LETTERS

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Clock Paradox of Relativity

Edwin M. McMillan attempts "to restate the situation" regarding the clock paradox of relativity, "in new words and all in one place" [*Science* 126, 381 (30 Aug. 1957)]. Since the controversy on this matter is so widespread and confused, this is indeed a worthy object, but, as he admits that he has "not read widely in the literature of this subject," it is not surprising that he has not succeeded in achieving his objective. His arguments, as he conjectures, have all been given before, and, on the other hand, he appears to have missed the answers to them. I, on the contrary, have been forced to read widely in the literature of the subject and realize, even more than he can do, how necessary it is to bring it to some sort of focus. I therefore willingly take this opportunity of summing up the situation as well as I can, with the double object of showing clearly how the problem now stands and of preventing, as far as possible, further repetition and confusion.

The first thing that calls for elucidation is the meaning of the word *paradox*. In this problem McCrea (*1*) says there is "no paradox," while Crawford (*2*) claims to give a "verification" of it; on examination it transpires that they mean exactly the same thing. Let me at least say what I mean by the word.

A paradox is an apparent contradiction. It arises when two arguments, each apparently sound, lead to incompatible conclusions. Clearly it can be resolved in only one way—namely, by showing that one of the arguments, though apparently sound, is in fact unsound. It cannot be resolved merely by producing additional arguments for one view and leaving the other untouched. To do that is to gild refined gold, which Shakespeare tells us is "wasteful and ridiculous excess"; let me not gild his description.

In this clock paradox the situation, expressed summarily, is this. Einstein (*3*) in 1905 proposed two postulates—the "postulate of relativity" and the "postulate of constant light velocity"—both of which have received abundant confirmation. According to the first, in any case of uniform relative motion (the restriction to uniformity of motion was removed later) there is no phenomenon which will enable one to decide which of the bodies is the absolutely moving one. According to the second postulate (summed up in the Lorentz transformation equations), if you suppose for convenience that one of the bodies is at rest, you must suppose that the other ages less rapidly. These led to contradictory conclusions in the case of two twins, one of whom moves out at a high velocity and returns. The postulate of relativity requires that, since either of them can be the moving one, their ages cannot differ on reunion or there would be a distinguishing phenomenon which would determine which had moved. The other postulate says that since, if we suppose the earthbound twin to be at rest, the other must be supposed to age less rapidly, he will appear younger than his brother when he returns.

My resolution of the paradox is this. I accept the former argument, which seems to me unanswerable. The conclusion drawn from the second postulate is false because the "less rapid aging" is a metaphorical expression. If a clock, B', say, leaves another, B, with which it agrees, and arrives at a distant point P, stationary with respect to B, we cannot tell whether it has kept time or not relatively to B unless there is a clock at P which is synchronized with B, and nature provides no unique means of synchronizing clocks at different places. Einstein's second postulate defines a way of doing this, which is determined by the requirement that the consequences of adopting it must not violate the postulate of relativity; and if this definition be accepted, then B' will be found to be behind a clock at P which is so synchronized with B, on the assumption that B and P are at rest. We rather misleadingly express this by saying that B' has "run slow" during the journey; it would be better to say that the clock at P has been set fast. The essential point, however, is that the equality of times of events at B and at P depends on a process of synchronization which is established by definition, *and which gives different results according to the common motion which we can arbitrarily assign to* B *and* P. Accordingly, if we take B''s point of view for instance, we see B and P moving together, first in one direction until P arrives at B' and then reversing until B again reaches B'. When P arrives at B' it is ahead of B', but B' says that that is because it was wrongly synchronized with B; B synchronized the clocks on the assumption that he and P were at rest, whereas they were both moving together; B has therefore set P too fast. As soon as B and P reverse their motion, however, the synchronization changes. Those same settings are now such that P is too slow (that is, B is too fast) by the same amount. Accordingly, what B' calls the return journey of B exactly compensates for the discrepancy found on the outward journey, and B and B' agree on reunion (*4*).

