The twins paradox of relativity

A composite reply to correspondence arising from Professor Dingle's October article

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I am grateful for the opportunity to reply to various letters that have been sent to the Editor in response to Professor Herbert Dingle's article[1]. I shall start with Dr Tom Wilkie's letter (June issue), since it was in response to discussion with him that the article came to be written.

I am sorry if Dr Wilkie feels that he has been singled out in an undesirable way by Dingle's article. I understand that Dingle had planned to rewrite the article in more general terms, without specific reference to Dr Wilkie, but he did not live to do this. I did think of making such alterations myself, but I was reluctant to tamper with what Dingle had written.

Since Dr Wilkie describes his conviction that Dingle is wrong as being "unshakable", there is little that I can reply to him; but I would like to make some comments about his letter which may say something to others who may view the question as being still open.

In reply to Wilkie's comment that "most academic journals have for some years rightly viewed the matter as settled and regarded more discussion of it as a waste of paper", and his final plea to "let it rest", I would simply observe that he was the one who published the item entitled "The Twin Paradox revisited" in *Nature* in 1977, which led directly to the writing of Dingle's paper[1].

One of the interesting features of the responses to Herbert Dingle's criticisms of special relativity has been the variety of attempts to answer Dingle's question about the relative rates of the equatorial and polar clocks mentioned by Einstein in his original paper. Wilkie's answer is to say that it may be that there is an error or ambiguity in this example in Einstein's paper, and he later states that original papers may not be definitive because "second thoughts may change the author's mind". It is interesting to note that, in the very example mentioned, we do have Einstein's second thoughts available to us. If one studies that example in the generally-accepted English version of Einstein's first paper[2] (translated from the text in a German collection published in 1922), one finds a footnote which excludes the case of pendulum clocks, *but that footnote does not appear in the originally published version of the paper*. The later addition of the footnote seems to me to confirm that Einstein did mean exactly what he said, and also confirms that the statement about the slowing of the equatorial clock was intended to refer to a real slowing, not merely something that depended on the point of view of the observer.

According to Wilkie, Max Born answered in technical terms whose meanings were precise and well-defined. As an example of Born's precision, consider the following statement, referring to the special theory[3]: "The simple fact that all relations between space co-ordinates and time expressed by the Lorentz transformation can be represented geometrically by Minkowski diagrams should suffice to show that there can be no logical contradiction in the theory." Since the Lorentz transformation is contained in the special theory, but is not the whole theory, it is illogical to claim that any property of the Lorentz transformation is a sufficient condition for the whole theory to be free of logical contradiction.

With reference to Wilkie's statement that the language of relativity is geometry, not English or German, Dingle did not question the impeccability of the mathematics of special relativity. But the theory is based on postulates expressed in words, and the mathematics is not the whole theory; it was the theory as a whole that Dingle criticized, not its geometry.

I agree with Dr Wilkie that some of Dingle's critics have tripped themselves up by their use of words. However, this is not always because of the difficulty of expressing abstruse technical matters in words. Consider, for example, a case that I have documented elsewhere[4], in which, in *The Listener* in 1971, one scientist stated that the results of the Hafele-Keating experiment supported special relativity, and another stated that the experiment had no relevance whatever to the special theory. Now, a statement that the results of a certain experiment support a certain theory is a perfectly simple factual statement (however abstruse and technical may be the reasoning that led to that conclusion), and the same applies to the contrary statement. The fact that the two statements are contraries of one another (they cannot both be true, though they might both be false) shows that one or other of the scientists (or both) misunderstood either the theory or the experiment (or both). Or it might mean that there is a contradiction in the theory.

In the note mentioned above[4], I documented several other unsatisfactory statements that have been published by defenders of the theory. These cannot be dismissed as being merely poorly worded, since most of them were uttered by scientists who are prolific authors of books and who may therefore be reasonably expected to be able to write what they mean. I think we should keep in mind the words of the anonymous diplomat (quoted by Sir Bruce Fraser in his revision of Sir Ernest Gowers' *The Complete Plain Words*) who said: "What appears to be a sloppy or meaningless use of words may well be a completely correct use of words to express sloppy or meaningless idea."

