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PETER A. BOWMAN

On Conventionality and Simultaneity— Another Reply

1. Introduction

In "Conventionality in Distant Simultaneity," Brian Ellis and I (1967) discussed the position Reichenbach and Grünbaum had taken on this issue. That article received considerable comment (Grünbaum and Salmon, 1969; Winnie, 1970; Feenberg, 1974), much of the critical part of which Ellis answered in "On Conventionality and Simultaneity—A Reply" (1971). Here I shall reformulate, extend, and supplement his answer to some of the critiques (Grünbaum, 1969; Salmon, 1969; van Fraassen, 1969). Elsewhere I treat the topic in a less polemical manner (Bowman, 1974 and 1976).

The conventionality of distant simultaneity, as maintained by Reichenbach and Grünbaum, is after all this commentary so widely known that it can be stated very briefly. Let us consider two points A and B which are separated from one another in an inertial frame K. For a light signal emitted from A and reflected at B back to A, we compare the time interval for the outgoing trip to that for the round trip. This ratio is called "epsilon" (ϵ). In formulating the special theory of relativity, Einstein effectively took ϵ to be $\frac{1}{2}$; thus we may use $\epsilon = \frac{1}{2}$ in defining what is now called "standard signal synchrony." Reichenbach views ϵ as restricted *only* by the causal relations involved in the signaling process. That is, the reflection of the light ray at B must take place after the ray's emission at A but before its return to A. These considerations require us to restrict ϵ between zero and one, but Reichenbach insists that within these limits values of $\epsilon = \frac{1}{2}$ "could not be called false" (1958, p. 127). He claims that there are no facts that would mediate against using these values in definitions that are now called "nonstandard signal synchrony." This allegedly

NOTE: This paper follows subsection J.1 of my dissertation (1972) with only minor expository changes except for the last page of the present subsection 2.c, which is a substantive revision.

physical possibility of choosing ϵ between zero and one is "the conventionality of distant simultaneity (or, *mutatis mutandis*, synchrony) as determined by signals." Grünbaum also argues for this thesis, making clear that it obtains within a single inertial frame.[1] In this paper I shall consider a nonsignaling definition of synchrony and discuss its implications for simultaneity in Newtonian mechanics.

The Reichenbach-Grünbaum approach establishes the basic concept of special relativity, distant simultaneity, through the transmission of signaling processes; e.g., electromagnetic, gravitational, or matter waves and particles. The approach I wish to take is characterized by the transport of actual clocks; e.g., mechanical, light, or atomic clocks. Since a clock is defined as any physical system which passes through the same process periodically, and since on some construal a signal might possess this property, it would not be possible to distinguish sharply between a clock and a signal in that sense.[2] However, for the purpose of my characterization of a signal, it has the salient feature of being an infinitesimal disturbance or a point mass which, except for its own presence and absence, carries no information.

2. Simultaneity in Newtonian Mechanics

Elsewhere (1972, section F.5; 1974, section 3; 1976, section 3), I have taken the position that distant simultaneity as defined by the transport of clocks is conventional in the same sense as any other quantitative equality at a distance. In doing so, I have followed the position taken by Ellis and myself in our joint article (1967, p. 134). Grünbaum (1969, pp. 26–27), Salmon (1969, p. 56) and van Fraassen (1969, p. 67) have argued in reply that this is not the case in a world like that described by Newtonian mechanics, in which distant simultaneity can be established as a matter of temporal fact through the use of arbitrarily fast causal chains. Ellis (1971, p. 179, section 2) rejoins that this conclusion is false if "the basic time-ordering relationships in the Newtonian world are taken *e.g.* to be those given by the *local entropic order*," i.e., by what I call "clocks." In the third part of this section I shall reinforce this argument; but in the first two parts I shall put aside temporarily the assumption that simultaneity can be defined by the transport of clocks in Newtonian mechanics, in order to show what happens when Grünbaum excludes the latter, fully legitimate procedure from his account.

