although it contained matter of historical interest, anyone who took it seriously would make himself ridiculous. I have met none willing to face that indignity merely because he cannot find a fault in what he knows by supernatural revelation (though he would not call it such, yet would be at a loss to find an alternative name for its source) must nevertheless be faulty. Unless, therefore, the facts related in Part One should lead to the awakening of physicists of influence – either directly or through the compulsion of outside pressure – to an awareness of the state into which they have unconsciously lapsed, it will remain unheeded until the time comes when they will bitterly but vainly regret the lost opportunity of merely making themselves ridiculous.

Appendix

I Editorial in *Nature*, October 14, 1967, p. 113.

II Article by H. Dingle in *Nature*, October 14, 1967, p. 119.

III Article by W. H. McCrea in *Nature*, October 14, 1967, p. 122.

IV Article by H. Dingle in *Nature*, January 6, 1968, p. 19.

(The headings of I, II and IV [and, presumably, III] were provided by *Nature*).

I

DON'T BRING BACK THE ETHER

It is probably too much to hope that the exchange of views between Professor Herbert Dingle and Professor W. H. McCrea which appears in this issue will put an end to a long-standing argument in special relativity. By now there is all too much evidence to show that issues like these have a habit of springing to life long after the stuffing seems to have been knocked out of them by the force of pure reason. It is Dingle who mentions Zeno's paradox, which shows that he too is fully aware of the pitfalls which can exist in what seem to be the most elementary kinds of arguments. But defining limits for the length of the diagonal of a square is child's play compared with the process of synchronising clocks in special relativity, which means that nobody should be surprised or even alarmed that distinguished people occasionally pay close attention to these matters.

Dingle's criticism of special relativity is, of course, pretty radical as these things go. He seems quite genuinely convinced that the theory of special relativity is seriously in error, and that there is a kind of conspiracy to hide from this supposedly self-evident and presumably unpalatable truth. He also considers that there has been something of a breach of professional propriety in the way in which his detailed assertion of his case against special relativity (*Nature*, **195**, 985; 1962) provoked only a reply from Professor Max Born (*Nature*, **197**, 1287; 1963). To many people, of course, that will seem an over-generous tribute to Dingle's case against special relativity, yet it is also true that Dingle's persistence with his argument is deserving of respect. He has been concerned not so much to twist the tail of orthodoxy as to draw attention to what he has for many years considered to be a serious defect in the accepted doctrine. McCrea's commentary on this argument will bring the controversy to an end for most people. It is earnestly to be hoped that it will also satisfy Dingle.

A part of the confusion which this controversy has occasioned arises because what may be called the Dingle contradiction is in many ways suggestive of other paradoxes in relativity – the clock paradox, for example. It is therefore important to be clear about the nature of Dingle's protest. He says, quite accurately, that Einstein's postulates in special relativity lead directly to the familiar Lorentz transformation for the co-ordinates of space and time and, in particular, to the notion that the time recorded by moving systems – clocks, for example, or radioactive atoms – appears to be dilated when matched against a stationary system for recording time. For example, short-lived mesons in the cosmic rays appear to observers on the surface of the Earth to last long enough to reach the ground. But time dilation works in both directions. From the point of view of quickly moving mesons in the cosmic rays, the lifetime of radioactive atoms on the surface of the Earth will seem to be unnaturally prolonged.

All this is entirely consistent with that one of the twin postulates of Einstein's special theory which has it that there is no objective difference between inertial frames of reference moving relatively to each other with constant velocity. Dingle constructs an argument by which he claims to show that this reciprocity of time dilation implies a contradiction. If it implies that some clocks in motion must run slow, it also implies that the same clocks run fast compared with some stationary system. What McCrea has done is to point out that Dingle's construction is an attempt somehow to evade the awkward truth that in relativistic mechanics there are serious physical limitations of the freedom with which separate events can be sychronised in time. In other words, McCrea argues that Dingle has thrown to the winds some of the quite elementary precautions which should be taken by those who choose to venture into this important field. In the circumstances, it is no great surprise that he is able to establish a *reductio ad absurdum*.

