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LETTER TO THE EDITOR

'Light ray and particle paths on a rotating disc': a reply to comments by Ashworth, Davies and Jennison

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In this letter we take issue with some of the comments by Ashworth, Davies and Jennison (1978, hereinafter referred to as ADJ) on our paper (McFarlane and McGill 1978) on light ray and particle paths on a rotating disc.

ADJ object first to our use of the metric

$$ds^2 = dt^2(1-\omega^2 r^2/c^2) - c^{-2}(dr^2 + r^2\,d\theta^2 + 2\omega r^2\,d\theta + 2\omega r^2\,d\theta\,dt) \qquad (1)$$

which, they say, is not generally accepted as being 'correct' for the description of a rotating system. This objection appears to be based on a trivial misunderstanding of the status of the transformation equations

$$\bar{r} = r, \qquad \bar{\theta} = \theta + \omega t, \qquad \bar{t} = t, \qquad (2)$$

which we used in deriving equation (1). This is the rotational analogue of the Galilean transformation equations for inertial frames mentioned in virtually all textbooks on the special theory of relativity. It is certainly true that the Galilean transformation is normally regarded as incorrect (except as a small-velocity approximation), to be replaced in the conventional analysis by the Lorentz transformation equations, *on the understanding that the symbols x, y, z and t have their usual, pre-determined physical meaning.* However if this restriction is relaxed, transformation equations of the Galilean type (and others) are perfectly capable of describing events in a reference system which moves with respect to an inertial frame, though the coordinates used now have a more complicated physical meaning than in the Lorentz case. For the case of the rotating disc, an analogue of the Lorentz transformation does not exist, and the simplest alternative is the Galilean transformation. There is no question of such a transformation being 'incorrect', however; only the correctness of the *physical interpretation* of the coordinates can be disputed, and ADJ do not attempt to do this.

In any case, if ADJ were right in suggesting that equation (1) is 'correct' only for small values of $\omega r/c$, we would have expected them to be able to demonstrate that our analysis gives results which are only approximately valid (to first or second order in $\omega r/c$, say), in contrast to the exact results found by different methods. Instead, however, ADJ acknowledge that some of our results are the same as theirs, which either implies that their results are inexact as well or calls for an explanation as to how an analysis based on inexact equations is able to produce exact conclusions.

ADJ lay special emphasis on the need to formulate a description of light and particle paths in terms of physical information directly available to a single, fixed observer. In

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our view this requirement, appropriately generalised, goes far beyond the requirement that a physical theory be testable, and is not satisfied in the conventional formulation of any branch of physics that we are familiar with. Whether or not their objective is desirable, it is certainly unnecessary and in practice appears to lead to a needlessly complicated description of events.

We note that ADJ now make only the restricted claim that *some* forms of distance in rotating systems (in particular, radar distance and parallax distance) suffer a radial contraction. This has certainly clarified a few matters, but we still fail to see why these concepts are useful in the context of the rotating disc. The concept of radar distance, for example, appears to explain nothing which cannot be understood in terms of the conventional frequency shift formula (equation (30) in our paper); and it does not seem possible to make radar distances the basis of geometrical calculations, or even (Davies 1976) to specify explicitly the radar distance between two arbitrary fixed reference points when neither coincides with the origin. The confusion which we think is likely to be caused by the use of radar distance in addition to the conventional notion of distance in accelerated reference frames (Møller 1952) is well illustrated by ADJ's semantic problem of distinguishing between 'distance' and 'length'.

Returning to the experiment by Davies and Jennison (1975), we note that ADJ do not challenge our argument that the null result achieved in the experiment is predicted by conventional theory and does not require to be explained in terms of contracted radar distance. It cannot, therefore, be taken as experimental 'proof' of such a contraction. Actually, the null result is a particularly insignificant consequence of the frequency shift formula, in the sense that causality alone requires the rejection of any theory which does *not* yield the null result. For suppose that the frequency of a system of light pulses, on arrival back at the centre of a rotating disc after a to-and-fro journey to a point on the periphery, were greater than the emitted frequency. Since the experiment can be prolonged indefinitely and since the duration of a to-and-fro journey for any one pulse must be independent of when the pulse is sent out, after a sufficient time has elapsed since the start of the experiment a greater number of pulses would have returned than had been emitted. The creation of pulses from nothing would then defy causality. By a similar argument the frequency ratio cannot be less than unity either if causality is to hold. The Davies–Jennison experiment can therefore be regarded as a (successful) test of causality, but in our view is incapable of shedding light on anything else whatsoever.

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