a. Without the Use of Transported Clocks

Let us imagine a point-mass P being sent as a signal (physical causal chain) between point-masses A and B at rest in an inertial frame (see figure). The departure of P from A we shall call E_1; its arrival and reflection at B, E'; and its return to A, E_2. Even though Newtonian mechanics allows arbitrarily fast causal chains, there is a unique event E on the world-line of A between E_1 and E_2 which cannot also be on the world-line of P if negative transmission times are precluded. For we may accelerate P as much as we wish, thereby making the transmission time approach zero, but we can never make P depart and return at the same instant: we can always find some instant E between P's departure and return. In other words, P is not a first signal in Reichenbach's sense[3] because we can always accelerate some other point-mass Q so that it will return before P. Still, there is a greatest lower limit, zero, to the transmission times of P or Q if negative transmission times are ruled out. Since it is impossible to connect the events E and E' in either direction by P used as a signal, they

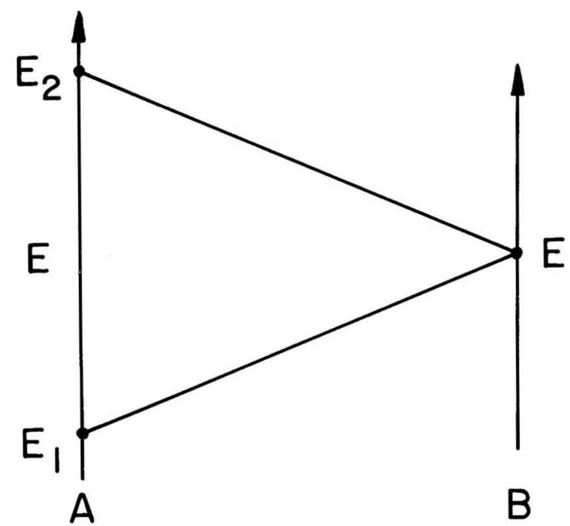

Figure 1

are indeterminate as to time order and hence simultaneous according to definitions given by Reichenbach (1958, pp. 144–45) and Grünbaum (1973, pp. 28–29). If the events were connectible, they would be temporally ordered and, with regard to a third event, they would exhibit temporal betweenness.

Now in Grünbaum's most recent treatment of simultaneity in Newtonian mechanics (1969, pp. 19–20), clocks play the roles of the point-masses A, B, and P. However, it does not follow from the above situation, as he would have us believe, that "the non-metrical, purely ordinal character of the unique simultaneity relation between E and E' [is] furnished by Newtonian clock transport." (1969, p. 21.) Rather, on the usual understanding of clocks and signals which was delineated in section 1, the simultaneity relation has been established by a signaling procedure. Although clocks could be substituted for the point-masses (as they were in Grünbaum's description), they do not have to be, since no clock was used as such.

Grünbaum goes on to provide a second characterization of "the relations of *temporal* betweenness and simultaneity in Newton's world on the basis of the causal betweenness defined by the following subclass K of its physically possible causal chains: K is the set of all those genidentical chains which are not spatially self-intersecting in at least one inertial frame." (1969, p. 22.) Once it is realized that the latter are infinitely fast influences (e.g., gravitational ones) and that those influences were also precluded from the first characterization, it becomes apparent that the first and second characterizations are identical in this respect: they are both based on the possibility of arbitrarily fast causal chains; neither is based on the possibility of obtaining consistent readings on transported clocks. Therefore the latter possibility, which was a legitimate physical procedure for synchronization in Newtonian mechanics according to Grünbaum's original position (1973, p. 370), has been effectively deleted from his revised position (1969, pp. 27–28). Concomitantly, the assumption of the impossibility of obtaining such readings, what he calls "assumption (i)," no longer characterizes for him the transition from Newtonian mechanics to special relativity (1964, section 1), and that assumption no longer represents for him a necessary condition for special relativity.