Wilkie's paragraph about all the scientists who did not choose to seek fame by dethroning Einstein is very interesting, but totally devoid of scientific basis. The pursuit of scientific truth is not aided by statements such as: "That no young student over the last 20 years has seen the chance to make his name by developing Professor Dingle's ideas is eloquent testimony to the erroneousness of these ideas."

Several other letters were received by the Editor with varying degrees of relevance to the problem at hand. Before dealing with individual letters, examine the nature of the problem. According to Dingle[5] a paradox arises when, from the same premises P, two (or more) apparently contradictory conclusions X and Y seem inescapably to follow. It can be resolved only if one of the following four things can be shown: (1) the conclusions are not in fact contradictory; (2) conclusion X does not follow; (3) conclusion Y does not follow; (4) the premises P contain an internal contradiction.

Furthermore, if we start with a pair of contradictory premises, then, as Popper[6] has pointed out, we can infer any conclusion we like *using valid rules of inference*.

How does this apply to our problem? Suppose that we have a set of premises P, and suppose that one scientist (D) deduces from those premises a conclusion X, and that another (E) deduces from the same premises a conclusion Y, which is directly contradictory to X. Each scientist may believe that he has shown *by his own deduction* that the other's deduction is faulty, but in fact both deductions might be perfectly valid deductions from premises P which contain an internal contradiction. Furthermore, even if hundreds of supporters of E come forth, each with a different argument showing that Y does in fact follow from P, these do nothing whatever to show that D's deduction of X from P is faulty. To refute D's argument it is necessary to examine that argument itself and show that there is an error in it – in other words, to show that conclusion X does *not* follow from the premises P.

In reading the literature on the twin paradox, one finds many articles showing ingenuity, with varying degrees of originality, and picturesque detail, that asymmetrical ageing can be deduced from Einstein's theory. Many of these articles present the arguments in such a way as to imply that they refute the deduction of the opposite conclusion (symmetrical ageing), when in fact their results merely *contradict* the opposite result and, for the reasons discussed above, contradiction does not imply refutation unless it is first proved that the theory from which the contradictory results have been derived is itself free from contradiction.

For example, one of the correspondents, T. de Limelette, writes: "But I agree that the solutions to the paradox found in some texts are not all one could wish. I propose here my own. It is contained entirely within the special theory." I think it is clear from the foregoing that yet another presentation of the derivation of asymmetrical ageing, without showing what is wrong with the other argument, is not a solution to the problem that Dingle raised. T. de Limelette also comments on Dingle's article as follows: "I wonder where Professor Dingle picked up the strange idea that two different observable descriptions of the same events are not permissible. A description requires observers, apparatus and measurement procedures before it can be observed. These are not left unchanged by a change in the reference coordinate system. So why should the results of the measurements have to remain the same?" I am not quite sure that is the point of this comment, unless it is to suggest that the rather bizarre set of observations envisaged by Dingle are in fact feasible.

N. Thomas comes closer to dealing with Dingle's argument. The relevant paragraph of his letter is as follows:

> The situation according to Special Relativity is as follows (for instance, see Introduction to Relativity by L. Marder, Longmans 1968). According to Paul the outward and return journeys take 1½ days each, whilst according to Peter they take 15 years each. Thus Peter judges Paul's clock to be running slow by the factor 15x365/1.5 = 3650. According to Paul, Peter's clock runs slow by the same factor, and at the end of his outward journey Paul says that 35½ seconds have elapsed on Peter's clock whilst 1½ days have elapsed on his own clock. Now suppose that Peter had previously placed a stationary clock synchronized with his own at the point where Paul reverses his journey. Both Peter and Paul will say that this clock reads 15 years at the end of the outward journey, and this is how Peter assigns a duration of 15 years to the outward journey. However, be-

