PHYSICS

Special Theory of Relativity

Two years ago I pointed out[1] what appears to be an inconsistency in the kinematical part of Einstein's special theory of relativity. I repeated this in a slightly different form in a volume published in December last[2]. No comment has been made on the former publication, either spontaneously or in response to individual requests, and in none of the many reviews of the latter has even an oblique attention to the criticism appeared. In view of its profound and far-reaching consequences if it is valid there can be no justification for leaving a twice-published criticism without a published refutation if it is not.

All experimental evidence yet obtained for the special theory is electromagnetic, and this would equally support Lorentz's earlier theory of an ether in which moving bodies are physically changed, and possibly other theories also. No test of the kinematical requirements of Einstein's theory has ever been made—doubtless because it has so far been impossible to reach sufficiently high velocities except by the action of electromagnetic forces on charged particles. The essence of the theory, however, lies in its kinematical requirements, and if these are falsified the whole theory collapses.

Einstein's special theory is so intimately interwoven with modern nuclear physics that its disproof would have revolutionary theoretical effects, but there is another aspect of the matter which is, if not ultimately more important, at least practically more urgent. Apart from this theory there is no reason to suppose (among other things) that the velocity of light, c, is unsurpassable, though if a higher velocity were reached in the laboratory the equations of the theory would necessarily register it as lower than c. All velocities would be under-estimated and, with the increasingly high speeds now being attained, the consequences of this might be extremely serious. In these circumstances the duty of publicly answering or accepting, at the earliest opportunity, a published criticism of the theory becomes compelling. I therefore appeal for an answer or acceptance before further risks are taken.

The alleged inconsistency lies in the fact that the argument used to prove that 'moving clocks run slow' (with which all the kinematical implications of the theory are bound up) proves, with exactly the same validity, that moving clocks run fast. Both cannot be right, so the basis of the theory must be faulty. To show this in the most fundamental manner possible, and at the same time to make the task of refutation as simple and direct as possible, I quote here the passage from Einstein's original paper[3] which purports to prove that moving clocks run slow, together with a parallel passage which similarly proves the opposite.

In the former passage two clocks are introduced, one of which is at rest in an arbitrarily chosen 'stationary' co-ordinate system, K (co-ordinates x, y, z, t), while the other is at rest in a 'moving' co-ordinate system, k (co-ordinates ξ, η, ζ, τ). The transformation equation between the time co-ordinates has previously been given in the algebraically equivalent forms:

$$\left.\begin{aligned} \tau &= \beta(t - vx/c^2) \\ t &= \beta(\tau + v\xi/c^2) \end{aligned}\right\}$$

where $\beta = 1/\sqrt{(1 - v^2/c^2)}$.

Here is the passage from Einstein's paper: "We imagine one of the clocks which are qualified to mark the time t when at rest relatively to the stationary system, and the time τ when at rest relatively to the moving system, to be located at the origin of the co-ordinates of k, and so adjusted that it marks the time τ. What is the rate of this clock, when viewed from the stationary system ?

"Between the quantities x, t, and τ, which refer to the position of the clock, we have, evidently, $x = vt$ and

$$\tau = \frac{1}{\sqrt{(1 - v^2/c^2)}} (t - vx/c^2).$$

Therefore

$$\tau = t\sqrt{(1 - v^2/c^2)} = t - (1 - \sqrt{(1 - v^2/c^2)})t$$

whence it follows that the time marked by the clock (viewed in the stationary system) is slow by $1 - \sqrt{(1 - v^2/c^2)}$ seconds per second, or—neglecting magnitudes of fourth and higher order—by $\frac{1}{2}v^2/c^2$."

And here is the parallel passage, leading to the opposite conclusion: We imagine one of the clocks which are qualified to mark the time t when at rest relatively to the stationary system, and the time τ when at rest relatively to the moving system, to be located at the origin of the co-ordinates of k, and so adjusted that it marks the time τ. What is the rate of this clock, when viewed from the stationary system ?

Between the quantities ξ, τ, and t, which refer to the position of the (other) clock, we have, evidently, $\xi = -v\tau$ and:

$$t = \frac{1}{\sqrt{(1 - v^2/c^2)}} (\tau + v\xi/c^2).$$

Therefore:

$$t = \tau\sqrt{(1 - v^2/c^2)} = \tau - (1 - \sqrt{(1 - v^2/c^2)})\tau$$

whence it follows that the time marked by the clock (viewed in the stationary system) is fast by $1 - \sqrt{(1 - v^2/c^2)}$ seconds per second, or—neglecting magnitudes of fourth and higher order—by $\frac{1}{2}v^2/c^2$.

I should point out that this contradiction has nothing to do with the mere paradox that, if there is an observer with either clock, he will find the other's clock running slower than his own. There are no such observers, and if there were they could be ignored. The situation, as presented by Einstein, is entirely objective. All the necessary readings could be recorded impersonally and the records studied later. Nor is it a matter of change of co-ordinate system. There is no such change: the same clock (A) is chosen as the 'stationary' clock, and the same clock (B) as the 'moving' clock, in both cases, and every symbol has exactly the same meaning in both cases. The conclusion of the first passage is that each reading of B is behind the corresponding reading of A, and that of the second passage is that each reading of A is behind the corresponding reading of B. Thus, if $\sqrt{(1 - v^2/c^2)} = \frac{1}{2}$, and $t = 12$, Einstein's equation gives $\tau = 6$, and when $\tau = 6$ the other equation gives $t = 3$. Hence, when B reads 6, A read both 12 and 3. That is a contradiction.

To avoid this outcome it must be explained not why the two cases are different—that is obvious—but *why, consistently with the theory, the former result must be accepted as true while the latter must be rejected as false.*

HERBERT DINGLE

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[1] *Philosophy of Science*, **27**, 233 (1960).
[2] Samuel, Viscount, and Dingle, H., *A Threefold Cord*, 270 (Allen and Unwin, 1961).
[3] *Ann. Phys.*, **17**, 891 (1905): the translation given here is taken from *The Principle of Relativity*, by A. Einstein *et al.*, 49 (Methuen, 1923).

SPECIAL THEORY OF RELATIVITY

By PROF. HERBERT DINGLE

THE replies which have been received to my communication to *Nature*[1], in which I pointed out a contradiction in the special theory of relativity, are too numerous for individual publication, and the Editor has asked me to deal with them (other than that of Prof. Max Born, which appears on p. 1287 of this issue) collectively. Apart from trivial misunderstandings, the criticisms of my argument fall into a few classes which I will consider separately, but I should first say that only one correspondent heeds the last paragraph of my letter, despite its italics. I there asked, not for the reason why Einstein's statement differed from mine, of which I was aware, but why the former should be considered true and the latter false. The correspondent in question—the only one, in my view, who has really faced the problem—admits that no such reason can be found, and concludes that the conception of an objective rate–ratio of two clocks is impermissible. I cannot agree, but since space prohibits an analysis of this suggestion I justify my dismissal of it here by the fact that I am examining only Einstein's theory, and since he asked: "What is the rate of this clock, when viewed from the stationary system ?" and gave an answer in "seconds per second", it is clear that he accepted a rate–ratio as a meaningful conception.

