ON INERTIAL REFERENCE FRAMES

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I

1. *Scope and Purpose of the Paper.* It is the universal practice in physics, when describing the motion of a body, to choose a *reference frame, i.e.* some real or imaginary physical structure which is said to be "at rest," and then the motion of any other body is defined as its motion relatively to that. Of the various possible frames of reference, that known as an *inertial system*—formerly, and still sometimes, called a Galilean or Newtonian system or frame of reference—appears in the literature of relativity with a frequency unapproached by any other, and motion with respect to it is generally regarded as having a special significance. It is therefore of the first importance, both for theoretical reasons and on account of the possible effects in circumstances in which miscalculations may have dire results, that the meaning of the term, *inertial system,* shall be clearly understood, and that the term shall be used always in the same sense.

Unfortunately, these conditions are far from being fulfilled. It is the purpose of this paper, first, to bring to light the widespread confusion that exists on this point ; secondly, to show that existing ideas, notwithstanding their variety, are almost unanimously incompatible with the conception of inertial systems held by Einstein and regarded by him as indispensable for the proper understanding of his theories ; and finally, to correct an outstanding example of this misunderstanding which might otherwise have unfortunate effects.

2. *The Present Situation.* Out of a wealth of material I give below fifteen examples of explicit or implied definitions of inertial systems which have appeared in recent papers. Many more could have been chosen, but these, from mathematicians and physicists of a considerable range of type in various parts of the world, will, I hope, be sufficient to show that the confusion is neither superficial nor restricted in scope :

(1) " *R* remains at rest in an inertial frame, that is, he remains in a free path " (*Nature*, **177**, 784 (1956)).

(2) " Whether or not a particular frame is inertial is determined by its relationship to the universe in the large " (*Nature*, **178**, 681 (1956)).

(3) " Throughout all but the negligible portion of this time, *M* is in one or other of two inertial frames " (*Nature*, **178**, 682 (1956)).

(4) " In the usual language of physics, a freely moving laboratory means one that remains in (or simply constitutes) an inertial frame " (*Nature*, **179**, 909 (1957)).

(5) " The crucial . . . role of the inertial frame as ' a frame in which special relativity holds ' " (*Nature*, **179**, 1072 (1957)).

(6) " Both goal and origin are in *S*'s inertial frame " (*Nature*, **180**, 499 (1957)).

(7) " *B* is at rest at the origin of an inertial coordinate system *S*, while *B'* has the same relation to an inertial coordinate system *S'* . . . he has dissociated himself from *S'* and attached himself to *S* " (*Science*, **126**, 381 (1957)).

(8) " In special relativity, as in classical mechanics, reference to the rest of the universe is made by using the concept of inertial frames of reference. Neither theory can say anything about the behaviour of a physical system unless the relation to an inertial frame is specified " (*Discovery*, April 1957, p. 175).

(9) " The *inertial* systems of reference mentioned in this statement are those which are unaccelerated and are free of gravitational fields " (*Bull. Inst. Phys.*, **8**, 211 (1957)).

(10) " We have been treating the earth as a realization of what the [special relativity] theory calls an ' inertial frame of reference ' " (*World Science Review*, April 1957, p. 7).

(11) " It is necessary to specify a procedure for setting up an inertial frame of reference. To do this we start with an observer who is not subject to a gravitational field . . . if the equations of special relativity are to be applicable, it is necessary to check the limits of the region within which the reference frame has an inertial character . . . it is necessary to repeat the tests at frequent intervals " (*Jour. Brit. Interplanetary Soc.*, **16**, 569 (1958)).

(12) " Suppose two frames of reference *S* and *S'* are such that (i) they move with constant velocity relative to each other, (ii) the ratio of lengths measured in them in the direction of motion is constant, and also the ratio of lengths perpendicular to the direction of motion is constant. We define *S* and *S'* as inertial

frames with respect to each other" (*Proc. Camb. Phil. Soc.*, **57**, 322 (1961)).

(13) "Even from the viewpoint of general relativity, inertial systems remain in a special position, constituting a set of coordinate systems in which the description of motion can be given in a particularly simple manner . . . it will be assumed that a coordinate system attached to the earth may be considered as a good approximation to an inertial system" (*Amer. Jour. Phys.*, **27**, 132–3 (1959)).

(14) "The only hypothesis that is tenable . . . is that there exists a unique absolute inertial system, such as the universe as a whole" (*Aust. Jour. Phys.*, **11**, 282 (1958)).

