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SPECIAL RELATIVITY AND THE
CONVENTIONALITY OF SIMULTANEITY

by

KLAUS THOMA

A THESIS SUBMITTED
IN PARTIAL FULFILLMENT OF THE
REQUIREMENTS FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

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INTRODUCTION

This essay is essentially critical and destructive in its overall tenor. My aim is to analyze and re-examine one of the most influential 'schools' in the area of the philosophy of space and time. No reconstructive attempts shall be made nor do I want to pursue such natural follow-up questions as to the correct philosophical interpretation of standard Einsteinian relativity. The scope of my thesis is therefore very narrow and well defined. I intend to isolate the basic, most fundamental tenets and assumptions of Geo-Chronometric Convention-alism (GCC) in order to expose them as paradigms of the kind of 'philosophy' that is conceived in confusion, kept alive with the help of a special, one-sided diet, and resulting in those characteristic "bumps that the understanding has got by running its head against the limits of language"1.

My approach will therefore not be historical or scholarly. I shall take GCC as I find it in its chief proponents. I intend to examine this body of arguments in a fresh light and shall simply 'wonder' anew at the kinds of dilemmas people are apt to encounter "when language goes on a holiday"2.

For these reasons I shall have but little use for the arguments and objections that others have raised against GCC in the voluminous literature in this field. GCC has survived
their attacks because none of these analyses has gone to the heart of the matter. These objections are no doubt valuable in many respects but they have generally missed the mark since they themselves are taking too much for granted in discussing 'alternate metrizations'. Rather than criticizing these critiques I want to deal with GCC directly and exclusively. I firmly believe that pinning down a conventionalistically inclined philosopher to a very specific, narrow, but central set of propositions is the only approach that will ever lead to a fruitful discussion in this slippery realm.
THE TASK AND THE PROGRAM

Two different kinds of analysis have long been recognized in the field of the philosophy of science\(^3\). One of them is primarily concerned with elucidating the logical structure of some particular problem or proposition occurring within certain scientific theories. The other approach is of a more 'synthetic' nature, attempting to draw similarities between different theories, thereby illuminating the structure (if any one is to be found) of 'the scientific enterprise' as a whole. Such essentially meta-scientific analyses usually deal with problems of testability, meaning-invariance, prediction, explanation, etc., in general. The aim and scope of my paper does not fall within that category. I want to examine only one very specific concept, i.e., that of the simultaneity of distant events within one and the same inertial reference frame. I propose to analyze in detail and exclusively the alleged "conventionality of intra-systematic simultaneity".

Based upon some very broad, diffuse assumptions about the nature of individual concepts and language in general, a certain school of philosophers has maintained that Einstein's Special Theory of Relativity (STR) requires and implies a total
rejection of the empirical, non-conventional status of the concept of "simultaneity" even within one and the same inertial frame. It is this very specific claim that I propose to re-examine critically in this essay.

This task will primarily involve us in analyses regarding the exact nature and logical implications of the conventionality of simultaneity thesis itself, rather than its metaphysical roots and origin. I shall attempt to set out its implications within (and its incompatibility with) the framework of orthodox STR since, after all, it is believed to be operative within, and a fully equivalent alternative to, that given framework. Only after we have thoroughly developed the various proposals for conventionalistically 'alternative descriptions' and verified whether or not they are indeed viable, sensible, and consistent proposals, will it be fruitful to add some remarks concerning abstract 'metrizability' and the general role of physical standards, concepts, and theories.

I shall first of all attempt a brief introductory outline of the logical structure as well as the historical development of the claims I want to attack. This prefatory overview will be followed by a more detailed exposition of the various claims involved. Subsequently, I shall analyze these propositions and examine in detail their logical implications, presuppositions, and merits.

The thesis we are about to discuss is generally known as
the "conventionality of simultaneity thesis" (CST). It is a particular branch of (in fact the only thoroughly developed application of) a more general philosophical thesis known as "Geo-Chronometric Conventionalism" (GCC). Even though that term itself is Gruenbaum's, his own particular version of conventionalism shall not be the main focus of this paper.

