THE CLOCK–RETARDATION PROBLEM*

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In his paper above (Dingle 1957) Professor Dingle attempts to show that my treatment of the problem of relative clock retardations (Builder 1957) is completely incorrect and that such retardations cannot occur.

It need hardly be said that my treatment is, in its essentials, that originally given by Einstein (1905) in his first paper on relativity, of which Professor Dingle has written elsewhere (1956*a*)—" That paper contains a most regrettable error, in a statement that a clock moving in a closed curve will be found, on returning to its starting point, to be behind a stationary clock ".

This treatment still seems to be accepted generally by physicists and previous efforts by Professor Dingle (1956*a*, 1956*b*, 1956*c*) to prove that it is wrong have been trenchantly critized by McCrea (1956*a*, 1956*b*), Crawford (1957), and Singer (1957). These criticisms are generally applicable to the arguments in his present paper.

Professor Dingle's forthright condemnation of my paper is based entirely on his proposition that it is impossible to distinguish an unaccelerated observer R (whom I supposed to be at rest in an inertial reference system) from an accelerated observer M (whom I supposed to travel away from R and then return to him). He claims that the " principle of relativity " allows us with equal justification to regard R as accelerated and M as stationary. He maintains that the situation is completely symmetrical, and that only the relative motion of M and R can have any physical significance.

Were this proposition tenable there would be nothing that could be said to refute Professor Dingle's main argument. In such case we would, of course, have to discard the restricted theory of relativity, because we could no longer identify the class of inertial reference systems to which this theory refers, i.e. those systems, in a region of the universe free of gravitational fields, which are unaccelerated.

Professor Dingle obviously bases his proposition on what may be called the " principle of relativity of accelerated motion ", according to which the absolute measurement of acceleration is said to be impossible. In his present paper, presuming that I do not think the general theory of relativity to be wrong, he infers that I " do not believe there are absolute accelerations " and that I must therefore accept his further proposition that the statement " that M is the accelerated observer " is meaningless.

It does indeed seem to be widely supposed that this " principle of the relativity of accelerated motion " is an inescapable consequence of the *principle*

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of equivalence of the general theory. According to the principle of equivalence, the description of phenomena in terms of the coordinates of an accelerated system of reference is indistinguishable from the description of the same phenomena in terms of the coordinates of a reference sytem at rest in an equivalent gravitational field. It is inferred from this that an observer, who is at rest in a system of reference, and who finds that the equations of mechanics do not hold in that system, will be unable to ascertain, by experiments or measurements *within* his system, whether it is in fact being accelerated in a region of the universe free of gravitational fields or whether it is in fact at rest in a gravitational field. If this is true, his measurement of the acceleration of a body can only be relative, i.e. relative to his own particular system of reference. This is the " principle of relativity of accelerated motion ". It is also applicable to the case of a physical system falling freely in a gravitational field. Such a system will satisfy the definition (Einstein 1905) of an inertial reference system, since observers at rest in it would find the equations of mechanics valid for phenomena occurring within the system. Such observers would be unable to ascertain, by analysis of such phenomena, or by means of any experiments carried out *within* the system, whether the system is inertial because it is at rest, or in uniform motion, in a field-free region, or because it is falling freely in a gravitational field. On this basis (however artificial this basis may seem) their measurements of acceleration could only be relative to their own system.

This " principle of the relativity of accelerated motion " will be assumed to be valid for the purposes of the present discussion* and its relevance to the clock retardation problem will be investigated. To this end, it is necessary first to make clear the context of that problem.

While it is true that all the clock-retardation experiments discussed by various authors are spoken of as being hypothetical, they are all regarded as being, at least in principle, physically possible.

This means that, in the description of these experiments, it is invariably implied that they would be carried out in the physical universe we know, and not in some abstract conceptual space unrelated to it. Otherwise, their discussion would fall outside the domain of physical enquiry.

Professor Dingle must, I think, agree with this. His own papers on " Relativity and space travel " bear it out; for he envisages a human being leaving the Earth and travelling through vast spaces of this universe before returning to the Earth.

Furthermore, it is always implied, if there is no specific statement to the contrary, that the experiment is to be performed in a region, of this physical universe, that is free of appreciable gravitational fields, i.e. in an inertial region

* I will show elsewhere that it is not a valid inference from the general theory. The fallacy in the inference lies in the identification of the coordinates of an accelerated reference system with the data accessible to an observer at rest in that system by means of his own perceptions and measurements. In my paper on " The resolution of the clock paradox " (Builder 1957) I have already shown that, in general, an observer at rest in such a system cannot, even in principle, measure the system coordinates of distant events.

in which the equations of Newtonian mechanics hold approximately and in which the restricted theory of relativity, and all the laws and equations of physics consistent with it, are applicable.

In this context we can, in company with Newton and all physicists, and in accordance with common-sense knowledge, indubitably distinguish between a body which is accelerated and one which is not, and we can indubitably determine whether a particular body is accelerated or not. Thus, in this context, the statements that " M is the accelerated observer " and that " R is not accelerated " have very definite and unambiguous meanings.

Furthermore, in this context, we can indubitably say that " something happens to M " when his motion suddenly changes (e.g. at the events E_2 and E_3 defined in my paper), even though we may not know or perceive any physical cause of this change. Equally, we can say with certainty that nothing corresponding to this has happened to R if his motion continues unchanged, i.e. if he remains at rest or in a state of uniform motion.