(15) "Relativity only postulates that observers moving in *inertial* systems cannot tell which of them is moving" (*The New Scientist*, August 31, 1961, p. 542).

It is evident enough that no definition of an inertial system is possible that conforms to all these statements. Nevertheless, a few ideas may be discerned that seem to be fairly widespread and not merely peculiar to individual writers: here are four:

(i) *Inertial systems may be identified with particular objects in nature* (4, 10, 13, 14).

(ii) *Inertial systems can exist only in certain circumstances*, i.e. *if certain natural conditions are realised* (2, 8, 9, 11, 12).

(iii) *It is a fact of nature, and not a matter of choice, whether a particular object is "in" an inertial system or not* (1, 3, 4, 6, 7, 15).

(iv) *Inertial systems cannot be defined independently of some theoretical description of nature* (5, 13).

I will now show that these ideas are all directly at variance with the idea of an inertial system which Einstein regarded as fundamental to his relativity theories.

3. *Einstein's Definition of an Inertial System.* Einstein stated what he meant by an inertial system on many occasions. I select here a passage from an early paper [1] which appears to be remarkably little known, yet which contains the clearest possible statement of his views on some recent controversies. It consists of a dialogue between himself and an objector to the relativity postulate in which the views he expresses are quite contrary to those of many who today accept his conclusions without realising that he would have rejected their reasons for doing so. In this paper he speaks of

ein galileisches Koordinatensystem im Sinne der speziellen Relativitätstheorie, d.h. ein Bezugskörper, relativ zu welchem isolierte, materielle Punkte sich geradlinig und gleichförmig bewegen. (A Galilean coordinate system in the sense of the special relativity theory, *i.e.* a reference frame relatively to which isolated mass-points move uniformly in straight lines.)

From this definition he never departed, and responsible writers, in describing the relativity theory apart from particular applications of it, accept it as authentic. Thus Whittaker [2], writing in 1953, defines "an inertial system of reference" as one in which "the velocity of free particles along their rectilinear paths is uniform". I shall accordingly take it that when Einstein speaks of an inertial system, this is what he understands by the term.

4. *Einstein's View of Coordinate Systems.* In commenting on this definition I must apologise for saying some elementary, and indeed obvious, things; this is necessary because it is just those things that have been overlooked. In the first place, then, we notice that, in Einstein's meaning of the term, an inertial system is a *coordinate* system, and therefore everything that is true of coordinate systems in general is true of this particular example. Now a coordinate system meant for Einstein something purely mental, quite independent of nature or of our observations of nature; it was a voluntarily chosen framework into which we placed those observations in order clearly to exhibit their relations with one another. He continually emphasised this; for reasons of space I again restrict myself to a single instance. In an article in *The Times*, reprinted in a later volume [3], he wrote:

> In order to describe the movement of a body, a second body is needed to which the movement of the first is referred . . . In physics the body to which events are spatially referred is called the coordinate system . . . Should the independence of physical laws of the state of motion of the coordinate system be restricted to the uniform translatory motion of coordinate systems in respect to each other ? What has nature to do with our coordinate systems and their state of motion ? If it is necessary for the purpose of describing nature to make use of a coordinate system arbitrarily introduced by us, then the choice of its state of motion ought to be subject to no restriction; the laws ought to be entirely independent of this choice (general principle of relativity).

The meaning is perfectly clear, and this view of coordinate systems is a necessary consequence of the general postulate, or principle, of relativity, which says that nature provides no absolute standard to which the motion of a body can be referred. If we have only one body, A, to consider, then we cannot speak definitely of its motion without inventing a second body, and then the motion of A is its motion relatively to that. This second body is a *coordinate system* (it has usually other characteristics, to enable us to specify the *position* as well as the *motion* of A, but these are irrelevant here), and since the state of motion voluntarily ascribed to it is to be entirely unrestricted—*i.e.* it can be regarded as moving in any straight, curved, smooth, jerky or any other fantastic way we like—the motion of A with respect to it can have any of these characteristics. This has nothing whatever to do with the actual state of A. A being whatever it is, its motion is, at one and the same time, of any conceivable kind we like, because its motion is not a property of A; it is a relation between A and a coordinate system, and the coordinate system being entirely arbitrary, the relation also is entirely arbitrary.