As has been pointed out by Gruenbaum himself as well as others⁴, Gruenbaum seems to believe to have provided an ontological basis for the 'merely' epistemological features of space and time allegedly uncovered by Reichenbach. Unlike Reichenbach, Gruenbaum thinks that alternate metrizability is not a sufficient criterion for the conventionality of any particular metric. Alternate metrizability is, however, a necessary condition for any brand of conventionalism, particularly with regard to intra-systematic simultaneity and the metric of one-way velocities (OWVs). It is this common core, then, that we shall refer to as GCC.

Disregarding the background of general philosophical arguments concerning the 'conventional' nature of all human discourse, GCC, so far as chronometry is concerned, rests squarely on the assumption that OWVs are not 'empirical magnitudes' at all but contain an inextricable, yet nontrivial, element of arbitrariness and choice. The choice involved here is said to be one among an entire class of equally sensible, equally justified, i.e., non-justifiable, possible alternatives.
Taking as his point of departure Einstein's famous remark\textsuperscript{5} that it is only by definition that the OWV of light is taken to be equal in all directions, Reichenbach went on to claim that any value between infinity and c/2 is equally legitimate, so long as the OWV in the opposite direction is suitably adjusted so as to render the (empirically verifiable) roundtrip velocity (RTV) equal to c. Using Reichenbach's \( \varepsilon \) factor to divide up opposite OWVs we can thus paraphrase the essence of the conventionality of simultaneity thesis (CST): For synchronizing distant clocks one can consistently and without any empirical distortion introduce an \( \varepsilon \) factor such that the reflection time \( t_2 \) of a signal from A to B can be chosen as lying anywhere between the emission time \( t_1 \) and the return time \( t_3 \) at A, i.e.,

\[
t_2 = t_1 + \varepsilon (t_3 - t_1) \quad \text{with } 0 < \varepsilon < 1.6
\]

This suggestion was taken up and expanded upon by a great number of authors. A generalized form of special relativity, i.e., generalized Lorentz transformations, were derived without the 'assumption' of isotropic OWVs\textsuperscript{7}. Subsequently this principle was consistently extended to cover the metric of all OWVs, not just that of light\textsuperscript{8}. An \( \varepsilon \)-generalized reformulation of relativistic mechanics and electrodynamics is currently being attempted\textsuperscript{9}. All along one should keep in mind, however, that the CST has not to do with the relativity of simultaneity between two or more different inertial frames. It rather
concerns itself with the allegedly prior and more fundamental conventionality of simultaneity within one and the same inertial frame\textsuperscript{10}.

The underlying chain of reasoning culminating in the CST can perhaps best be summarized in the following way:

A.1. Only RTVs can ever be measured empirically without already presupposing the motion of distant simultaneity. It is logically impossible to measure the true OWV of light signals with the help of only one clock. Measuring "velocity" means relating measurements from either two points in space and one clock (RTVs only), or from two points in space and two clocks—yet-to-be-synchronized. There is, in principle, no way to establish distant simultaneity which does not already presuppose knowledge of OWVs\textsuperscript{11}.

A.2. There is at least one other method, however, which prima facie seems to be free of logical circularity. It is commonly referred to as synchronization by slow clock transport\textsuperscript{12}. In the last analysis the conventionalistic answer to this argument amounts to an attempt to reduce that procedure to merely one special kind of inter-point signalling process, thereby viewing it as being on a level with, rather than independent of or prior to, the light propagation method\textsuperscript{13}. We are said to possess no independent or overriding reasons for initially setting up intra-systematic simultaneity with the slow clock transport method (naturally leading to $\varepsilon=1/2$)
rather than by commencing with freely stipulating values for \( \varepsilon \) \textit{ab initio}.

A.3. The class of inter-point signalling procedures is therefore thought to be exhaustive of all the permissible kinds of synchronization methods. There is, generally speaking, no conceivable way to physically link distant points in space which, at bottom, does not involve some signalling process between these points. Any such linkage, however, is subject to the inherent 'finiteness of physical propagation velocities'\(^\text{14}\). It is this logical restriction which allegedly precludes the establishment of intra-systematic simultaneity in any empirically unique and logically non-circular manner.

