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THE ORIGIN AND PRESENT STATUS OF THE SPECIAL RELATIVITY THEORY

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INTRODUCTION

THE special theory of relativity is now so much an accepted part of physics that its origins and early history tend to be forgotten, and they are largely unknown to the younger theoretical physicists of to-day. In view of recent difficulties that have arisen in connection with the theory, and particularly in view of the fact that an alternative theory of Ritz's, which was thought to have been disproved, has now been shown to be a distinct possibility,* the occasion seems opportune for a review of the circumstances in which the theory arose and developed and for an appraisal of its present status. The purpose of this article is to supply these desiderata. The historical sketch which forms Section I is of necessity a mere outline, with attention confined to the work which now seems relevant. The guiding motive in Section II, in which Einstein's theory is brought into focus against the background of history, has been to distinguish as clearly as possible between the logical consistency of the theory on the one hand, and its relation to observation on the other. This distinction has not, I think, as yet been sharply enough drawn, with the result that the experimental evidence needed to confirm the theory has often been misunderstood. A theoretical demonstration that the theory contains no internal contradictions—that it *could* be right—has frequently been regarded as a proof that it *is* right; while experiments have been held to confirm the theory which in fact show only its compatibility with the Maxwell-Lorentz electromagnetic field equations, and not with the facts which those equations are designed to represent. The result of our enquiry will be to show that at present there is no

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* *M.N.R.A.S.*, 119, 67 (1959).

experimental evidence for or against the *kinematical* requirements of the theory, and a crucial experiment is proposed.

These Sections I believe are either statements of facts or requirements of pure reason, and therefore not controversial; they are demonstrably either right or wrong. I believe also that it is possible to show by pure reasoning that the present physical interpretation of the coordinates occurring in the Lorentz transformation leads to observations that are mutually impossible. This may be done in several ways, of which I have given one elsewhere.* In this paper, however, I do not wish to introduce controversial matters (I do not see why they should be controversial, but in fact they are), and therefore leave the question as one to be decided experimentally.

I

About the beginning of the twentieth century the fundamental equations of Newtonian mechanics were regarded as so firmly established as to be axiomatic in the original sense of the word—beyond possibility of question. Maxwell's electromagnetic theory, originally formulated for a stationary system, had been generalized by Lorentz to include systems containing moving charges, and equations had been derived which had received impressive experimental confirmation. The theory depended on the assumption of a fixed ether, filling all space, so that "moving" had the quite definite meaning, "moving with respect to the ether". In some respects, however, the predictions of the theory were not realized in experiment. These were all concerned with bodies whose velocities (v) were sufficiently large compared with the velocity of light (c) for the quantity v^2/c^2 to have a measurable effect on observation: in all such cases (particularly those involving the motion of the Earth), effects predicted were not observed. The natural explanation was that something had been overlooked, with the result that either the field equations were themselves imperfect or else their application to the conditions of the experiments was misconceived.

The first important attempt at a general solution of the problem came from Lorentz. He showed† that if it be assumed that electrons (which were already recognized as constituents of all material bodies) were contracted, when moving, in the direction of motion by a definite amount, and that if, in brief, uncharged bodies behaved like charged ones in their relations with the ether, then moving bodies would suffer such changes that it would be impossible to tell from

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* *Bull. Inst. Phys.*, 9, 314 (1958).

† *Proc. Amst. Acad.* (English edition), 6, 809 (1903).

observations made within a system whether the system was moving (with respect to the ether) or at rest. If the system were moving uniformly with a velocity less than that of light, its dimensions and the periods of its rhythmical processes would be so changed that all internal measurements would be exactly the same as though it were at rest.

The changes in question were given by the now famous *Lorentz transformation*, *viz.*:

$$x' = \frac{x - vt}{\sqrt{1 - v^2/c^2}}$$

$$y' = y$$

$$z' = z$$

$$t' = \frac{t - vx/c^2}{\sqrt{1 - v^2/c^2}}$$

Here (x, y, z) are the space coordinates, and t the time, of an event according to measuring rods and clocks stationary in the ether. (x', y', x') and t' are the space position and time of the same event according to measuring rods and clocks moving in the x-direction with uniform velocity $v < c$. Their values are different, but Lorentz showed that the concomitant changes in the values of all electric and magnetic magnitudes would be such that the same general field equations would hold good in both cases; in other words, the field equations were invariant to the Lorentz transformation.