Of course, if we are considering a problem concerning many bodies, we must refer them all to the *same* coordinate system, and therefore the motion of one body *relatively to another* is *not* arbitrary. If, for example, A is at rest with respect to another body, B, then the motion of A with respect to our arbitrary coordinate system will be the same as that of B, and therefore A and B will be relatively at rest in *all* coordinate systems. The motion of A with respect to B is an objective fact of nature, but the particular motion ascribed to each of them is at our choice. The purely fictitious character of coordinate systems is an expression of the fact that the motion ascribed to a single body is a fiction, and to deny that coordinate systems are fictitious is to deny the postulate of relativity. This consideration alone is sufficient to show that idea (i) mentioned above is incompatible with Einstein's view.

5. *The Distinguishing Feature of Inertial Coordinate Systems.* An inertial system, then, being a coordinate system, is a fiction. But there are many kinds of fiction; what particular kind is an *inertial* system? According to Einstein it is a coordinate system with respect to which an *isoliert* (or *free*, to use Whittaker's translation) body moves uniformly in a straight line. Now superficially this seems to violate the absolute separation, essential to relativity, between the phenomena of nature on the one hand, and the coordinate systems to which we arbitrarily refer them on the other. A free material body seems to be a part of nature, and if it is introduced into the very definition of an inertial system, then that system, it would seem, must necessarily to some extent depend on nature for its properties. This would seem at least to give a

possible justification for the ideas which I have described as contrary to those of Einstein.

I think it extremely likely that it is this thought that has given rise to those ideas; nevertheless, there can be no doubt whatever that it is false, and it is impossible to understand Einstein if its falsity is not clearly realised. The crux of the matter lies in the word "free," for this itself is arbitrary. If there were some objective sign by which we could distinguish, irrespective of all arbitrary choice, a free body from a body which is not free, then an inertial system, as defined by Einstein, would be objectively identifiable if it existed, but in fact there is none. An inertial system therefore remains as fictitious as any other coordinate system.

To see this we have only to look at Newton's mechanics, and it at once stares us in the face. Newton's first law of motion is a statement that he is going to use only inertial systems. Let us follow him, and then examine nature to see how a body moves when we release it from all obvious constraint. Suppose, for instance, we consider an apple which has just broken away from a tree on the Earth. Here we have two bodies—the apple and the Earth—neither of which is subject to any observable force, yet one of them at least is not moving uniformly in a straight line. Must we, then, say that we cannot use inertial systems ? Not at all; we simply say that the bodies are not "free"—there is "a gravitational force" acting on them.

This is no sophistry. For more than 250 years it has been taken as, and it is, an entirely legitimate procedure. Since coordinate systems are fictitious we can *always* choose any we like. If we choose an inertial system, then the facts of nature compel us to say that undisturbed bodies are not "free", *i.e.* we must admit gravitational force as *a fact of nature*. No subsequent discovery can invalidate this procedure. If, as is now generally believed, Newton's law of gravitation has failed, that simply means that his formula for calculating gravitational force is inaccurate; it cannot proscribe the search for an exact formula by anyone who thinks it worth while.

6. *Newtonian and Einsteinian Mechanics*. Einstein, as he often said, followed the same procedure as Newton. Where he differs from Newton is simply and solely in the fact that Newton held that we *must* use an inertial system, whereas Einstein holds himself free to use any coordinate system at all. Newton's first law of motion says that if you have a single body alone in space, the only motions you may allow it are uniform rectilinear ones; *i.e.* the only coordinate systems you may use are inertial ones. Of course, such a

restriction, imposed without adequate reason, would be entirely arbitrary: what is necessary to justify it is a demonstration of some actual phenomenon which it is impossible to ascribe to anything other than the acceleration of a body with respect to an inertial system. Newton believed he had found this in his famous bucket experiment.* Its essential characteristic, which it shares with later observations such as that of Foucault's pendulum and other "accelerometers" as such devices are called, is that when a body, B, is accelerated relatively to a system of coordinates in which it was previously at rest or moving uniformly, certain physical phenomena become observable which affect B but not other bodies whose motion relatively to that system is not changed. If those effects arose only from the acceleration of B relatively to the other bodies, they should appear in those bodies also, for they are accelerated relatively to B just as B is accelerated relatively to them. Since they do not, B must experience an *absolute* acceleration, not shared by them. Hence the coordinate system in which they remain at rest or in uniform motion has an absolute character, acceleration with respect to which is an objective fact of nature, attended by phenomena which enable us to discover it.

