

PROBLEMS IN SPECIAL RELATIVITY

Recent issues of *Wireless World* have seen writings by many people who feel disenchanting with the Special Theory, but whose case has been put in such a way as to cause further polarization of respective camps.

As a student, I was privileged to be lectured by Dr G. J. Whitrow, then Reader in Mathematics at Imperial College, who was then, and still is, one of the world's foremost authorities on this subject. I vividly remember the model posed by Whitrow in which the time-travellers would be taken round a circle at infinity, thus avoiding the problems of accelerated frames of reference. As a mere student, my protestations at the physical unreality of this model were, I feel, looked on as based on youthful inexperience. Many years later during the course of one of my many public lectures in an unrelated field I was charmed by the attendance of Otto Frisch, the pioneer of nuclear fission, himself a considerable mathematical physicist. In conversation we lightly stepped on the territory of Special Relativity and I found that the same feelings were aroused in me as to the response of what I might call the hierarchy of the world's physicists. I found again the attitude of the master talking to the schoolboy.

Undoubtedly, there may be many of us who are intellectually ill-equipped to appreciate the foundations of something as profound as the Special Theory, but I must stick to my feelings that the application of theoretical structures in those areas in which their approximations are so clearly invalid is extremely dangerous. Furthermore, the "instantaneous" light signals which formed a key element of teaching in my days as a student of this subject seem to me to be totally divorced from physical common sense.

Surely, if signals are to be sent, reflected from a moving body and then received by a detector in the frame of the sender, the entire mathematical problem must be worked out clearly and with great attention to a "feet on the ground" approach. Without labouring the point we should have to ask when is a signal regarded as being received by the detector (how much of it do we have to perceive before we draw useful conclusions)?

Overall, as an average mathematical physicist, I still feel as unconvinced by the use of Special Theory in conditions of accelerated frames of reference as I did as a student some 25 years ago. It is, therefore, a great pleasure to see a level-headed article such as this essentially reiterating those doubts I have as a non-member of the family of scientists who are brow-beaten into believing in the general applicability of a theory in those areas in which its validity is in doubt. At the same time, I have sufficient humility to accept that there are many people of greater intellect than myself but, sad to say, that large body has been incapable of presenting its case to me in a convincing fashion.

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Before worrying too much about 'Problems in Special Relativity' (Prof. I. McCausland, October issue) it would be as well to find out just what the relevant predictions of Special Relativity really are. Suppose that one has a set of observers at rest with respect to one another and spaced out along a line, that they synchronise their clocks according to conventional procedures, and that another observer B is in motion relative to them along the same line. Then

Special Relativity predicts that each time he encounters a new member of the initial set of observers he will find that observer's clock registering a time further in advance of his own. From the way this statement is framed it evidently doesn't matter whether B is considered to be moving relative to the other observers, or to be at rest while they move relative to him.

The key point here is that one observer is encountering a sequence of observers. The situation can be reversed by associating B with another string of observers moving along the same line, but this time at rest relative to him and with their clocks synchronised with his. Then each observer of the first set will have the same kind of experiences as B as he encounters in succession the observers of B's set. This seems thoroughly paradoxical until one realises that simultaneity does not transfer between inertial frames, i.e. that when the first set of observers synchronise their clocks B's set claim that they have made systematic errors in the synchronisation, and conversely when B's set synchronise theirs. This appears to be the situation envisaged by McCrea (M12), where the M denotes a McCausland reference. Dingle never did catch on to the failure of simultaneity, and some of his most impressive paradoxes result from ignoring it¹.