McCrea has done a public service by the trouble he has taken in his demonstration of these points. The chances are that most people will be persuaded by what he has to say. It does not follow, of course, that those who, for one reason or another, find Einstein's version of the theory of special relativity unpalatable will promptly be forced to toe the conventional line. After all, there is nothing to prevent those who want to bring back the ether, or who would like the velocity of light to be otherwise than constant, from seeking other ways of saving the appearances. As the saying goes, it is a free country, and there is nothing to prevent people from tilting at windmills if they choose. It is also worth recalling that McCrea's demonstration will not put an end to discussion of that paradox which, in its most popular form, alleges that a relativistic space traveller will be found to have aged less quickly than those who have stayed at home. The point here is that the symmetry of the problem in special relativity is upset by the mere fact that the traveller is set in motion by the deliberate application of external forces.

There remain two quite serious aspects of the charges which Dingle has levelled against orthodoxy, and they also deserve an answer. The least contentious of them concerns the way in which theories of any kind can be challenged and destroyed. There is, of course, no question that a flagrant internal inconsistency is intolerable, and must be got rid of if the theory is somehow to survive. In practice, however, the special theory of relativity has been enormously successful in the past half-century, and in spirit as well as in detail has come to pervade the whole of modern physics. Even though Dingle may say that an abandonment of strict relativity in the sense of Einstein's essential postulate would not necessarily imply the end of Lorentz transformations, it is exceedingly hard to believe that a postulate which seems entirely helpful in most of its applications should nevertheless lead to a real inconsistency, and it is no surprise that the resolution of the paradox which McCrea has provided is in a sense semantic. 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.

There is also the problem of whether Dingle is right in saying that there has been a breach of professional concern for professional integrity in the way in which his original assertion has not been answered in the detail which he himself considers to be necessary. This raises interesting questions about the function of the scientific literature as a whole. It is, in particular, important to know just to what extent the profession of science should come smartly to a halt when somebody cries scandal and says that there is something wrong with the essential character of an important theory. Dingle is right to seize on the importance of propaganda. The man who first spots an inconsistency has a duty to bring it to the attention of others, if

necessary with vigour. But, especially now that most people are disciples of Popper, an inconsistency is more likely to be welcomed than ignored. Science may not be conspicuously more free from self-deception than other kinds of intellectual activity, but everybody knows that it flourishes by upsetting apple-carts. If, in these circumstances, an allegation of scandal should be ignored, that in itself is an entirely proper reason for asking whether the allegation can be well founded. It is immodest of Dingle to have plumped for the alternative supposition that the profession of science is at fault.

II

THE CASE AGAINST SPECIAL RELATIVITY

by H. Dingle

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 recognised 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 there-

fore 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.

$N^{\cdot}$ $\qquad\qquad$ $B^{\cdot} \longrightarrow v$

$\qquad\qquad\qquad$ $A^{\cdot}$ $\qquad$ $H^{\cdot}$

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.

$N^{\cdot}$ $\qquad$ $B^{\cdot}\ t' = 0$

$\qquad\quad$ $A^{\cdot}\ t = 0$ $\qquad$ $H^{\cdot}$ $\qquad$ (event E_0)

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

$N^{\cdot}$ $\qquad\qquad$ $B^{\cdot}\ t' = t_1'$

$\qquad$ $A^{\cdot}$ $\qquad$ $H^{\cdot}\ t = t_1$ $\qquad$ (event E_1)

Here H is adjacent to B, H is observed to read t_1 and B to read t_1'.

$N^{\cdot}\ t' = t_2'$ $\qquad$ $B^{\cdot}$

$A^{\cdot}\ t = t_2$ $\qquad$ $H^{\cdot}$ $\qquad$ (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

$$\left.\begin{array}{l} t'_1 = at_1 \quad (1) \\ t_2 = at'_2 \quad (2) \end{array}\right\} \quad a = (1 - v^2/c^2)^{\frac{1}{2}}$$

(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 synchronises 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 synchronises 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 characterises 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 characterising a theory by its mathematics alone, he saw it as merely a wrong assignment of priorities, and entitled his chaper 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 synchronised does not synchronise 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 synchronised, 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 synchronised 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 vaccum 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 realised 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 Einstein'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 theory...' 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 realised 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.

[1] Dingle, H., *Nature*, **195**, 985 (1962).
[2] Born, M., *Nature*, **197**, 1287 (1963).
[3] Dingle, H., *Nature*, **197**, 1288 (1963).
[4] Einstein, A., *Ann. d. Phys.*, **17**, 891 (1905). English translation in *The Principle of Relativity*, by Einstein and others, 49 (Methuen, 1923).
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III

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 synchronised 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 synchronised 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 $2t'$; 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 synchronises 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.

