THE LORENTZ TRANSFORMATIONS*

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Sir Harold Jeffreys (1958) has asserted that my analysis of the clock paradox (Builder 1957) is based on concealed hypotheses ; but the hypotheses to which he refers are ones that he claims are involved in the derivation of the Lorentz transformations and in the currently accepted methods of using them. He holds that the transformations cannot be shown to be unique and he infers that predictions based on them are not necessarily correct and " cannot be right in any case " when applied to systems that are inertial at the time considered but that have been subject to acceleration at some other time. He claims that " standard works on relativity still start from the postulated invariance of the velocity of light, which can be stated in the form $\mathrm{d}s=0$ is equivalent to $\mathrm{d}s'=0$ " and asserts that it is inferred, without any disclosed justification, first that $\mathrm{d}s'=k\mathrm{d}s$ and then that $k=1$ and $\mathrm{d}s=\mathrm{d}s'$. He holds that the last step cannot be rigorously justified, for he mentions the two possibilities $k=1$ and $k=(1-u^2/c^2)^{-\frac{1}{2}}$ and remarks that : " The question is which, if either, is right ".

Whatever some standard works may do, the literature also abounds with standard works and with a wealth of papers which trace carefully the empirical and logical basis of the restricted theory. It seems clear that Jeffreys has himself taken insufficient account of the essentially empirical character of the theory. He does not adduce the *principle of relativity* or discuss its empirical background either in his present paper (1958) or in his book " Scientific Inference " (1957) ; it is not surprising that his discussion indicates that the " postulated invariance of the velocity of light " is not by itself sufficient to establish a unique set of coordinate transformations.

Jeffreys shows that the inference that $\mathrm{d}s'=k\mathrm{d}s$ is justified if it refers only to the restricted class of coordinate systems that are inertial ; but this restriction

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was already implied in the empirical basis of the theory and in the consequent formulation of the equality $ds'=kds$. He then states that the further step of putting $k=1$ requires " some comparison of scales " in the two systems ; and he seems to suggest that this is possible only if there are observers in both systems who can make *mutual* scale comparisons. On the contrary, the empirical evidence generalized in the principle of relativity makes such direct scale comparisons superfluous. This evidence, based on the Michelson-Morley and related experiments, was obtained solely by sets of terrestrial measurements, each set being made using a single arrangement of measuring apparatus which was subjected to a spatial rotation and/or was subjected to acceleration by the orbital motion of the Earth so that its motion, in the astronomical inertial coordinate system, was different at different times. Thus the formulation of the principle of relativity was based on experience of situations in which the same measuring devices were utilized in different inertial coordinate systems and in which the measuring devices were transferred from one such system to another by means of non-destructive accelerations.

The empirical background of the principle of relativity thus showed that the scales of measurement in different inertial coordinate systems could be specified by requiring that they be compatible with the transfer of measuring devices from one system to another. This was implicit in Einstein's (1905) discussion of coordinate systems in which the measuring devices used would be identical if compared together at rest, and also in his discussion of the behaviour of devices initially at rest and then set into uniform motion. This specification of scales, when taken together with the prescribed method for synchronization of clocks and with the principle that all inertial coordinate systems are equally privileged in the description of natural phenomena, requires that the coordinate transformations form a group with $k=1$ (e.g. Einstein 1905 ; Poincaré 1905 ; Eddington 1920). Thus, in the context of the restricted theory, just as in the context of Newtonian relativity, every physical device, so long as it does not lose its identity by destructive treatment, itself remains a standard for the scales of measurement of any inertial coordinate system in which it is at rest.

The enormous power of useful prediction displayed by the restricted theory of relativity depends on the fact that this specification of scales eliminates the need for mutual scale comparisons in each particular case ; at the same time, the accuracy of the predictions demonstrates the validity of this specification. Thus, if a device be in uniform motion in our own inertial coordinate system S, we can simply assert that the description of its behaviour that would be given by hypothetical observers in the coordinate system S' in which it is at rest would be the same as the description that would be given by us if it were at rest in our own system S ; we can then use the Lorentz transformations to predict uniquely how the moving device will appear to behave when observed in our own system S. The accuracy of predictions made in this way has been repeatedly confirmed by experiment and this experimental evidence is quite incompatible with $k=(1-u^2/c^2)^{-\frac{1}{2}}$.