Now, clearly, if this is right it is a resolution of the paradox; it shows the error in the false argument. So far, no one has found a flaw in this reasoning. Those who hold that asymmetrical aging will occur, however, have *not* offered a resolution of the paradox. To do that they must show that the argument from the postulate of relativity is fallacious, and they have not done so, or even attempted to do so. To simplify refutation I have given the argument from the first postulate in the simplest and most precise form, in a single syllogism (*5*). Let me repeat it here:

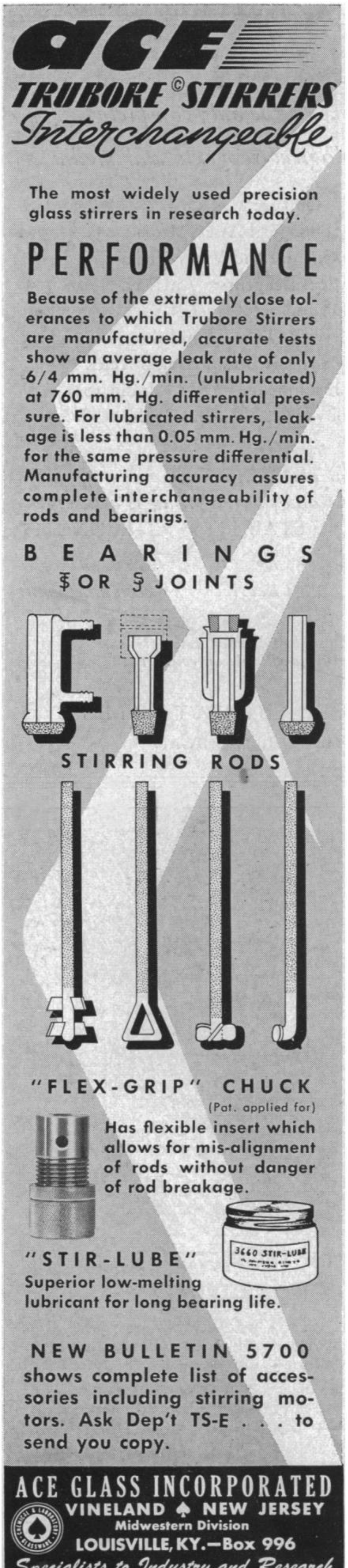
1) According to the postulate of relativity, if two bodies (for example, two identical clocks) separate and reunite, there is no observable phenomenon that will show in an absolute sense that one rather than the other has moved.

2) If, on reunion, one clock were retarded by a quantity depending on their relative motion and the other were not, that phenomenon would show that the first had moved and the second had not.

3) Hence, if the postulate of relativity is true, the clocks must be retarded equally or not at all; in either case, their readings will agree on reunion if they agreed at separation.

I have appealed repeatedly for enlightenment as to the flaw in this argument, but without result; I have not even drawn a single comment on it, let alone an answer. I again appeal to those who hold it to be fallacious to tell me where the fallacy lies.

The numerous arguments for asymmetry, on the other hand, show a pitiable state of affairs. Whatever the truth of the matter, they show beyond question that there is a profound and widespread lack of understanding of what relativity means. They fall into the following three broad classes, in each of which there are minor variations: (i) arguments that the asymmetry follows from the Lorentz equations alone, quite independently of the accelerations which occur at starting, reversal, and stopping [Builder (*6*), Fremlin (*7*), Cochran (*8*), and so on]; (ii) arguments that the asymmetry in the problem arises from the fact that one of the bodies, but not the other, must inevitably be accelerated by some mechanical device, but that the nature of this device and the kind of acceleration it produces are immaterial [McCrea (*1*), Crawford (*9*), McMillan (*10*), and so on]; (iii) arguments that the paradox cannot be resolved within the field of special relativity but requires a precise evaluation of the effect of the reversal of motion in terms of Einstein's theory of gravitation [Einstein (*11*), Born (*12*),



Tolman (*13*), Møller (*14*), and so on].

It is obvious that if any one of these groups of arguments is sound, the others must be wrong. McMillan, for instance, is definitely at variance with Einstein on this point. As I shall show, Einstein's writings are evidently among the literature which he has not read.

I have attempted to clear up a part of the discrepancy, at least, by analyzing a problem in which there are no accelerations (*4*). *B′* passes *B*, moving at uniform relative velocity *V*, and they observe that their clocks agree at the moment of passing. Some time later, *B′* meets *C* coming from the opposite direction (that is, towards *B*) at velocity *V* with respect to *B*, and *B′* and *C* note that their clocks agree when they pass. How will *C*'s clock compare with *B*'s when they meet? This is identical with the ordinary clock problem except that there are no accelerations of any kind in it. Those who, like Crawford for instance, accept argument ii or iii should maintain that *B*'s and *C*'s clocks will agree, since they hold the accelerations to be the cause of the asymmetry. Crawford (*9*), however, holds that the same asymmetrical aging should occur here also. I have asked him what causes it in the absence of the accelerations (*5*) but have received no reply.