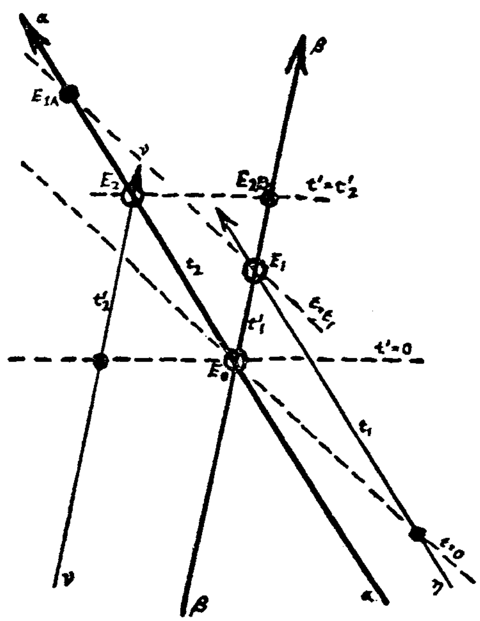
About the first thing that relativity theory does is to deny any operational meaning to the notion of simultaneity at two different places. Naturally, this fundamental feature in the theory is not affected in the slightest by any arbitrary conventions we may adopt for the synchronisation of clocks. The 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.

[1] Dingle, H., *Nature*, **195**, 985 (1962).
[2] Born, M., *Nature*, **197**, 1287 (1963).

IV

THE CASE AGAINST THE SPECIAL THEORY OF RELATIVITY

by H. Dingle

Professor McCrea's reply[1] to my disproof of special relativity[2] is both gratifying and disappointing. It is good that, at long last, some comment has appeared; regrettable that this one contains nothing to the point.

One simple thing only is needed to refute the disproof, and it is essential – to show an error in the derivation of my equation (4) that does not invalidate equation (3). This I showed with unmistakable clearness. McCrea's only contribution to it is the following: 'Dingle's (3) is meaningless. Correspondingly his (4) is meaningless.' This, if true (it is not), would merely kill the theory in another way, for (3) is Einstein's deduction and that of all his followers until now.

Because this conclusively nullifies McCrea's rejoinder, I should leave the matter here, with a final appeal to him now to agree frankly that the theory is untenable, but for the fact that the overlooking of the irrelevant bulk of his statement would, in the prevailing state of thought, be misinterpreted. *Nature's* prediction[3] that 'The chances are that most people will be persuaded by what [McCrea] has to say' would only too probably be verified. It is the general view that relativity is beyond the understanding of most, but must be accepted because some mathematicians, who alone understand it, have endorsed it: criticism of it, being on this view merely a sign of incomprehension, can therefore be ignored if a sufficiently imposing mathematical dismissal, intelligible or not, is forthcoming. Euler faced the non-mathematical sceptic, Diderot, with the challenge 'Sir, $a+b^n/n=x$, hence God exists; reply!'. Diderot did not reply and Euler's case prevailed. McCrea's statement has the same relevance and cogency as Euler's and if met with silence would produce (indeed, has produced) the same conviction. Reluctantly, therefore, I dissect it.

First, all the didactic, as distinct from the polemical, part which expounds the mathematics of the theory is superflous; I do not question it. I distinguished clearly between (*a*) the mathematics and (*b*) the identification of the mathematical symbols with observable quantities. I have enough mathematical insight to see that it is a waste of time to look for mathematical flaws in the theory. Hence McCrea's argument (i), which he says 'alone is sufficient to refute Dingle's contention', does not touch that contention. Of course, 'equations (III) and (IV) [my (1) and (2)] concern two distinct sets of events, and so they cannot contradict each other'. But what McCrea has to show, and has not shown, is why the physical result (3), deduced from one set, can be held true, while the physical result (4), similarly deduced from the other (non-contradictory) set, must be held false.

Not only do I agree that my equations (1) and (2) are mathematically free from contradiction; I agree also that it is perfectly possible (though, of course, not necessary) that if the experiment were made the clocks described would give readings conforming to (1) and (2) (in which case, as far as can be seen at present, we should have to accept Lorentz's theory). But what is impossible is that, in that case, the settings of H and N in relation to A and B, respectively, which, according to Einstein's definition, synchronise the pairs, A,H and B,N, would be such as to entitle us to infer both his reading of A for an event occurring on H (thus yielding (3)) and a similarly determined reading of B for an event occurring on N (thus yielding (4)). This I clearly stated.