If this conclusion were valid, then the existence of gravitational force as an objective part of nature would be inescapable. But, in the too little known *Naturwissenschaften* paper already referred to, Einstein points out that a "gravitational field," like the "freedom" of an undisturbed body, is not an objective thing whose existence one can test by observation, but something which, according to Newton's own procedure, we dismiss or call into being whenever it is necessary to do so in order to account for observations. He takes an extreme example of an "accelerometer," in which a rapidly moving train is suddenly brought to rest by collision and is smashed to pieces, while a neighbouring church steeple is unharmed. The cause of the damage is not the *acceleration* suffered by the train, but the *impact with another body*, which the steeple does not undergo. The acceleration of the train relatively to the steeple can with equal right be ascribed to the steeple, rela-

* Newton rotated a cylindrical bucket, containing water, around its axis. At first, before the motion of the bucket was communicated to the water, the surface of the water remained plane. Later, when the water and the bucket were rotating with the same angular velocity, the surface of the water was concave. He concluded that, since a visible effect (the concavity of the water surface) appeared when there was no *relative* rotational motion between the water and the bucket, but did not appear when the relative motion was greatest, it could not be an effect of relative motion, but must indicate an *absolute* rotation.

tively to the train which remains at rest all the time; and in that case the steeple, together with all the rest of the universe except the train, suddenly moves under the influence of a "gravitational field," which does it no more harm that the Earth's gravitational field does to a falling apple; only when the apple hits the ground is it bruised.* The "accelerometer" thus detects an acceleration of one body relatively to another, but does nothing at all to compel the ascription of the acceleration to one body rather than the other. The *account* of the phenomena changes with change of coordinate system, but the phenomena themselves remain the same. Hence, accelerometers notwithstanding, you are free to choose any coordinate system you please, and (apart from convenience in certain problems) inertial systems have not the slightest claim to special consideration.

As everyone knows, Einstein solved magnificently the problem thus posed of describing the motions of bodies, in what was previously regarded as an objective gravitational field, in a form applicable to all coordinate systems, but it is no disparagement of that achievement to say that it was of secondary significance. He was convinced before he had reached this culmination of his thought-process that the fundamental thing was to release the approach to the problem from the quite unnecessary slavery to inertial systems—in other words, to admit the *general* postulate of relativity, that *all* coordinate systems are on the same footing as fictions between which our liberty to choose must be absolutely unrestricted. The opening of his great paper on the general theory [4] makes it perfectly clear that he was convinced of this principle on grounds quite independent of its success in leading to a better law of gravitation than Newton's, and, on the other hand, his later adducing of the success of that law as evidence for the legitimacy of the general postulate of relativity shows what he considered to be its primary claim to respect.

7. *Einstein's Struggle against Misunderstanding.* It follows, then, that none of the ideas (i)–(iv) set out in paragraph 2 is compatible with Einstein's view of inertial systems or of the relation of coordinate systems to nature in general. A survey of his writings from 1916 onwards shows that he himself had frequently to contend with those who, because of the success of his special theory in which only inertial systems were appealed to, thought that such

* Whether it is proper to call this fictitious force by the same name—gravitational field—as that used for the naturally occurring and not mentally avoidable fields surrounding massive bodies, is another story: I am here describing, not criticising, Einstein's argument.

systems must have objective existence. His most detailed protest against this illusion is probably that contained in the book which he wrote with L. Infeld [5], in which the authors take great pains to discover whether inertial systems can in fact, by any possible observation, be distinguished. They reach the conclusion that this is possible only if absolute motion exists. "The difficulties mentioned, that of an inertial system and that of absolute motion," they say (p. 223), "are strictly connected with each other. Absolute motion is made possible only by the idea of an inertial system." They then devote a dozen pages to an examination of the necessity or otherwise of recognising inertial systems and absolute motion, and ultimately decide (p. 235): "The ghosts of absolute motion and inertial coordinate systems can be expelled from physics and a new relativistic physics built."

The "Autobiographical Notes" which Einstein contributed to the volume, *Albert Einstein, Philosopher-Scientist* [6], again show his insistence on the denial of all special significance to inertial systems. "So," he wrote (p. 67), "if one regards as possible, gravitational fields of arbitrary extension which are not initially restricted by spatial limitations, the concept of the 'inertial system' becomes completely empty." And a footnote on p. 77 shows clearly enough how he had to struggle, as those who understand him today have to struggle, with the impercipience of those who think they can accept his general theory of relativity and at the same time grant significance to inertial systems in other connections:

> To remain with the narrower group [*i.e.* the Lorentz-Group of transformations between inertial systems] and at the same time to base the relativity theory of gravitation upon the more complicated (tensor-) structure implies a naïve inconsequence. Sin remains sin, even if it is committed by otherwise ever so respectable men.