It remains for me to consider McMillan's arguments very briefly. He has simply evaded the problem. He says: "The usual next step is to have one of the observers reverse his motion, so that ages can be compared directly when the two come together again. However, the argument is just as cogent if we have them come to a state of relative rest, where age comparisons are meaningful even at a distance, and this approach is used because of its simplicity." He then shows that, to use the former notation, *B′* differs from the clock at *P* when it meets that clock. But that has never been in dispute. I have had to correct this mistake more than once, in correspondence which he himself has cited (*15*). There is no "state of relative rest, where age comparisons are meaningful even at a distance." The event in California which is simultaneous with an event this moment in London depends, for instance, on whether we regard the earth or the sun as being at rest, notwithstanding the fact that London and California are at relative rest in both cases. I have already reminded Crawford of this fact, without response.

McMillan finds an asymmetry in terms of "inertial systems," as though these were facts of nature instead of arbitrary frames of our own choosing. This error also I have pointed out before (*16*). How remote McMillan is from understanding the matter is shown by his remark that his demonstration "does not involve any arguments from general relativity (except insofar as it may give meaning to the concept of an inertial system)." What general relativity did was, in fact, to *take away* meaning from the concept of an inertial system. It will be sufficient to quote one sentence from Einstein and Infeld's book, *The Evolution of Physics,* occurring in the section on general relativity: "The ghosts of absolute motion and inertial coordinate systems can be expelled from physics and a new relativistic physics built" (page 235).

Finally, I would like to remark that if Crawford would reply to the questions I put to him in my letter in *Nature* of 15 June 1957 (*5*), we might make some advance. There is nothing to be gained by successive writers repeating the old statements and ignoring the answers to them.

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References and Notes

1. W. H. McCrea, *Nature* 167, 680 (1951); 177, 784 (1956).
2. F. S. Crawford, Jr., *ibid.* 179, 35 (1957).
3. A. Einstein, *Ann. Phys.* 17, 891 (1905).
4. H. Dingle, *Proc. Phys. Soc. (London)* A69, 925 (1956). In this paper the statements made in the text are proved. An alternative and more general proof of the theorem concerning the change of coordinate times on reversal is given in 5.
5. ———, *Nature* 179, 1242 (1957).
6. G. Builder, *Bull. Inst. Phys.* 8, 210 (1957); *Australian J. Phys.* 10, 246 (1957).
7. J. H. Fremlin, *Nature* 180, 499 (1957).
8. W. Cochran, *ibid.* 179, 977 (1957).
9. F. S. Crawford, Jr., *ibid.* 179, 1071 (1957).
10. E. M. McMillan, *Science* 126, 381 (1957).
11. A. Einstein, *Naturwissenschaften* 6, 697 (1918). Einstein merely outlined the argument but made no calculations; had he done so, I have no doubt that he would have seen that the suggestion would not work. It must be remembered, however, that at that time the problem was a mere curiosity, not worth serious attention, and it was sufficient for his purpose to show that the general theory, which he had recently completed, provided an escape from the contradiction which he agreed the special theory alone could not avoid. Einstein's idea has since been worked out algebraically by the other authors mentioned, but only for small velocities; I have shown that even in these cases it does not remove the contradiction.
12. M. Born, *Einstein's Theory of Relativity* (London, 1924), pp. 216, 282.
13. R. C. Tolman, *Relativity, Thermodynamics and Cosmology* (Oxford, 1934), p. 194.
14. C. Møller, *The Theory of Relativity* (Oxford, 1952), pp. 48, 258.
15. H. Dingle, *Nature* 178, 680 (1956); 179, 865 (1957).
16. ———, *ibid.* 180, 500 (1957).

The editor of *Science* has kindly given me the opportunity to reply to the foregoing letter by Professor Dingle. The next three paragraphs are in the nature of explanations of intent and terminology; following these I shall discuss the more important question: Who is out of step in this parade?