The remaining correspondents make the following points (it will be remembered that my communication contained two statements, one by Einstein claiming that moving clocks run slow, and one by me claiming that moving clocks run fast): (1) In Einstein's statement the interval considered is that between the separation of the clocks and an event on the moving one, but mine refers to the interval between the separation of the clocks and an event on the stationary one. I have therefore myself introduced the contradiction which I ascribe to Einstein's theory. (2) The internal consistency of the Lorentz transformation can be demonstrated mathematically; it is therefore unquestionable. It gives a unique value for the time of every event in every inertial co-ordinate system, and so cannot possibly contain a contradiction. This reply is usually accompanied by a Minkowski diagram. (3) Both statements show how one clock appears to an observer on the other; they are not contradictory because they relate to different co-ordinate systems.

None of these replies meets the point of my criticism. I can best explain this, I think, by slightly amplifying the situation as described by Einstein, so as to make explicit some steps there tacitly assumed and make the situation more vividly imaginable.

$\dot{Y} \rightarrow \underset{A}{.}$ $\qquad\qquad$ $\dot{X} \rightarrow \underset{B}{.}$

A and X are twins who separate at birth at a speed v such that $\sqrt{(1 - v^2/c^2)} = 1/5$. Each carries a clock which reads 0 at the moment of separation and thereafter reads the age of its bearer. Ahead of A, in the direction of X's motion, and keeping at a constant distance from A, is another child B, born at the same moment as A in A's and B's common time system and carrying a similar clock synchronized with A's. Likewise, in the rear of X, and keeping at a constant distance from X, is a child Y, born at the same moment as X in X's and Y's common time system and carrying a similar clock synchronized with X's.

When X is 6 years old he passes B and they exchange photographs which have just been taken. B, and therefore A, is then 30 years old according to the Lorentz transformation. Further, when A is 6 years old Y passes him and they also exchange recent photographs. The Lorentz transformation then shows that Y, and therefore X, is then 30 years old. All assemble later and agree, on the evidence of the photographs, that A is 30 when X is 6 and X is 30 when A is 6. I call this a contradiction.

The readings can be set out in more detail: Tables 1 and 2 readily follow from the Lorentz transformation.

Table 1 corresponds to Einstein's statement, with A fixed at the origin of the K system and X at the origin of the k system, and we see that X ages more slowly than A, as he concludes. But we also have Table 2.

Table 1

		X is born	X meets B
'Stationary' (K) system	t =	0	30
'Moving' (k) system	τ =	0	6

Table 2

		A is born	Y meets A
'Stationary' (K) system	t =	0	6
'Moving' (k) system	τ =	0	30

Table 2 corresponds to my statement, with A and X still fixed at the origins of the same systems as before, and we see that A ages more slowly than X.

I can now deal with the above objections.

(1) Of course I have referred to a different pair of events from Einstein; vain repetition would be foolish. If a thing has contradictory aspects, you will not discover the fact merely by inspection of one, however often repeated. Einstein's statement and mine are neither paraphrases of one another nor two halves of a single argument. They are independent statements, running in parallel, not in series, and if one is valid, so is the other; but they lead to incompatible conclusions. There is no reason why the persons or clocks should be compared by Table 1 rather than Table 2. True, I do not get X's age directly but have to infer it from Y's; but Einstein does not get A's age directly but infers it from B's. If Y's photograph is not evidence of the age of X, thus discrediting my conclusion, then B's is not evidence of the age of A, thus discrediting Einstein's. It seems to me self-evident that any objection to the implications of Table 2 is *ipso facto* an objection to the corresponding implications of Table 1.

(2) The internal consistency of the Lorentz transformation is not in question. The figures in both tables, as the Minkowski diagram shows clearly, are unambiguous, and not, as numbers, conflicting; but no clock or person can behave in such a way as to fulfil their requirements. One correspondent urges that Einstein's theory is that the time of a distant event can be assigned only by definition and not by natural necessity, and a definition cannot be disproved. But that is not Einstein's theory; that merely clears the ground to make room for a theory (it was, in fact, at the time a piece of supreme intellectual insight). If that were all, then any fantastic figures could appear in the tables, and could not be disputed. But Einstein proposed a particular definition, and his theory was that actual clocks would conform to it. It required them to behave so that the figures in each horizontal line of Tables 1 and 2 were the readings of clocks synchronized according to his prescription, and those in each vertical line were related by the Lorentz transformation. That determined them uniquely. This was, and is, too difficult to test experimentally when it differs perceptibly from the Galilean requirements, but what Tables 1 and 2 show is that the clocks cannot conform to the theory, for in order to satisfy some of its requirements (Table 1) they must necessarily violate others (Table 2). Hence the theory fails—not because of mathematical inconsistency but because of inapplicability to the facts of nature.

(3) This point is met in my reply to Prof. Born (p. 1287); there has been no change of co-ordinate system. But, in any event, co-ordinate systems are merely means to an end; the contradiction here is in the end. Suppose A and B are black and X and Y white. Then what is proved is that blacks are men when whites are boys, and blacks are boys when whites are men. You do not change their skins when you bring them mentally to rest.

An analogy will pinpoint the problem. To find the effect of latitude on clock-rate I use two clocks differing only in latitude. Calculation then shows me that A reads 11 o'clock when B is observed to read 10, and that B reads 11 when A is observed to read 10. Neither clock goes backwards. I conclude that the calculation is wrong. Substitute 'uniform motion' for 'latitude' and 'special relativity theory' for 'calculation', and the analogy becomes an identity. The substitution is not invalidated by any differences between latitude and motion: write x for latitude and y for calculation, and the argument is unaffected. My question is: What other conclusion is possible? It is not a mathematical question: a mathematical answer is simply a sign that the point has been missed. Only a sentence is needed, and no correspondent gives that sentence.

Even a brief reflexion on the consequences of a disproof of the special relativity theory shows how fundamental they are. In the first place, 'space-time', as something having objective significance, is abolished, and 'universal time', independent of space, is restored—not, however, as a necessity of thought as in the old days, but as the only concept of time that makes possible a consistent description of nature. There is no limit to the velocities attainable by material bodies, and—unless there is, in fact, an ether of the Lorentz type which modifies bodies moving through it—there is no contraction of rods, slowing-down of clocks, increase of mass, and so on, accompanying motion.

Electromagnetic theory, on the other hand, needs revision, and three possibilities seem open. First, the *ad hoc* assumptions just mentioned, which were introduced by Lorentz to protect his equations from the assaults of observation, may be again called into service. This, however, would raise the new problem, as difficult as that which it displaces, of finding a reason for such arbitrary changes. Secondly, the converse process to Einstein's might be tried. His theory was devised to reconcile the equations of mechanics with those of the Maxwell–Lorentz electromagnetic theory by changing the independent variables in the former so that the equations obeyed the Lorentz transformation to which the latter were invariant. That having failed, the independent variables of the electromagnetic equations might be regarded as functions of the space and time co-ordinates of Newtonian mechanics, and the functions sought which would render those equations invariant to the Galilean transformation: this would be, in effect, to locate the 'space–time' fiction in electromagnetism instead of in mechanics and ordinary experience. It is a purely mathematical problem to discover whether such functions exist. If not, then the only remaining possibility would seem to be a fundamentally different electromagnetic theory, perhaps on the lines adumbrated by Ritz[2] or those which Faraday outlined and Maxwell mistakenly identified with his own[3], or something directly introducing quantum conceptions. This might seem inadmissible in view of the impressive evidence for the Maxwell–Lorentz theory, but it would be no more remarkable than the supersession of Newton's gravitation theory by Einstein's —which, incidentally, need not share the breakdown of the special relativity theory.

