THE CONSTANCY OF THE VELOCITY OF LIGHT

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Summary

The principle of the constancy of the velocity of light has generally been given a prominent place in discussions of the restricted theory of relativity. Yet lack of any clear and unambiguous statement of the principle has led to its frequently being misunderstood and to its sometimes being adduced as a basis for fallacious conclusions.

The principle is discussed from a historical point of view, is carefully analysed, and is stated in a form which seems free from ambiguity. In the context of the restricted theory the " velocity of light " must always be interpreted as " the velocity of light measured in an inertial reference system " and it must be understood that this measured value is the average speed measured over a go-and-return path. The principle can then be stated in the form : " The principle of relativity precludes any possibility of ascertaining how light is propagated relative to any inertial reference system. Measurements made in inertial reference systems, using the methods of measurement prescribed by the restricted theory, always give the same value c for the speed of light irrespective of the direction of its propagation and irrespective of the motion of its source."

Fallacious inferences about the propagation of light, arising out of an incorrect definition of " relative velocity " and out of the incorrect appellation of the relativistic law of transformation of velocities as the " law of addition of velocities " are discussed ; and it is shown that the ballistic theory of light is quite incompatible with the restricted theory.

Finally, it is pointed out that the measurement procedures prescribed by the restricted theory are conventional ; failure to recognize this has led many exponents of the restricted theory to assert, without sufficient justification, that these procedures demand a complete revision of the older concepts of space and time.

I. Introduction

The " principle of the constancy of the velocity of light " has generally been given a prominent position in expositions of the restricted theory of relativity. Following the lead given by Einstein in his early papers (1905, 1907), it appears frequently as one of the fundamental postulates of the theory. In most other cases it is presented as a consequence of the Lorentz transformations, when these have been otherwise established.

In spite of this, it does not seem possible to derive from the literature a statement of the principle that is clear and unambiguous and that would command universal approval. Indeed, it seems apparent that it is taken by various authors to mean quite different things. The consequent vagueness as to the exact significance of the principle leaves it open to quite improper applications. This is so not only for laymen and philosophers but even for scientists themselves.

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The present investigation shows that misapplication of the principle has led to some quite erroneous inferences. It will be seen that the inherent difficulty that has to be dealt with lies in the translation of the precise mathematical statement of the principle, taken together with its proper context, into a verbal statement which is not open to misinterpretation. The difficulty is therefore similar to, but far more complex than, that which led to the occurrence of the " clock paradox " of the restricted theory of relativity, and which was resolved by showing that the mathematical definition of the " rate " of a moving clock had been translated into a verbal statement lacking the precision necessary to prevent its being misapplied (Builder 1957).

To avoid any confusion in the ensuing discussion, it is necessary to state precisely what we will mean by the *restricted theory of relativity*. There is only one statement of this theory that can command universal assent, that is subject to experimental verification, and that is equally appropriate whether one ascribes the theory to Poincaré and Lorentz or to Einstein, namely,

> " The restricted theory of relativity is the theory that the spatial and temporal coordinates of events, measured in any one inertial reference system, are related to the spatial and temporal coordinates of the same events, as measured in any other inertial reference system, by the Lorentz transformations."

Every prediction of the theory, thus defined, that has yet been tested, has been verified.

Thus defined, the theory is a verifiable statement about measurements made in inertial reference systems. It does not offer any causal explanation for the validity of the Lorentz transformations as relations between measurements made in different systems; it is derivable, as Einstein showed, from two postulates which are verifiable statements about the characteristics of natural phenomena, and the validity of this derivation is completely independent of any hypotheses or theories which might be held to give a causal explanation of these characteristics.

The theory was developed independently by Poincaré and Lorentz (Whittaker 1953) and by Einstein (1905). The points of view adopted in these two lines of development were so markedly different* that it will be necessary here to consider both.

II. Silberstein's Statement of the Principle

To illustrate the nature of our problem we take, as an example, the form of statement of the principle given by Silberstein (1922) and typical of a number of serious and responsible authors.

* This applies more particularly to Einstein's early papers. His views tended to approach those of Poincaré and Lorentz somewhat more closely with the passage of years (Builder 1958); but we are here more concerned with his early views because these have been so widely quoted and adopted by exponents of relativity theory that they may be said to have dominated the now-current attitudes to the subject.

Silberstein's Statement

> "Light is propagated *in vacuo*, relatively to any inertial reference system, with a velocity c, constant and equal for all directions, no matter whether the source emitting it is fixed or moving with respect to that system."

He goes on to say: "This is shortly referred to as uniform and isotropic light propagation in any inertial system."

To appreciate the spectacular character of this statement, it is necessary to recall that the Maxwell-Lorentz theory supposed that light is propagated in a universal stationary medium (ether) with a velocity c characteristic of the medium and independent of the motion of its source. On this theory it would seem to be incontrovertible that the propagation of the light, relative to any inertial system in motion relative to the ether, would be non-uniform and anisotropic, i.e. it would be propagated relative to the inertial system with different speeds in different directions. This inference fits in with our commonsense view of motion.

Silberstein's statement explicitly denies this anisotropy of light propagation relative to any inertial system.

Is this what Silberstein meant to convey by his statement? This question can be answered definitely in the affirmative by considering how he applied the principle in discussing the ether hypothesis. His argument (Silberstein 1924) can be set out in the form:

(*a*) The restricted theory of relativity requires acceptance of the principle of the constancy of the velocity of light.

(*b*) According to the Maxwell-Lorentz theory it is necessary to postulate the existence of a universal, all-pervading, stationary ether to account for the phenomena of electrodynamics and, in particular, to account for the fact that the propagation of light is independent of the motion of its source.

(*c*) Yet according to the principle, as stated above, light is propagated isotropically with the same constant speed c, irrespective of the motion of its source, relative to every inertial reference system.

(*d*) Hence, if it is necessary in the Maxwell-Lorentz theory to postulate the existence of an ether, it is equally necessary to postulate a similar ether associated with every one of the infinity of possible inertial reference systems.

(*e*) This is clearly absurd and inconceivable, so that the ether concept breaks down and must be discarded completely.

This argument can be briefly summarized by the following quotation from Sommerfeld (1952, p. 235):

> "In the earlier but long since discarded theory of the universal *ether*, the independence of the light wave from the state of motion of the emitting body was readily understood . . . The constancy of velocity of light is today the only valid *remnant of the ether concept*. If at present we should speak of an ether, we would have to assign a separate ether to every frame of reference, i.e. speak e.g. of a primed and an unprimed ether. We now regard Lenard's 'absolute ether' merely as a freak . . ."

The same argument seems clearly to be implied by the numerous authors who have stated that the Michelson-Morley experiment proved the ether hypothesis to be wrong; it is difficult to envisage any other argument that would lead to this conclusion.

These arguments demonstrate clearly that Silberstein's statement of the principle was understood by him, and by others, to be a substantial statement about the propagation of light relative to each and every inertial system *in exactly the same sense* that the Maxwell-Lorentz theory claimed to be a substantial statement about the propagation of light relative to one particular inertial system at rest in a universal stationary ether. If this were not so, step (*d*) of the foregoing argument would fail.