B. We are therefore forced to make stipulations and 'assumptions' of non-empirical, i.e., non-verifiable and non-falsifiable nature about the magnitude of OWVs. The absence of any physical differences between different directions in space is not sufficient to make the 'choice' \( \varepsilon = 1/2 \) the only sensible, natural, and unique one, i.e., the one without conceptually justifiable alternatives.

C. Such 'alternatives' are not only not ruled out \textit{ab initio}, it even turns out that proper limits can be given within which \textit{any} value for the ratio of opposite OWVs can be freely chosen. This can be done consistently and without a chance of empirical falsification. All of the physics
within any given inertial frame can thus be rewritten and described without the 'assumption' of isotropic propagation speeds: "...no distinct hypothesis concerning physical facts is made by the choice \( \varepsilon = 1/2 \) as against one of the other values"\(^{15}\).

All of these claims shall be examined in due course. I shall not follow that same order, however, and will begin with step (B) of the conventionalistic argument. Even if the basic premises (A.1) through (A.3) were sound and unassailable, the resort to stipulations, or, what is the same thing, the absence of overriding 'good physical reasons' for retaining one and only one particular 'assumption' does not automatically follow. This is a much neglected topic in the literature of the CST. I shall therefore try to assemble various physical arguments for the preferential treatment of isotropy. It is, so I shall argue, not just the ledger of bare space-time coincidences of geometrical points that would be affected by the conventionalistic proposals, it is the entire web of genuinely physical concepts that would have to go with it. Conventionalists have traditionally failed to even perceive the need for such vast conceptual (rather than purely quantitative) 'metrical' alterations. These kinds of arguments and objections shall constitute the core of chapter III of this essay.

Chapters IV, V, and VI shall then attack the basic
presuppositions (A.1) - (A.3) head on. I shall argue that the conventionalistic restriction of the class of permissible synchronization procedures to inter-point signalling processes is unduly narrow and entirely unwarranted. I shall first deal briefly with (A.2) and the conventionalists' alleged rebuttal of the slow clock transport argument. In chapter V I will then tackle (A.3) directly or, more precisely, undercut (A.3) completely. A new, logically independent and operationally unambiguously verifiable synchronization procedure shall be outlined which is not subject to the vicious-circle argument mentioned in (A.1).

With that the primary task of this essay is essentially completed. There will be no need to dissect in great detail the heavy epistemological and ontological machinery surrounding the CST. Such diagnostic efforts have largely proved futile in the past, mainly because the direct counterarguments assembled in this thesis were unavailable. Chapter VII will therefore close our discussion with only a cursory treatment of some of the more blatant philosophical confusions which have contributed to the epistemology of the CST. Our treatment there will necessarily be very brief and open-ended. Reconstructive remarks, however, must remain outside the scope of this essay.

First of all, however, I would like to set out in chapter II what the specific contentions of the CST are and how they
have been shaped by the various contributions of its chief proponents.
REFERENCES TO INTRODUCTION AND CHAPTER I


2 Ibid., §138.


10 Winnie, op. cit., part I, p. 81.


II.

THE CST AND ITS HISTORICAL DEVELOPMENT

It is of crucial importance to distinguish clearly between the Einsteinian relativity of simultaneity and the Reichenbachian 'conventionality of simultaneity'. Only the former has anything to do with what one might call 'the denial of absolute simultaneity' (relative to all inertial frames). The latter, however, is a denial of the unique, empirically verifiable nature of the concept of a 'correct magnitude' of OMY relative to any one inertial frame.

The term "relativity of simultaneity" used in this context refers to the fact that "distant events which are simultaneous in a given inertial frame of reference are not simultaneous events in any other inertial reference frame in constant (non zero) motion with respect to the first".1 The CST, on the other hand, is thought to clarify "the status of simultaneity within an inertial frame of reference by virtue of the claim that the relation of simultaneity within each inertial reference frame contains an ineradicable element of convention which reveals itself in our ability to select (within certain limits) the value to be assigned to the oneway speed of light in that inertial frame."2
The separation of the relativity of simultaneity from the CST allows us to make the following 'assumptions':

1. The CS-thesis is a claim about light propagation in the rest-frame. It has nothing to do with complications introduced through the inertial motions of the frame as a whole, nor need we worry about possible agreement or disagreement with observers from other frames of reference. The CST purports to make a point about the spreading of simultaneity via the propagation of light or other 'first signals' within one and the same reference frame. For this reason we may simply call the inertial frame in question 'the rest-frame'.