But, whatever may be the right solution, electromagnetic theory is so intimately involved in the whole of sub-atomic physics that the entire subject must be submitted to revision if the special relativity theory is disproved, and the importance of this is so great that a clear settlement of the question, before kinematical velocities not negligible compared with that of light become possible, is imperative. This demands a genuine effort to examine the matter with a completely unprejudiced mind. The general impression which these replies give is that the obstacles to the acceptance of my argument are mainly psychological: my communication has been read, not to see if what I say is right, but to see where it is wrong. If my critics could only manage to conceive the possibility that it might be right, I think they would at once see that it is; it is so very simple.

[1] *Nature*, **195**, 985 (1962).
[2] Ritz, W., *Ann. Chim. Phys.*, **13**, 145 (1908).
[3] See *Philosophy of Science*, **27**, 233 (1960).

LETTERS TO THE EDITOR

PHYSICS

Special Theory of Relativity

PROF. H. DINGLE has sent me a reprint of his communication published under the above title on p. 985 of *Nature*, September 8, 1962, with the handwritten remark: "With kindest regards. Test case for the integrity of scientists".

Though former experience has taught me that discussing relativity with Dingle leads to no agreement I have to answer a challenge which is directed against the "scientific integrity" of myself and of others.

I use Dingle's notation. He restricts himself to the case of a linear motion along the x- or ξ-axis.

$K(x,t)$ is a 'stationary' system of reference, $k(\xi,\tau)$ a 'moving' one. Dingle writes down half the Lorentz transformation expressing τ by t and x, and half its inverse expressing t by τ and ξ. Then he quotes a passage from Einstein's paper, the first paragraph of which ends with the question: "*What is the rate of this clock, when viewed from the stationary system?*". Then follows the well-known derivation of the formula

$$\tau = t\sqrt{(1-v^2/c^2)} \qquad (1)$$

Dingle now proceeds in this way: "And here is the passage leading to the opposite conclusion". The first paragraph of this new passage is completely identical with that of the original including the last sentence just quoted (in italics). Then he exchanges the co-ordinate systems and so obtains the result:

$$t = \tau\sqrt{(1-v^2/c^2)} \qquad (2)$$

which he regards as a contradiction to formula (1).

The mistake is in the first paragraph quoted above (in italics); it should read, in the two cases:

1st case, clock at rest in k: *What is the rate of the clock in k, when viewed from the 'stationary' system K?*

2nd case, clock at rest in K: *What is the rate of the clock in K, when viewed from the 'moving' system k?*

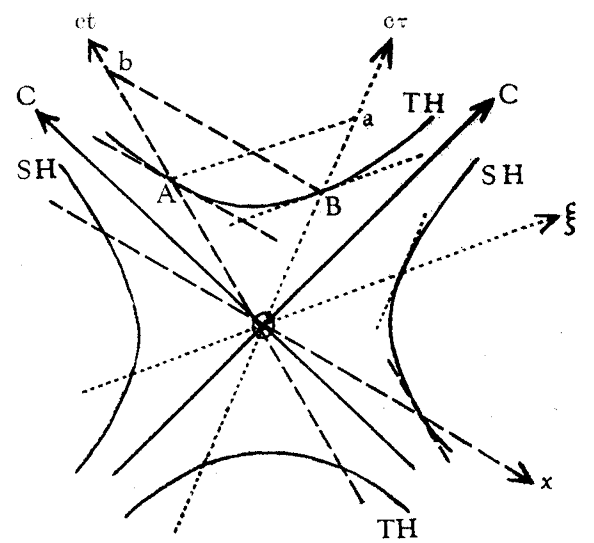

Fig. 1. C,C, Cross-section of light cone; SH, space calibration hyperbola; TH, time calibration hyperpola; x,ct, conjugate diameters = axes in K; $(\xi, c\tau)$, conjugate diameters = axes in k; OA, represents the same time-interval in K as OB in k:

$$OA \sim OB$$

$$\text{Clock at rest in } K \left\{ \begin{matrix} OA \sim ct \\ Oa \sim c\tau \end{matrix} \right\} \begin{matrix} Oa > OB \sim OA \\ \tau > t \end{matrix}$$

$$\text{Clock at rest in } k \left\{ \begin{matrix} OB \sim c\tau \\ Ob \sim ct \end{matrix} \right\} \begin{matrix} Ob > OA \sim OB \\ t > \tau \end{matrix}$$

In other words: formulæ (1) and (2) refer to different physical situations; t and τ have not the same meaning in (1) and (2).

If the clock is at rest in k and in motion relative to K, the reading of the hands of the clock at the beginning of the time-interval t in k is taken at the same place in k as the reading at the end of the time-interval. But the reading of the hands of this clock taken in K at the beginning and at the end of the time-interval are at different positions of the clock in K, as the clock is moving relative to K.

If, on the other hand, the clock is at rest in K and in motion relative to k, the situation is exactly reversed: the beginning and the end position of the clock are read at the same place in K, but at different places in k.

The two cases are therefore different, the symbols t and τ in expressions (1) and (2) referring to different physical situations; these are inverse and must of course correspond to an exchange of the symbols t and τ. This is exactly what expressions (1) and (2) express. There is no contradiction.

Dingle's objections are just a matter of superficial formulation and confusion. 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. The use of such diagrams is explained, for example, in my book *Einstein's Theory of Relativity* of which a new cheap edition (Dover Publications, Inc., New York) is now available. I give here the diagram for the time dilation with a detailed legend (Fig. 1); it shows why the slowing down of a clock at rest in one system when observed from another system in relative motion is reciprocal in the two systems. Further explanations seem to be superfluous.

MAX BORN

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I AM grateful to Prof. Born for replying to my communication, and am especially pleased that one scientist of outstanding eminence has passed the integrity test: would there were others.

I must maintain, however, that he has not met the point I raised: this is shown by the following passage in his letter: "The mistake is in the first paragraph . . . the same meaning in (1) and (2)". In making this statement Prof. Born is clearly exceeding his rights: a question cannot be dismissed as a "mistake". It is my question, and I meant it exactly as I put it. Prof. Born substitutes a different question, which he assumes I should have asked, and gives an answer that does not contradict Einstein's conclusion. I agree, but the answer to my question does.

