apply.

Edwards is correct in asserting that the doubling of the spectral lines from rotating binaries is observed fact and that Nedvěd's conclusion from his analysis that the effect could be observed has probability zero, therefore vitiates either his arguments or his postulates.

After all the whey is drained out of Whittaker's argument, it leaves behind one or two solid bits of evidence: that (uncited) astronomical evidence has been marshalled by several writers; that direct experiment by Majorana counters the c ± v assumption; and, that the R. Tomaschek experiment of 1924 has definitely disproven the ballistic hypothesis. The astronomical evidence is DeSitter's argument that Nedvěd is challenging. The Majorana experiment has been mentioned and is not without some questionable aspects. At the moment the reviewer does not have in hand Tomaschek's original report as it has been delayed in getting it through interlibrary loan in the usual hassle that is associated with that service. If it arrives before press time, it will be included somewhere in this issue; if not, it will be found in the October issue.*

With modern laboratory equipment it is not a difficult undertaking to test the c ± v hypothesis by direct measurement. It is no trick at all to electronically measure the time of transit of a light pulse over a path 20 cm long or even as short as 2 cm if necessary; the reviewer has done it a hundred times with little difficulty. What is difficult to instrument is some sort of shutter that will open or close in the order of a nanosecond; perhaps a Kerr cell might provide the answer. Two beams of light, one from a distant star known from its Doppler shifting to be travelling at some rate like c/10 away from us, and the other from a laboratory based source, are to be passed through the shutter simultaneously, both to be intercepted together. At a distance of a meter or so away, the leading or trailing edges of the pulses may be

* Added in proof: See pages 3675-86, this issue. Editor.

observed very readily with a simple solid state photocell. The difference in the time of arrival should correspond to 9/10c and c, respectively. What is lacking is the telescope capable of focussing on the star, of which there are an adequate number but which are usually very far distant from us. Probably a large instrument of an observatory is essential.

3. Kantor's Opinion. Kantor is generally very detailed in his supportive arguments for the $c \pm v$ hypothesis and equally so in his rebuttals of counter-evidence and argument to it. In the matter of the Tomaschek experiment, he is, however, strangely silent. We give his complete analysis of it:

> Tomaschek's repetition of the Michelson-Morley experiment used sunlight and starlight which were first reflected from heliostats into a telescope and then passed through a laboratory window to finally enter the interferometer. Naturally, the intervening glass predetermined a null result just as in Tolman's experiment.

We find that in the face of the major experiment negating his contention, he has nothing to say except to depart from his principle and now assert that it no longer applies when light has gone through intervening glass. Atmospheric air apparently does not have such an effect; he ignored its effect completely in the report of his famous experiment, and in private correspondence with the reviewer he has denied it has any. It is this sort of inconsistency of theoretical reasoning that has so discredited much of what Kantor has done.

In private correspondence with Tedenstig who also denies the DeSitter argument, Tedenstig likewise resorts to the same dodge of claiming that the lenses and the other parts of the optical equipment employed have altered the velocity of the stellar ray. We agree that this is the case for that is just what the transmission theory is all about, but both Kantor and Tedenstig reject the contention out of hand; which is a very inconsistent manner of thinking. Tedenstig goes further, declaring that we really do not know that the Doppler shifts of spectroscopic binaries are actually due to their rotating around one another, since we cannot see them doing it. He neglects the fact that there are many visible binaries and that the effects are correlated with their motions in an entirely reasonable manner and that the conclusion arrived at for them carries over continuously to successively more remote visible binaries and then to so-called spectroscopic binaries. Kantor shifts ground in exactly the same way and we quote the meaningful portion of his remarks on DeSitter's argument:

> The de Sitter argument (sometimes even referred to as an experiment) predicts the appearance of stellar ghosts and the distortion of the orbits of the double stars. The argument is utterly misleading in that it tacitly supposes an unambiguous knowledge of the stellar motions. The only *direct* knowledge of the stellar motion is provided by the light received from the stars. It is therefore circularly unproductive to infer the speed of propagation of the light emitted from the moving stars on the basis of the motion of the stars, since the only *direct* knowledge of the stellar motion is provided by the very same light whose speed of propagation will have its effect in the observed motion of the stars. Thus the appearance of ghosts and distorted orbits could not be recognized as such, unless the speed of propagation of the light were known independently beforehand.
>
> If the stellar motion were known independently of the received light, the "distorting" effect of the speed of the light could be recognized. Such knowledge is not available. The exact indirect inference of binary stellar motion from celestial mechanics is also circular, since all celestial motions are only known exactly by the light or other radiation received from the celestial bodies.

