A Question of Relativity

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Professor Herbert Dingle was a long-time critic of the special theory of relativity, who believed for many years that the theory was self-contradictory. Although he was unsuccessful in persuading the scientific world of the inconsistency of the theory, his questions and arguments were not satisfactorily answered during his life. Now, thirty years after his death, the subject is of historical interest. This paper examines two main problems that have contributed to the confusion that still surrounds this issue. The first problem is the fact that some scientists answered Dingle's Question, which is explicitly about the special theory, by invoking the general theory. It is argued that, if there is a valid answer to Dingle's Question, it would have been valid if the same question had been asked in 1905 before the general theory appeared. The second problem is that many scientists have claimed that Dingle's thesis has been refuted by experiment, although experimental results cannot disprove the existence of an internal contradiction. An answer to Dingle's Question is still wanting.

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Introduction

Professor Herbert Dingle (1890-1978) was a distinguished scientist and philosopher of science, who was well known within the scientific community as a long-time critic of Einstein's special theory of relativity. Now, thirty years after his death, his criticisms of the theory are a suitable subject for study by scientists and historians of science.

Dingle's criticisms of special relativity can be divided into two separate phases. At first he criticized the derivation from the theory that is commonly associated with the term clock paradox, according to which a clock that moved at high speed around a closed path, starting and finishing at the position of a stationary clock with which it was initially synchronized, would show a lower reading than the stationary clock when they again came together. An extension of that problem was the twin paradox, in which one of a pair of twins would make a long high-speed journey into space and return to the other twin, and according to the theory the travelling twin would be younger than the stay-at-home twin when they again came together. Dingle's argument was, essentially, that the theory deals only with relative motion so the clocks or twins would both age at the same rate.

The second phase of Dingle's criticism of the special theory started when he realized that he could not disprove the asymmetrical ageing, and he concluded that, if symmetrical and asymmetrical ageing were both predicted by the theory, the special theory must be self-contradictory.

The story of Dingle's criticism of the asymmetrical ageing associated with the clock or twin paradox has been told comprehensively by Chang [1]. Although it is clear from the history of that phase of the controversy that Dingle did not disprove the asymmetrical ageing, he did ask a question about the physical cause of that ageing that Chang believed was not answered. Chang wrote that most of Dingle's opponents were not concerned with physical explanations, and concluded (p. 782) that "What their responses to Dingle came down to was an implicit rejection of his question about the physical cause of the asymmetrical ageing, rather than an answer to it. In my opinion, Dingle's question remains unanswered to this day."

Since Chang's paper was so comprehensive a study of Dingle's arguments about the clock or twin paradox, the present paper does not attempt to deal with that problem any further. Instead, we concentrate on a much more important question that Dingle asked, which in my opinion remains unanswered to this day.

Dingle's Question

The question that Dingle asked was the central theme of his book *Science at the Crossroads* [2], and he claimed that, unless that question could be answered, the special theory failed. This question might be worded very briefly as follows: *Which of two clocks in uniform relative motion does the special theory require to work more slowly?* However, in order to present the story satisfactorily we should consider the question in its extended form, as it is presented on pages 45-46 of his book:

THE QUESTION

> *According to the special relativity theory, as expounded by Einstein in his original paper, two similar, regularly-running clocks, A and B, in uniform relative motion, must work at different rates. In mathematical terms, the intervals, dt and dt', which they record between the same two events are related by the Lorentz*

> *transformation, according to which dt ≠ dt'. Hence one clock must work steadily at a slower rate than the other. The theory, however, provides no indication of which clock that is, and the question inevitably arises: How is the slower-working clock distinguished? The supposition that the theory merely requires each clock to appear to work more slowly from the point of view of the other is ruled out not only by its many applications and by the fact that the theory would then be useless in practice, but also by Einstein's own examples, of which it is sufficient to cite the one best known and most often claimed to have been indirectly established by experiment, viz. 'Thence' [i.e. from the theory he had just expounded, which takes no account of possible effects of acceleration, gravitation, or any difference at all between the clocks except their state of uniform motion] 'we conclude that a balance-clock at the equator must go more slowly, by a very small amount, than a precisely similar clock situated at one of the poles under otherwise identical conditions.' Applied to this example, the question is: what entitled Einstein to conclude from his theory that the equatorial, and not the polar, clock worked more slowly?*