My statement that I had not read widely in the literature of the subject was not meant as an "admission" (presumably of ignorance), as interpreted by Dingle; it was put in to forestall any implication of plagiarism in connection with the material given in the second and third sections of my article. I suspected (correctly) that computations similar to those presented in the third section, on the implications for space travel, had been published elsewhere, while I supposed that my treatment of accelerated coordinate systems in the second section was probably new, and have so far heard nothing to the contrary. The arguments in the first section, on the "paradox" itself, largely follow familiar lines, and I claimed no more than a "restatement" of the situation.

In Dingle's classification of the arguments for asymmetry, mine is placed in the second category, depending on the accelerations, rather than in the first, depending on the Lorentz transformation. In actual fact, all the authors quoted under both of these categories seem to me to understand the problem in the same way, differing only in their presentations. All, in effect, use the Lorentz transformation to compute the time intervals; the acceleration is used only as a kind of "key" to identify changes of coordinate systems, no specific effect of accelerations on clock readings being assumed. In my previous article [*Science* **126**, 381 (1957)], I gave a simple mathematical transformation by which the interpretation of this "key" in terms of time differences can be obtained directly (last equation in the first section).

The phrase "clock paradox" is sometimes used loosely, and in my opinion incorrectly, in referring to the asymmetrical aging itself. Crawford preferred to use it in that sense, although I argued with him at the time that it was an illogical usage. Although we disagreed in a matter of wording, Crawford and I are in complete agreement in matters of principle.

The disagreement between Dingle and the other parties to this controversy is fundamental; someone must be wrong, and the controversy can continue indefinitely unless the error is recognized. In his letter, Dingle uses two independent types of argument, each containing an error which I shall now point out.

One argument is given in the form of a syllogism, whose major premise is: "If two bodies separate and reunite, there is no observable phenomenon that will show in an absolute sense that one rather than the other has moved." This is clearly false. Each body can carry an inertial navigation device in a sealed box (and therefore making only absolute observations); after the completion of the journey, the boxes can be opened, and the recordings made by the devices will contain sufficient information for the construction of complete maps, including velocities, of the motions of the bodies. If one body remains at rest while the other makes a round trip, the two recordings will obviously not be the same. The recordings will not indicate any superimposed uniform motions, but this circumstance is not important; the bodies cannot "separate and reunite" (or, in my treatment, separate and then come to relative rest) if their *only* motions are uniform. Dingle's example of the three uniformly moving clocks B, B', and C is not a correct parallel to the journey of the twins; to be correct, one twin would have to transfer from B' to C in passing, and his motion would then not be uniform.

Dingle's other argument, used three times in the above letter, is based on a misuse of the concept of simultaneity. He says: "Nature provides no unique means of synchronizing clocks at different places," and from the context it is clear that he means this to apply to cases in which the clocks are at relative rest. Now there is a perfectly good way (in fact, there are many ways) to synchronize distant clocks fixed in the same inertial system. This fact is, as far as I know, recognized in all the standard works on the subject. Dingle, while refusing to accept a criterion of simultaneity in the only case where it is valid, has used it in the case of clocks with relative motion, where it is not valid, in an earlier paper [*Nature* **179**, 1242 (1957)].

In closing, I would like to say that both Crawford and I feel that we have said all that we usefully can on this subject; if further rejoinders should be made, we would be inclined to leave the privilege of replying to others.

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Transport of Molecules into Cells against a Concentration Gradient

In an excellent review of endocrine control of amino acid transfer, Noall, Riggs, Walker, and Christensen [*Science* **126**, 1002 (1957)] assume an enzymatic process in concentrative transfer across cell barriers, or of transport of molecules into cells against concentration gradients. Is it not pertinent here to suggest the possibility that the process may be pinocytosis, the remarkable phenomenon of "cell drinking" which was first described by Warren Lewis [*Bull. Johns Hopkins Hosp.* **49,** 17 (1931)]? This may be beautifully demonstrated in phase-contrast time-lapse movies, as has been done for so many tissues by many competent cytologists.

The free edges of cells in tissue culture, as recorded by phase-contrast time-lapse movies, are in continual wavelike movement, laving the medium around the cells and often sweeping over and engulfing a portion of the surrounding fluid, forming what is usually called a "vacuole," but which contains the various molecules that may be in the surrounding liquid. This "vacuole" may be seen to move toward the center of the cell, into which it has been swept by the curling edges, and, as it moves, the phase boundary between it and the cyto-