Silberstein's statement, thus interpreted, is indeed revolutionary in its implications. It confounds all our commonsense and physical notions and denies to us any comprehensible physical picture of the nature and behaviour of light.

Must we then accept it as a correct and necessary consequence of relativity theory? Before doing so we must at least examine it with great care.

III. Logical Objections to Silberstein's Statement

There are serious logical objections to Silberstein's statement if it is taken in the literal sense indicated by his own and other applications of it in discussion of the ether hypothesis. In this sense it is a statement about the propagation of light.

The principle of the constancy of the velocity of light can only be justified by means of the restricted theory of relativity. Such justification might be achieved in one of two ways. On the one hand, the *principle* may be regarded as an essential postulate of the theory and the success of the theory may be regarded as evidence for the validity of this postulate. On the other hand, the *principle* may be regarded as an inference from, and a consequence of, the restricted theory. In either case, the justification rests on the validity of the restricted theory.

Now it has been pointed out, in Section I above, that the only statement of the restricted theory that can command universal assent, and that is subject to direct experimental verification, is a statement about measurements made in inertial reference systems.

Thus, so far as it is based on the validity of the restricted theory, the principle can only be a statement about measurements made in inertial reference systems.

Any further inference about the propagation of light must therefore necessarily depend on the introduction of some additional theory or hypothesis which satisfactorily relates such measurements to the physical phenomenon of light propagation. Thus, to ascertain whether Silberstein's statement about the propagation of light is justifiable, it is necessary first to analyse the measurement techniques and procedures themselves and then to investigate how the measurement results so obtained can be used to provide information about the propagation of light.

A detailed analysis of this sort is presented in the following sections of the present paper and it is shown that Silberstein's statement, taken literally, cannot be justified. On the contrary, it is shown that the *principle*, as a statement about measurements based on the restricted theory, taken together with the only available physical theory of the propagation of light, i.e. the Maxwell-Lorentz theory, leads to quite different conclusions.

It is also worth noting that, in any case, it is fairly obvious that Silberstein's statement could not be a substantial statement about the behaviour of light in *exactly the same sense* as would the corresponding statement of the Maxwell-Lorentz theory. This theory claimed to give a description of the behaviour of light about which all observers would agree, after having made appropriate measurements and after having made appropriate corrections for the effects on their measurements of their own motions. In this, the Maxwell-Lorentz theory itself provided the necessary link between the measurements and the phenomena under investigation.

The character of Silberstein's statement is quite different. It asserts that light is propagated isotropically with speed c relative to any and every inertial reference system. But this is not a statement that can command universal assent, for the following reasons.

Consider any arbitrarily selected inertial reference system S. According to the restricted theory, measurements made by observers in S of the speed of light will all give the value c, irrespective of the direction of propagation of the light and irrespective of the motion of its source. These measurement results would therefore be compatible with the S-observers supposing that light is propagated isotropically relative to their system ; but they would not be proof of such isotropy without an additional hypothesis according to which the measurement results are direct evidence of physical isotropy.

Consider now a second inertial reference system S' in motion relative to S with speed v. Measurements by observers in S' will also give always the value c for the velocity of light relative to their system, and would be compatible with their also supposing the light to be propagated isotropically in their system. However, their measurements of the velocity of light relative to the system S would give values ranging between the limits $c+v$ and $c-v$, depending on the direction of the light propagation relative to the direction of the observed motion of S. These measurements would be compatible with the anisotropy of propagation of the light relative to S.

Now, if one attempted to persuade the S'-observers that the light is in fact propagated isotropically relative to S, with the constant velocity c, they could reconcile this with their own measurements only by making an appropriate correction for their own motion relative to S. This correction would, however, result also in their corrected measurements giving different values for the velocity of light in different directions relative to their own system.

It is therefore difficult to see any way in which the S'-observers could be persuaded that their measurements are compatible with the same rays of light being propagated isotropically relative both to their own system S' and to the system S. Thus predictions of *the restricted theory can only show that the measurements made in each inertial reference system of the velocity of light are compatible with isotropic propagation of light relative to that system and anisotropic propagation of light relative to every other such system.*

The only possible objection to the foregoing argument is that it is at fault in stating that, according to the measurements of the S'-observers, the speed of light relative to the system S will vary between the limits $c+v$ and $c-v$, depending on the direction of propagation of the light. The basis for such an objection would be that, in calculating the speed of the light relative to S, the S' observers should have used the so-called " relativistic law of addition of velocities ". It is, however, shown in Section VII below that this objection must be rejected because the application of the so-called law in such a calculation is not permissible.

It is therefore concluded that Silberstein's statement cannot command universal assent and cannot therefore be a substantial statement about the physical behaviour of light in exactly the same sense as the corresponding statement of the Maxwell-Lorentz theory. Thus step (d) in Silberstein's argument, as set out in Section II above, cannot be sustained.

IV. Poincaré and Lorentz

The Maxwell-Lorentz theory envisaged, and indeed required,* the existence of a universal stationary ether as the bearer of electromagnetic fields and as the medium of propagation of disturbances of such fields with a definite velocity c characteristic of the ether and independent of the motion of the source of the disturbance.

The principle of the constancy of the velocity of light could therefore be stated in terms of Maxwell-Lorentz theory in the form :

Statement according to the Maxwell-Lorentz theory

> According to the Maxwell-Lorentz theory light is propagated isotropically in a universal homogeneous isotropic and stationary medium in a manner uniquely determined by the properties of the medium, and therefore independent of the direction of propagation and of the motion of the source of the light.

This is obviously a substantial statement about the physical characteristics and nature of light. Thus it was to be expected that an observer in uniform motion relative to the ether would be able to detect his motion by detecting the anisotropy of the propagation of light relative to him, even if the light source were moving with him.

* The Maxwell-Lorentz theory, as such, is stated explicitly in terms of such an ether. The equations of this theory, in which the velocities are *defined* as velocities relative to the ether, must therefore be distinguished sharply from the relativistic equations of electrodynamics, which are identical in form, but in which the velocities are defined as velocities measured in the particular inertial reference system being used. The corresponding distinction between the Fitzgerald-Lorentz contraction, defined as a contraction caused by motion relative to the ether, and the observable relativistic length contraction, is also desirable. Whether the ether hypothesis remains essential in a causal relativistic description of electrodynamical phenomena is a wider question with which I have dealt elsewhere (Builder 1958) ; but whatever the final answer to this question, these distinctions remain desirable.

This expectation was, of course, refuted by the Michelson-Morley experiment and by all similar experiments which followed it, thus preserving the principle of relativity of uniform motions which Poincaré (1904) restated in the form :

> "The principle of relativity according to which the laws of physical phenomena should be the same, whether for an observer fixed, or carried along in a uniform motion of translation, so that we have not and could not have any means of discerning whether or not we are carried along in such a motion."