Let us examine the answer to this question that appeared in Ziman's review [3] of Dingle's book. Ziman quoted Dingle's Question, as it is shown above, up to and including the sentence "How is the slower-working clock distinguished?" Immediately after quoting the question, he wrote: "This is a perfectly reasonable question to which science should indeed give an answer." Later in the review he wrote: "To distinguish the slower-working clock (as demanded by Professor Dingle) one must use the theory of *general* relativity, which takes account of accelerations." He gave his answer to the question in the following words: "In fact, the answer to Dingle's 'question' is simple: the fastest working clock between any two events is one that travels between them by free fall."

These quotations make it obvious that Ziman's statement is not a valid answer to Dingle's question. It is clear from the question that the answer is to be provided from the special theory, not the general theory. It is equally clear that Ziman's statement fails to answer the part of the question referring to the polar and equatorial clocks, which Ziman did not include in his quotation of the question.

Since Dingle's question was explicitly asked about the special theory and only the special theory, as it was described in Einstein's 1905 paper, any valid answer to Dingle's question must have the property that it would have been a valid answer if the question had been asked in 1905, just after Einstein's paper was published and before the general theory appeared.

Another scientist, G. F. R. Ellis [4], described Ziman's review as "admirable" but wrote that "Ziman invokes general relativity at a stage when it is not really needed." He went on to say: "In special relativity, just as in general relativity, the answer to Professor Dingle's 'question' is: the fastest working clock between any two events is one that travels between them by free fall."

We see that Ziman wrote that the answer to Dingle's question requires the use of the general theory, whereas Ellis wrote that it does not require it: that is a clear contradiction, on an important property of a theory. If Ziman and Ellis had been interested in the search for the truth of the matter, they might have been expected to try to resolve that contradiction, but there is no record of any further discussion between them on that subject. Although they disagreed on an important property of a theory, they agreed that Dingle was wrong in saying there was a problem. One is reminded of remarks by Cullwick

[5] which, although they were written about the earlier debate over the clock paradox, seem appropriate to the later controversy:

> *On one thing Professor Dingle's critics are all agreed, that he is wrong. They do not all agree, however, on the nature of his error. Some give arguments which are no more than illustrations of the obvious fact that the reciprocal Lorentz transformation is algebraically consistent; some claim that the problem requires the General Theory of Relativity; and some appear to regard the matter as settled by their knowledge of four-dimensional space-time. Some argue with patience, while others thinly disguise their irritation.*

Ellis's answer [4] does not pass the test of being valid in 1905, before the general theory appeared. Consider the following argument. Suppose that there is a perfectly spherical, non-rotating planet without an atmosphere, and that a clock is carried on a satellite in a circular orbit that is just above the surface of the planet. (For a planet having the same size and mass as our Earth, such an orbit would have a period of about 84 minutes.) Suppose that the orbiting clock passes over a stationary clock on the surface of the planet once every orbit, and that the clock readings are compared at each of these close encounters. According to Einstein's 1905 paper, the reading of the clock that goes round a closed path and returns to the other clock would fall behind the reading of the clock on the surface, but according to Ellis's answer the orbiting clock would work faster than the other because the clock on the satellite is in free fall. Although there were no clocks on satellites in 1905, someone with enough imagination and knowledge could easily have suggested such a thought experiment then to refute that answer to the question.

Like Ziman, Ellis did not show how his answer applied to the polar and equatorial clocks. Dingle [6] replied to him and Ziman as follows: "Professor J. Ziman and Mr. G. F. R. Ellis seem not to have read my 'question', let alone answered it, though Ziman quotes it correctly. Neither of the events need be at either of the clocks concerned, so the statement, 'the fastest working clock between any two events is one that travels between them by free fall', is futile."