In discussing the relative retardation of clocks, Jeffreys supposes that an observer R remains at rest in an inertial coordinate system, and that an observer

M travels away from R with uniform velocity u_1 in R's system to a distance X and then returns with uniform velocity $-u_2$; R and M are supposed to have identical clocks which are to be compared at the beginning and end of the experiment; the time recorded on R's clock between M's departure and return would be $T_R = X(1/u_1 + 1/u_2)$ and that recorded on M's clock would be

$$T_M = X\left\{\frac{k_1}{u_1}\left(1-\frac{u_1^2}{c^2}\right)^{\frac{1}{2}} + \frac{k_2}{u_2}\left(1-\frac{u_2^2}{c^2}\right)^{\frac{1}{2}}\right\}.$$

He then comments: "If $k=(1-u^2/c^2)^{-\frac{1}{2}}$, $T_M = T_R$ and Dingle's result follows. If $k=1$ Builder's result* follows. The question is which, if either, is right."

However, it has been shown above that the restricted theory entails that $k=1$. It is also to be noted that the value $k=(1-u^2/c^2)^{-\frac{1}{2}}$ would require that the observed "rate" of M's clock would be unaffected by its motion, for in this case each of the terms in the expression for T_M would become identical with those in the expression for T_R. This is completely incompatible with available experimental evidence, for Ives and Stilwell (1938, 1941) and Otting (1939) have confirmed, by measurements on the frequency of the light emitted by atoms in canal rays, that the observed rate of processes in systems in motion is reduced by the factor $(1-u^2/c^2)^{\frac{1}{2}}$, as predicted by using the Lorentz transformation; this has also been confirmed by observations that the decay time of μ-mesons increases enormously with increase in their velocity. Thus the suggestion that k might have the value $(1-u^2/c^2)^{-\frac{1}{2}}$ is clearly wrong.

Jeffreys then puts forward a further argument which I am quite unable to follow. He states that "M's clock is compared with R's before departure and $k=1$". He then assumes that it is necessary that M and R should "compare scales" during M's journey, e.g. by measurement of transverse displacements, but he points out that M must be subjected to some elastic deformation while being set in motion at the beginning of his journey and claims that this will vitiate any comparison of scales. This leads him somehow, and certainly without any justification, to infer that "Builder's result cannot be right in any case".

Even if such a "comparison of scales" were necessary, which it is not, it would need to be made during the intervals X/u_1 and X/u_2 during which M moves uniformly, and not during the short intervals τ required for acceleration. Jeffreys explicitly concedes in his previous paragraph that "the time τ does not matter since it is small anyhow and does not increase indefinitely with X". Thus any elastic deformation suffered by M's equipment during its acceleration is irrelevant. On the other hand, there would be no elastic deformation of M's equipment during its uniform motion and any comparison of scales that might then be made would obviously be consistent with $k=1$ just as when M and R were at rest together.

However, it is worth remarking that, once we start to consider the elastic properties of real bodies, or even of ideal bodies, we cannot pass lightly over the question of the behaviour of M's clock during acceleration. There would be no difficulty in taking this into account in the hypothetical experiment here

* That is, the result predicted by Einstein (1905).

considered, since the only accelerations involved are those necessary to set M into uniform motion away from R, to reverse his motion, and bring him to rest again with R. Effects of these accelerations could be calculated, or measured experimentally, and allowance could be made for them ; they could in any case be made negligible by using moderate accelerations followed by long periods of uniform motion. On the other hand, in the general case in which the motions of the clocks are arbitrary, the effects of acceleration on the rates of real clocks might well far exceed the predicted relativistic effects. Even an assumption that the clocks are to be ideal in some relativistic sense is not without difficulties, and this is important in considering the postulatory basis of the general theory of relativity.

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