Prof. Born is wrong in saying that my expressions (1) and (2) refer to different physical situations and that t and τ have not the same meaning in (1) and (2). This is not a matter of opinion. I constructed the second statement, and I say categorically that if, in Einstein's statement, A is a clock fixed at the origin of what he calls the 'stationary' system, and X is a clock fixed at the origin of what he calls the 'moving' system, then in my statement also, A is fixed at the origin of that same 'stationary' system and X at the origin of that same 'moving' system. Furthermore, in both statements t is the time of an event according to clock A, and τ the time of the same event according to clock X.

What Einstein's statement shows is that, on his theory, between the two events, (1) the coincidence of the clocks, and (2) a later event occurring on X, the time-interval according to A is greater than that according to X, so that X is running slower than A. What my statement shows is that, in the identical physical situation, between the two events (1) the coincidence of the clocks, and (2) a later event occurring on A, the time-interval according to A is smaller than that according to X, so that X is running faster than A. The only difference between the two cases is that the intervals compared are those between different pairs of events; and the only conceivable way of avoiding the contradiction is by showing (consistently with the relativity postulate, that is, without assuming an ether in which A is uniquely stationary and X uniquely moving) that Einstein's interval between two events on X is valid for comparing the rates of the clocks, and that my interval between two events on A is not.

I repeat that there is only one physical situation—two clocks, A and X, in uniform relative motion; that there is no change of co-ordinate system in going from Einstein's statement to mine (A 'rests' and X 'moves' in both); and that there is no change of meaning of t or τ (t refers in both to readings of A, and τ to readings of X). Hence equations (1) and (2) show that A goes steadily both faster and slower than X.

The effective criterion of truth in these matters still resides in the pronouncements of mathematical physicists, none of whom, of stature comparable with that of Prof. Born, appears to share his sense of obligation on this point; and as I have been informed, with various degrees of emphasis, that my utterances on such things are imponderable, there is an urgent need for the weight of his unquestioned authority to reinforce a clear demonstration of the truth of the matter, whatever it may be.

HERBERT DINGLE

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[See also p. 1248 of this issue.]

The Case Against Special Relativity

by
H. DINGLE

In 1962 Professor H. Dingle published an argument that the theory of special relativity is invalid. In what follows he restates his case, which is then answered by Professor W. H. McCrea.

FIVE years ago[1] I gave, as the culmination of several similar efforts, a simple proof that the special relativity theory was untenable. This received only one reply from an acknowledged authority, namely, Professor Max Born, who unfortunately, as he himself said[2], assumed that I had expressed myself badly, and replied to what he thought I had meant to say. My assurance that what I had meant was what I had said[3] has remained unnoticed. Nevertheless, the theory has continued to be accepted and used as though it were unquestioned.

This does not accord with the general view of the ethics of scientific practice, and, in a matter so fundamental as this, it is not only abnormal but dangerously so. It is understandable that there should be hesitation in believing that a theory so firmly established, and apparently supported by a great weight of evidence, should be disproved as simply as my letter suggested, but it is equally hard to believe that, if such a simple disproof contained a fallacy, no exposure of that fallacy (which, it may be added, there have been numerous private but unsuccessful attempts to extract from recognized authorities), should have been forthcoming. This criticism of the theory, in various forms, has been published repeatedly, during a period of almost nine years, in physical, astronomical and philosophical journals and in four books, in Britain and in America, without eliciting a single published comment. Reluctance to correct errors in such matters is not a customary feature of scientific discussion, so the natural inference is that there is here no error to correct. The balance of probabilities is therefore fairly even, but in fact that is all irrelevant because the matter must be settled not in that way but by reasoned argument. What my argument showed was that the theory was untenable because it required each of two clocks to work steadily and continuously both faster and slower than the other. I do not think it can be maintained that this is physically possible, and therefore the decision rests on the validity or otherwise of the proof that the theory does in fact require that. That is a matter of pure reason, not of opinion or probability, and therefore it admits of a conclusive solution here and now. Furthermore, the point in question is quite specific and must be dealt with specifically, not submerged in more general considerations concerning the abstract functions of scientific theories. It is of course quite permissible, and indeed inevitable if progress is to be made at all, to use theories that are unproved: it is another, and quite impermissible matter, to base experiments on a theory known to be false. To facilitate assessment of the argument I give it here in an extended form, including explicitly details which were only implicit in the former statement in *Nature*.

Consider the following situation.

$N\bullet \qquad\qquad B\bullet \longrightarrow v$
$\qquad\qquad\qquad A\bullet \qquad H\bullet$

A and H are two relatively stationary, regularly running, clocks. B and N also are two similar relatively stationary, regularly running, clocks, moving with uniform velocity v with respect to A and H. (The distances AH and BN are independent and arbitrary.) A and H are set so that a pulse of light which leaves A when A reads T_1, and is instantaneously reflected back from H when H reads T_2, returns to A when A reads $T_3 = 2T_2 - T_1$. N is similarly set in relation to B. The readings of A and H are denoted by t, and those of B and N by t'.

The following are three successive events during the process.

$N\bullet \qquad B\bullet\ t'=0$
$\qquad\qquad A\bullet\ t=0 \qquad H\bullet$ (event E_0)

Here B is adjacent to A and both are observed to read 0.

$N\bullet \qquad\qquad B\bullet\ t'=t'_1$
$\qquad A\bullet \qquad H\bullet\ t=t_1$ (event E_1)

Here H is adjacent to B, H is observed to read t_1 and B to read t'_1.

$N\bullet\ t'=t'_2 \qquad B\bullet$
$A\bullet\ t=t_2 \qquad H\bullet$ (event E_2)

Here A is adjacent to N, A is observed to read t_2 and N to read t'_2.

All this is quite independent of theory: it is a simple description of a possible physical process. A theory is required when we wish to determine *two independent things*: (i) the values of t'_1, t'_2 for given values of t_1, t_2, or vice versa; and (ii) the relative rates of A and B which these values imply.

Now apply Einstein's theory[4], supposing A fixed at the origin of the K co-ordinate system and B fixed at the origin of the k system.

(i) t and t' are related by the Lorentz transformation, so that

$$t'_1 = at_1 \qquad (1)$$
$$a = (1 - v^2/c^2)^{\frac{1}{2}}$$
$$t_2 = at'_2 \qquad (2)$$

(ii) This is determined by choosing a pair of events and comparing the intervals between the readings of A and B at those events.

Einstein chose events E_0 and E_1. At these events A reads 0 and t_1, respectively, and B reads 0 and t'_1, respectively. The reason why A must be held to read t_1 at E_1 is that H reads t_1 at this event, and on this theory the process by which A is set in relation to H synchronizes it with H.

Thus, between events E_0 and E_1, A advances by t_1 and B by $t'_1 = at_1$ by (1). Therefore

$$\frac{\text{rate of } A}{\text{rate of } B} = \frac{t_1}{at_1} = 1/a > 1 \qquad (3)$$

But now choose events E_0 and E_2. At these events A reads 0 and t_2, respectively, and B reads 0 and t'_2, respectively. The reason why B must be held to read t'_2 at E_2 is that N reads t'_2 at this event, and on this theory the process by which B is set in relation to N synchronizes it with N.