cause he is moving relative to Peter, Paul says that this additional clock is not synchronized with Peter's own clock but rather leads it by 15 years minus 35½ seconds: this is an example of the relativity of simultaneity. As soon as Paul reverses direction he judges that Peter's clock now leads the local clock (which reads 15 years) by 15 years minus 35½ seconds. Paul measures 1½ days on his own clock for the return journey (making a total of 3 days) whilst he judges that only a further 35½ seconds elapse on Peter's clock (making a total for the trip of 30 years). According to Paul, Peter's clock therefore races forward by 30 years minus 71 seconds during the reversal; as discussed by Einstein this can be explained using General Relativity. (Alternatively, since Paul changes inertial frames it can be attributed within Special Relativity to a change in his definition of simultaneity). Special Relativity does not therefore predict that Paul is rejuvenated at the beginning of the return journey, and Dingle's refutation of the theory on this basis is not valid.

It seems to me that his is not a satisfactory answer to Dingle's article. It should be recalled that Dingle was discussing Einstein's own resolution[7] of the twin paradox, and that this resolution required the use of general relativity. (Einstein's article[7] takes the form of a discussion between a relativist and a critic; the discussion of the paradox starts from special relativity but the critic asks for a resolution that satisfied the general theory, and it was that resolution that Dingle discussed in his article[1].) This seems to me to suggest that Dingle's argument must be met in terms of the general theory, not the special theory.

The other point to be noted about Einstein's resolution is that he agreed that it is perfectly valid to consider Paul to be fixed throughout the whole course of events, provided that the appropriate fields of force are invoked. This means that we could rephrase a passage in N. Thomas's letter as follows: "Now suppose that *Paul* had previously placed a stationary clock synchronized with his own at the point where *Peter* reverses his journey. Both Peter and Paul will say that this clock reads 15 years at the end of the outward journey, and this is how *Paul* assigns a duration of 15 years to the outward journey." (In case it may be argued that the fields of force associated with the initial parting of Peter and Paul might upset the synchronization of that clock, one can assume that Peter and Paul are moving uniformly relative to one another at the start of the process, so that no fields of force are needed at the original parting.)

W. James writes that "Dingle gives a wholly spurious symmetry to the problem by assuming that the Universe is empty but for the two clocks in his analysis (although in the statement of the problem he also refers to the earth.)" I can find no such assumption stated; in fact Dingle talks about the earth and a distant planet, whereas Einstein's statement of the same problem defines it wholly in terms of reference frames. Einstein's article[7] does not use *any* other objects except the travelling twins to resolve the paradox, except that later in his paper, when his supposed critic suggests that the gravitational fields are fictitious, he states that "all the stars in the firmament can be conceived as participating in the creation of the gravitation fields". I do not think that Dingle would have objected to this statement, and the fact that Dingle did not happen to mention all the stars in the firmament can scarcely be taken as equivalent to an assumption on his part that the universe is empty except for the two clocks.

W. James also states: "The clock paradox of special relativity is stated in McCausland's article 'if there are two clocks in *uniform* relative motion the special theory of relativity requires each clock to run faster than the other'. . . ". In fact my article[8] does not even mention the clock paradox, much less *state* it. In the relevant context I quoted a passage from Davies[9], and then suggested that passage provided strong support for Dingle's claim that "if there are two clocks in uniform relative motion, the special theory requires *each* clock to run (not merely *seem* to run) faster than the other." If the passage I quoted from Davies does *not* support that claim of Dingle's, then I think that someone should state clearly what it *does* mean.

I. M. Crann states that Dingle tacitly assumes some form of universal time, and that this assumption of "absolute" time guarantees that contradictory results will be obtained. I do not think that Dingle makes such an assumption, any more than Einstein did. Einstein stated quite clearly, in the passage Dingle quoted, that retardation of a clock during one phase of the experiment was over-compensated by faster working during another phase, and that a clock works faster if located at a point of higher gravitational potential. I think that Dingle merely followed Einstein's argument to its inevitable conclusion.