For brevity this will here be referred to as the *principle of relativity* since the present context naturally excludes any wider connotation of this term.

The task faced by Poincaré and Lorentz was therefore the reconciliation of :

I. The principle of relativity.

II. The principle of the constancy of the velocity of light, according to the Maxwell-Lorentz theory, as stated above.

These may therefore be regarded as the postulates from which they derived the restricted theory.

Neither postulate could very well be discarded. The experimental evidence in favour of the principle of relativity was overwhelming. On the other hand, the Maxwell-Lorentz theory had, in all other respects, proved to be a wholly successful description of all the available empirical data relating to light and electrodynamics. There was no available alternative. The ballistic theory of Ritz (1908) is incompatible with these data ; in particular it is incompatible with the fact, now securely established experimentally, that the velocity of light is independent of the motion of its source. This last is further discussed in Section VIII below.

Poincaré and Lorentz, by work extending over the period 1892–1904, succeeded in establishing the restricted theory of relativity and thus reconciling the two postulates given above ; a detailed account of this development is given by Whittaker (1953) and need not detain us here. In achieving this they did not need to modify the concept of the ether in the Maxwell-Lorentz theory nor did they need to modify the principle of the constancy of the velocity of light according to that theory. The ether hypothesis was not only retained ; it also provided the basis for a causal explanation for the fact that measurements made in different inertial reference systems are related by the Lorentz transformations.

The hypothesis, put forward independently by Lorentz (1892) and by Fitzgerald (see Lodge 1893), that bodies in motion relative to the ether with speed v are contracted, in the direction of their motion, by the factor $\sqrt{(1-v^2/c^2)}$, was sufficient by itself to account for the negative result of the Michelson-Morley experiment. Moreover, it entailed* the consequence that clocks in motion

* I have pointed out elsewhere (Builder 1958) that the Fitzgerald-Lorentz contraction and the clock-rate reduction are not two independent hypotheses. This seems first to have been shown by Larmor (1900) and can readily be illustrated by considering a simple clock consisting of a rigid rod fitted with reflectors at each end so that a ray of light will be propagated backwards and forwards along the length of the rod. It can be shown that, if the rod suffers the Fitzgerald-Lorentz contraction when moving, the frequency with which the light traverses the go-and-return path along the rod will be reduced by the factor $\sqrt{(1-v^2/c^2)}$.

relative to the ether with speed v should also suffer a reduction in rate by the factor $\sqrt{(1-v^2/c^2)}$.

The Fitzgerald-Lorentz contraction and the entailed clock-rate reduction, taken together, are sufficient to explain the fact that measurements made in different inertial reference systems can be related by the Lorentz transformations; it was only necessary to recognize also that the "local time" of Lorentz, which he had originally devised as a mathematical trick to achieve covariance of the Maxwell-Lorentz form of equations to the Lorentz transformations, is the only "time" that can be established in an inertial reference system moving with unknown velocity through the ether. Poincaré recognized this in 1904* and in the same paper prescribed the now well-known relativistic method of synchronization of clocks in any inertial reference system.

The Poincaré-Lorentz postulates I and II themselves *entail the impossibility of ever discovering how light is propagated relative to any inertial reference system.* In other words, *they preclude the possibility of making any measurements that could reveal whether or not light is propagated isotropically relative to any particular inertial reference system, or of revealing the degree of anisotropy.* This is obvious: propagation of the light in the ether is isotropic (postulate II) but the detection of anisotropy in the propagation relative to an inertial reference system would reveal the motion of the system relative to the ether in contravention of the principle of relativity (postulate I).

More specifically, this entails the impossibility of measuring the velocity of light relative to any inertial reference system by determining the time taken for a light signal to travel over a unidirectional path from one point in the system to another, i.e. the impossibility of measuring the unidirectional velocity of light over a one-way path in the system. For were such measurements possible they would reveal any anisotropy of light propagation relative to the system in contravention of the principle of relativity.

Let us suppose that observers in an inertial reference system were to attempt such measurements. They would have to establish the facilities necessary to measure the time taken for a light signal to travel over a measured distance from one point A in the system to another point B. This would require having at A and B clocks known to be synchronous.† To achieve such synchronization it would be necessary to relate the readings of the clock at A to the readings of the clock at B by some signalling method. The fastest available signal is a

* Minkowski (1908) was incorrect in ascribing the first recognition of this to Einstein. On the other hand Einstein (1907) stated, as quoted in Section VI below, that this recognition was all that was essential to solve the basic problem.

† It is sometimes erroneously stated that measurements of light velocity over a unidirectional path can be made in a terrestrial laboratory without this provision, e.g. by the use of a pair of toothed wheels running synchronously on a common shaft. This is fallacious. It presupposes that the common shaft is a guarantee of synchronization. This is not so. The shaft cannot be set in rotation by torques applied synchronously at its two ends without having available two synchronized clocks to ensure that the application of the torques is synchronous. If it is set in rotation by a torque at one point, the time required for transmission of the torque along the rod will upset the synchronization by an amount which could not be ascertained unless synchronized clocks were available. This remains true however short the shaft may be.

flash of light; thus it would be necessary to allow for the time of transmission of the light signal from point A to point B, or vice versa. To make this allowance it would be necessary to know the velocity of light, in each direction, relative to the system. *Thus the clocks cannot be synchronized unless the unidirectional velocity of light relative to the system is known, and the unidirectional velocity of light relative to the system cannot be measured without synchronizing the clocks.* It can be shown that all other methods of synchronization, e.g. by slow transport of clocks from one place to another, are subject to the same limitations.

It clearly follows that when we speak, as we often do, of the "velocity of light measured in an inertial reference system" we cannot be speaking of a measurement of the unidirectional velocity; nor can we be speaking of a measurement which contains any information about the propagation of light relative to the system or which can be used as a basis for any substantial statement about the propagation of light relative to the system.

What then are we to understand by "the velocity of light measured in an inertial reference system"? *It means the average velocity of a light signal propagated over a go-and-return path.*

This average velocity is obviously measurable. This requires only the use of a single clock, located at the point A of emission of a light signal, to measure the time taken for the signal to reach a distant point B in the system and to return to the point A after reflection at B. Knowing the time taken and the total distance travelled, the average velocity can be calculated immediately.

Since this average velocity is measurable in any inertial reference system, the principle of relativity (postulate I) requires that its measured value must be the same for all directions in any one system and must have the same value c in all such systems as in the ether. Otherwise such measurements, e.g. using the Michelson-Morley experiment, would enable the motion of the system, relative to the ether, to be detected, in contravention of the principle of relativity.

This is the principle of the constancy of velocity of light of the restricted theory. To avoid confusion it should be referred to more explicitly as *the principle of the constancy of the measured average value of the velocity of light over go-and-return paths*, and it should be stated in some such form as the following:

> The principle of relativity precludes any possibility of ascertaining how light is propagated relative to any inertial reference system. The only measurements possible are measurements of the average speed of light over a go-and-return path and these always give the same value c irrespective of the direction of transmission of the light and irrespective of the motion of its source.