Physically, this meant that there was a real difference between a state of rest and a state of uniform motion, but no experiment could reveal it because the effect of motion on the bodies concerned would be such that the same observations would be made in both cases. The Galilean transformation, *viz.*:

$$\begin{aligned} x' &= x - vt \\ y' &= y \\ z' &= z \\ t' &= t \end{aligned}$$

which embodied the essence of Newtonian mechanics, was fundamentally true, but would *apparently* be violated because measurements of moving bodies would not reveal their true properties. There were thus two sets of transformation equations between the space and time coordinates of systems in relative motion—one which was true but at variance with observation, and the other false but agreeing with observation. This was æsthetically unsatisfying but neither contradictory nor impossible. The world might be like that, and apparently was like that. The only alternative seemed to be that the electromagnetic field equations were wrong, and no better ones were forthcoming.

This theory was not inconsistent with a conviction that was growing in the minds of physicists at this time, that—in the words of one of them, Poincaré—" we have not, and cannot possibly have, any means of discerning whether we are, or are not, carried along in . . . a motion [of uniform translation] ". Poincaré called this the *principle of relativity*. Since we can certainly discern that two bodies, or systems of bodies, are *relatively* in motion, this principle would mean that we could share the relative motion between them in any way at all, and no observation could tell us that one choice was more " real " (it might be more convenient) than any other. The Galilean transformation was already consistent with this (Newton's first law of motion provides no distinction between a state of rest and one of uniform rectilinear motion) and if the equations of all phenomena had been necessarily invariant to the Galilean transformation, the principle of relativity would have been automatically true universally. But the conditions under which the Lorentz transformation had been established showed that, on Lorentz's theory, for electromagnetic (and therefore, through the electromagnetic theory of light, for optical) phenomena, the relativity principle was true for practical rather than fundamental reasons, and there was still the possibility that some phenomenon of a quite different kind might be discovered that would reveal the actual distinction between a stationary and a moving system. In other words, the principle of relativity expressed a fact about the moving bodies so far examined, but not necessarily a property of motion itself.

A new turn was given to the matter by Einstein in 1905.* He began by postulating that the principle of relativity was necessarily true—that motion, *by definition*, was a relation between two things and not a property of either. This meant the abandonment of the ether in the Lorentzian sense—the sense in which it had been assumed in order to generalize Maxwell's equations—for so long as there was such an ether there was a physical difference between a state of rest and a state of motion of a single body. Consequently, Einstein could not avail himself of the Lorentz " contraction " and " time dilatation ", for it was impossible, even in thought, to identify the bodies that experienced those changes. The distinction between the " real " Galilean transformation and the " observed " Lorentz transformation therefore disappeared also, and

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* *Ann. Phys.*, 17, 891 (1905).

Einstein was faced with the problem of choosing one or the other for *all* phenomena, mechanical, electromagnetic, optical, and any other that might later come to light.

He did this by analysing the meaning of time in relation to events at a distance from the standard clock used to define the measurement of time. We shall consider the theory in more detail in the next Section, but it may be said at once that, whatever may be its ultimate fate, the realization that such an analysis was necessary marks Einstein out as a man of quite exceptional insight and represents a permanent and fundamentally important contribution to physical theory. It is necessary here only to say that by first pointing out that a definition of time at a distance was necessary, and then defining it in a permissible and reasonable way, Einstein showed that (granting the definition) the Lorentz transformation was true automatically for all phenomena of whatever kind —not as a consequence of our inability to frustrate nature's attacks on our instruments, but because of the very meaning of time as he defined it. All the consequences of Lorentz's theory then followed —the electromagnetic field equations could be retained, the " contraction " and " time dilatation " corresponded to observation, and so on—but not because of the effect of motion through ether; they followed because what we call " length ", " time interval ", " mass ", etc. are not objective properties of external events or things, but the results we obtain when we perform certain operations of measurement. When A and B are in relative motion, A observes B's measuring rod to be contracted and B observes A's to be contracted, each relatively to his own. As physical effects such " contractions " are plainly contradictory. But there is nothing contradictory in the statement that the observations and calculations which A must make to determine the length of B's rod, being necessarily different from those which he employs for his own, yield a smaller " length ", while the corresponding observations which B makes yield a reciprocal result. Such an effect would be precisely similar to the " contraction " which each observes in the other when the distance between them increases. A observes B to have shrunk, and B observes A to have shrunk. We do not have to say that it is B who has really shrunk, and A only appears to B to have done so because B has had his instruments disturbed by his change of position.