Thus, between events E_0 and E_2, B advances by t'_2 and A by $t_2 = at'_2$ by (2). Therefore

$$\frac{\text{rate of } A}{\text{rate of } B} = \frac{at'_2}{t'_2} = a < 1 \qquad (4)$$

Equations (3) and (4) are contradictory: hence the theory requiring them must be false. Einstein[4], in his paper, gave only (3), and accepted it as giving the unique value of the rate-ratio: he did not check the result by considering the interval between E_0 and E_2. Had he done so he would undoubtedly have seen that his conclusion was erroneous.

I regard this as a conclusive proof that the special relativity theory is untenable, and the consequences of this fact, however improbable they may seem now (they would certainly not have seemed so in 1905), must therefore be accepted. The resistance to acceptance arises not from reason, as my long experience shows, but from incredulity, and this, in its turn, from some very deep-seated misapprehensions which it is impossible here to explore fully, but which can be indicated sufficiently, I hope, to remove something of the almost compulsive predisposition to regard criticism of special relativity as necessarily misconceived.

(I) It is often held that the logical structure of the theory is unassailable, and therefore the theory can be disproved, if at all, only by experiment: hence, any such paper disproof as the foregoing must necessarily be fallacious and there is no need to waste time in discovering where the fallacy lies. This was expressed by Professor Max Born, in the letter previously referred to, in the following terms[2]: "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."

The error here lies in oversight of the fact that a physical theory must contain not only a mathematical structure but also a correlation between the mathematical symbols and observable quantities: a perfectly logical theory may therefore fail physically in the second of these requirements. This oversight calls for much more general consideration, because it characterizes almost the whole of modern physical theory, in which so often a mathematical possibility is assumed automatically to be a physical possibility also, whereas mathematical symbols have a far wider range of significance than is possible to the physical objects whose properties they are taken to represent. This is a matter for later discussion: here I must restrict myself to a single example showing the inapplicability of Professor Born's statement.

The equations, $8-6=2$ and $6-8=-2$, are mathematically valid and equivalent examples of the general equation, $a-b=c$. They are both geometrically applicable to a physical situation: thus, if we walk 8 miles north (+) and then 6 miles south (−) we end 2 miles north of our starting point; and if we walk 6 miles north and then 8 miles south we end 2 miles south of our starting point. But they are not both applicable to physical objects: you can get 6 apples from 8 by leaving 2 behind, but you cannot get 8 apples from 6 by leaving −2 behind. If Professor Born's argument were sound we should be able to say: the simple fact that all numerical values of a, b and c expressed by the equation $a-b=c$ can be represented geometrically by lines drawn to north and south should suffice to show that there can be no logical contradiction (and, by implication, nothing wrong) in the theory that you can get 8 apples from 6.

(II) The resistance most commonly felt by practical physicists to the disproof of the theory arises from a conviction that the experimental evidence for it is too strong to be overcome by a mere piece of logical jugglery which, in face of it, has no more weight than Zeno's proof that Achilles could not overtake the tortoise. This again reveals a misconception needing far more extended treatment than is possible here, where all that can be said is that it is due to an oversight or misreading of the facts of history. There is no existing experimental evidence for Einstein's theory that does not give exactly the same support (whatever that may be) to a quite different theory advanced earlier by Lorentz[5]. Both theories have the same mathematical structure (the Maxwell–Lorentz electromagnetic equations plus the equations of the Lorentz transformation) but give it quite different physical interpretations. All that the experiments so far performed (for example, those showing increase of mass with velocity, extended lifetimes of cosmic ray particles, etc.) show is that if we assume the electromagnetic equations we must correct them by the Lorentz transformation; they throw no light at all on the physical interpretation of the equations.

The physical differences between the theories are profound: here are a few. Lorentz ascribes the contraction of rods and slowing down of clocks to an *ad hoc* physical effect of the ether on moving bodies; Einstein ascribes them to an *ad hoc* modification of kinematics at high velocities. Lorentz's theory is impossible without an ether; Einstein's (because of its relativity postulate) is impossible with one. Einstein's theory makes a velocity greater than c logically impossible; Lorentz specifically restricted his theory to "a system moving with any velocity less than that of light", and, from the nature of its effects, it must break down well short of that velocity, just as Boyle's law breaks down well before the volume of a gas shrinks to nothing; it makes the "light barrier" no more necessarily impassable than the "sound barrier". Einstein's theory merges space and time into an unimaginable "space-time"; Lorentz's leaves them independent, as in ordinary understanding. The physical consequences of these differences when very high macroscopic velocities are attained are enormous and ominously incalculable.

Until the First World War, Lorentz's and Einstein's theories were regarded as different forms of the same idea, but Lorentz, having priority and being a more established figure speaking a more familiar language, was credited with it: thus Poincaré, as late as 1912, spoke of "le principe de relativité de Lorentz", even in a paper in which he was discussing Einstein's view of the action of light on molecules[6]. It was not until 1919, when the eclipse observations compelled acceptance of Einstein's general theory, that "the special theory of relativity" became uniquely ascribed to Einstein, and the ideas associated with the name in the minds of physicists became an incompatible mixture of Lorentz's and Einstein's—a fact that preserved the theory from disproof, since any attack on the relativity aspect could be met by an appeal to Lorentz's non-relativistic ideas, and criticisms of those could be disposed of by a reversion to relativity. Thus, for example, the "FitzGerald contraction" was variously regarded as an actual physical effect and as a mere appearance, according to the needs of the occasion.

Whittaker[7] partly exposed the confusion, but, as a pure mathematician characterizing a theory by its mathematics alone, he saw it as merely a wrong assignment of priorities, and entitled his chapter on the supposedly single theory, "The Relativity Theory of Poincaré and Lorentz". The fact that there were two distinct theories, physically poles apart, was thus obscured. If Einstein's paper, however, had never been written, all the experiments now held to "prove" Einstein's theory would still have been performed and held with the same conviction to prove Lorentz's. Is it conceivable, it would have been asked, that a moving body can experience a resistance to acceleration (increase of mass) unless there is an ether to provide the resistance ? Indeed, this very phenomenon was cited by Lorentz in support of his theory before Einstein's paper appeared[5]. The very experiments now held to prove a theory dismissing the ether would have been held to prove its indispensability.

An important point in the present discussion, however, is that the disproof of Einstein's theory given above leaves Lorentz's intact. Both agree down to equations (1) and (2), but the process by which, according to Einstein, A and H, and B and N, respectively, are synchronized does not synchronize them on Lorentz's theory, because one pair, at least, must be moving in the ether. If we suppose the other pair at rest, then they are truly synchronized, but the moving pair are not, any more than clocks on relatively stationary aeroplanes, moving rapidly through the air along the line joining them, would be synchronized by a similar process with sound waves. If, then, we attach conclusive weight to already performed experiments, we must consider Lorentz's theory proved and seek a rational basis for his *ad hoc* postulates.