There then follows another paragraph or so of totally irrelevant remarks which are nothing more than a deliberate attemt to draw a red herring across the path of the curious investigator and throw him off the scent of the issue involved. We do not bother to reproduce these remarkes here. Finally, the dogmatic assertion is made:

> Thus the de Sitter argument is both wrong and inconclusive. Related arguments based on light or other radiative signals received from pulsating stars are also similarly circular unproductive speculations.

We have not bothered to reply to either Kantor or Tedenstig (or to Hobson either, for that matter) for when a debator has to resort to questioning evident fact so as to support an untenable but favoured position, the debate is over and one has reduced himself to being a wrangler instead of a truth seeker. Nothing is to be gained by involving himself in wrangling contention. Perhaps, we may fare better with Nedvěd, but we doubt it, as in seven years of publication of this **Journal** only a single dissident other than ourself has ever admitted an error or changed an opinion, however irrational it is proven to be; they are always right and commit no wrong - ever.

4. Some Inclarity On The Part Of Nedvěd And Of Edwards. It would seem that neither Edwards or Nedvěd is fully clear on the nature of the line shifting of spectroscopic binaries. Neither of them seems to have had recourse to DeSitter's original and easily read short note on the matter, but both seem to have gained their awareness from a secondary source which is itself misinformed, it seems. The doubling of the lines is not like the splitting of spectroscopic lines into doublets due to some cause such as a strong magnetic field or the like. The lines are separately due to individual moving light sources; one set of lines is pertinent to one of the stars and the other to the second. They regularly cross one another in a periodic fashion as the two stars both come into the position where they are moving transverse to the line of sight to the Earth; then they reverse their positions relative to one another in a periodic fashion again, with the Doppler shifting indicating an approach of one of the stars towards us while the other is retreating from us during the half period; with the reverse taking place during the second half period. In many instances, due to different spectroscopic characteristics of the two stars, each may be traced separately from the other. Nedvěd seems to think that the shifting of the lines has something to do with the interference of light waves and interprets this misconception according to his own theories. Edwards seems to accept this interpretation as if it were pertinent to the phenomenon; but it is not.

The significant factor in DeSitter's argument is that there is no outphasing between the periods of velocity changes of one star as compared to those of the other. The periodic changes stay together despite the long distances to the stars, during which time the signals that might be moving at c + v would be expected to outrun those at c - v, if the c ± v hypothesis were true. This is the crux of the negative inference which the proponents of the c ± v hypothesis have to rebut satisfactorily and with clarity of implication, in logical detail, so as to revitalize their theories.

5. The Principal Error Involved In Nedvěd's Analysis. Nedvěd appears to have neglected in his analysis that the two stars in circulating one about another, do not do so independently. By mechanical principle, they must rotate about their common center of gravity, which in the simplified case studied in which the two masses and radial distances are presumed to be the same, implies that the two stars are at the ends of a diameter through this point. Using figure 1, which retains the same notations and pertinent detail as in the original paper, this implies that the two stars C_1, C_2 are at all times diametrically opposed to the inertial center S, and that therefore at all times $\alpha_2 = \alpha_1 + \pi$. This point has been neglected and it has been

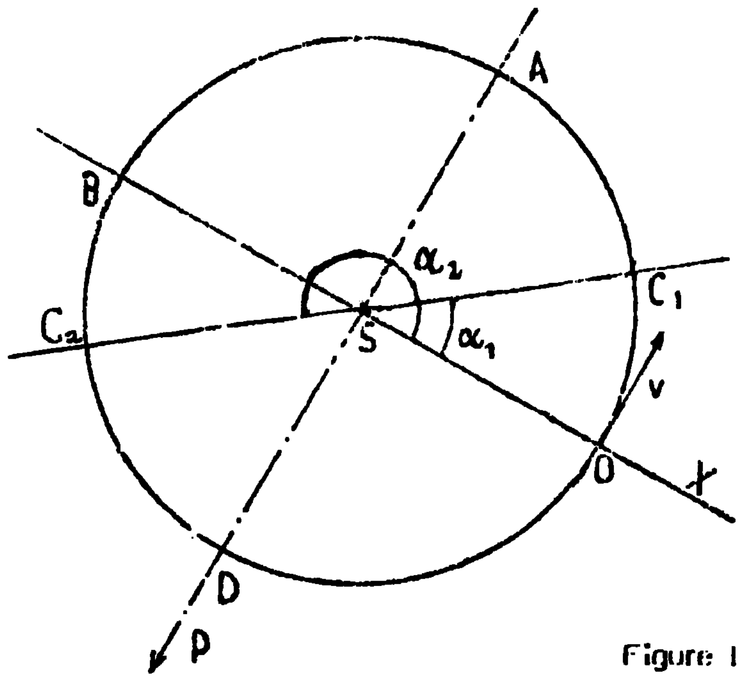

Figure 1.

presumed that the stars emit light from anywhere on the orbit and can be independently positioned there so that the angles α_1 and α_2 can satisfy the relations (2, 3, 4) of the original paper. This is false. Without those relations there simply is nothing more that can be said and the rest of the analysis amounts to nothing.