McCrea's comments on this essential point of synchronisation, which alone enables us to compare the readings of separated clocks, are revealing. If Einstein's comparison were, as he says, merely one of 'two different ways of putting the readings of A,B into 1–1 correspondence with each other', the whole theory would be an idle mathematical fancy, and a space traveller would return either older or younger than his twin brother according to our capricious choice of correspondence. This, which I also clearly pointed out, McCrea ignores. We have here another equivoque of the kind which I exemplified by the oscillating interpretation of the 'FitzGerald contraction'. Either special relativity says nothing physical, or its (physical) statements are contradictory. To establish his choice regarding asymmetrical ageing, McCrea supposes that it does speak,[4] and selects arbitrarily only one of its contradictory utterances. When asked to face the other he shifts his ground and denies that the theory has said anything at all. The recognition and abjuration of this specious ambivalence are long overdue.

But the passage which shows the deepest misconception is this: '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 synchronisation of clocks.' What Einstein pointed out (and this shows his great insight into the matter) was that there was no *natural* meaning of such simultaneity; one could freely be given '*by definition*'. This released him from the previously assumed necessity to assign distant times in conformity with Galilean kinematics and set him free to base a theory on whatever operational definition he chose – the theory then, of course, being riveted to that definition and subject to test by observation. But when the observation which it requires is seen to be that one clock goes both faster and slower than another, you cannot plead that that is only because of the arbitrary definition, which does not affect the theory in the slightest. The theory is based on the definition, and if you want to regain your freedom to choose another, you must first repudiate the theory and then start again from scratch. McCrea's assertion is equivalent to saying that one's freedom to marry any eligible woman who will consent is not affected in the slightest by the fact that one is already married.

Here is Einstein's own account of the matter:[5] 'In order to give physical significance to the concept of time, processes of some kind are required which enable relations to be established between different places. It is immaterial what kind of processes one chooses for such a definition of time. It is advantageous, however, for the theory to choose only those processes concerning which we know something certain.' How it could be advantageous for the theory to be so particular about a process that does not affect it in the slightest is incomprehensible.

McCrea's remaining arguments are based on an indefensible misreading of my phrase, 'A must be held to read t_1 at E_1'. He takes this to imply that I imagined A to be at the *place* of the event E_1, which no sane person could do. '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',

he writes, and he proceeds to draw a space-time diagram that 'makes it perfectly clear that α [the world-line of A] does not go through E_1, and so there cannot possibly be a reading of A "at" E_1.' I myself had defined A as a clock permanently distant from H (on which E_1 occurs) and had so shown it in each of four drawings. I cannot believe that anyone, reading the phrase in its context, could fail to see that 'A must be held to read t_1 at E_1' meant that the reading of A, which, according to the theory, must be associated with the event E_1, is t_1. My definition of E_1 as the coincidence of H and B was accompanied by one of the drawings showing A unmistakably at a distance from that event, and, similarly, another drawing showed B at a distance from E_2. That McCrea should have found it necessary to draw a complex of world-lines which obscures for the non-specialist what my simple figures had already made clear to all can be explained only in Eulerian terms. That is not the way to get at the truth, and I am not Diderot.

I now sum up the situation by stating again what must be done to avoid my conclusion. Either my equations (3) and (4) are contradictory or they are not. If they are, at least one must be wrong, and if Einstein's (3) is right, then a false step must exist in the deduction of (4) from the commonly agreed (1) and (2) which has no repercussions on the deduction of (3): this false step must be pinpointed. If, on the other hand, (3) and (4) are not contradictory, then it must be explained why Einstein's deductions from (3) – for example, that an equatorial clock goes slower than a polar one – are true, while the similar but opposite deductions from (4) – for example, that an equatorial clock goes faster than a polar one – are not equally true. In each case, therefore, either the necessary physical implications of (3) must be vindicated and those of (4) discredited, or the theory fails. No solution which makes the equations equivalent, whether meaningful or meaningless, has any bearing on the matter.

1 McCrea, W. H., *Nature*, **216**, 122 (1967).
2 Dingle, H., *Nature*, **216**, 119 (1967).
3 *Nature*, **216**, 113 (1967).
4 McCrea, W. H., *Nature*, **179**, 909 (1957).
5 Einstein, A., *The Meaning of Relativity*, fourth ed., 27 (Methuen, 1950).