The second prediction involves introducing a kinetic assumption to the effect that at any instant an accelerated clock keeps the same time as the clocks in the frame in which it is instantaneously at rest (see Hill²). Originally Einstein appears to have made this assumption implicitly rather than explicitly, since it follows naturally from the idea that world lines in Minkowski space must be continuous. It then becomes possible (pace G. Staden (M11)) for Special Relativity to deal with accelerated clocks, including a polar clock and a clock located at the equator, provided that one ignores gravitation effects. As a result two or more encounters between two clocks may occur, and one is faced with the phenomenon of differential ageing, as in the so-called twin paradox. These are the conditions Einstein had in mind in making the statement about an equatorial clock losing time with respect to a clock at one of the poles. Professor McCausland didn't try very hard to arrange a meeting of clocks: a clock carried by a jet aircraft flying round the equator in the opposite direction to the earth's equatorial motion at appropriate speed would have done very nicely. This is essentially G. J. Whitrow's response (M7) to Dingle's supplementary question.

Dingle's original question is paradoxical from the beginning, it does not correspond to any specific prediction of Special Relativity, and therefore it cannot be answered without making some guess as to what he might have had in mind. J. M. Ziman's response, with a clear indication in the quotes round "Dingle's 'question'" that he thought the 'question' should be rephrased (M5), was the General Relativity answer to the question of which clock registers the greatest time between any two events at which it is present when there are gravitational fields to consider.

Finally Professor McCausland might have mentioned why Einstein excluded pendulum clocks from his observation about the time-keeping of equatorial clocks. The reason is that a pendulum does not in itself constitute a clock; the clock consists of the pendulum together with the earth.

C. F. Coleman

1. H. Dingle, *Nature* Vol. 197 1963, 1248.
2. E. L. Hill, 'The Theory of Relativity', *Handbook of Physics*, ed E. U. Condon and H. Odishaw (McCraw Hill, 1967).

The theories of relativity and quantum mechanics are the two major leaps forward in physics this century, and they appear to have attracted more than their fair share of controversy. One reason for this may be that most of our everyday experience of physical phenomena happens to be in the area where both theories agree with Newtonian mechanics.

As far as we know, neither relativity nor quantum mechanics contain any inconsistencies – and this is despite the effort put in to trying to discover them, by people of Einstein's calibre. Special Relativity is so well established among physicists that attempts to discredit it tend not to be taken seriously. However, a theory as rich as Special Relativity cannot be demonstrated to be consistent – just as we know that arithmetic cannot be shown to be consistent.

There are problems with both theories, and these arise from the fact that while the assumptions on which they are based are simple, the application of the theories contains subtleties. These subtleties lead exponents and opponents of the theories to make slips of thought which lead them to the conclusion they require.

For example, people often claim that they have found an inconsistency in Special Relativity by applying it to a physical example. They claim that when they attempt to do this, they obtain a result which is clearly false.

Problem $\xrightarrow[\text{+ Newtonian Mechanics}]{\text{Special Relativity}}$ falsity

In fact, they have inadvertently added some Newtonian idea (which is inconsistent with Special Relativity). It is this combination of theories which produces the false result.

Problem $\xrightarrow[\text{Relativity}]{\text{Special}}$ falsity

It is this slip which Dingle makes. Although (as I pointed out earlier) we cannot prove that Special Relativity is consistent, we can at least conclude that as there are mistakes in Dingle's argument, his case is not proven.

To turn to the specific example of the two clocks, Special Relativity does not say that one is faster than the other – in fact it denies the existence of absolute speed both of objects and of clocks. Special Relativity is a theory of measurement denying the existence of absolute space and time against which to measure the speed of material particles and clocks.

In McCausland's reference 10 Einstein is writing many years before formulating his general theory of relativity, and is using a very simplified model of two clocks. One is at a pole (i.e. stationary with respect to the fixed stars), the other is moving with the equator. He concludes that an observer who is stationary with respect to the fixed stars measures the clock on the equator as going more slowly than that situated at a pole. His argument here avoids the complication of gravitation, except in so far as it is the mechanism by which the moving clock traces its path. He excludes pendulum clocks from the argument, not through oversight, but because he realised that to include them he would have to include the effects of gravity. This would have complicated the argument unnecessarily.