Einstein's theory was therefore essentially different from Lorentz's in a physical sense, though mathematically identical with it. Of the alternatives mentioned at the beginning of this Section —that the electromagnetic equations were wrong or had been wrongly applied to observations—both chose the second. According to Lorentz, the physical effects of motion through the ether had been ignored, and, according to Einstein, the wrong meaning had been assigned to the times of distant events. The other alternative was chosen by Ritz.* He accepted the principle of relativity as a fundamental fact of nature, and accordingly discarded the ether. This, as we have seen, removed the basis on which the electromagnetic equations had been reared, and he therefore rejected those equations, pointing out, at the same time, a number of independent difficulties which their acceptance entailed. New electromagnetic equations were therefore called for, and this problem he began to investigate, but unfortunately died before the work could be completed. The chief stumbling-block of the older theory, however—the failure of all optical experiments to justify electromagnetic theory by revealing the absolute motion of the Earth—was readily explained by the assumption that light always issued from its source with the same velocity, however the source was moving, just as though it were a material particle fired by an unvarying mechanism. No change was necessary in the conception of time at a distance (*i.e.* the Galilean transformation for the *t*-coordinate was held to represent the relation between actual clock readings), and the Galilean transformation in general was held to be universally valid. In all respects this explanation was simpler than that of Lorentz or Einstein, but it had the one glaring disadvantage that it left electromagnetism without a general theory.

About the year 1909, therefore, when Ritz died, there were three theories in the field (as well, of course, as minor suggestions which came to nothing), which may be summarized as follows:

(1) *Lorentz.* A stationary ether; Maxwell-Lorentz electromagnetic equations valid; relativity of motion not fundamental but apparently so because of physical changes caused by motion through ether; Galilean transformation fundamentally true, but Lorentz transformation apparently operative because of these changes.

(2) *Einstein.* No ether; Maxwell-Lorentz equations valid; relativity of motion fundamental; Lorentz transformation universal and fundamentally true.

(3) *Ritz.* No ether; Maxwell-Lorentz equations false; relativity of motion fundamental; Galilean transformation universal and fundamentally true.

A decision between Lorentz and Einstein seemed impossible unless some totally unpredictable phenomenon should turn up which would enable an absolute distinction to be made between the motion

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* *Ann. Chim. Phys.*, **13**, 145 (1908).

of A with respect to B and that of B with respect to A. Failing this, a choice could be made only on æsthetic grounds. Accordingly the theories tended to be regarded as identical, and this is no doubt the origin of the confusion which has existed ever since as to whether the Lorentz "contraction" etc. are or are not "real physical effects."* A decision between Lorentz-Einstein on the one hand and Ritz on the other, however, was easily seen to be possible in theory, though, at that time at least, very difficult in practice. It lay in the fact that, according to Lorentz-Einstein, light from a distant source should reach an observer at velocity measured as c, no matter how the source was moving, whereas, according to Ritz, it should reach him at velocity measured as $c + v$ (vectorial addition), where v is the velocity of the source with respect to the observer. This was not then testable in the laboratory, but a few years after Ritz's death, de Sitter† pointed out that if—as he assumed Ritz's theory required—light from a component of a double star, whose velocity with respect to the Earth was oscillating between v and $-v$, travelled towards the Earth at a velocity which similarly oscillated between $c + v$ and $c - v$, the observed motion of the star would show complete confusion instead of the simple Keplerian ellipse which we observe. This was held to dispose of Ritz's theory, which has accordingly received scant attention since.

On examination, however, it becomes clear that this conclusion was at least premature. What de Sitter had shown was that light did not issue from the star at velocity c *with respect to the star* (and therefore at velocity $c \pm v$ with respect to the Earth) and thereafter maintain a velocity $c \pm v$ *with respect to the Earth.* His considerations gave no answer at all to the question whether or not light issued from the star at velocity c with respect to the star and thereafter maintained a velocity c *with respect to the star.* (Strictly speaking, Ritz's theory, like Einstein's, is concerned only with uniform velocities, and is therefore inapplicable to the orbital motion of a star. But, if that strict interpretation is pressed, these observations have no bearing at all on the matter and therefore do not disprove Ritz. If we assume them to be relevant, the only form of Ritz's theory that is at all acceptable as a law of nature is that light issues from its source at a constant velocity with respect to the source;

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* Lorentz was never fully reconciled to Einstein's theory. As late as 1921 (*Nature*, **106**, 794) he wrote: "there can be no question about the reality of this change of length", and compared it with the changes produced by temperature. This is incompatible with Einstein's theory, which denies the possibility of ascribing the change to one rather than the other of the relatively moving bodies.

† *Proc. Amst. Acad.*, **15**, 1297 (1913); *B.A.N.*, No. 64, **2**, 163 (1924).

it would be fantastic to suppose that, having once been emitted, it must keep a constant velocity with respect to an arbitrary body like the Earth.) Other proposed tests, always regarded as less crucial than this, are equally inconclusive, so we must recognize that, so far as observation is concerned, the Lorentz-Einstein and Ritz theories are equally in the field, and there is no reason on observational grounds to suppose that one rather than the other is correct.