Rather than admitting that the slow transport of clocks provides a legitimate physical means of synchronizing clocks in special relativity, Grünbaum excludes synchronization by clock transport in Newtonian

mechanics. If he is to be consistent, he has to put reliance on a clock's periodic processes as durational measures either in both theories *or* in neither, and he takes the latter implausible option. Grünbaum acknowledges that he modifies his position in that the absoluteness of simultaneity is no longer a sufficient condition for its nonconventionality (1969, p. 28). But he does not admit that even in Newtonian mechanics he must exclude clock transport as a physical means of synchronization distinct from signaling, because in that theory as well as in his "quasi-Newtonian universe"[4] the assertion that E and E' are simultaneous according to the readings of transported clocks (as distinct from signaling) rests on a convention in what he would consider a nontrivial sense: the assertion makes "a metrical appeal to the *durational congruence* of intervals on different world-lines" (1969, p. 25).

While Grünbaum concludes that simultaneity in a "quasi-Newtonian" universe is conventional in this sense, Putnam comes to a conclusion the statement of which is diametrically opposed to Grünbaum's, yet apparently similar to that of Ellis and myself:

> In the quasi-Newtonian world, the customary correspondence rule for simultaneity involving the transported clocks does not lead to inconsistencies that cannot be explained as due to the actions of differential forces upon the clocks. There is then, in my view, no reason to regard simultaneity as a notion needing a definition except in the trivial sense in which every notion requires a definition (TSC). It is, in my view, an empirical fact in such a world that the to and fro velocities of light are equal relative to anything else. We *could*, of course, define the to and fro velocities of light to be equal relative to some system in motion and put up with the incredible complication that would result in the statement of all physical laws. This is, however, just an instance of the possibility of altering the nomenclature in connection with any physical magnitude and as such is an instance of TSC [= "trivial semantic conventionalism"] (1963, pp. 237–238).

Our conclusion is more restricted in that it refers to any physical magnitude that depends on local comparison. This is necessary to preclude quantities such as those depending on fundamental charge e and Planck's constant h, which are "physical magnitudes" but which are based on manifolds having intrinsic properties that provide a built-in measure. In contradistinction to such magnitudes there are magnitudes like spatial and temporal congruence that represent continua devoid of such intrinsic metrics; these magnitudes that have to have a metric provided for them by fiat or convention Grünbaum calls "Riemann-conventional" (1969, pp. 25, 33). Ellis and I[5] accept the existence of the latter conventionality, while pointing out that in certain theoretical contexts there are legitimate physical procedures (like slow-transport synchronization in special relativity) that override or subjugate it. Conversely, in the absence of such procedures, there are contexts in which the Riemann-conventionality of continuous manifolds prevails.[6]

However, as for the *grounds* for conventionality, we seem to have more in common with Grünbaum. In view of the theoretical-context-dependence of our version of Riemann-conventionality, we must distinguish it from a fact-dependent formulation like Grünbaum's. But even more we must separate it from Putnam's "trivial semantic conventionalism" (TSC), which is based on purely linguistic considerations. We can, like Grünbaum (1970, pp. 471, 476), imagine theories other than Newtonian mechanics (about which we disagree) in which simultaneity would not be Riemann-conventional. For example, we can conceive of physicists, in trying to reconcile quantum mechanics with relativity theory, devising a notion of time which would be quantized like charge and action and would not be based on a continuum of instants. Presumably, Putnam would say that such a magnitude can be altered so as to be a continuous one (cf. n. * on his p. 222). However much this may *simplify* (rather than complicate, as in Putnam's statement above) the mathematical representation, physicists would regard it as an approximation just as they do quantized magnitudes whose quantum numbers become very large (correspondence principle). The underlying reality, they would say, is nonetheless discrete, and an exact treatment must reflect this fact.

b. Without the Use of Gravitational Influences

Clock transport is not the only physical procedure for synchronizing clocks in Newtonian mechanics that Grünbaum eliminates. He also precludes gravitational influences from his characterization, evidently for the reason that "the *K*-defined relations of temporal betweenness do not allow the deduction of the paradoxical conclusion that either of two simultaneous events belonging to a gravitational chain is temporally between the other and some third event." (1969, p. 23.) Nevertheless, except for creating problems for Grünbaum's philosophical account, both of these procedures are perfectly legitimate alternative criteria for distant simultaneity in Newtonian mechanics. And he ought to admit them into his

account if he really sees theoretical terms in physics as "open multiple-criterion concepts." He criticizes "the crude operationist claim that any particular 'definition' chosen by the physicist exhaustively renders '*the* meaning' of spatial congruence in physical theory." (1973, p. 15.) However, in practice, Grünbaum is doing this himself by restricting his characterization of distant simultaneity in Newtonian mechanics to the "subclass *K* of its physically possible causal chains."