But those experiments are not conclusive, for they do not dispose of the alternative possibility, advanced by Ritz, that the velocity of light is c with respect to its source alone[8]. Einstein's theory is a logical deduction from two postulates: (*a*) the postulate of relativity—the absence of an absolute standard of rest, that is, of an ether, and (*b*) the postulate that the velocity of light in space is *c*, whatever the motion of its source. Lorentz's theory denies (*a*) and accepts (*b*); Ritz's theory accepts (*a*) and denies (*b*). Contrary to general belief, Ritz's theory (that is, the simple hypothesis just stated, not necessarily his tentative development of it, which he later[9] described as a "Scheusal-Theorie"—horror theory) has never been tested. Deductions from double star observations are inconclusive[10], and the various laboratory experiments with hypothetical particles as sources and the assumption of the wave equation, $c=n\lambda$, with its usual interpretation, all involve a circular argument. If Ritz's hypothesis is correct, the electromagnetic theory of light, in its present form at least, is not, for that requires the velocity of light to be independent of that of its source. Thus we must not presuppose any part of the electromagnetic theory in testing Ritz's hypothesis. But all tests involving hypothetical particles, or interference as it is usually understood, do just that. To take but one example, in the experiment of Alväger, Nilsson and Kjellman[11], beams of γ-radiation from a vacuum tube, showing spectrum shifts suggesting sources moving with high velocity, travelled through space with the same velocity as beams from particles in the tube showing no spectrum shift, and it was concluded that Ritz's hypothesis was disproved. But suppose the beams had travelled with different velocities. Then the electromagnetic theory would have been disproved, and so the evidence that the sources were particles moving with the supposed velocities would have disappeared. Such an experiment therefore could not possibly have tested Ritz's hypothesis. For a true test the source must be a body observed to move with a known velocity and not one inferred from a theory that rules the hypothesis out of court before the test has begun.

The following aspect of the situation may clarify it for some readers. The Maxwell–Lorentz electromagnetic equations and the Newtonian mechanical equations had in common the co-ordinates (x, y, z, t) which were related to space and time measurements in an understood way, and their values when the physical system under consideration was referred to a relatively moving system of co-ordinates were taken, in 1905, to be given by the Galilean transformation. This left the mechanical equations unchanged in form (that is, they were relativistic), but not the electromagnetic equations. ("The [electromagnetic] theory appeared to be unsatisfactory only in *one* point of fundamental importance. It appeared to give preference to one system of co-ordinates of a particular state of motion. . . . In this point the theory seemed to stand in direct opposition to classical mechanics, in which all inertial systems which are in uniform motion with respect to each other are equally justifiable as systems of co-ordinates"[12].) Electromagnetic theory was accordingly taken to require certain observable events to occur (for example, a fringe-shift in the Michelson–Morley experiment) when a piece of apparatus was moved. In fact these events did not occur, that is, electromagnetic phenomena were relativistic (invariant to motion), but the equations were not. The latter, however, would be relativistic if the transformation equations were not those of Galileo but those of Lorentz. Einstein's theory was that they were so, and the effect of this on mechanics was then far beyond the possibility of experimental test because the necessary velocities in a mechanical experiment were unattainable.

But if the Galilean transformation is the correct one, the assumption of the Lorentz transformation must give discrepancies with observation in mechanics corresponding to those found in electromagnetism under the Galilean transformation. This is what is now shown to be the case; the assumption of the Lorentz transformation in mechanics requires one clock to work both faster and slower than another. The fact that this can be seen to be contradictory in advance of observation, whereas the result of the Michelson–Morley experiment could not be foreseen, is due simply to the fact that we already know far more about clocks than about light. Whether or not a particular mathematical possibility can be realized physically can be known prior to experiment only when we have sufficient knowledge of the physical situation concerned, and we know enough about clocks to know that one cannot, at the same time and in the same sense, be working both faster and slower than another. If we had as much knowledge of the structure and behaviour of light sources and light beams as we have of clocks (or apples), a fringe shift in the Michelson–Morley experiment would be as obviously impossible as the contradictory behaviour of clocks (or the obtaining of apples by the compensating creation of negative ones). And, just as the Michelson–Morley experiment is only one of a number showing the breakdown of electromagnetic theory under the Galilean transformation, so the experiment with moving clocks is only one of a number showing the breakdown of mechanical theory under the Lorentz transformation. Another, for example, is revealed in the possibilities of mutual observation by widely separated observers[13]. It is clear that a change of transformation equations, as proposed by Einstein, merely transfers the discrepancy with observation from one set of phenomena to the other: a change in the theory of one set (almost certainly electromagnetism, as quantum phenomena more than suggest), by giving the ether additional properties (Lorentz) or discarding it (Ritz) or by some other means not yet conceived, would now seem to be the only possibilities open of reconciling mechanical and electromagnetic phenomena in a single theory (which may or may not be a unified *field* theory).

The net result, then, of these considerations concerning experiments is that none yet performed disproves either Lorentz's or Ritz's theory, and because neither theory is disproved by the earlier rational argument which was fatal to Einstein's (on Ritz's hypothesis, equations (1) and (2) become simply $t_1' = t_1$ and $t_2 = t_2'$, leading to a rate-ratio of unity for all event-intervals), these theories remain in the field. A valid experiment to test Ritz's hypothesis (such, for example, as that suggested earlier[14], in which observable sources are used) is clearly called for.

(III) Another apparent possibility of saving Einstein's theory lies in the supposition that equations (3) and (4) are not really contradictory because they refer not to objective phenomena but merely to appearances: *A* appears to go slow when observed from *B*, and *B* appears to go slow when observed from *A*. Again it would take too much space to show—although I do not think anyone familiar with the subject will have much difficulty in perceiving it—that if this were so the whole theory would be concerned merely with appearances and could not possibly lead to an explanation of any of the objective phenomena for which the theory was designed. All that is practicable here is to point out that this was not Einstein's interpretation of the result, nor has it been that of any of his followers when dealing with this point alone and not seeking an interpretation that will dispose of some other difficulty. Here are the deductions which Einstein makes from equation (3)[4]: "If one of two synchronous clocks at *A* is moved in a closed curve with constant velocity until it returns to *A*, the journey lasting *t* seconds, then by the clock which has remained at rest the travelled clock on its arrival at *A* will be $\frac{1}{2}tv^2/c^2$ second slow. Thence 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". We need not ask if these deductions are valid; all we need to notice is that precisely the opposite deductions, valid or invalid, can be made from equation (4). It is inconceivable that if Einstein had noticed this he would have selected only the equatorial clock as the one which was going slower than the other. Moreover, he added a footnote to say that the result did not apply to a pendulum clock. This would have been meaningless if it were not the actual physical working of the clocks that was in question but merely an accident of the observer's standpoint. As evidence that the general interpretation of the result agrees with Einstein's, it is sufficient to cite the universal belief that asymmetrical ageing of separated and reunited clocks or persons is required by Einstein's theory.

Allied to this is the misconception that equations (3) and (4) refer to different physical situations. That is not so. The events E_0, E_1, E_2, are successive events in a single process; there is no change in the physical conditions during that process. Also, there is no "change of co-ordinate system". Such systems appear in the argument only implicitly in the use of the Lorentz transformation to derive equations (1) and (2), and there is no change of system anywhere in the derivation. Whether you regard A as stationary and B as moving, or vice versa, makes no difference whatever: throughout, the primed symbols refer to the clocks B and N and the unprimed symbols to the clocks A and H, no matter how you describe them.