These statements are, of course, statements about accelerations relative to the physical universe. They are, nevertheless, appropriate statements in the specification of any physical problem, because the whole of our physical knowledge relates to this universe, and to it alone.

It is true that, when we consider the quantitative measures of the accelerations of bodies, in a field-free region of the universe, we are limited to measurements made in inertial reference systems. The quantitative measures so obtained of the acceleration of a particular body will differ from one such reference system to another and are in this sense relative; *but all such measurements will agree as to whether the body is accelerated or not.* Nor do the differences in measure disturb us; for the restricted theory of relativity has shown us that the same physical equations are valid, in each such inertial reference system, as relations between all physical quantities measured in that system. Moreover, if we know the measures of these quantities in any one such system, we can utilize the Lorenz transformations to calculate their measures in any other such system.*

In the specification of any physical experiment, to be carried out in a region of this universe free of gravitational fields, it is therefore sufficient to specify the motions of the various bodies and any other relevant data, in terms of the measures of any one arbitrarily selected inertial reference system in that region. This specification is quite definite, and the result of the experiment may be properly calculated using the laws and equations of physics known to hold in every such system. In general, it will be necessary to utilize the Lorentz transformation if it is desired to express these calculated results in terms of the measures of another inertial reference system; *but this is unnecessary if the calculated quantity is the relative retardation of two clocks during the interval between their successive coincidences; for this quantity is an invariant for all systems of reference and for all observers.*

* All this could be expressed more succinctly and elegantly in four-dimensional language. In particular, the invariant four-acceleration of a body is finite if the body is accelerated and is zero if it is not.

Thus the explicit statement of the context of the clock retardation problem results in a clear and indisputable definition of the various terms involved and leads to a definite prescription of the methods of calculation that must be used in the investigation of the problem.

What then of the " principle of relativity of accelerated motion "? The context shows that it is quite irrelevant to the problem, and the reasons for this are fairly obvious.

The " principle " cannot be relevant to experiments which are, by their specification, to be carried out in regions of the physical universe free from gravitational fields. The observers who are to conduct the experiments must first verify that this condition is satisfied. All our experience and knowledge of the universe leads us to believe that this verification is feasible and that, once it is complete, the observers can then ascertain whether any particular body is accelerated or not.

Are we deluding ourselves in this? It has been pointed out above that the " principle " would make it impossible for observers to ascertain, by experiments carried out *within* their own physical system, whether the space of this system is inertial *sui generis* or because the system is falling freely in a gravitational field. Could it then be that vast regions of our universe are not inertial *sui generis*, but only because our universe is falling freely in an even vaster gravitational field? If we admit this possibility, are we then to infer that a body which we know to be unaccelerated relative to our universe may in fact be accelerated in some wider abstract sense of the " absolute "?

Obviously such questions and speculations are meaningless. Even if they were not, they would remain irrelevant to the present discussion because we are here concerned only with the accelerations, relative to our physical universe, which enter into the equations of physics. Whether we call these accelerations " absolute ", or not, is immaterial.

Thus the " principle " gives us no grounds for supposing that we are deluded in believing that the observers can verify that the regions of the universe in which they are to experiment are in fact free of gravitational fields.

Under these circumstances the "principle" is wholly irrelevant. Indeed it is applicable only in a situation in which the observers making measurements of acceleration are unable to ascertain the relation of their own system of reference to the rest of the universe; otherwise they could detect the acceleration of a body " absolutely ", i.e. relative to the universe. Indeed, it is clear that the " principle ", even if it is valid, is essentially artificial in that it presupposes that the observers measuring the acceleration of a body are so restricted in their inspection of the universe, or are so limited in physical knowledge, that they are unable to ascertain their relation to the rest of the universe. In this respect, it contrasts strongly with the principle of relativity of uniform motion which has, ever since its statement by Newton, survived all the most ingenious, and quite unrestricted, attempts to find an exception to it.

Thus there is no trace of justification for Professor Dingle's contention that the general theory of relativity renders meaningless any concept of acceleration other than that of the acceleration of one body relative to another. Nor is there any reason for supposing that only such accelerations, of one body relative to another, can have physical significance.

His proposition, that the statement " that M is the accelerated observer " is meaningless, arises only because of his abstraction of the problem from its physical context and his consequent application of a " principle " which is wholly irrelevant in that context.

No one would dispute that, if we postulated an observer M located in an abstract conceptual space unrelated to the physical universe, we could not attach any meaning to the statement that M is accelerated ; but such abstract hypothetical cases lie outside the realm of physics.

His objections to my treatment of the clock retardation problem are therefore based on propositions which, even if they have any meaning at all, are irrelevant to the physical problem.

An examination of Professor Dingle's present paper shows that his detailed arguments are entirely dependent on these defective propositions. There is therefore nothing to be gained by discussion of these arguments. In any case they differ little from those, presented in his earlier papers, which have been effectively analysed and criticized by McCrea (1956*a*, 1956*b*) and Crawford (1957).

I would like to remark only on two points of detail. The first is that I agree with Professor Dingle that there is a distinction between " coordinate time " in general, and what he calls " observed time " ; the latter is always referred to by physicists as the " proper time " and this term was used in my paper, particularly in Section V. The second point is that I cannot agree that the expression " a moving clock runs slow " is a proper or precise statement of what we can infer from the restricted theory ; nor can I agree that, in this form, " it is equivalent to the Lorentz transformation ". Sections III and IV of my paper show clearly that it was just such looseness of statement and definition that gave rise to the ambiguity which resulted in the " clock paradox ".

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