The situation which called forth that comment speaks eloquently to us in the quotations (1)–(15) in paragraph 2 and in the investigation now to be discussed.

II

8. *Recent Experimental Evidence.* The foregoing considerations have recently acquired a special importance by reason of a paper by C. W. Sherwin [7], in which observational evidence is claimed for the objective existence of privileged inertial frames in nature. One of the experiments referred to has been repeated at Harwell and shown on the television screen in this country, in a programme en-

titled "Insight," and described by Dr. J. Bronowski as, at the same time, establishing the objectivity of inertial systems and yet confirming Einstein's theories. This would be at least paradoxical, and since, by a process which I have not succeeded in following, this experiment is held also to have established the reality of the so-called "asymmetrical ageing" (the retardation of ageing by fast travel), it is necessary to examine the argument somewhat closely.

Two experiments are described by Sherwin—those recently performed by Pound and Rebka [8], and by Hay, Schiffer, Cranshaw and Egelstaff [9], respectively. Since, for our present purpose, they are precisely equivalent, and the latter was the one repeated by Bronowski in his "Insight" programme, I will consider that alone: I describe it in Bronowski's words [10]:

> The experiment has been made possible in the last years by the discovery of the Mössbauer effect, in which gamma ray photons are emitted and absorbed in a very sharp frequency interval. The experimental apparatus consists of an aluminium disc of six inches diameter, on the spindle of which is mounted a radioactive isotope of iron. Round the circumference of the disc is mounted a resonant absorber of iron, and beyond the circumference is a stationary counter which records those photons which pass through the absorber. Because the Mössbauer effect is so sharp, this arrangement makes it possible to compare the rate of physical processes (used as clocks) to an accuracy of one part in 10^{12}. In the film that I showed, the number of photons counted was significantly different when the disc was rotated at fifty and five hundred revolutions a second, each time for thirty minutes.

On this experiment, in its original form, Sherwin comments:

> In his original paper on special relativity, Einstein predicted that a clock which departed from a given point in an inertial frame S . . . and returned to the starting point, would there be found . . . retarded compared to an identical clock . . . which remained at the starting point . . . this is associated with the fact that the rest clock remains in the inertial frame S, but the frame in which the traveling clock is at rest is not inertial, since it experiences accelerations. The purpose of this paper is to point out that recent experiments have provided the first direct experimental verification of [this] prediction.

Sherwin claims quantitative as well as qualitative verification, and Bronowski echoes this claim. The radioactive specimens are regarded by both writers as equivalent to clocks.

9. ***Einstein's "Prediction."*** It is unfortunate that Sherwin and Bronowski were not better acquainted with the history of the subject, or they would have known that Einstein very soon realised that this prediction was invalid—and indeed, had it been verified it would have disproved his theories. The prediction, as Sherwin says, appears in his original paper on the special theory [11], in which, by applying the Lorentz transformation which he had independently derived, he deduced that "a balance-clock at the equator must go more slowly, by a very small amount, than a precisely similar clock situated at one of the poles under otherwise identical conditions." His later paper on the general theory (*loc. cit.*), however, opens with a modification of some of the assumptions of the earlier one, and among them is the assumption that the Lorentz transformation is applicable to a "rotating" body such as the Earth. He points out that no observation confined to a single such body is evidence of its rotation, and any phenomenon observed on such a body which it has been customary to regard as arising from its rotation might equally well be ascribed to the influence of *external* forces acting on a non-rotating body. The example of such phenomena which he gives is the equatorial bulge which the Earth is known to display, but the effect on an equatorial clock is clearly of precisely the same character, except that it is hypothetical while the bulge is an observed fact. The apparent relative motion of a polar and an equatorial clock is therefore something that can be transformed away by a change of coordinate system. But to obtain a physical effect from an application of the Lorentz transformation you must have a relative motion that cannot be so transformed away. Hence, whether or not an equatorial clock goes more or less slowly than a polar one, and if so by how much—which are questions to be settled by observation or by a more comprehensive theory than that of the 1905 paper—it is quite impossible to deduce that fact from the Lorentz transformation.