To this it may perhaps be objected that observers in an inertial reference system can in fact make measurements of the speed of light propagated over a one-way path in their system when once they have synchronized their clocks by the method specified by Poincaré (1904) and by Einstein (1905) and that, if they do so, they must, in accordance with the principle of relativity, always obtain the value c.

This objection cannot be sustained. Although, in such a measurement procedure, the light is propagated over a one-way path from one point A in the system to another point B, the result is still a measurement of the average velocity over a go-and-return path from A to B because of the procedure, necessarily involved in the measurements, of synchronizing the clocks at A and B.

The prescribed method of synchronizing the two clocks is as follows. Let a ray of light be emitted from A when the clock there reads t_A; let it reach B when the clock there reads t_B and let it be then reflected back to reach A again when the clock there reads t'_A. The two clocks are synchronized if

$$t_B - t_A = t'_A - t_B.$$

In other words, the clocks are to be set so that measurements made by these clocks will give the same time for transmission from A *to* B *as from* B *to* A. Thus, according to these clocks, the time taken for light to travel in either direction will be equal to the time it would take if it were propagated at a speed equal to its average speed from A to B and from B to A.

This method of setting the clocks is a *convention* which leads to a *conventional measure* of the velocity of light from A to B or from B to A. This conventional aspect of the restricted theory is discussed in Section IX below. Thus, in the context of the restricted theory, the *velocity of light* must be interpreted as a conventional reference to the measured average value of the speed of the light over a go-and-return path.

In this context, measurements of the " velocity of light " clearly lack any simple and direct relation to the physical propagation of light relative to the inertial reference system in which the measurements are made. If we do wish to infer from such measurements anything about the propagation of light relative to an inertial reference system, the only tenable physical theory at our disposal is that of Maxwell and Lorentz. And it has been shown above that the results of such measurements are predicted by, and are therefore compatible with, this theory.

V. Einstein (1905)

It can readily be shown that the conclusions reached in the last section are completely consistent with the expositions of the restricted theory given by Einstein (1905, 1907).

In Section 1 of his 1905 paper, Einstein sets out the following statements about the synchronization of two clocks A and B at rest at different points in an inertial reference system:

(i) " A common time for A and B . . . cannot be defined at all unless we establish *by definition** that the ' time ' required for light to travel from A to B is equal to the ' time ' it requires to travel from B to A."

(ii) "Let a ray of light start at the 'A-time' t_A from A towards B, let it at the 'B-time' t_B be reflected at B in the direction of A, and arrive again at A at the 'A-time' t'_A.

In accordance with definition the two clocks synchronize if

$$t_B - t_A = t'_A - t_B."$$

* The italics are Einstein's.

(iii) " In agreement with experience we further assume the quantity

$$\frac{2AB}{t'_A - t_A} = c$$

to be a universal constant—the velocity of light in free space."

His first statement (i) expresses the conclusion reached in Section IV that clocks can be synchronized by light signals in each inertial system only if we accept a conventional *definition*, that the " time " required for light to travel from A to B is the same as that required for it to travel from B to A. This is equivalent to Poincaré's assertion (1904) that the " local time " of Lorentz is the only time that can be established in an inertial reference system and, like it, is convenient because the principle of relativity precludes our ever discovering how light is propagated relative to the system.

His second statement (ii) prescribes the method of synchronization, previously given by Poincaré in 1904, for establishing the " local time " of Lorentz in the system.

His third statement (iii) declares explicitly that the quantity c *is equal to the average velocity of light over the go-and-return path from* A *to* B *and back to* A, i.e. it is twice the distance AB divided by the time $t_A - t'_A$, measured on a single clock, for the go-and-return transmission. Moreover, it declares that *in agreement with experience*, i.e. the results of the Michelson-Morley and other experiments, this quantity c is a universal constant. This is in precise agreement with the conclusion reached in Section IV that the principle of relativity precludes our ascertaining how light is propagated relative to an inertial reference system but requires that the average velocity of the light, measured over a go-and-return path, shall have the same value c in all such systems.

Statement (iii) concludes with a parenthetical phrase identifying the universal constant c with the " *velocity of light in empty space* ". The meaning of this is by no means clear. It might, on the one hand, be taken to be a definition of the " velocity of light in empty space " in any inertial reference system ; if this is so, it is clear that this definition precludes any possibility of this " velocity " implying any information about the anisotropy of light propagation relative to such a system. It might, on the other hand, be taken as an identification of the quantity c with the velocity of light in the ether according to the Maxwell-Lorentz theory ; but this interpretation seems to be excluded by Einstein's claim, in the introductory paragraphs of the same paper, that the concept of the ether is superfluous.*

Thus Einstein's statements (i), (ii), (iii) are completely compatible with, and may even be held to express concisely, the conclusions reached in Section IV above. In particular, they are clearly compatible with the statement there given of the principle of constancy of light velocity and the corresponding interpretation of the " velocity of light " in the context of the restricted theory.

* Though he did not long adhere to this view (Builder 1958), the fact that he did indeed hold it in 1905 is confirmed by his more emphatic statement in his 1907 paper.

Yet, in Section 2 of his paper, Einstein immediately, without any further definition of terms, sets out his two postulates, which he refers to as the *principle of relativity* and the *principle of the constancy of the velocity of light*, as follows :

I. " The laws by which the states of physical systems undergo change are not affected, whether these changes of state be referred to the one or the other of two systems of coordinates in uniform translatory motion."

II. " Any ray of light moves in the ' stationary ' system of coordinates with the determined velocity c, whether the ray be emitted by a stationary or by a moving body. Hence

$$\text{velocity}=\frac{\text{light path}}{\text{time interval}}$$

where the time interval is to be taken in the sense of the definition of Section 1."

The " stationary " system he had defined in Section 1 as any arbitrarily selected inertial reference system.

Taken literally, the statement of postulate II is, like that of Silberstein, a statement about *how light moves* relatively to an arbitrarily selected inertial reference system. Taken thus, it asserts unequivocally that light *is propagated* isotropically relative to such a system.* This is undoubtedly how it has been interpreted by Silberstein and others, as discussed in Section II above.

Yet the context shows that this literal interpretation is quite untenable because it is wholly incompatible with the statements (i), (ii), (iii) quoted above from Section 1 of his paper. The possibility of making any assertion about how " light moves " in the system had been specifically denied in statement (i). The " velocity c " of light had been defined in statement (iii) as the average speed over a go-and-return path.

Thus, in the full context, we can only properly interpret postulate II as a statement in which the " *determined velocity* c " is that defined by statement (iii). We must therefore infer that the statement that " *light moves* " with the " *determined velocity* c " can be interpreted only to mean that measurements of the velocity of light rays, made by observers in the system, utilizing the conventions prescribed in statements (i) and (ii), must always give the same value c " *whether the ray be emitted by a stationary or by a moving body* ".