The situation is quite clear: the only difference between the arguments leading to (3) and (4) is in the events chosen for comparing the clock rates. If Eintsein's theory is valid the following questions arise. How is it possible for the ratio of the intervals recorded by two identically constructed, regularly running, clocks, between the same pair of events, to vary with the events chosen (in other words, how can the ratio of two constant quantities be variable)? Second, if it is possible, why must the events that alone give the "correct" ratio be chosen from the set occurring on one and not the other of the clocks? Third, if they must be so chosen, how does one (consistently with a theory in which the only feature in which the clocks differ—motion—can be ascribed indifferently to one or the other) discover on which clock the valid set of events occurs? I think it is self-evident that these questions are unanswerable. There can be no doubt that, if this criticism of the theory had been made in 1906, it would at once have been seen to be fatal and Einstein would have been the first to acknowledge it, for then reason was the *de facto* as well as the *de jure* arbiter in such a matter. In 1967, however, the obvious has become the inconceivable, and it has to meet the prejudice, independent of reason, that every apparent objection to special relativity is merely evidence of incomprehension and can accordingly be ignored. Unless faith in reason is restored, and prejudice determinedly uprooted, the outlook in the present age is black indeed.

I have introduced a discussion of the implications of the matter, not at present for their own sake but in order to remove obstacles, which experience has shown to be formidable, to concentration of attention on the simple alleged disproof of the theory. I hope it will not have the opposite effect of diverting attention to itself. As I have said, most of the points raised demand fuller treatment later. But the disproof is complete in itself. Unless some specific error is found, and clearly exposed, in the passage of the foregoing argument extending from the words, "Now apply Einstein's thoery . . ." to ". . . hence the theory requiring them must be false"—an error of such a character that it invalidates equation (4) without, at the same time, invalidating equation (3)— it must be accepted that the special relativity theory is untenable, no matter how unexpected or unwelcome or perplexing or fraught with difficulties the implications and consequences may be.

I would point out also that what I have advanced is not a theory which, in the traditional scientific manner, can be left to be justified or condemned by experiment. No experiment can do either, for the conclusion follows rationally from the premises. If there is no error in the reasoning, the only relevant experiments—and they are urgently demanded—are those designed to show where, and not if, the theory is wrong. Furthermore, it does not seem yet to be sufficiently realized that the nature of modern experiments makes imperative a change of attitude to the relegation of fundamental problems to decision by experiment. It was safe enough to await a measurement of the velocity of light through air and water before deciding for the wave or particle theory of light, and the convincing nature of the result justified suspension of judgment. But, so deeply involved are the special relativity theory and the electromagnetic theory of light in the whole of modern physics, that if experiments of the modern type continue on the assumption that special relativity is tenable when it is not, the results, sooner or later, are as likely as not to lay waste a county. Truth is immortal but human lives are not, and they have claims to protection, even at the cost of admitting an error in physical theory that should never have been made. The recent tragedy at Aberfan shows how bitterly regrettable the consequences may be when hindsight is not anticipated by foresight, and the consequences there were slight compared with those conceivable here. I hope, therefore, that this matter will no longer be allowed, by neglect, to take its own natural and possibly disastrous course, but will be faced squarely and promptly, with no aim but that of arriving at the truth, whatever it may be.

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Why the Special Theory of Relativity is Correct

by
W. H. McCREA

I GIVE first a brief presentation designed to facilitate a reply to Professor Dingle's present statement and to the one[1] he gave in 1962. So far as applicable, I use Dingle's present notation (which is not identical with what he used in 1962).

Let Ox be a rigid rod graduated in the usual way; let similar clocks be fixed to the rod at points along the rod, and let them be synchronized by a standard procedure (that described by Dingle). If anything happens at the position x of any one of the clocks, let t be the reading of that clock at that event E, say. We speak of the event E as the event (x, t). Let $O'x'$ be a second rigid rod in motion along Ox with uniform velocity v $(\neq 0)$. Let $O'x'$ be graduated in the same way as Ox; let clocks similar to those attached to Ox be fixed to $O'x'$ at points along $O'x'$, and let them be synchronized amongst themselves by the standard procedure. If anything happens at the position x' of any one of these clocks, let t' be the reading of that clock at that event E', say. We speak of the event E' as the event (x', t').

According to the theory of special relativity, this system is possible, supposing Ox, $O'x'$ to belong to inertial frames

K, k, say. The theory then asserts that E, E' are one and the same event if and only if the parameters satisfy the relations

$$at' = t - vx/c^2 \qquad \text{(I)}$$

$$at = t' + vx'/c^2 \qquad \text{(II)}$$

where $a = (1 - v^2/c^2)^{\frac{1}{2}}$, supposing $0 < a < 1$ and supposing the zero points of the various quantities are suitably chosen. This is one way of writing the Lorentz transformation (being the one used by Dingle in his earlier paper[1]).

Consider in k the particular clock B permanently fixed at O', so that every event at B has $x' = 0$. Then from (II) for every such event

$$at = t' \qquad \text{(III)}$$

[Take, for example, the case $a = \frac{1}{2}$. Equation (III) means that if clock B reads t' then that K-clock past which B is moving reads $2\,t'$; at 1 o'clock by B it passes a K-clock reading 2 o'clock, at 2 o'clock by B it passes a K-clock (a different one, naturally) reading 4 o'clock, and so on.]

In the immediate operational interpretation of (III), as just illustrated, t' is the reading of one and only one clock and t is the reading of a different clock for each different value of t. I repeat that, so far as our discussion is concerned, every event to which (III) applies happens to clock B.

If we next consider in K the particular clock A permanently fixed at O, then every event at A has $x = 0$ and from (I) we have for every such event

$$at' = t \qquad \text{(IV)}$$

This is obviously what we expect from (III) because now K, k have exchanged roles. In (IV), t is now the reading of one and only one clock, and t' is now the reading of a different clock for each different value of t'. Manifestly the parameters t, t' do not have the same meanings in (III), (IV). Every event to which (III) applies happens to the clock B; every event to which (IV) applies happens to the clock A.

[If we do require both (III) and (IV) to hold good we get simply $t = 0 = t'$, since $a^2 \neq 1$. That is, (III), (IV) are both satisfied for the unique event that happens to both clock A and clock B, namely their single mutual encounter. This is obviously entirely consistent with what has just been said.]

No particular or preferred observer is concerned in these results. If a cine-camera anywhere in any state of motion takes a sequence of pictures of clock B, each picture will show clock B with some reading t' and, adjacent to B, a K-clock reading t'/a, the K-clock being a different one in each picture. If the same or any other camera takes pictures of A, each picture will show A with some reading t and, adjacent to A, a k-clock reading t/a, the k-clock being a different one in each picture.

I turn now to Dingle's allegation that the theory used above "must be false". In his present paper, this is based simply on his claim to have inferred the contradictory statements (3) and (4) of his paper from the theory. So we have to do only with the logical consistency of the theory. It may help if I enumerate a sequence of arguments; the first alone is sufficient to refute Dingle's contention, but I hope the rest throw further light on the subject as a whole.