K. Burnett (May letters) asks "Am I the only reader of *Wireless World* with an interest in physics who finds the long series of articles on special relativity somewhat boring?" After making some interesting comments about theories of modern physics, he ends his letter by writing: "When a new more inclusive theory arises, which will enbrace quantum mechanics and general relativity, I suspect that few 'anti-relativists' will like the result. But boring it won't be."

I do not know the grounds on which Burnett bases his suspicion that few anti-relativists would like such a result. There seems to me to be a suggestion here that those who criticize relativity are like Luddites longing for a retreat to pre-Einstein physics, whereas in fact they are trying to suggest that it is time for the scientific world to consider the possibility of moving on to post-Einstein physics.

Some correspondents, such as W. James, M.H. de la Rica, R. V. Harvey, and A. B. Starks-Field, present alternative resolutions, or partial resolutions, of the clock paradox. For the reasons given in my earlier comments, I believe that they do not meet Dingle's argument, because they do not identify a fault in his reasoning. M.M. Albahari (February letters) suggests a new experiment to test the validity of relativity by a test of the constancy of the velocity of light, using time intervals four orders of magnitude greater than those in the Michelson-Morley experiment. A.H. Winterflood states that Dingle is wrong in believing that the mathematics of special relativity is impeccable; he states that the mathematics of the theory is wrong, and refers to his recently-published book "Einstein's Error". Other correspondents, such as C.L. Thomson, W.T. Morris, J. de-Pière, F. Allen and J.A. MacHarg, contributed interesting comments and suggestions, and V. Halsall contributed a discussion relating to Dr Essen's article in *Wireless World* dated October 1979.

There is another letter which I think requires comment, namely a letter by J.H. Fremlin, which appeared in *New Scientist* last year[10]. Some of the comments below were made in a letter that I sent to the editor of *New Scientist* in October 1980, but to the best of my knowledge my letter has not been published.

Professor Fremlin stated that he would "like very much to refute the suggestion that opponents of the theory of relativity find it difficult to get a proper hearing". He might *like* to refute the suggestion, but his letter certainly does not do so. The only evidence he presents in support of his "refutation" is about things that were published, whereas the suggestion that he claims to refute is related to the fact that papers have been denied publication. There is no contradiction between the fact that some papers have been published and the fact that others have been denied publication. Unfortunately few people, except those who have direct experience, are aware of the difficulty of having *any* paper published if it is critical of relativity. Part of the problem is that almost all the evidence about papers that have been rejected is hidden from public view.

To take a specific example, Professor Dingle's paper[1] was rejected by another journal before his death. I have in my possession a copy of the relevant correspondence (which spanned a period of several months) between Dingle and the journal, but the journal has refused my request for permission to publish its part of the correspondence.

Professor Fremlin's letter also dealt with his own personal correspondence with Dingle, and his (Fremlin's) demonstration of the difference to be expected between the ages of the pair of twins in the twin paradox. Although it is difficult to comment on this without seeing all of the relevant correspondence, I suspect that Dingle considered the finding of an error in Fremlin's analysis to be a non-existent problem. He was convinced that the special theory contained an internal contradiction, and he knew that meant that it was possible, using valid rules of inference, to deduce from the theory any conclusion that one wished.

Dr Wilkie did not think it was wise to publish Professor Dingle's article, because Dingle is unable to defend himself. As I pointed out in my note accompanying the article, Professor Dingle sent the article to me in the hope that it would eventually be published. I am conscious of the inferiority of my qualifications as a defender of Herbert Dingle, but perhaps I amy excuse my attempts by quoting a sentence from his last book, *The Mind of Emily Bronte:* "To disinter from a mass of diverse writing a common substratum demands penetration of a far higher order, and the only ground on which I claim justification for attempting the task is the absence of competitors."

In fact there is a significant number of scientists dissatisfied with the special theory of relativity. Anyone who doubts this should read the August 1979 and October 1980 issues of the journal *Speculations in Science and Technology*. I happen to believe that Herbert Dingle was right in his thesis that the special theory is untenable, but I would not be so rash as to claim that I have an "unshakable conviction" on this. I am, however, firmly convinced that the problems raised by Professor Dingle have not been satisfactorily solved.

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