Historically, however, as has been said, the Ritz theory was regarded as disproved, and the Lorentz-Einstein theory has been dominant ever since. In view of the uncritical way in which this supposed disproof of Ritz was accepted, it is hard not to believe that it was actually dominant before, and a study of the literature bears this out. Physicists were prepared to sacrifice almost anything rather than the electromagnetic equations, and a reason for shaking off a nuisance rather than a genuine test between equally valid possibilities was what was sought. It is impossible otherwise to account for the readiness with which such an inconclusive argument was accepted as a final disproof.

Another factor which almost certainly contributed to the acceptance of Einstein was the work of Minkowski in 1908.* What in fact Minkowski did was simply a piece of pure mathematics, contributing nothing at all to the physical problem. The Lorentz-Einstein theory had previously been presented in algebraic terms. Minkowski transformed it into geometry, and so discovered a quantity *ds*, now known as the *interval,* which remains invariant under all Lorentz transformations. So far as the relation of the theory to observation was concerned, this made no difference whatever, but the psychological effect was great. One of the chief stumbling-blocks to the acceptance of Einstein's theory was its apparent absurdity: one could not *picture* these apparent reversals of the time order of events. Minkowski made the theory conceivable. Moreover, he made it possible to accept the theory without sacrificing belief in the objectivity of nature. Space and time had " vanished to shadows ", but " space-time " appeared in their place as a physical reality. The significance of this achievement was enormously enhanced later, when Einstein made it the basis of his general relativity theory of gravitation, and the view became widespread—and still is so—that Minkowski had made an essential contribution to the *physical* theory of relativity. Even to-day we find the Minkowski description of what the theory requires presented as though its self-consistency were a proof that the theory

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* *Nach. K. Ges. Wiss. Gött.,* 1908, p. 53.

is physically true.* This makes it all the more necessary to distinguish as sharply as possible between the pure mathematics of the theory on the one hand and its relation to observation on the other, and to this we turn in the next Section.

II

At the very origin of Einstein's theory lies his realization that the " time " of an event, at a distance from the clock which is accepted as the standard for time measurement, needs to be *defined.* It had previously been assumed that it was something *given* to us by nature. But for Einstein the " time " of a distant event is not an objective physical quantity which we must discover: we ourselves choose what operations of measurement we shall perform, and give names to the results they yield. If we choose one such operation connected with the event which is so closely related to the reading of our standard clock that it seems appropriate to call its result the *time* of the event, then we can use that term; otherwise it is simply a meaningless sign.

This means—and the fact cannot be too strongly stressed—that Einstein's theory rests entirely on a freely chosen definition. All that it has to say about time, and everything related thereto, is said about time as so freely defined. This is not a defect of the theory. It is a *necessary* characteristic of every theory, and pre-relativity physics differs from the physics of to-day simply in the fact that it was unconsciously instead of consciously based on a definition.

In choosing a definition Einstein considers first of all a system of bodies all relatively at rest. He assumes that we have standard measuring rods for measuring spatial distances, and a clock which measures standard time when at rest at a point A beside us. Thus equipped, we can say without ambiguity that all bodies in the system *are* relatively at rest, when the distances between them remain the same for all readings of the clock. (The question of rotation of the system as a whole is not considered.) To speak of the time of an event at a distant point B, in a manner compatible with the already defined time at A (*viz.* the reading of the clock at A), we shall, says Einstein, " establish *by definition* that the ' time ' required by light to travel from A to B equals the ' time ' it requires to travel from B to A ". In other words, if we place at B a clock similar to the one at A, and set it so that it records the time of reception of a ray of light from A, which it immediately reflects back,

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* See, for example, H. Bondi, *Discovery,* 18, 505 (1957).

as halfway between the times by the clock at A of emission and return of the light, then the "time" of any event occurring at B will be the reading of this clock at the moment at which the event occurs. The clock at B is then said to be "synchronized" with the clock at A. B may be any point, so this definition gives a meaning to the time of an event anywhere in a system of relatively stationary bodies. If a moving body is then introduced into the system, its velocity is precisely definable as the rate at which its space position changes with the time.

We notice at once that the mention of "light" here is entirely gratuitous. No physical properties of light are called upon, and anything else would do, since we establish *by definition* that the to and fro journeys take the same time: we are not, let it be repeated, dependent on the assumption that they *do* take the same time, because that has no meaning at all until time at different places has been defined. What *is* essential, however, is that if the clock at B is synchronized with that at A, then the same process must show that the clock at A is synchronized with that at B. This does not necessarily follow if we use some particular physical agency, such as light, for the synchronization. Einstein, of course, recognizes this, and states that he "assumes" it to be true for light. If (to use terms which are not permissible at this stage, but which do help us to realize the situation) the to and fro velocities of light were not the same, then this reciprocity of synchronization would not hold, and only confusion would result from defining the times of distant events in this way. The essence of the definition, therefore—and indeed of any definition that is to stand any chance at all of being useful—is that we must use some messenger (let us call it X) which does make synchronization mutual. This is the only restriction that at this stage we need place on X. According to our present knowledge, either light or sound, for example, would do equally well, and would give exactly the same settings for all the clocks in the system within the range in which their use was practicable.