But, indeed, this had already long been realised, quite apart from considerations arising from the general postulate of relativity, on account of an objection first raised by Ehrenfest [12] which became famous as the "problem of the rotating disc." Ehrenfest pointed out that the application of the Lorentz transformation to such a disc led to an absurdity, for every element of the circumference would be moving along its length, and therefore would be contracted, while every element of a radius would be moving at right angles to its length and would not be contracted. As the speed of rotation increased, therefore, the circumference would continually shrink while the radius remained unchanged, which is clearly impossible.

This made it necessary to abandon all such "predictions" as that concerning polar and equatorial clocks, but at that time no grounds existed for any expectation at all concerning their relative behaviour. Later, however, after the general relativity theory had become established, a solution of the rotating disc problem was given by Lorentz [13], and an independent one, leading to the same result, by Eddington [14]. These required an effect different in character, and much smaller in amount, from that wrongly deduced from the Lorentz transformation, and this is now generally regarded as the solution of a troublesome problem.

If, then, Sherwin and Bronowski are correct in holding that the effect originally predicted has been quantitatively verified, the consequences—not to exaggerate—are disconcerting. Einstein's theories lie in ruins, and the unresolved dilemma of the rotating disc rises again, phoenix-like, from the ashes. However, experiment is unanswerable, and if in fact it has so decided the consequences must be faced. Nevertheless, we are justified—indeed, compelled—to look very carefully at the experiments in question to see if in fact they have the implications asserted.

10. *The Objectivity of Inertial Systems.* Sherwin bases his interpretation of the experiments on the assertion that they establish the objective existence of privileged inertial systems. This, he says, "raises from a new standpoint, the fundamental question: Why are inertial frames privileged above all other reference frames? For, the two clocks are calculated to behave differently for precisely the reason that inertial frames *are* unique. Put another way, noninertial frames may be identified experimentally."

Now if this were so, then inevitably, as the passages quoted in the first part of this paper show, Einstein's general theory must be abandoned, and he himself would have been the first to admit this. In his view, as we have seen, inertial frames and absolute motion were inseparable, and if inertial systems are objectively identified, then so is absolute motion. We must therefore consider carefully the reasons which Sherwin gives for holding that the experiments demonstrate the objective existence of inertial frames.

Unfortunately, however, such reasons are not discoverable; we cannot even tell what he understands by an inertial frame. In the Abstract of the paper the two specimens are said to "pursue independent paths (at least one of which involves accelerations) in a common inertial frame." In the paper we read: "Given that the rest clock is in an inertial frame (as indicated by the null reading of attached accelerometers, for example) . . . " And later: "one is comparing a proper time interval in one inertial frame to what might be described as the sum of proper time intervals which were collected by the travelling clock in several different inertial frames." Bronowski makes confusion worse confounded by asserting that the travelling clock is *not* in an inertial frame.

What are we to make of all this? Certainly Sherwin and Bronowski accept implicitly the idea (iii)—that it is a matter of objective fact, and not of choice, whether or not a clock is "in" an inertial frame or not—but it is difficult to reach any more precise understanding of what they consider such a frame to be. Whether the travelling clock (Bronowski's "absorber" is Sherwin's "travelling clock") is in the same inertial frame as the rest clock, as we gather from one passage, or in a succession of different ones, as we gather from another, or in none at all, as Bronowski has it, remains obscure. Furthermore, accelerometers are held to be able to show objectively that a body is "in" or not "in" an inertial frame, though in fact the experimenters do not appear to have used them. Whatever all this may mean, it is obvious to anyone who reads the passages cited in Part I of this paper that it is entirely foreign to Einstein's ideas. We are bound to conclude, therefore, that Sherwin is speaking a different language from Einstein. Whatever he means by an inertial frame, it is patently not a frame in which a free body rests or moves uniformly in a straight line—certainly the frame in which his "resting" specimen is in fact resting is not such a frame unless the objective existence of Newtonian gravitational force is allowed, for we may take it for granted that the specimen did not rest in mid-air—but some physical object such as the laboratory in which the experiment was performed. He is, of course, at liberty to introduce conceptions of his own, which need not be those of Einstein, though it is unfortunate that he does not choose fresh names for them, and still more unfortunate that he should say that the experimental results he describes are "in full agreement with the conventional theory which assigns a unique status to inertial frames." Most readers would assume "the conventional theory" to be that of Einstein, but to Einstein, inertial frames were "ghosts" which should be "expelled from physics," and the concept of an inertial frame had become "completely empty." Sherwin and Einstein are thus in complete contradiction with one another.