2. In it 'good local clocks' may be assumed to exist at all and any points. Furthermore, a net of unit rods may be assumed at relative rest and of equal lengths everywhere. There are no 'relativistic effects' due to the motion of the frame itself to contend with.

3. In principle, any local clock will do. Even if we restrict ourselves to 'light clocks', however, certain things are not called in question. 'Light clocks' here are rigidly connected mirrors between which light bounces back and forth. It is important to keep in mind here what is and is not called in question by the CST. The uniformity of time, for example, is taken for granted. That, e.g., the intervals between subsequent light pulses remain the same is presupposed, similarly the constancy of the time intervals between each series
of emissions and returns. In principle, then, all local 'light clocks' can be made to run, or be presupposed to run, at the same 'pace'.

4. The linear uniformity or inertial nature of the propagation of light is also not called in question. All signals emitted from some point O are still presumed to travel at a uniform and constant rate of speed. Conventionalists do not want to claim that they experience varying accelerations (except for the discontinuous reversal upon reflection) even though they are not directly 'observed' along their way not to do that either. Furthermore, it is not 'doubted' that light signals always travel in straight lines between two points O and A, even though, once more, they are strictly observed only in O and A but not in between. What the CS-thesis does allege, however, is the conventional freedom, nay necessity, to stipulate the magnitudes of the uniform rates of speed along different paths and in opposite directions. All we can ever measure or 'observe' are the local coincidences of complete 'units' of emissions and returns. In other words, all we can ever 'measure' in that sense are round-trip velocities (RTVs) of signals. It remains entirely up to 'choice', so it is claimed, to subdivide such units as to one-way ratios.

This alleged freedom to assign the initial settings of intra-systematic simultaneity, i.e., the introduction of non-isotropic OWVs, coupled with the claim that such 'assumptions'
will lead to no empirical differences with regard to customary physical descriptions and predictions, constitutes the CS-thesis in a nutshell.

As will become apparent once the historical expository part is complete, the CST basically claims that different sets of observers within the same reference frame, i.e., at rest with respect to one another, may each set their clocks differently upon the arrival of one and the same 'synchronizing' signal:

\[ t_o - \frac{\Delta x}{c^2} (2 \varepsilon - 1) \]

\[ t_o + \frac{\Delta x}{c} (2 \varepsilon - 1) \]

All C clocks are taking into account the 'standard delay' of the synchronizing light signals. Emitted from O at time \( t_o \), each C clock will be set to \( t_o + \Delta x/c \) upon arrival of the signal such that, at any subsequent time, all C clocks will show identical \( t_1 \) readings at any given instant. The \( C^* \) clocks, on the other hand, do not agree with these readings portrayed by their C clock counterparts, even though they are set with the help of the same signal. Their dials are set, however, as if the signal emanating from O had traveled with a speed other than \( c \). As we shall see shortly, according to the CST formalism each \( C^* \) clock would be out of phase compared to a C clock at the same position by an amount of \( \pm \Delta x (2 \varepsilon - 1)/c \).
Such differing clock settings, it is claimed, would lead to physical measurements of velocities, lengths, etc., which are different but entirely consistent within each set. No one class of observations and measurements could be called 'truer' or more correct than any other. Each of these 'synchronizations' is said to be equally 'justified' and will never lead to contradictions or clashes with unambiguous observations. Non-standard synchronizations are therefore said to be totally indistinguishable from, and entirely equivalent to, standard synchronization as far as their verifiable 'empirical content' is concerned.

We shall now endeavour to briefly trace the development of the CS-thesis through the work of its chief proponents. Although Einstein cannot properly be said to belong to this group I shall begin with a summary of the position attributed to him by the conventionalists.

The 'conventionalistic Einstein':

The first section of Einstein's 1905 paper on Relativity is interpreted as having established the following points:

1. Temporal order, i.e., 'time', is inherently 'local'. Any counting device or periodic series of events ('periodic' here meaning issuing from and returning to one and the same point) will constitute a 'local clock'. Only events in the immediate neighborhood will be directly 'measurable' on such
clocks. In principle, 'good local clocks', meaning periodic mechanisms of identical frequency, can be established or be assumed to exist at any point.