* It is important to note that, taken literally, the postulate would be indistinguishable from the *principle of constancy of light velocity according to the Maxwell-Lorentz theory* if one supposed the " stationary " system to be at rest in the ether. The " time interval ", " taken in the sense of the definition of Section 1 ", would then be identical with the " absolute " time of a system at rest in the ether.

Nor would Einstein's derivation of the restricted theory be in any way affected by such an identification *because the definition of the stationary system does not enter into, or affect, his utilization of the postulate.*

It is indeed difficult to avoid the feeling that Einstein's use of " stationary " was an unconscious reflection of the fixed ether of the Maxwell-Lorentz theory. That this may have been the case is to some extend supported by the introductory paragraphs of his 1907 paper.

Thus interpreted, postulate II becomes, once again, a statement about measurements, and not a statement about the propagation of light relative to the system. And it expresses the conclusion, required by the principle of relativity of postulate I, that such measurements must fail to reveal any anisotropy of light propagation relative to the system. Once again, any inference from the postulate about the propagation of light relative to the system would require a further physical theory about the relation of the measurements to the phenomenon of propagation. Such a theory would lie outside the context of the restricted theory as it was defined in Section I; in any case, the only tenable theory available is that of Maxwell and Lorentz.

VI. Einstein (1907)

The conclusions reached in the last two sections are further supported by reference to Einstein's second comprehensive paper on the restricted theory, published in 1907.

In the introduction to this paper he again specifically rejected the ether hypothesis. Moreover, he rejected the Fitzgerald-Lorentz contraction hypothesis as an *ad hoc* assumption and an artificial device to rescue the Maxwell-Lorentz theory from the results of the Michelson-Morley experiment. He then states:

> "However, it turned out surprisingly that it was only necessary to define sufficiently precisely the concept of time to overcome these difficulties. It required only the recognition that the auxiliary 'local time' of Lorentz can be defined simply as the 'time' . . . The Fitzgerald-Lorentz hypothesis then appears as a necessary consequence of the theory."

I have quoted this statement in full partly because of its importance in the assessment of the historical significance of Poincaré's recognition of this in 1904, as discussed in Section IV above.

It is, however, also important as contextual background for his subsequent treatment of the problem of clock synchronization. He assumes an inertial reference system to be equipped with ideal standard clocks and, without further discussion, simply states:

> "We now assume that the clocks can be so adjusted that the velocity of propagation of a light ray in vacuum—measured with the aid of these clocks—will everywhere be equal to a universal constant c.
>
> If A and B are two points fixed in the coordinate system . . ., whose separation is r, and if t_A is the reading of clock A when a light ray is emitted in the direction AB, and t_B the reading of the clock B on arrival of the light signal then, irrespective of the motion of the light source,
>
> $$r/(t_B - t_A) = c.$$
>
> That the assumption here made, which we will call the 'principle of the constancy of the velocity of light' should be satisfied in nature is by no means obvious, yet this . . . is made plausible by the confirmation it has been given by experiments."

This restatement by Einstein confirms entirely the interpretation, given in Section V above, of his 1905 paper.

The principle, as he now states it, is simply " *that the velocity of propagation of a light ray—measured with the aid of these clocks—will everywhere be a universal constant* c " and it is here clearly given as a *prescription of how the clocks shall be set.*

It has thus clearly become a statement about a conventional procedure of measurement. Any simple and direct inferences about the propagation of light relative to inertial reference systems have been omitted and have, in fact, been clearly precluded by the context.

VII. The Relativistic Law of Transformation of Velocities

The principle of the constancy of the velocity of light can be inferred from the relativistic law of transformation of velocities. This law is itself simply derivable from the Lorentz transformations and provides a relation between the velocity of a thing as measured in any one inertial reference system S and the velocity of the same thing as measured in any other inertial reference system S'. In particular, if the measured value of the velocity of light in the system S is c, the transformation gives the same value c for its velocity measured in the system S'. Thus the principle of the constancy of the velocity of light appears once again as a statement about measurements made in inertial reference systems, in agreement with the conclusions reached in previous sections.

Nevertheless, it is necessary to discuss the law in some detail here because it has sometimes been misapplied and has thereby led to some erroneous inferences which might be presented as objections to the conclusions reached in previous sections. Such inferences have arisen out of a serious ambiguity in the meaning of the term *relative velocity.* It is therefore necessary to set out here a definition of this term (to be referred to as the *old* definition) and to show that an alternative definition sometimes used (the *new* definition) is untenable.

The velocity of a thing is its velocity stated in terms of the coordinates of some specified inertial reference system S. We will refer to it as *the velocity of the thing measured in* S or, more briefly, as *the velocity of the thing in* S. This definition is not to be taken to imply the existence of any physical system corresponding to the inertial reference system S. On the contrary, the system S is to be understood primarily as a system of coordinates in the mathematical sense, and the phrase " measured in S " is to be understood primarily to mean " expressed in terms of the measures, i.e. coordinates, of S ". It is true that the actual measurement of such a velocity implies the use of some physical system corresponding to at least one inertial reference system but, once this measurement has been made, the velocity in any other inertial reference system may be calculated by utilizing the relativistic law of transformation.

The relative velocity of two things is the velocity of one thing A relative to another thing B, measured in some specified inertial reference system S as the simple vector difference $\mathbf{u}_a - \mathbf{u}_b$ between the velocities $\mathbf{u}_a$ and $\mathbf{u}_b$ of the two things in S (*old definition*).

Thus a relative velocity measured in S is a simple vector relation between the velocities in S of two real things such as bodies, light quanta, light rays, etc., and is to be calculated by the parallelogram or vector law of addition. Thus any statement that implies such a relation is essentially a statement about relative velocities. To illustrate this, consider the postulate that " the velocity of light is independent of the motion of its source ". This is a statement of relation between the velocity of light and the velocity of its source, both measured in the one inertial reference system. It could obviously be restated, explicitly in terms of relative velocities, in the clumsy but equivalent form : " If a ray of light is propagated in any direction $\hat{\mathbf{i}}$ in S, after being emitted from a source moving with velocity $\mathbf{v}$ in S, its velocity relative to its source, as measured in S, has the value $c\hat{\mathbf{i}}-\mathbf{v}$, whatever the value of $\mathbf{v}$ and whatever the direction $\hat{\mathbf{i}}$."

Now, if one of the things B, referred to in the definition, is at rest in S, so that $\mathbf{u}_b=0$, the velocity of A relative to B, measured in S, is $\mathbf{u}_a-\mathbf{u}_b=\mathbf{u}_a$ and is equal to the velocity $\mathbf{u}_a$ of A in S. It would therefore seem that one could, if one wished, speak of the velocity $\mathbf{u}_a$ of A in S as *the velocity of* A *relative to* S *measured in* S, though it would be a rather superfluous and rather clumsy way of referring to what is already sufficiently and concisely described as *the velocity of* A *in* S. It might, however, properly be objected that the concept of relative velocity is essentially one of relation between the motions of two real things, whereas the inertial reference system S does not necessarily imply the existence of any physical system or of any real thing at rest in S.