11. *The True Meaning of the Experiments.* Let us, however, try to get beneath language difficulties, and eliminate the unfortunate

term, "inertial system," altogether. Then the difference between Einstein and Sherwin is that, according to Einstein, there are no privileged coordinate systems in nature, and therefore no possible experiment or observation can indicate that any frame at all is more legitimate (it may, of course, be more convenient) than any other; whereas, according to Sherwin, the experiments he cites indicate that there *are* privileged frames. This is a clear issue; let us see what the experiments in question have to say about it.

Reduced to their bare essence, they present us with two bodies, A and B, in relative motion, as a result of which certain phenomena are observed. Now concerning the *relative* motion of two bodies, no question arises; it is undoubtedly an objective physical phenomenon, attended by observable phenomena. Equally undoubtedly, there are objectively different *states* of relative motion, distinguishable from one another by observation. In general there are five such states:

(*a*) A state of relative rest.
(*b*) A state of uniform relative motion at one velocity.
(*c*) A state of uniform relative motion at another velocity.
(*d*) A state of relative motion with a given acceleration.
(*e*) A state of relative motion with another acceleration.

It scarcely needs saying that if one draws up a table of the measured positions of A and B, in any coordinate system at all, at successive times, then that table will give a clear and unambiguous criterion for determining which of these five states of relative motion is the one actually existing. There are, of course, other criteria also for particular cases, such as the Doppler effect, accelerometers, etc. We need not enumerate them, for the point is clear enough that no new experiment is needed to establish the objective distinguishability of these five states.

The difference between Einstein and Sherwin is this—that, according to Einstein, in none of these states is it possible, by any observation, to say that the relative motion, if any, must be shared between A and B in some particular way. In case (*a*), for instance, A may be given any motion at all, regular or irregular, so long as B is given the same motion; or, what is the same thing, we may choose any coordinate system at all, with respect to which either body has any motion we like, so long as the other has the same motion with respect to that system. Sherwin, however, holds that there are particular coordinate systems which are "privileged," *i.e.* they are such that the motion of A with respect to them (not to B) is distinguishable *by observation* from its motion with respect to other systems; and he claims that the experiments now under discussion provide that observation. Let us see if this is so or not.

In the experiment which I have described in Bronowski's words, A is the iron isotope mounted on the spindle of the disc, which remains stationary in the laboratory throughout the experiment, and B is the absorber on the circumference, which is made to revolve around A at various speeds. What is observed is that B absorbs the radiations of A at different rates for different speeds of revolution of the disc.

But what does that demonstrate? Only that there is an observable criterion for distinguishing between states (*d*) and (*e*), which stood in no need of demonstration. It tells us precisely nothing about the individual state of motion of either A or B. We are still at liberty to choose a coordinate system in which A is at rest, or travelling round the Sun at 18½ miles a second, or round the Galaxy at some hundreds of miles a second, or doing the twist or moving in any other way we like, and the experiment remains exactly the same. There is nothing whatever in this experiment to indicate that any one coordinate system is privileged above any other. The other experiment cited by Sherwin, which I have not described, is equally ineffective in this respect. It gives a new criterion for distinguishing between states (*b*) and (*c*), but in neither of these states does it enable an observable distinction to be made between the motion of A with respect to B and that of B with respect to A.*

12. *Conclusion.* We must conclude, therefore, that Sherwin has not only misunderstood Einstein, but has misunderstood also the significance of the experiments which, if his interpretation of them had been correct, would have disproved Einstein's relativity postulate. The moral of it all seems to be that one should always understand clearly the definitions of the terms he uses, and use them only in accordance with their definitions. When an inertial frame is regarded indiscriminately as a coordinate system, the Earth, the universe, a freely moving body, and so on, creditable results are hardly to be expected, and when all these various conceptions are automatically ascribed to Einstein and his clear statements are ignored, gross disrespect is shown to the memory of a great man.

It need scarcely be added that the conclusions concerning the clock paradox which Sherwin and Bronowski draw from their arguments are without significance.

* I am, of course, making no *general* comment on these experiments, which are of great interest and importance in relation to nuclear theory. I am commenting only on their significance in relation to the relativity of motion.

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