Another reviewer of Dingle's book also used the general theory to try to rescue the special theory from its problems. Referring to the polar and equatorial clocks, Stadlen [7] wrote as follows:

> *But the relative motion involved in this case, being circular, is non-uniform. I submit, therefore, that Einstein was wrong in saying that his prediction followed from the special theory, which deals only with the effects of uniform motion. This is not to say that the prediction was invalid. For Einstein was, intuitively, anticipating his later general theory, according to which the equatorial clock runs slower because of the centripetal force exerted upon it.*

The fact that the predicted slowing follows from the general theory does not make Einstein's prediction *from the special theory* valid; it is a well known fact of logic that the truth of the conclusion of an argument does not guarantee the validity of the argument. If Einstein's prediction does not follow from the special theory, then his inclusion of that prediction in his 1905 paper was irrational and, therefore, not valid. Also, the suggestion that Einstein was so easily able to anticipate his general theory, which took him about another decade to develop, is highly unconvincing.

Obviously Stadlen's statement could not have been a valid answer to Dingle's question if it had been asked in 1905. Also, anyone who claims that Einstein's statement about the polar and equatorial clocks is wrong, as Stadlen does, ought to be able to state categorically at exactly what point in Einstein's reasoning he went wrong.

The Problem of Inconsistency

One of the biggest problems in dealing with Dingle's criticisms of the special theory is that many scientists seem to have very poor knowledge of simple logic. Because of their poor knowledge, they do not fully understand the properties of a theory that has an internal inconsistency or contradiction, and this causes them to use irrational arguments in trying to refute Dingle's claim that the special theory is internally inconsistent.

It is a well-known fact of simple logic that, if a theory has an internal contradiction or inconsistency, it is possible to derive from that theory any result that we wish. As Popper [8] puts it: "But the importance of the requirement of consistency will be appreciated if one realizes that a self-contradictory system in uninformative. It is so because any conclusion we please can be derived from it."

That fact has enormous significance in assessing the responses of scientists to Dingle's claim that there is an internal contradiction. If a theory is self-contradictory, then it is possible to derive results from the theory to match the results of any physical experiments that we choose. That means that the claim that a theory is self-contradictory cannot be refuted by saying that the theory matches various experimental results, since a self-contradictory theory could match all possible results.

The question of whether the special theory is self-contradictory can be answered only by studying the theory. Imagine that, in 1905, a suitably-qualified scientist had been confined to a sealed room with a copy of Einstein's paper and asked to find out whether the theory described in the paper was self-consistent. He might not have been able to prove that the theory was self-consistent, but it is quite certain that, if he had found a contradiction, nothing that ever happened outside that room, either the performance of experiments or the publication of the general theory, could remove that contradiction.

In short, in assessing the merits of Herbert Dingle's claim that the special theory of relativity is self-contradictory, all experimental results whatever are completely irrelevant.

Consider, for example, another review of *Science at the Crossroads* that appeared in *Nature* in the form of an unsigned editorial article [9]. The last two sentences of the review, which are presented triumphantly as if they were a refutation of Dingle's thesis, read as follows:

> *And is there any hope that he will now be satisfied with the demonstration that moving clocks run at different speeds from clocks at rest which has been provided in the past few months by the experiments in which Hafele and Keating have flown caesium clocks in different directions around the world (Science,* ***177****, 166; 1972, see also Nature,* ***238****, 244; 1972)? It will be sad to see the clock paradox disappear, but this work is the last nail in the coffin.*

It would seem reasonable to expect that a scientist who was qualified to write a *Nature* editorial would know enough elementary logic to realize that the experiment did not refute Dingle's thesis, but unfortunately that condition is not satisfied. Also, the writer seems not to have noticed Dingle's statement that he had for years held an open mind on the subject of asymmetrical ageing, or his statement that the scientific issue was not what is normally associated with the expression "clock paradox", both of which appear in the Preface of *Science at the Crossroads.*