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The author replies

Mr Coleman raises several interesting points. Referring to my statement that Ziman's answer

does not apply to the polar and equatorial clocks because they do not meet, he says that I didn't try very hard to arrange a meeting of clocks. I had thought that it was Ziman's responsibility to show how his answer applied to that case, not mine. However, let us consider Coleman's suggested clock carried by a jet aircraft flying round the equator in the opposite direction to the earth's equatorial motion at appropriate speed. I assume that by "appropriate speed" he means a speed equal to the earth's peripheral velocity at the equator; such a clock would be stationary relative to the polar clock, and would presumably work at the same rate. In that case, Einstein's prediction corresponds to a prediction that the airborne clock would work faster than the earthbound equatorial one. Now, if Ziman's answer is applicable to the comparison of those two clocks, as Coleman implies it is, then in order to deduce Einstein's result using Ziman's answer one would first have to show that the airborne clock was in free fall between the two meetings of the clocks, or for one full circuit of the earth. It is fairly obvious that the clock in question does not travel between the two meetings by free fall, but could perhaps be made to do so if one made the small extra step of removing the earth; however, Coleman does not seem to have that possibility in mind, since he stipulates that the clock is to be carried by a jet aircraft. It is also unclear how he uses this example to justify Whitrow's answer, since what he says does not alter the fact that the earthbound equatorial clock is not in an inertial frame.

Coleman also tells us that Ziman's response shows a "clear indication" that Ziman thought Dingle's question should be rephrased. But Ziman did not say it should be rephrased; he said it was "a perfectly reasonable question to which science should indeed give an answer". Professor Ziman is a prolific writer who may be assumed to have sufficient command of the language to be able to say what he means without requiring readers to indulge in mind-reading. If he believes that Dingle's question ought to have been rephrased, he should tell us so himself.

Coleman goes on to say that Ziman's response was the General Relativity answer to the question. But the whole point of Dingle's question was to find out what justification was given by the *Special Theory* for one clock to work faster than the other. So Coleman is supporting the view that Dingle's perfectly reasonable question has not been answered.

Finally, let us consider one of the most important topics of all — the synchronization of clocks. Coleman mentions synchronization and then goes on to say that Dingle never did catch on to the failure of simultaneity. Let us consider this problem in more detail.

First of all, Dingle was careful to distinguish between simultaneity of events and synchronization of clocks; see, for example, his letter in *The Listener* dated 30 December 1971. He also pointed out, in *Science at the Crossroads*, that when a pair of relatively stationary clocks are synchronized they are synchronized for all observers. Although this is a crucial part of Dingle's argument, I can recall only one review of his book that discusses synchronization, and it agrees with Dingle that synchronization is independent of the observer; that review is Staden's, which was cited in my article.

Since Einstein argued, in his original paper on Special Relativity, that observers moving relative to the pair of synchronized clocks would find that they were not synchronized, let us now

consider Einstein's original definition and argument.

Einstein gave a definition of synchronization in the following way. Two clocks A and B are at rest relative to one another, and a flash of light is emitted from A and reflected back from B to A. If the reading on B at the moment of reflection is halfway between the readings of A at emission and return of the flash, the clocks are synchronized. Any observer, in any state of motion, would see the same set of three readings, and would reach the correct conclusion about the synchronization of the clocks. (If desired, the experiment could be done in darkness, and the only three clock readings seen by anyone would be the readings illuminated by the flashes; the observer need not consult his own clock, nor indeed need he possess one.)

Now consider the argument by which Einstein concluded that observers moving relative to a pair of clocks would find that they were not synchronized. The argument involves a rigid rod aligned with the x axis of a stationary reference frame, and moving longitudinally along the x axis; at its ends A and B are two clocks, and along the x axis are several stationary clocks which are synchronized with one another. A flash of light is emitted from A and reflected back from B to A to test for synchronization.