2. Distant 'good local clocks' need to be connected, then, by some kind of physical linkage before their strictly local 'times' can be related to any kind of common temporal metric.

3. If discrete pulses or signals of some finite propagation speed are used for providing this physical linkage, then the OWV of these signals must already be known before we can 'spread simultaneity' or synchronize clocks in this way.

4. Given the above definition of 'local time', such knowledge of OWVs becomes a logical impossibility -- as long as we have no other means for empirically establishing distant simultaneity (or no independent reasons for 'assuming' it). As long as we restrict ourselves to discrete point-like signals as the only permissible synchronization linkages we are forced to concede that, other things being equal, we 'merely assume' the OWVs in different directions to be isotropic and equal to the round-trip value. (Whether or not this 'assumption' can be justified on the basis of other criteria shall be discussed in later sections).

Considered not as a subthesis of GCC but standing on its own, the CS-thesis, if built upon that basis, is essentially a misinterpretation of Einstein's STR and needs to be discussed
and treated in that context. The following famous passage from Einstein's original 1905 paper forms the historical as well as logical core and basis of the CS-thesis:

"... it is not possible without further assumptions to compare, in respect of time, an event at A with an event at B. We have so far defined only an 'A time' and a 'B time'. We have not defined a common 'time' for A and B, for the latter cannot be defined at all unless we establish by definition that the 'time' required by light to travel from B to A." (Italics in the original)³

This passage, in isolation, is easily misconstrued. It is the words "so far", found in the second sentence, which ought to have been italicized as well. This would serve as a reminder that this passage is geared only to the context of the particular paradigm or example Einstein is elaborating. Conventionalists, however, generalized the "by definition" phrase into an iron-clad philosophical principle without paying sufficient attention to sense or context. As Gruenbaum reads it:

"In discussing the definition of simultaneity, Einstein italicized the words 'by definition' in saying that the equality of the to and fro velocities of light between two points A and B is a matter of definition. Thus, he is asserting that metrical simultaneity is a matter of definition or convention."⁴

Conventionalists place much too much emphasis on the short passage quoted above, an emphasis that is not borne out by
Einstein's finished product, i.e., his STR. In addition, much of the conceptual force apparently conveyed by this passage is simply due to a simple translational error. In the original German edition Einstein never claimed that the relationship of a common 'time' can **never** be defined at all unless ... etc. The German text simply reads: "Die letztere Zeit kann nun definiert werden, indem...". Perry and Jeffrey totally distorted this sentence by translating "**nun**", meaning "now" as if Einstein had written "**nur**", i.e., "only" or negated, "not at all". This translational error has recently been acknowledged by the editor himself. 5 I do not want to make much of this purely linguistic point but it should be kept in mind whenever conventionalists claim to be the faithful executors of Einstein's very own words and meanings. If this passage is to be the cornerstone and foundation of the CS-thesis then let us at least get the initial facts straight: Even in explicating the features of his particular, highly restrictive Gedankenexperiment, Einstein never claimed more than that "time can **NOW** be defined by establishing by **definition** that... etc."

Reichenbach

At this point Reichenbach went on to claim that
5. There is no conceivable way ever to verify or falsify this 'assumption' about the isotropy of OWVs. He explicitly
ruled out clock transport synchrony and also what he called linkages of "infinite limiting velocity"\(^6\), thereby elevating synchronization by discrete \(\phi\)W signals to a logically unique and supreme position. Given that basis, the 'choice' of isotropy seems to be a forever non-verifiable 'assumption'.

6. Going beyond even that point, Reichenbach added the following claim: not only must we make an 'assumption' concerning the magnitude of \(\phi\)WVs before we can synchronize distant clocks, we can even make any 'assumption' we like, as long as the combined values for the to and fro travel times add up to the customary RTV or light.

"First, it is impossible to ascertain whether two distant clocks are set 'correctly' in their indication of time; second, they can be set arbitrarily and yet no contradiction will arise.