However this may be, it will be shown that *it is certainly not permissible* to refer to the velocity of A in S as *the* velocity of A relative to S or as *the* velocity of A relative to some thing at rest in S. To do so would be to utilize a *new* definition of relative velocity which will be shown to be untenable. This *new definition* would state that *the velocity of* A *relative to* B *is the velocity of* A *in the rest system of* B, *i.e. expressed in terms of the measures of the inertial reference system in which* B *is, at least momentarily, at rest.* It is to be noted that this would exclude from the concept of relative velocity the corresponding relation between the velocities of A and B measured in any other inertial reference system. In other words, it would imply that the only significant relation between the velocities of bodies is the relation between their velocities in the rest system of one of them.

The consequences of using this *new* definition can be illustrated thus. According to it, the velocity of light relative to its source is its velocity measured in the rest system of the source. The velocity of light is c in every inertial reference system. Therefore the velocity of light relative to its source is c, and this would remain so irrespective of changes in the motion of the source. We would then have, as a *consequence* of the restricted theory, the statement that " the velocity of light relative to its source is c irrespective of the motion of the source ", while we have, as a *postulate* of the theory, the statement that " the velocity of light is independent of the motion of its source ", and we have shown above that this can only mean that the velocity of light relative to its source depends on the motion of its source.

This apparent contradiction is of course due to the fact that different definitions of relative velocity have been used in the two statements. To avoid such contradictions we must make a choice between these definitions. *There are grave objections to the new definition.*

In the first place, it should be noted that in the context of Newtonian relativity the *new* and *old* definitions would always result in the same value for the relative velocity, simply because the measured value of the relative velocity would be the same for all inertial reference systems ; it might therefore be thought that there would be no essential difference between the two definitions. But this is not so. For, even in this context, the *new* definition conceptually contravenes a principle which is basic in the formulation of the laws and equations of physics, i.e. *that in every statement of a physical law, in every physical equation, and in every description of natural phenomena, all the quantities referred to must be stated in terms of the measures of one and the same reference system.* The necessity for this principle was in no way affected by the fact that the laws and equations of Newtonian mechanics retained the same form in every inertial reference system ; indeed this covariance itself depends on all the quantities specified in the laws and equations being measured in the particular reference system being used. Thus the new definition is conceptually inadmissible in the context of Newtonian relativity even though it would not lead to obvious contradictions.

But in the context of the restricted theory the relations between velocities are no longer independent of the reference system. Thus any departure from the principle of stating these relations in terms of the measures of the single reference system implied in any physical statement, whether it be a law or an equation or a description of phenomena, can only lead to chaos. The *new* definition clearly contravenes this principle in the worst possible way in that it leads to physical statements in terms of the measures of an unspecified multiplicity of unspecified inertial reference systems, e.g. to a statement about the velocity of light relative to its source, measured in all the rest systems of a source of which the motion is completely unrestricted and unspecified.

In the second place, it is to be noted that the rejection of the *old* definition and the adoption of the *new* would be abortive. To illustrate this, consider once again the postulate that " the velocity of light is independent of the motion of its source ". This is a description, in terms of the measures of any one arbitrarily selected reference system, of a physical relation which could be expressed by saying that " the motion of light is independent of the motion of its source ". If we were to adopt the *new* definition and reject the old, we would then have to formulate a description, in terms of the *new* definition, corresponding to this physical relation. Since the only statement of relation that is admissible according to the *new* definition is the relation measured in the rest system of the source, we would be forced to the statement that " the velocity of light relative to its source always has the same value c, irrespective of the motion of its source " ; but this statement fails to characterize uniquely the postulated physical relation, for it would remain true even if the emission were ballistic, i.e. if the motion of the light were determined by the motion of its source.

We thus conclude that the *new* definition must be rejected and the *old* definition retained in the context of the restricted theory.

Thus we must continue to calculate relative velocities by the simple vector law, i.e. the parallelogram law, of addition of velocities, even though we must replace this law by the relativistic law of transformation for the transformation of velocities from the measures of one inertial reference system to the measures of another.

Thus the claim, frequently made, that the parallelogram law of addition of velocities has been replaced by the relativistic law of transformation is true *only* in regard to transformations of velocities from the measures of one inertial reference system to the measures of another. It is not true in regard to calculations of relative velocities. It is perhaps worth also making the obvious, but not trivial, remark that it is certainly not true of the resolution and composition of velocities which is the basis of all mathematical formulations of kinematics; in spite of the restricted theory we continue freely and without embarrassment to use the parallelogram law to resolve velocities into their tangential and normal components, to resolve them into their Cartesian components, to add such components, or to add the velocities of two or more simple harmonic motions, and so on.

Thus the practice of referring to the relativistic law of transformation of velocities as the "*relativistic law of addition* (*or composition*) *of velocities*", although originated by Einstein (1905, Section 5, "The Composition of Velocities"; 1907), and, although adopted by many authors such as Whittaker (1953) and Sommerfeld (e.g. 1952), is a misnomer which is both pointless and dangerously misleading. It is pointless because it is, at best, an inaccurate description of what is in fact a law of transformation. It is dangerous because it may be taken to imply (as it was meant to imply) that the law is to be used for purposes other than transformation. The *new* definition of relative velocity, discussed above, was also suggested in Section 5 of Einstein's 1905 paper when he, apparently quite casually, referred in effect to *velocity in* S as *the velocity relative to* S.

The appalling confusion to which this has led can be well illustrated by a further example. Eddington, who was precise and careful in his formal presentations of relativity theory, made the following statement in a popular exposition (1928).

> "A feature of the relativity theory which seems to have aroused special interest among philosophers is the absoluteness of the velocity of light. In general velocity is relative . . . But it is a curious fact that if I speak of a velocity of 299,796 kilometres per second it is unnecessary to add the explanatory phrase. Relative to what? Relative to any and every star or particle of matter in the universe."

It is in fact necessary to add a number of explanatory phrases, i.e. to avoid this statement being taken seriously by laymen and philosophers or even by physicists.

The fallacy is obvious. The argument leading to the statement must take the form: The old parallelogram law has been replaced by the new relativistic law of addition of velocities. Therefore, to calculate the velocity of light relative to any star or relative to any particle of matter, we must use the relativistic law of addition, i.e. we must calculate the relative velocity as measured in the momentary rest system of the star or particle. When we do this we always obtain the value c irrespective of how the stars or particles are moving and irrespective of how their motions are changing.

The statement must therefore be rejected as being based on an untenable definition of relative velocity. If we translate it into proper physical terms it becomes trivial and uninteresting, for all it amounts to is that, if we were to measure the velocity of light in the rest system of a star or particle, we would obtain the value c, just as we would in any other inertial reference system.

The discussion in this section also demonstrates the fallacy in the possible objection mentioned in the penultimate paragraph of Section III.