The next step is to compare two systems of relatively stationary bodies which are moving uniformly with respect to one another. Within each of them we can time every event that occurs: the problem is to compare the times of the same event in the two systems. (It is assumed, of course, that spatially they completely overlap, so that every event may be considered as occurring in both.) We first obtain a point of reference by supposing that the original clocks in the systems—at the points A and A′, say—both read zero at an instant at which they are at the same place. Thereafter they separate at uniform velocity v along a definite direction which we will call the x-axis, and for simplicity we consider only events at points on that axis. We then require the time of an event at such a point, B, in each of the two systems.

To solve this problem we must specify X more precisely. We must state the relation between the X emitted from A and that emitted from A′ at their moment of coincidence. Among the many possibilities, two stand out as obviously simpler than all others. First, we can specify that X shall move with respect to its source as though it were a shot fired from a gun. In that case, the X from A′, which is moving towards B, would reach B before the X from A; and it follows (we omit calculations in order to concentrate on the principles; they are not a subject of controversy) that the times of events have the same measure in both systems. This is equivalent to Ritz's theory and to the Galilean transformation. The alternative is that the two beams, from A and A′, travel together towards B, forming a single beam. This, in fact, would be the case if X were a sound wave in still air, for the speed of sound through air is determined solely by the properties of air, no matter how its source is moving. The two beams would then reach B at the same instant, but the reflected beam would reach A′ before it reached A, since A′ would be travelling to meet it. The mean of the times of emission and return would therefore be earlier for A′ than for A, and therefore, by definition, the time of the event which is the arrival of the joint beam at B would be earlier for the A′ than for the A system. In other words, the clock of the A′ system situated at B at the event in question must be set earlier than the corresponding clock of the A system, and so the time interval between the emission of X and its arrival at B would be shorter in the A′ than in the A system. This is equivalent to Einstein's theory.

Now X is still at our choice. We are merely forming definitions, and we are equally free to make X behave like a gun-shot or like a sound wave, or, indeed, like anything else we can precisely imagine and describe. Einstein in fact identified X with light, and then he had to *assume* (there was no experimental evidence on the point at the time, and there is still none) that light behaves in this respect like sound. We can free the theory from such assumptions by *defining* time in terms of X and *defining* X as something whose relation to its source is that which Einstein *assumed* to be true for light. In this way we make the theory invulnerable to observation, and logically impeccable. The physical problem is then to find the relation between time as so defined and the observations we make with clocks and light, or whatever else we choose to experiment with, and the theory will become of physical importance if that relation is a simple one. For the moment, however, we consider the concepts alone.

It is now a matter of pure mathematics (provided that one principle, to be mentioned presently, be granted) to show that the relation between the times and positions of events in our two relatively moving systems is that given by the Lorentz transformation. If the position and time of an event are (x, y, z) and t in the A system and (x', y', z') and t' in the A′ system, if v is the velocity of the A′ system, and V that of X, both with respect to the A system, then the Lorentz transformation equations *must* be true. With our definitions this is a logical necessity, but it controls only the quantities we have defined, not necessarily anything observed in measurements with ordinary instruments.

The principle which must be granted is the *principle of relativity*, which asserts that the motion of A′ with respect to A shall be in all respects (apart, of course, from conventional differences of sign) identical with the motion of A with respect to A′. This can be shown to require that V is the same for both systems, so that for a beam of X along the x-axis, for example, no matter from what body it is emitted, $dx/dt = dx'/dt' = V$. The principle of relativity is, in a sense, an empirical principle, but it is not an *assumption* in the sense in which Einstein's postulate that two beams of *light* from A and A′ would travel together through space is an assumption. The latter, but not the former, could be proved or disproved by a single experiment. The principle of relativity is empirical in that it sums up the universal failure to observe any physical phenomenon by which a definite velocity can be assigned to a single body irrespective of other bodies. It could, however, be regarded as a definition. We could *define* motion as a property of a body relative to something else—as in practice it always is—and designate any intrinsic property of a single body that might later be discovered by some other name. Certainly velocity as here defined is meaningless without the postulation of a standard of reference, A or A′. Nevertheless, it is not a logical necessity, apart from the principle of relativity, that V shall have the same value in both systems, so the principle of relativity must be accepted as a necessary postulate, provided the other definitions are accepted, in order to justify the Lorentz transformation on logical grounds.