Consider also the following quotation from another unsigned *Nature* editorial that is reproduced as an appendix in Dingle's book [2, p. 226]:

> *But in circumstances like these, where a theory is lent conviction by the sheer breadth of its agreement with experiment, it would seem incumbent on those who would overthrow it to produce not merely a contradiction but a constructive alternative.*

Not only does the writer of this strange argument not appear to understand that the agreement with experiment does not disprove the possible existence of a contradiction, but he also refuses to accept that a contradiction—an obviously fatal flaw in a theory—is sufficient to require scientists to discard the theory unless the person who demonstrates the contradiction also presents an alternative theory. An argument like this was criticized by Nordenson [10], who called it a grotesque argument and made the following comment:

> *It is as if a judge, who has established that a person, accused of murder, has brought out evidence of his innocence, would declare: 'It may well be that you have proved your innocence but I cannot release you unless you find who the murderer is.'*

It is interesting that Dingle, even though he reproduced the above-mentioned *Nature* editorial as an appendix in his book, did not make the obvious criticism that "the sheer breadth of its agreement with experiment" did not refute his claim of a contradiction. I think it needs to be said that Dingle himself did not seem to realize fully the irrelevance of experimental evidence. In various places he discussed the experimental evidence for the theory, saying for example that much of the evidence in its favour depended on circular arguments because the velocities of elementary particles were inferred from electromagnetic theory rather than being found by measuring the distance actually travelled by an individual particle in a given time. Perhaps he should simply have pointed out that all experimental evidence is irrelevant in attempting to refute a claim that the theory is self-contradictory.

The fact that any statement we wish can be derived from a self-contradictory theory is also highly significant in assessing the debates about the twin paradox that have gone on for many years. For example, suppose that one scientist argues that, according to the special theory, an astronaut who makes a long round-trip journey at very high speed would age less than his sibling who stayed at home, and suppose that another scientist argues that the two would age equally. It is natural for each scientist to believe that, since his own argument appears to be correct, the other scientist's argument must be wrong. But it is not necessarily so: if the special theory was self-contradictory, both arguments might be correctly based on the postulates of the theory. In other words, the possibility that the theory is self-contradictory could account for the long inconclusive series of debates on the twin paradox. If the theory was self-contradictory it would also make it impossible to disprove the theory by experiment, because supporters of the theory could show that the theory matched any experimental results whatever.

Discussion

As mentioned above, Ziman wrote that Dingle's Question is "a perfectly reasonable question to which science should indeed give an answer." I believe it is obvious that the question has not been satisfactorily answered, but it is not so obvious who should answer it. Ziman did not say who should answer it: he asked “what do we mean by ‘science’ in this context?” and went on to say that “physics has no Pope with authority to proclaim doctrine.” Of course he is correct in saying that, and it is obvious that science, as an abstract idea, cannot give an answer; only scientists can answer the question.

I believe that future historians of science will be very puzzled by the fact that, in spite of the ineptitude of the published attempts to answer Dingle’s Question and his other arguments, the scientific world remains almost unanimous to this day in its belief that Dingle was all wrong and his opponents all right. Although I have quoted in this paper only a few attempts to answer Dingle’s arguments, I have shown elsewhere [11,12] that several of Dingle’s opponents contradicted one another in their attempts to show that there is no contradiction in the special theory.

I suggest that scientists need to answer Dingle’s Question. Since the question was explicitly about the special theory of relativity, I suggest that the answer to the question should have the following properties: it should provide a clear criterion to distinguish which of two clocks in relative motion the special theory requires to work more slowly, the applicability of the criterion to the case of the polar and equatorial clocks should be clear, and the answer should not depend in any way on the general theory of relativity. The world has been waiting more than thirty-five years for Herbert Dingle’s perfectly reasonable question to be answered.

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