Not only is he methodologically inconsistent in this respect, but he also weakens his argument for the nonconventionality of distant simultaneity in Newtonian mechanics beyond the point of collapse. As it now stands that argument rests merely on the lack of a limit on Newton's second law, i.e., the physical possibility of particles being accelerated without limit (1973, p. 350n). To be sure Newton himself believed that his law was limitless. And, of his famous Rules of Reasoning in Book III of the *Principia*, Rule III provided him sufficient grounds for such a belief: "The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever."[7] However, it is doubtful that he would have sustained his belief in the face of an empirically based challenge to the unlimited speed of gravitation. Rule IV provides just this necessary condition: "In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exception." (1966, p. 400.) Thus the second law is to be regarded as unlimited until such time as there is discovered some actual phenomenon which seems to limit it.

Now Newton was well aware from Roemer's observations of Jupiter's moon that the speed of light was finite. But he had no similar grounds for believing that gravitational propagations were limited. Thus he was justified in holding that particles could be accelerated without limit. However, the lack of a limit was by no means essential to Newtonian mechanics, and as J. D. North shows, there were attempts to modify Newtonian mechanics in this respect both from inside and from outside the theory (1965, chap. 3, section 5). First he generalizes the sort of external criticism of Newtonian mechanics that, as a matter of historical fact, Poincaré gave: "anyone who hoped to cast a new theory of gravitation in the form of Maxwell's electromagnetic field equations was committed to forces which could not be supposed to act simultaneously over finite distances."[8] What needs to be brought out explicitly here is that the modifications electromagnetic theory forced one to make in the laws of gravitation and acceleration were physically rather than logically grounded: the modifications were being made to obtain empirical agreement between two different theories (or three, if Newton's three laws are regarded as a theory of inertia distinct from his law of gravitation); logically, there is no reason why the theories could not disagree.

It is true that the foregoing modifications were made in the context of an electron-theoretic modification of Newtonian mechanics. But, as logical possibilities, they could have been made on the basis of some theory other than the electron theory, or they could have been made for reasons internal to Newtonian mechanics. North argues secondly that, at one time, Newton's gravitational law appeared unsuitable for the explanation of what were perceived as "anomalies" in the motions of the planets. Thus, in order to account for an apparent secular acceleration of the moon, Laplace considered the possibility of a finite velocity for gravitation, one with a lower limit of 7×10^6 times the velocity of light (North, 1965, p. 44). Thus, from within Newtonian mechanics, Newton's second law could have been altered prior to the electron theory if there had been no overweighing reason to believe that gravitational influences propagated faster than light and probably with infinite speed. Indeed, if such influences were unknown to the Newtonian, then he would have been even freer to require, in response to some perceived anomaly, that the actually known speed of light serve as a limit for the possible speeds of particles. Just as Poincaré felt obliged on physical grounds to modify Newton's second and gravitational laws to make them agree with electromagnetic theory, so also the Newtonian would have felt obliged under these hypothetical circumstances to modify the second law.

What are the implications of this discussion for Grünbaum's account of distant simultaneity in Newtonian mechanics? If he precludes faster-than-light gravitational propagations as a criterion for distant simultaneity, as he apparently does, then hypothetically one could insist that the acceleration of particles be limited by the speed of light and, concomitantly, that the criterion for distant simultaneity making use of accelerated particles agree with the light-signaling criterion. Under these conditions,

then, Grünbaum's truncated version of the Newtonian world (not that world itself) can be identified with what he calls a "quasi-Newtonian world." And the Riemann-conventionality which he takes to characterize the latter is also found in the former. In particular, the Reichenbach-Grünbaum definition of simultaneous events as being those that are indeterminate as to time order is seen to be compatible with this interpretation of Grünbaum's Newtonian world. Or, by transposition, any two events belonging only to the career of the fastest causal chain (light) must be time-ordered and hence nonsimultaneous (Grünbaum, 1969, pp. 21–22.) This being the basis of Grünbaum's coordinative definition of nonsimultaneity, it is available in his version of the Newtonian world "to permit the inference that E and E' are paradoxically nonsimultaneous," despite his claim to the contrary (1969, p. 23).