The crucial fact about this experiment is that each of the clocks at A and B is constrained to give the same reading as the stationary clock that happens to be adjacent to it at any instant. I say "constrained" deliberately, because it turns out from results derived later in the theory that the clocks at A and B, if they were running freely, would not continue to give the same readings as the stationary clock adjacent to them as they move along, but would fall further and further behind the stationary clocks. To make them continue to show the same readings as their stationary neighbours they would have to be continually readjusted, in which case they would not be regularly-running clocks. To put it more bluntly, they would not be clocks at all, for their clock works could be removed and their readings adjusted by demons to correspond to the readings of the adjacent stationary clocks. Even more simply, the "clocks" could be removed altogether and replaced by mirrors which would simply reflect the appropriate readings.

In the experiment, the flash of light reflected from B arrives back at A, the end of the rod from which the flash was emitted. Since A has by then moved on, relative to the stationary row of clocks, the clock then opposite A is not the same one as the one that was opposite A when the flash was emitted; the reading at B is therefore not halfway between the two clock readings at end A of the rod. Therefore, according to Einstein "observers moving with the moving rod would thus find that the two clocks were not synchronous".

But Einstein is not using his definition of synchronization in reaching that conclusion. The "clocks" at the ends A and B of the rods are not regularly-running clocks, but merely objects which reflect the readings of the stationary clocks beside them. Since the definition requires the reflected flash of light to return to the regularly-running clock from which the original flash was emitted, and since it does not do so until after it has passed the new position of end A of the moving rod, it is not valid to make any inference about synchronization of clocks from the reading of the clock at the new position of A. Einstein's conclusion is therefore unjustified.

Reply to A. D. Vella

Dr Vella states that Dingle made an error, but does not identify a specific error. He goes on to say, referring to the two clocks, that "Special Relativity does not say that one is faster than the other — in fact it denies the existence of absolute speed both of objects and of clocks." I do not think that a statement that one clock works faster than another is a claim about absolute rates of clocks, but in any case it was Einstein himself who stated explicitly that the equatorial clock must work slower than the polar one.

Vella goes on to say that the polar clock is stationary with respect to the fixed stars, which is not true. He then says, referring to Einstein: "He concludes that an observer who is stationary with respect to the fixed stars measures the clock on the equator as going more slowly than that situated at a pole." Vella implies that it is the state of motion of the observer that determines which clock is measured as the slower one, but this is not what Einstein said; he stated that the equatorial clock must work more slowly than the polar one.

In view of the two statements that I have quoted from Dr Vella's letter, I would ask him to answer, with a simple yes or no, the following question: Would an observer on the equator measure the clock at the pole as going more slowly than that situated on the equator?

Reply to J. C. Laine

After a fairly obscure derivation, Mr Laine concludes that "it is the travelling clock which runs slower than the stationary clock". Exactly. But the theory says that either clock can be taken to be the stationary one (as Laine seems to agree when he says that "stationary" is a relative expression), so Laine's statement supports Dingle's claim that the theory requires each clock to work slower than the other.

Laine then goes on to talk about observation, in an apparent attempt to avoid the obvious result of the statement quoted above. But that does not remove the problem. As I pointed out in *Wireless World* in October 1980, Professor P. C. W. Davies, in his book *Space and Time in the Modern Universe*, makes the following statement about two clock-carrying observers in uniform relative motion: "It is not that each observer merely sees the other clock running slow, it actually is running slow — a real physical effect." [Emphasis in the original.] In any case, the observer is not an essential part of the special theory, as has been pointed out by H. Reichenbach, one of the contributors to the book *Albert Einstein: Philosopher-Scientist*, edited by P. A. Schilpp, who wrote that "In a logical exposition of the theory of relativity, the observer can be completely eliminated".

General comments

Without exception, critics of my article have failed to answer my main point, which is that defenders of the theory have published arguments which are inconsistent with one another and/or with Einstein's own statements. Clear evidence that there are problems in the theory is provided by the fact that these inconsistent statements remain uncorrected. The alternatives are clear: either some of those scientists' statements are wrong, or the theory from which those scientists claim to have deduced their statements is internally inconsistent. Therefore, unless the defenders of the theory can remove the inconsistencies by showing that some of their statements are wrong, they have themselves proved that the special theory is untenable.