... It can easily be seen that the time comparison of distant events is possible only because a signal sent from one place to another is a causal chain. This process leads to a coincidence, i.e., a comparison of neighboring events, and from the time measurement thus obtained we can determine the time of the distant event only with the help of an inference. What assumptions are contained in this inference?

This inference requires besides the knowledge of the distance also the knowledge of the velocity of the signal. How can this velocity be measured?

In principle, there exists only one method, which we shall schematize as follows. The signal leaves a point \(P_1\) at the time \(t_1\) and reaches a point \(P_2\) at the time \(t_2\). Its velocity is given by the quotient of the time interval \(t_2-t_1\) and the distance \(P_2-P_1\). Therefore, two time measurements are required
which have to be made at different places. We can think of them as given by two clocks located at \( P_1 \) and \( P_2 \). If the indication of the time interval \( t_2 - t_1 \) is to be meaningful, however, the two clocks must have been synchronized previously, i.e., it must have been determined whether their hands occupied the same positions at the same time. In order to measure a velocity, therefore, the simultaneity of distant events must already be known.

...Thus we are faced with a circular argument. To determine the simultaneity of distant events we need to know a velocity, and to measure a velocity we require knowledge of the simultaneity of distant events. The occurrence of this circularity proves that simultaneity is not a matter of knowledge, but of coordinative definition, since the logical circle shows that a knowledge of simultaneity is impossible in principle.

We also notice that the second characteristic of a coordinative definition, namely its arbitrariness, is satisfied. It is arbitrary which time we ascribe to the arrival of the light ray at \( B \).

...This definition determines at once the velocity of light and simultaneity, and such a determination can therefore never lead to contradictions. If we wish to determine by velocity measurements which events are simultaneous, we shall always obtain that simultaneity which has already been introduced by definition.

It is this consideration that teaches us how to understand the definition of simultaneity given by Einstein

\[
t_2 = t_1 + 1/2(t_3 - t_1) \quad (1)
\]

which defines the time of arrival of the light ray at \( B \) as the mid-point between the time that the light was sent from \( A \) and the time that it returned to \( A \). This definition is essential for the special theory of relativity, but it is not epistemologically necessary. Einstein's definition, too, is just one possible definition. If we were to follow an arbitrary rule restricted only to the form

\[
t_2 = t_1 + \varepsilon(t_3 - t_1) \quad 0 < \varepsilon < 1 \quad (3)
\]

it would likewise be adequate and could not be
called false. If the special theory of relativity prefers the first definition, i.e., sets $\mathcal{E}$ equal to $1/2$, it does so on the ground that this definition leads to simpler relations. It is clear that we are dealing here merely with descriptive simplicity, the nature of which will be explained in §27. The arbitrariness is restricted only by condition (3) which specifies that $t_2$ must lie between $t_1$ and $t_3$; otherwise the signal would arrive at $B$ at a time earlier than its departure from $A$.

The transition between the last two points is by no means self-evident. Even if it were true that the OWVs of light signals cannot be measured without making prior 'assumptions' about distant simultaneity, it does not follow that they do not in fact possess a unique velocity. Apart from allegedly deeper epistemological and ontological underpinnings, the CS-thesis stands and falls with the feasibility-in-fact of alternate metrizations of OWVs which do not engender empirical contradictions. Throughout this survey I shall already pass some preliminary judgment on the few examples of actual 'alternatives' given by the various proponents of the CS-thesis. Einstein, as we have seen can at most be credited with having espoused a weak CS-thesis. All the proponents of the stronger claim, however, regardless of the epistemological or ontological conclusions they care to draw from this thesis, will have to prove the possibility of alternative 'assumptions' that are consistent not only with one particular context but with all of 'customary physics'.

Let us note then that the only example of such an 'alternative' ever presented by Reichenbach himself is precisely one that will in fact not stand up to scrutiny. In (8) pp. 162 Reichenbach attempts to define a non-standard signal synchrony with ε given by the following equation:

$$\varepsilon = \frac{c}{2 \left[ a \cdot \cos \varphi + (c^2 - a^2 \sin \varphi)^{1/2} \right]}$$

In this formula ϕ stands for the direction of the ray, a is an arbitrary constant, and c is the numerical value of the RTV of light. As has been pointed out by others (9), this 'non-standard alternative' is demonstrably inconsistent unless a=0, i.e., ε = 1/2. As Edwards put it10:

"Reichenbach used such a contradictory synchronization, though he apparently wished to retain the principle of causality. If the value of his parameter 'a' is taken so that the speed of light in the positive x-direction is 3/2c then, for instance, the value of the speed of light of a signal emanating from the negative x-axis and traveling parallel to the y-axis to the point equidistant from the two axes is -3.1c, a negative value!"