Having just adduced one argument which purports to show that the Newtonian world as described by Grünbaum is Riemann-conventional, I shall now consider a second. In his rejoinder to Grünbaum, Salmon, and van Fraassen (abbreviated "G. S. & F." below), Ellis argues against their[9] attempted demonstration of the contrary conclusion by trying to show that

> . . . if time in a Newtonian world is a quantity for which the basic ordering relationships are considered to depend for their determination upon local comparisons, then distant simultaneity in such a world is Riemann-conventional, and the conclusions of G. S. & F. are false.
>
> If, on the other hand, the time order is defined by the causal order and not say the *local* entropic order, then in the Newtonian world, the basic time-ordering relationships do *not* depend only on local comparisons, and the argument is irrelevant. Our claim was that distant simultaneity is conventional in the way that any relationship of quantitative equality *which depends upon local comparisons* is conventional. It is irrelevant to show that it is conventional in a way different from that of some relationship of quantitative equality which in some possible world would not depend on local comparisons (1971, p. 179).

In section 2 of his paper Ellis explicates "the temporal order at any place A" in terms of "the local entropic order for certain classes of closed and isolated systems at A." Since a "clock" in the sense in which I have been using the term provides an instantaneous measure of the entropy of such systems, I can incorporate Ellis's explication. Then I can define the *distant* entropic order in terms of transported clocks.

This allows me to clarify the role of my conclusions in (a) and (b) above.

gards the weight of a chicken . . . as . . . intrinsic to the chicken" (pp. 337–8). Even in the case of the butcher, I think some preliminary verbal clarification is needed, if only to determine whether he might distinguish in any way between what is measured by a spring balance and by a beam balance; but this is minor, of interest chiefly for the moral that verbal usage ought never to be taken quite for granted, except under the most closely controlled conditions. As to your *harder* problem, I think it may in one way be a quite *impossible* one, because ill-defined—but therefore no problem at all; and in another way (namely if, through adequate explication, it becomes well-defined), perhaps in principle not so hard. For consider: when the butcher's usage (and the intended sense of "intrinsic") has been fixed, the answer to the butcher question is logically determined by the body of knowledge codified in the theory of gravity (Newton's will serve the purpose). I ask, if the use of the phrases 'metricality of space-time' and 'ontologically absolute or relational' is made entirely clear, should we not expect that the answer to your question will also be determined by the content of the relevant physical theories—say, the general theory of relativity? Of course, the discussion of the theory—criteria for its application to phenomena, derivation of consequences, evaluation of evidence, etc.—may involve hard problems; but assuming these under sufficient control, it would seem prima facie that your ontological question, *if* it is clear, ought to be answered with comparative ease (unless, that is, contrary to what seems to me at all likely, the answer should turn out to depend upon further difficult *purely mathematical problems*; in which case these should at least be precisely posed). On the other hand, if the ontological question is not clear, there is no point in speaking of it as hard.

This complaint, lack of clarity, is just the one I have previously made—see, e.g., your quotation from me on pp. 328–9, and the end of my letter to you of February 16, 1975—against your attempt to explicate "intrinsic" in "Space, Time, and Falsifiability"; and in my letter, I excused myself from extended discussion of the matter, in part because, as I wrote, "I think it possible that you may have come to similar conclusions yourself." You have not confirmed or rejected this conjecture; but since you do not appear to rely, in your present discussion, on that earlier attempt, I shall still refrain from detailed comment on it. However, elaboration of my similar complaint against your latest discussion now seems obligatory. I have already indicated the general point, which can be put as follows:

Although I believe that in your quotation from *Faust* (p. 348) it is the student's sentiment you commend (*"Doch ein Begriff muss bei dem Worte sein"*), not that of Mephistopheles (*"eben wo Begriffe fehlen, da stellt ein Wort zur rechten Zeit sich ein"*), your practice in connection with certain crucial words—notably, 'ontological' or 'ontologically constitutive,' and 'metrical' or 'geometric'—seems to me rather of the devilish kind. I want to be unmistakably clear about this: I am not objecting to the fact that your use of these terms is different from mine—in such matters I am an extreme libertarian, believing not only that the utterer is entitled to any usage he finds apt, but also that the auditor has a certain obligation to be open and attentive, and to try to construe what is said in the way it is intended. (Just for this reason, I think it bad practice, except in connection with technical terms of quite established scientific usage, to assume that a word can be employed in systematic discussions without any need for elucidation.) What I object to is that, as it seems to me, you use the terms in question in a way that *is not clear at all*; or—to state a more modest claim, but with greater assurance—it not only "seems to me" but is indisputably true that you use those terms in a way that *is not clear to me*.

Perhaps it will be a help, in trying to give a more detailed account of what puzzles me about your point of view, if I first briefly sketch my own answer to the question raised a few pages back ("By virtue of what is the tensor-field g to be regarded as representing the metrical structure of space-time?" or: "Just what do we mean when we say that g describes the metrical structure of space-time?"). I do not think my answer differs very much, in substance, from yours: it has a good deal to do with the theory of the behavior of such things as measuring rods and clocks. But I would begin by making a preliminary, and in my opinion quite crucial, remark. This is that there is no "categorical" (in Kant's sense: derived from "categories" or "reine Verstandesbegriffe")—no innate, a priori, or (in your language) "canonical"—notion of *the metric of space-time*. Indeed, nobody before Minkowski employed such a notion at all. And when Minkowski invented, or discovered, this concept, what exactly did he do? He showed that the special-relativistic theory of space and time was tantamount to the statement that space-time has a particular structure, whose attributes are suggestively (although not perfectly) analogous to those of a Euclidean metric structure (thus, by the usual and useful liberty one takes in mathematics to *extend* or "generalize" a notion, a structure is simply a single universal time" (1972, p. 118). But anyone who has read Newton's *Principia* knows that, as a description of his mechanics, this is at best misleading and at worst false. For in addition to what he calls "absolute, true and mathematical" time (also called "duration"), he allows that there exists "relative, apparent, and common time, [which] is some sensible and external (whether accurate or unequable) measure of duration by the means of motion, which is commonly used instead of true time. . . ." (Newton, 1966, p. 6). Thus, contrary to what Weingard (1972, p. 119) would have us believe, we already have in Newtonian mechanics the "notion of time [with] respect to a frame of reference." The difference vis-à-vis special relativity is that this time, if accurate, agrees with universal time; in special relativity, there is, of course, no such notion as universal time. At a practical level the problem exists just as much for the Newtonian as for the (special) relativist to find a proper frame of reference in which to record the time: for the Newtonian it would be the true or universal frame, whereas for the relativist it would be an inertial frame. (One cannot simply choose "a frame of reference whose space axes are at rest with respect to the physical system," as Weingard (*Ibid.*) seems to think, as that frame might be noninertial.)

Finally, let us examine the implications of all this for the possibility of negative (one-way) signal or transport times in Newtonian mechanics. In the situation in which something travels from event b to event a according to the universal frame, one cannot say in Newtonian mechanics, as Weingard tries to, that an observer who judges by the experiences of his rest frame that events at b occur after those at a "would be wrong since events at a are simply after those at b" (*Ibid.*) To say this one would have to know that his frame is equivalent to the universal frame. But this is a proposition which one might not know (much less know simply) in Newtonian mechanics, just as in special relativity one might not know whether his frame is inertial. Therefore, Weingard's objection is obviated, and Ellis can assert in Newtonian mechanics, just as well as in special relativity,[12] the possibility of negative times in the following sense: if, with respect to the time in an observer's rest frame, events in that frame are temporally ordered one way (with respect to earlier-later) while they are ordered the reverse way with respect to a second observer's frame of reference, then with respect to the second observer's frame of reference the first is going back in time (paraphrase of Weingard, *Ibid.*). Indeed, I do not see how Weingard can consistently deny for Newtonian mechanics the possibility

of going backward in time when he later says that one can divorce the ideas of backward time travel from the particular space-time structure of special relativity (1972, pp. 119–120).