This completes the structure of the theory as a mathematical system. Any event is specifiable by four "coordinates", x, y, z, t, of which (x, y, z) specify its location with respect to a standard position—the origin of coordinates—and a standard spatial framework, and t specifies its time of occurrence, defined in terms of the standard clock at the origin and the messenger X. The relation between the coordinates in two systems in uniform relative motion is that given by the Lorentz transformation. We can now bring this ideal system into relation with observation.

We know by experience that an event can be uniquely located by three space measurements with standard measuring rods, and one time measurement. The first step is therefore to identify (x, y, z) with the space measurements and t with the time measurement. So long as we consider only one system, this raises no problem at all. We can choose the *coordinates* (x, y, z) as we wish (provided, of course, that they satisfy certain trivial conditions, such as being single-valued for each event, and so on), so we can without question choose them to be identical with the readings of our measuring rods when set out along the axes in the ordinary familiar way. Similarly, we can identify t with the readings of clocks distributed in the system, provided that we synchronize such clocks by some travelling agent whose velocity, when the clocks have been so synchronized, is found to be the same in opposite directions, and which is reflected instantaneously. We have no direct experimental evidence that this condition is satisfied by light, though we believe we have for sound. However, it is highly unlikely that light differs from sound in this respect, and we may take it as compatible with all our existing knowledge to identify the t coordinate with clock readings when the clocks are synchronized by sound or by light signals.

It is when we compare systems in relative motion that the real test comes. To take the simplest case, suppose our clock, A′, which agrees with A at zero reading when it is coincident with A, moves at constant velocity, v, along the x-axis to a point B, distant r from A, and suppose that at this point there is a clock, stationary with respect to A, which has been synchronized with A by X-signals. Then, according to the Lorentz transformation, the reading of this clock when A′ reaches it (*i.e.* the t coordinate of this event in the A system) will be r/v, while the reading of A′ at this event (the t' coordinate in the A′ system) will be r/v multiplied by $\sqrt{1 - v^2/c^2}$. This can be tested experimentally, provided that we can find a messenger having the properties assigned to X.

In Einstein's theory, X is light, and this, as we have seen, involves the *assumption* that the passage of a beam of light through space is independent of the motion of its source. But in order to understand clearly the relation between the theory, as an axiomatic system independent of possibly false assumptions, on the one hand, and observation on the other, let us take X to be sound, which we know satisfies all the conditions imposed on X. If, then, we suppose all our observations to be made in still air, of unchanging properties, and all the velocities to be less than V, the velocity of sound, we shall get a perfectly accurate idea of the relation between the theory and observation (apart, of course, from the numerical magnitudes involved).

Let us suppose, then, that we have two very long straight rods, ABC and A′B′C′, relatively at rest side by side, A being coincident with A′, B with B′, and C with C′. Suppose B and B′ are at the mid-points of the respective rods. Each rod has clocks distributed at frequent intervals along its length, and these clocks are synchronized with the clocks at B and B′, respectively, by sound signals. At the moment when the B and B′ clocks read zero, the rod A′B′C′ starts to move with uniform velocity, v, in the direction B ⟶ C, and for convenience we will thereafter call ABC "stationary" and A′B′C′ "moving". As soon as the velocity has become uniform (we assume the period of acceleration to be brief and negligible) we resume the synchronization test for the clocks along A′B′C′, and make whatever adjustments are necessary to synchronize all the others with the clock at B′, leaving that to behave naturally, without interference. It will then follow logically, from the accepted properties of sound signals, that adjustments will be necessary, and that when they are made the adjusted clocks will not agree with those along ABC which they successively encounter. Those between B′ and C′ will show an earlier time, and those between B′ and A′ a later time, than the neighbouring stationary clocks. This is not a fact about nature. It is not a statement concerning the natural behaviour of clocks, but simply a statement about their readings when we adjust them to accord with our definition of synchronization.

But we cannot conclude anything about the clock at B′ from pure reasoning from the properties of X alone. It does not follow at all that B′ will be found to lag behind the clocks which it passes between B and C. Why, then, does the Lorentz transformation imply that B′ will appear to run slow? Simply because of the principle of relativity. If it did not, we should be able to say, not merely for convenience but as an observable fact, that A′B′C′ was moving and ABC stationary, for B′ would agree with the clocks between B and C, but B would be behind those between B′ and A′. If, on the other hand, we had set ABC moving in the direction B′ ⟶ A′, producing the same *relative* motion, B′ would be behind the clocks between B and C, while B would agree with those between B′ and A′. The two descriptions of the same relative motion would thus be observationally distinguishable. And, in fact, in the case of sound they would be so distinguishable, because we have tacitly assumed that the air remains at rest with respect to ABC in the first case and with respect to A′B′C′ in the second, so the cases are physically different. In Einstein's theory there is no ether to bear the same relation to light as air does to sound, so there is no possible way of making the two descriptions of the motion equivalent except by making the moving clock B′ run slow. The Lorentz transformation, since the principle of relativity was embedded in its derivation, automatically provides for this.