(i) Dingle's assertion is obviously and demonstrably wrong. Using no more than the Lorentz transformation in his algebra, he claims to derive two different values for the same quantity. But the transformation is linear and any result it gives can only be unique. It is trivially impossible for it to give two different answers to the same question. If Dingle obtains two different answers it must be because (a) he has made a slip in the algebra, or (b) his quantities are not well defined, or (c) what he treats as the same quantity are two different quantities.

(ii) Dingle has not made any mistake in the algebra, but in his present paper he deals with objects to which the theory explicitly denies a meaning. We consider events E_0, E_1, E_2 defined and described in frames K, k as follows (these being apparently the events similarly denoted by Dingle):

Event	K-description	k-description
E_0 A, B encounter each other	$x=0, t=0$	$x'=0, t'=0$
E_1, H, B encounter each other	$x=x_1, t=t_1$	$x'=0, t'=at_1$
E_2 A, N encounter each other	$x=0, t=at'_2$	$x'=x'_2$ $t'=t'_2$

Here, and in physics generally, event means something happening at a particular position at a particular instant. The crucial feature is that an observer experiences an event if, and only if, the event is part of his own history, that is the event is in his own world-line.

In Dingle's system in his present article A and B are the only observers who experience the event E_0, or are "at" event E_0; H and B are the only observers at E_1; A and N are the only observers at E_2. Dingle arrives at his conclusions because in practice he does not adhere to the standard concept of an event. He asserts, "The reason why A must be held to read t_1 at E_1 is that H reads t_1 at this event, and on this theory the process by which A is set in relation to H synchronizes it with H . . . The reason why B must be held to read t'_2 at E_2 is . . .". A is not "at" E_1 in any sense admitted by the theory and it simply has no meaning whatever within the theory to speak of what A must be held to do at E_1. B is not at E_2 and it has no meaning to speak of what B must be held to do at E_2.

Just before his formula (3), Dingle proceeds to state "between events E_0 and E_1, A advances by t_1 . . .". Because A is never at E_1, this phrase is meaningless and so Dingle's (3) is meaningless. Correspondingly his (4) is meaningless.

(iii) Naturally there is an event E_{1A}, say, at which A reads t_1. This event has $x = 0$, $t = t_1$ and so clearly $E_{1A} \neq E_1$, thus corroborating what has just been said.

(iv) Dingle's language requires a meaning for what the clock A reads "at" some event involving B even though A is not at the place of that event. In other words, he wants to say what A does "when" B does something, although A and B are not adjacent. Indeed, Dingle expressly uses this phraseology in his 1962 paper. But this restores the notion of distant simultaneity.

About the first thing that relativity theory does is to deny any operational meaning to the notion of simultaneity at two different places. Naturally, this fundamental feature in the theory is not affected in the slightest by any arbitrary conventions we may adopt for the synchronization of clocks. The latter is merely a particular way of putting the readings of two relatively stationary clocks into 1–1 correspondence with each other.

(v) While Dingle's (3) and (4) are meaningless as they stand, the quantities involved can of course be assigned operational meanings in terms of readings of the relatively moving clocks A, B. The formulae do not then tell us about the "rates" of the clocks. They become simply two different ways of putting the readings of A, B into 1–1 correspondence with each other. There are infinitely many different ways of doing this! Being no more than ways of attaching labels, there can be no question of any two of these ways being "contradictory".

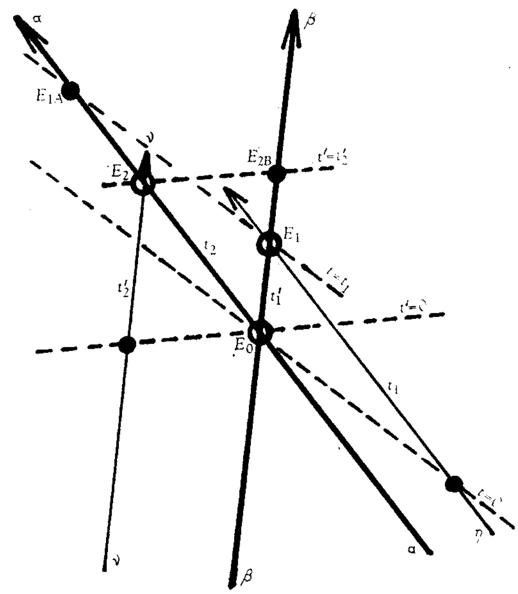
(vi) In his 1962 paper, Dingle started from equations (I), (II) as we have written them (but in his earlier notation) and then derived precisely our equations (III), (IV). He then asserted, "every symbol has exactly the same meaning in both cases", and he claimed to infer a contradiction. His assertion is false, because here he is not talking about the same thing, but two different things. As we have seen, equations (III) and (IV) concern two distinct sets of events, and so they cannot contradict each other. More exactly, the equations concern distinct sets apart from the unique common event for which $t = 0$ and $t' = 0$, and for this event (III) and (IV) are clearly both satisfied.

(vii) While in Dingle's system A and B are the only observers who experience event E_0, it is of course meaningful to say that other observers can observe event E_0. Indeed, if any observer Ω anywhere in the universe takes a motion picture of A or B and if one exposure shows the encounter of A with B, then we say that Ω has observed the event E_0. This exposure would show A and B in juxtaposition with both clocks in this case reading zero. Every observer who observes E_0 will get precisely this same picture.

Suppose now that the motion picture taken by Ω shows also the clocks H, N of Dingle's system. Then in the exposure showing the event E_0, clocks H, N will appear showing some particular readings. If another observer Ω^* at some different place in the universe makes a corresponding motion picture, then in Ω^*'s exposure showing the event E_0 clocks H, N will appear showing some other particular readings different (in general) from those in Ω's exposure. This is because the various light-travel times from the clocks to Ω and to Ω^* are all different. Thus there is no unique reading, and no preferred reading, of H or of N to be associated with the event E_0. This inference does not depend on any arbitrarily selected graduation of the clocks. Thus we have another, possibly more "operational", refutation of Dingle's criticism.

(viii) We may draw a simple space-time diagram in which α is the world-line of A, and so on. Then the events E_0, E_1, E_2 are as shown. This makes it perfectly clear that α does not go through E_1 and so there cannot possibly be a reading of A "at" E_1, this having nothing to do with the manner in which the clocks happen to be graduated. This was the essential point of Born's comment[2].

In this diagram we may treat t, t' as oblique cartesian co-ordinates. Then, using these co-ordinates, equation (III) is the equation of the world-line β and equation (IV) is the equation of the world-line α and E_0 is their unique point (0,0) of intersection. This shows more clearly than anything else the difference between the two sets of events for which (III) and (IV) hold good.



The diagram shows the line $t = t_1$ through E_1 meeting α in the event E_{1A}. Thus E_{1A} is the event at which A reads t_1. As we have said, Dingle's formula (3) has to do with the correspondence between events E_1, E_{1A}; but we learn nothing by setting up this correspondence and so there is nothing in it to be contradicted, or to contradict anything else.

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