3. Conclusion

Ellis's answer to Grünbaum, Salmon, and van Fraassen on the simultaneity of Newtonian mechanics was complete and cogent only in response to Salmon. In reformulating it so as to make clear its applicability to Grünbaum and van Fraassen, I have no doubt rendered it incomplete. However, the reformulation is easily completed in the way I have just indicated. Further, I have argued that Grünbaum's position on simultaneity in Newtonian mechanics, insofar as it excludes the determination of simultaneity through the use of gravitational influences and transported clocks, suffers difficulties of its own: that simultaneity can be interpreted as being Riemann-conventional, notwithstanding his claims to the contrary.

Notes

1. 1973, p. 353. Ellis and Bowman (1967, section 1) derive an expression for one kind of nonstandard signal synchronization of the clocks of any inertial frame.

2. The construal mentioned above occurs in Grünbaum (1969, p. 21), who uses a clock as a signal. This conflation produces a certain difficulty, as I point out in subsection 2.a below.

3. A "first-signal" is, according to Reichenbach, "the fastest message carrier between any two points in space" (1958, p. 143).

4. A "quasi-Newtonian universe" is a world described by Newtonian mechanics except that the speed of light limits all causal chains; thus initially synchronized clocks continue to give the same readings despite being separated and rejoined.

5. In my case, at least, this is what I shall call "preanalytic acceptance," i.e., acceptance based on the way scientists actually operate. It is to be distinguished from acceptance based on the rational reconstruction of what scientists do. Since the behavior of scientists is sometimes based on mistaken judgments, it obviously cannot be the ultimate criterion for what is correct. However, it can serve as an interim criterion in the absence, for whatever reason, of an ultimate one. In other words, scientists behave rationally until they are proved to be mistaken.

6. I can mention two simple contexts in which the transport of clocks fails to provide a legitimate physical means of synchronization. The first is Reichenbach's "static gravitational field" (1958, p. 259), a static non-Euclidean space with an orthogonal time axis; i.e., the components of the field, $g_{\mu\nu}$, are functions of spatial coordinates alone (they are independent of time) and $g_{\mu 4} = 0$ for $\mu = 1, 2, 3$. The second is a special case of Reichenbach's "stationary gravitational field" (1958, pp. 261–262). The latter differs from the static in that the time axis is not orthogonal; i.e., $g_{\mu 4} = 0$ for $\mu = 1, 2, 3$. The special case results from making the space Euclidean, i.e., letting $g_{\mu\nu} = 0$ for $\mu \neq \nu = 1, 2, 3$. For this case Eddington (1924, p. 15) shows that even synchronization by infinitely slow transport along a straight line is impossible.

7. 1966, p. 398. In his elaboration he mentions some such qualities: gravitation (i.e., the mutual attraction of bodies) and what are now called the "primary qualities" of Newtonian mechanics (extension, hardness, impenetrability, mobility, and inertia).

8. 1965, p. 44. Cf. also Bowman, 1972, subsection B.4, part a.

9. Ellis can direct his argument at all three of our detractors, since it applies equally to slowly transported clocks or arbitrarily fast accelerated particles or infinitely fast gravitational influences. In contrast, my argument applies only to particles (it assumes the absence of the other methods), so I have had to restrict its effect to the position of Grünbaum, the only one of the three who seems to preclude gravitational signals. Salmon remains agnostic on them (1969, p. 52n); and van Fraassen does not mention them, although he may imply that consideration of them is not necessary (1969, p. 67, note 1), in which case his position is the same as Grünbaum's.

10. 1969, p. 61. Cf. Bowman, 1972, subsection K.3.

11. Apparently, a situation is a "conceptual possibility" if it can be described "in terms of our present concepts of time, travel, and change," i.e., without "a change in usage or meaning from the ordinary way of speaking" (Weingard, 1972, p. 120).

12. This is true of special relativity only insofar as the present problem is concerned. There are other problems in saying in that theoretical context that things can go back in time, as Earman (1967, section IV) has shown.

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