Einstein's theory, then, depending, as he clearly pointed out, on two postulates—the postulate of relativity and the postulate that light behaves like X—can be tested by two experiments: (i) an experiment to determine the velocity of light in relation to that of its source; (ii) an experiment to determine if a moving clock runs slow. The position may be summarized by considering what we should have to infer in each of the four eventualities which these experiments make possible.

Case 1a. Light from relatively moving sources travels as a single beam, and a moving clock runs slow (by the required amount, of course). In this case light can be identified with X, the principle of relativity is (really or apparently) satisfied, and the coordinate t in the Lorentz transformation represents the readings of synchronized clocks stationary in the system considered. In short, the Einstein-Lorentz theory is completely justified.

Case 1b. Light from relatively moving sources travels as a single beam, and stationary and moving clocks run at the same rate. In this case light can be identified with X, but the principle of relativity is violated. The Lorentz transformation is inapplicable to observation with any interpretation of t, and motion with respect to an " ether " becomes measurable. Light is kinematically identical with sound. In view of the failure of all experiments to detect the motion of the Earth, this case is scarcely conceivable.

Case 2a. Light from relatively moving sources travels as a double beam, each component having velocity c with respect to its source, and a moving clock runs slow. This case is impossible, since the synchronization process would ensure that moving and stationary clocks agreed. Of course, in one sense anything imaginable is a possible result of experiment, but if this case were realized we should have to give up the idea of light as a travelling agent and account otherwise for the experience of sight. In the present state of knowledge this contingency may be ignored.

Case 2b. Light from relatively moving sources travels as a double beam, and stationary and moving clocks run at the same rate. In this case light cannot be identified with X, and the Lorentz transformation becomes a purely artificial construct. The Galilean transformation is universally valid, and the principle of relativity is satisfied. Ritz's theory is completely justified.

For all practical purposes, then, the choice lies between Einstein and Ritz, and at present there is not a scrap of experimental evidence even to make one appear more probable than the other. This is not generally realized, partly because it has been thought that Ritz's hypothesis has been disproved, and partly because Einstein's hypothesis has been supported by a number of observations which have been thought to admit of no other explanation. But these observations, when examined, turn out to depend for their interpretation on the Maxwell-Lorentz electromagnetic equations. Our only practicable means of producing and measuring velocities high enough to decide between the Galilean and the Lorentz transformations lies in the use of electromagnetic fields, and the accepted field equations are expressed in terms of coordinates (x, y, z, t) which we know obey the Lorentz transformation. Consequently, when we find by experiment that these coordinates obey the Lorentz transformation and not the Galilean transformation, we are simply confirming experimentally what the mathematics already requires. In other words, the t coordinates in the electromagnetic equations and in the Lorentz transformation are identical, but that throws no light at all on the question whether they are identical with actual physical clock readings, and that is the question in which we are interested.

To illustrate this by one example, take the well-known experiments in which the mass of a body is found to increase with its velocity. The body—an electrically charged particle—is accelerated in an electric field and subjected to a deflecting force. The deflection, for a constant force, is found to decrease with increase of speed of the particle more rapidly than the assumption of an invariant mass would require, and the particle behaves, in fact, as if its mass at velocity v were $m/\sqrt{1 - v^2/c^2}$. Now this is just what the laws of conservation of mass and momentum would require if space and time measurements followed the Lorentz and not the Galilean transformation. Consequently, it is concluded that the Lorentz transformation represents the actual relations between space and time measurements.

But what does the quantity v represent here? Not the velocity measured by rods and clocks, but the quantity dx/dt, where x and t are coordinates occurring in the electromagnetic equations; and what the experiments actually show is that, for a particular experimental arrangement, the momentum of the particle is $mv/\sqrt{1-v^2/c^2}$, where v is thus defined. Now if we suppose that the coordinate t is identical with standard clock readings, then v is velocity in the ordinary meaning of the word, and the quantity which is to be multiplied by the velocity to give the momentum (*i.e.* by definition the mass of the particle) is $m/\sqrt{1-v^2/c^2}$: in that case mass is definitely a function of velocity. But if we suppose that the Galilean transformation is valid, and accordingly that m is invariant, then the quantity which is to be multiplied by the mass to give the momentum (*i.e.* by definition the velocity of the particle) is $v/\sqrt{1-v^2/c^2}$. These experiments afford no evidence whatever as to which choice is to be made: they simply show—superfluously—how the electromagnetic field equations, which are invariant to the Lorentz transformation, require mass to behave if the equations of mechanics also are to be invariant to the Lorentz transformation.