VIII. The Ballistic Theory of Light

We have now to consider a serious misapplication of the relativistic law of transformation of velocities which, in some recent discussions of the ballistic theory of light, has led to an absurd conclusion.

In the ballistic theory it is postulated that light is emitted with a definite velocity relative to its source. Whittaker (1953, p. 38) treats the corpuscular theory as being synonymous with the ballistic theory. He states that, according to the corpuscular theory,

> " the corpuscles emitted by a moving star would have a velocity which is compounded of the velocity of the star and the velocity of the light relative to a source at rest, just as an object thrown from a carriage window in a moving railway train has a velocity which is obtained by compounding its velocity relative to the carriage with the velocity of the train (the *ballistic* theory)."

There is, of course, no *a priori* reason why light thus emitted ballistically should consist of simple corpuscles totally devoid of wave-like characteristics. An analogy, between the wave-like characteristics of electrons and the corpuscular-like characteristics of light quanta, has suggested to some authors the possibility that light quanta might be emitted ballistically ; but the general acceptance of the restricted theory of relativity has made it necessary for them to consider this possibility in the context of that theory.

The great interest in the ballistic hypothesis at the beginning of this century, which led to the remarkable attempt by Ritz to develop a complete ballistic theory of electrodynamics, arose out of the fact that the ballistic hypothesis is compatible with the negative result of the Michelson-Morley experiment and would, if it were tenable, provide a complete and satisfactory explanation of that result within the context of simple Newtonian relativity.

It is in fact easy to show that, in the strict logical sense, the ballistic theory of light and the principle of relativity, taken together, are *equivalent* to simple Newtonian relativity. Taken together, they *entail* this. Moreover, simple Newtonian relativity would *entail* the principle of relativity and would also *entail* the ballistic theory of light.

On the other hand, it has frequently been shown that the principle that the velocity of light is independent of the motion of its source and the principle of relativity, taken together, are *equivalent* to the relativity* of the restricted

* I here distinguish the relativity of the restricted theory from the restricted theory of relativity, in accordance with the conclusions reached in Section IX below.

theory. Taken together they *entail* this. Moreover, the relativity of the restricted theory *entails* the principle of relativity and also *entails* the principle that the velocity of light is independent of the motion of its source.

These statements are indisputable. Since the relativity of the restricted theory is incompatible with simple Newtonian relativity, it follows necessarily that *the ballistic theory is incompatible with the relativity of the restricted theory.*

This same conclusion would also follow directly from a statement that the ballistic theory is incompatible with the principle that the velocity of light is independent of the motion of its source. This statement is certainly entailed by the definitions of the ballistic theory and of the principle. Any doubt about this could arise only out of the ambiguity in the definition of relative velocity discussed in Section VII above.

The experimental evidence against the ballistic theory is quite overwhelming. Strong experimental evidence has led to general acceptance of the restricted theory of relativity and this entails acceptance of the principle that the velocity of light is independent of the motion of its source. Experimental evidence has also rendered untenable any theory of electrodynamics based on the ballistic hypothesis. Finally, it has now been well established directly by astronomical data, presented by de Sitter, Comstock, and others, as well as by terrestrial experiments, that the velocity of light is in fact independent of the motion of its source ; the wealth of such evidence has been summarized briefly by Whittaker (1953).

In spite of all this, Matthias, Whittaker, and Sommerfeld have recently claimed that a ballistic theory of light emission is reconcilable with the restricted theory of relativity.

The logical incompatibility of the ballistic hypothesis and the restricted theory, demonstrated above, is sufficient justification for rejecting this claim. Yet the matter is one of such fundamental significance that it seems necessary here to analyse the claims and demonstrate the fallacies in the arguments put forward.

Whittaker (1953), having summarized the strong experimental evidence against the ballistic hypothesis, continues as follows :

> " It was now recognised that these observational findings, which in the nineteenth century might have been supposed to tell in favour of the wave theory, were actually without significance one way or the other . . . For, according to relativity theory, even on a corpuscular hypothesis, a corpuscle which had a velocity c relative to its source would have the same velocity c relative to any observer, whether he shared in the motion of the source or not."

The fallacy in this is obvious. In the context of the restricted theory, the statement that a corpuscle has a velocity c relative to its source has no definite meaning unless the reference system, in which this is claimed to be true, is specified. If it is supposed that the instantaneous rest system of the particle is implied, the statement is insufficient to entail that the emission be ballistic, for the statement is equally true of emission in accordance with the principle that the velocity of light is independent of the motion of its source. Thus the inference from this statement, that the emitted light has the same velocity c relative to any inertial system of reference, tells us nothing about the ballistic theory.

Furthermore, if it is held that the statement is meant to imply that the corpuscle has the velocity c relative to its source as measured in any and every inertial system, this is in direct contradiction to all the experimental evidence which shows, for example, that in the inertial reference system used by astronomers the velocity of light does not have the velocity c relative to its source but has the velocity c in that inertial system. This contradiction could be avoided only by claiming that the astronomers should calculate the velocity of the light relative to its source by the " relativistic law of addition of velocities " ; but it has been shown in Section VII that this would give the velocity of the light relative to its source as measured in the rest system of the source and not as measured in the reference system being used by the astronomers.

Sommerfeld's case depends directly on such a misapplication of the law of transformation of velocities. He writes (1952),

> " The fact that Newton's emission theory could, in a sense, experience a resurrection in the present theory of light quanta rests solely on the addition theorem of the theory of relativity according to which $c+v=c$ (c=velocity of light quanta, v=velocity of emitting body)."

The fallacy here has been exposed in the previous paragraph and in Section VII.

The argument of Matthias (1939), though put forward in considerable detail, is essentially the same as that of Whittaker and must be rejected for the same reasons.

Thus any claim that the ballistic theory could be reconciled with the relativity of the restricted theory is absurd and must be rejected vigorously.

IX. Conventional Aspects of the Restricted Theory

In Sections III–V above it has been suggested that the method of synchronizing clocks specified in the restricted theory is a convention. Whether or not this is true is a question of considerable interest and may even perhaps be of fundamental importance from the point of view of critical philosophy.

The obvious objection to this suggestion is that the restricted theory does in fact lead to some remarkable conclusions about the characteristics of physical phenomena, although it is derivable from only two basic postulates. For example, it leads to the conclusion that energy and inertial mass are equivalent. How then can one regard the basic measurement procedures of the theory as conventional when the powers of physical prediction of the theory are so great ?

It is first of all necessary to distinguish between the restricted theory itself, in the sense defined in Section I, and the physical theories of dynamics and electrodynamics which have been formulated in terms of the measures prescribed by the restricted theory. At the same time it must be recognized that the restricted theory itself does in fact also imply some of the physical characteristics of nature ; it entails that nature is such that we cannot ever, by observations of dynamical or electrodynamical phenomena, measure absolute velocity ; it entails also that the propagation of light is independent of the motion of its source.