6. De Sitter's Point Elaborated Upon. Let us consider a time at which C_1 is at A and therefore C_2 is at D. By the c ± v hypothesis the components of velocity projected on SP are zero so that the velocity of the light reaching the far distant observer at P is simply c. Both signals from A and D arrive simultaneously at P (or very nearly so since the diameter AD is presumed to be insignificant to the analysis). When the light reaches P, perhaps years later, its Doppler shifting indicates no radial motion in the direction SP, so that the spectral lines coincide. We take this to represent the beginning of the period.

When the stars reverse positions and C_2 is at A and C_1 is at D, the spectral lines again coincide. This is a half period later. The next time the coincidence takes place, a full period has elapsed and the stars have returned to their original positions. This goes on regularly and based on the observations that can be made of visible binaries, the coincidence of the spectral lines agrees with the times when the stars are in the line of sight. This conclusion is transferred to spectral binaries; and validly, the reviewer believes.

Now, let us observe the state of affairs when C_1 is at B and C_2 is at O. If the c ± v hypothesis were valid, then the light from C_1 would travel at velocity c + v towards P, while that from C_2 would travel at velocity c - v. When C_1 is at B, the Doppler shifting apparent to the observer when the signal finally arrives at P, would be maximum, occurring at time $t_B = L/(c + v)$ later, where L = distance[S, P]. But when C_1 is at B, C_2 is at O, and the time at which the signal informing the observer of the fact, under the c ± v hypothesis, would be at $t_O = L/(c - v)$. This is when the Doppler shifting would again be maximum but in the opposite direction to the above. Now it is easily seen that $T_B \neq T_O$, unless v = 0, which is a trivial case of no significance.

However, the observed fact is that $T_B = T_O$; that is to say, there is no outphasing between the events as seen at P. Indeed, the maximal Doppler shiftings occur together, and, moreover, at intervals corresponding to the quarter and three-quarter intervals of the period.

Since the c ± v hypothesis has led to a conclusion not accordant to the physical facts, we conclude it was an erroneous supposition. It must be discarded in favour of something else that is.

As one can see, Nedvěd's analysis is skew of the point, which has nothing to do with the type of interference effect he has chosen to consider. So far off is it, indeed, that it has no relation to De Sitter's objection whatever.

7. Concluding Comment. This Journal has welcomed the present attempt to rebut DeSitter's argument, even if it has failed and been shown to be a misanalysis. It will also welcome further efforts to do the same validly, up to the point where it becomes evident, one way or the other that DeSitter was either in error or correct.' It is only by such testing and retesting of theory that we can become confident of the validity of such arguments advanced in science. Errors of logic are often very subtle; errors of interpretation of factual evidence, even subtler; finally, the experimental evidence itself can be misleading or be drawn by the experimenter's bias, terribly. Continual checking and rechecking of what is accepted opinion needs to go on all the time, with challenges being made such as this one to the most obvious seeming facts. After all, it did seem obvious, and still does to the unsophisticated mind, that the Earth is flat. No challenge was permitted, therefore, from the dawn of man's existence until comparatively recent times in the Middle Ages, to what was self-evident, seemingly. The old journals simply did not allow it; the religious tenets of the past forbade it as atheistic. Eratosthenes, who did challenge the universal consensus of all intelligent scientists of his age, was laughed at and ignored as just another dissident.

On the other hand, nonetheless, having challenged, but failed to carry the point, the intelligent man should be wise enough to check and see if his own thinking need not be challenged. We hope this will be the case here with the modern advocates of c ± v. If they are in error, at least they have been in good company along with Newton, Euler, De la Place, Biot, Poisson and Ritz. What is not to be condoned is the stubborn refusal to acknowledge error and then move on to an improvement of ideas, merely to save face and admit being in the wrong and not that godlike intelligence self-estimation may have led one to believe himself to be.This last is, unfortunately, the dissident's heel.