If, indeed, velocities (w) measured by rods and light-synchronized clocks are related to velocities (v) defined by dx/dt by the equation $w = v/\sqrt{1-v^2/c^2}$, we get a simple interpretation of the invariant ds discovered by Minkowski; it is simply ordinary clock time interval.* From the point of view of physical measurements we can then say that the t coordinate of the electromagnetic equations is a function of space and time measurements—*i.e.* a sort of " space-time " measurement—and that Minkowski, thinking he had discovered " space-time ", had actually disinterred pure time from its burial in this hybrid. In that case the electromagnetic equations might be entirely valid, provided the t coordinate is recognized as merely a coordinate, and it might be more convenient to continue to use that coordinate for the purposes of electromagnetism rather than to re-express the equations in terms of a coordinate representing clock readings directly. On the other hand, Ritz may have been right in holding that the equations are more fundamentally defective than this.

This is not the place to anatomize electromagnetic theory. It need only be remarked that the *experimental basis* of that theory involves only measurements in which the velocities are so low that the Galilean and Lorentz transformations yield results which are observationally indistinguishable. Also, as has already been pointed out, both Maxwell and Lorentz assumed a stationary ether in order to derive the equations, and they have not been derived otherwise. We no longer believe in the stationary ether, but we retain the

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* See *Bull. Inst. Phys., loc. cit.*

equations—hanging, as it were, in mid-air. There is no evidence whatever to show whether velocity, as measured by rods and light-synchronized clocks, is to be represented by v or by w, or indeed by any other expression sufficiently close to v for small velocities.

Similarly, all experiments in which an atom is regarded as a clock are powerless to answer our question. An atom is a clock only by virtue of its interpretation in terms of electromagnetic theory. We never observe an atom as a periodic system. We receive light in which we observe a periodicity, but the assignment of that periodicity to the atom is a purely theoretical matter, depending on the wave equation obtained from the electromagnetic equations. (On the quantum theory, incidentally, what we call the "frequency" of light is regarded as energy. That conception affords no passage at all to a "clock" as the source of the light.) Hence we should again expect that if we make that assignment we shall arrive at a "clock" whose behaviour is invariant to the Lorentz transformation. This is confirmed by such experiments as that of Ives and Stilwell, which are often cited as a proof of "time dilatation". They prove nothing except the invariance of the electromagnetic equations to the Lorentz transformation.

We can see in an interesting way how irrelevant is the Doppler effect—the "frequency" phenomenon most often advanced as demonstrating the "reality" of time dilatation—to the actual question, by returning to our example of sound. We have seen that when A′B′C′ moves, its clocks must be resynchronized because sound waves are legitimate examples of X. Are we to conclude that they have changed their rate because of the motion? We know very well that we are not: the change in this case is necessary because sound has the properties assigned to X, and an observer stationed at B would not expect to see, and would not see, any change at all in the A′B′C′ clocks if the resynchronization were not artificially brought about. But if he regarded the sources of sound in the A′B′C′ system as clocks, and deduced their frequency from the sound radiations he received from them, he *would* discover a change in them. The Doppler effect would tell him that the receding clocks were slowed down.

We can see the fallacy easily enough in this case. The frequency which has changed is the frequency of B's reception of the sound waves, not the frequency of vibration of their sources. With light we have much less knowledge of the relation between the atom and its radiation, and we cannot therefore so indubitably derive a relation between their characteristics. But this example shows clearly enough that it is quite wrong to identify observed radiation frequencies with " frequencies " of vibrations in the source of radiation. If we do so, and interpret the former in terms of equations invariant to the Lorentz transformation, then the latter also will inevitably be found invariant to the Lorentz transformation. We have proved nothing at all.

The present situation, therefore, is that the issue between Einstein and Ritz is quite open. We have not sufficient evidence on observational grounds to give one even a greater or less probability than the other: we await the verdict of experiment. Since the two theories differ in their requirements concerning both the experiments mentioned earlier, it would be sufficient to perform one of them, and the simpler would probably be the comparison of the velocity of light, with respect to the laboratory, from relatively moving sources, *taking care, to avoid de Sitter's erroneous deduction from the double star observations, that the sources did not change their motion during the passage of the light.* It is true that there is a theoretical possibility that either Case 1b or 2a corresponds to fact, but this is so remote that an experiment with moving clocks could be dispensed with unless some quite unexpected difficulty should arise. It is greatly to be hoped that the crucial experiment will be undertaken without delay if it is practically possible.