Starting from the other end, we find that the task faced by Poincaré and Lorentz, and by Einstein, was the reconciliation of the two empirical generalizations, the principle of relativity and the principle that the velocity of light is independent of the motion of its source. The essential factor in effecting this reconciliation was the recognition that we cannot, by observations of dynamical and electrodynamical phenomena, achieve measurements corresponding directly to the concepts of absolute time and of absolute space (Einstein 1907). This recognition was, by itself, sufficient to demonstrate that the two principles were not necessarily incompatible, for any measurement of absolute uniform velocity, in contravention of the principle of relativity, would necessarily require absolute measurements of space and time.

Having, by this recognition, shown that there is no fundamental incompatibility between the two principles, it remained necessary to decide what convention should then be adopted in physical measurements. Some such convention was of course necessary once it was recognized that the unique measurements implied in the absolute concepts were impossible.

The most desirable convention seemed obviously to be that of synchronizing the clocks in each inertial reference system by means of light signals or by some other means which would permit a system time to be established uniquely without need to refer at all to any other possible reference systems. This was the convention adopted in the restricted theory. In particular, the procedure adopted was that of setting the clocks to the local time of Lorentz ;* this had

* It has been previously pointed out by Ives (e.g. 1951) and by Grünbaum (1955) that the adoption of this particular procedure also involves a further conventional choice. The condition for synchronization of clocks in the restricted theory may be written, using the nomenclature of Sections IV and V above, in the form $t_B=t_A+\varepsilon(t'_A-t_A)$, providing that ε has the value $\frac{1}{2}$.

Grünbaum has pointed out (1955) that " no fact of nature found in the objective relations of physical events precludes our choosing a value of ε between 0 and 1 which differs from $\frac{1}{2}$ " and, after a critical discussion of possible objections to this statement, he concludes that " the value of $\varepsilon=\frac{1}{2}$ is simpler only in the *descriptive* sense of providing a *symbolically* simpler representation of these data ".

Similarly, Ives (1951) wrote : " A point of great importance may here be noted. It is that we do not need to assign individual values to c_o and c_b (i.e. the velocities of light in the outward and backward directions on a moving platform), such for instance as calling them equal as is done in Einstein's arbitrary ' definition ' of simultaneity. We carry these quantities as real although undetermined quantities . . . " This view was based, not on a logical analysis of the restricted theory as such, but on a careful analysis made in the course of his own critical and independent investigation, in terms of the ether hypothesis, of measurements that are possible in inertial reference systems. His generalized transformations cover the general case in which ε is unspecified, and they reduce to the Lorentz transformations for $\varepsilon=\frac{1}{2}$.

Ives's work is valuable, and it is important in the present context. It demonstrates that the restricted theory, as defined in Section I above, can be rigorously established on the basis of the ether hypothesis and, at the same time, draws attention strongly to the conventional character of the choice $\varepsilon=\frac{1}{2}$ in the Poincaré-Lorentz development of the theory.

Ives's work resulted from his rejection of the restricted theory. It is significant that this rejection was due to the fact that he, like many others, had been misled into believing that the principle of the constancy of the velocity of light, according to Silberstein's statement and interpretation as given in Section II above, is an essential feature of the restricted theory ; unlike most others, he stoutly maintained the view that this was " not merely ' un-understandable ', it is *not* supported by objective matters of fact ; it is untenable, and . . . unnecessary ". This view is in agreement with the conclusions reached in the investigation presented here.

the inestimable advantage that the equations of physics then had precisely the same form in every inertial reference system.

But this was not the only possible choice. It would also have been quite possible to have adopted the convention of referring all measurements to one particular inertial reference system such as that in which the solar system is at rest, i.e. of arbitrarily selecting this system as a " conventionally absolute " system replacing in practical measurements the elusive " absolute " system of the Maxwell-Lorentz theory.

There could be no logical objection to such a convention. Nor could there be any fundamental physical objection; the laws of electrodynamics had in fact been established in this system on the tacit, but probably mistaken, assumption that it was at rest in absolute space. Moreover, there would be no fundamental difficulty in referring all measurements to this conventionally absolute system. It had of course to be recognized that, in systems in conventionally absolute uniform motion, the measuring devices, such as rods and clocks, would be affected by the motion in accordance with the hypothesis of Fitzgerald and Lorentz and the entailed slowing down of clocks in motion (and in agreement with the predictions of the restricted theory).

There would in fact have been considerable conceptual advantages in such a convention. The relativity of simultaneity and the reciprocity of the relativistic variations of the restricted theory would not have obtruded, simply because the spatial and temporal coordinates of events in various inertial reference systems would not have been of conceptual interest. It would also have resulted in a description of the universe in terms of the measures of only one reference system and this would have avoided difficulties of definition such as that discussed in Section VII above.

Indeed, it can well be argued that such a convention would have been the obvious and sensible transition from the then-current ideas of absolute spatial and temporal measurements. For, as was shown elsewhere (Builder 1958), the concepts of absolute space and time remained essential in our physical description of the universe, while the fact that the absolute reference system could not be identified by observations of dynamical and electrodynamical phenomena (principle of relativity) could not in principle preclude its identification by purely geometrical and kinematical measurements; it has in fact always been believed, and with good reason, that the absolute velocity of the solar system, i.e. its velocity relative to the universe as a whole, must be very small indeed compared with the velocity of light.

The critical question to be answered is this: Would the adoption of such a convention have precluded the physical discoveries that are credited to the restricted theory?

It is clear that it could not have precluded such discoveries. The restricted theory itself, as defined in Section I, is a purely deductive inference from two postulates which are themselves statements, generalized from experience, of the physical characteristics of natural phenomena. The particular form of statement chosen for this inference, based on a particular convention about the synchronization of clocks, added to the postulates nothing more about the physical characteristics of natural phenomena. Thus the restricted theory, by itself, could entail nothing about the characteristics of natural phenomena that was not already entailed by the postulates. Similarly, the restricted theory itself, taken together with the laws of electrodynamics and of dynamics, as modified by the postulates, could entail nothing about the characteristics of natural phenomena that was not already entailed by the postulates taken together with these laws.

It follows that the physical consequences deduced by means of the restricted theory could, from a purely logical viewpoint, have been equally well deduced using some alternative convention such as that suggested above. There is of course little doubt that the road to the discovery of these consequences would then have been longer and more arduous ; no one would be inclined to dispute the elegance and effectiveness of the restricted theory formulation. We are, however, concerned here with assessing the fundamental character of the theory and not with assessing its elegance and its practical advantages.

We are, moreover, much concerned with the conceptual difficulties that have been caused by exponents of the restricted theory who have failed to recognize its conventional character and who have consequently asserted that its conventions demand a complete revision of our fundamental concepts of space and time.

Such exponents of the restricted theory will no doubt consider the point of view presented here as being, like the Fitzgerald-Lorentz hypothesis,* nothing but an " artful " device to rescue old concepts.

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* " Diese ad hoc eingeführte Annahme erschien aber doch nur als ein künstliches Mittel, um die Theorie zu retten " (Einstein 1907, pp. 412–13).

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