Gruenbaum

Gruenbaum has contributed more to the philosophical underpinnings allegedly supporting the CS-thesis than to the detailed technical development of this thesis itself.
As I mentioned before, the alleged 'ontological lessons' uncovered by the CS are of secondary importance at this point. We first want to set out the much-heralded 'discovery' in all its details and see whether or not it in fact satisfies the absolutely necessary requirement of providing genuine, physically unobjectionable 'alternative descriptions'.

Looking at his contributions from this angle it is safe to say that Gruenbaum merely elaborated on points already made by Reichenbach, perhaps stating them in a bit more complicated fashion:

"... consider four events $E_1$, $E_2$, $E_3$, and $E$ which satisfy the following conditions represented on the accompanying events diagram ('world-line' diagram), from which the 'arrow' of time has been omitted: (1) $E_1$, $E_2$, and $E_3$ are causally connectible by a light ray in vacuo, and $E_2$ is temporally between $E_1$ and $E_3$. (2) $E_1$, $E$, and $E_3$ are causally connectible other than by a light ray, $E$, being temporally between $E_1$ and $E_3$; these three events all occur at the same space point $P_1$ of an inertial system, whereas $E_2$ occurs at a different space point $P_2$ of that same inertial system. Furthermore, let $E_x$, $E_y$, and $E_z$ be events which sustain the relations:
of temporal betweenness to $E_1$ and $E$ and to one another, as shown on the diagram.

Similarly, let $E_\alpha$, $E_\beta$, and $E_\gamma$ be temporally between $E$ and $E_3$. The given facts that temporally $E_2$ is between $E_1$ and $E_3$ and that $E$, $E_\alpha$, $E_\beta$, $E_\gamma$, $E_\delta$, and $E_\epsilon$ are each between $E_1$ and $E_3$ as specified do not furnish a basis for any relations of timelike separation or topological simultaneity between $E_2$ and any one of the events at $P_1$ lying within the open interval between $E_1$ and $E_3$. Whether the latter relations exist will depend, therefore, on whether it is physically possible for there to be causal chains of which $E_2$ and a particular event in the open interval at $P_1$ would be the termini."

I shall later argue that there are indeed such 'causal chains' which do not violate the STR 'discovery' of the finite propagation speed of physical signals. Gruenbaum is therefore distorting the essential difference between Einstein's STR and Newtonian mechanics by injecting the element of 'conventionality' into 'relativity':

"Newtonian physics grants that no light ray or other electromagnetic causal chain could provide a link between $E_2$ and $E_\alpha$ or $E_\beta$ and $E_\epsilon$. But it adduces the second law of motion to assert the physical possibility of other causal chains (such as moving particles) that would indeed furnish these links. It is precisely this latter possibility that is denied by Einstein's STR. The STR enunciates the following topological postulate, to which we shall refer as the 'limiting assumption': it is physically impossible for there to be causal chains that would link $E_2$ with any event lying within the open time interval $E_1 E_3$.

"In Newtonian physics there is no scope whatever for choice of the event that can warrantedly be metrically simultaneous with $E_2$. By contrast, the physical facts postulated by relativity require the introduction, within a single inertial frame $S$, of a convention specifying which pair of topologically simultaneous events at $P_1$ and
P2 is to be metrically simultaneous.\textsuperscript{12}

What Gruenbaum is claiming here may perhaps be true of discrete 'point-particle signals' but there are other physical linkages between distant points which do allow for unique and empirically unambiguous OWV measurements within a given inertial frame. But more of this in Chapter V.

Gruenbaum is also more explicit than Reichenbach about Einstein's alleged rejection of clock transport as a means for establishing \textit{intra-systematic} synchronization of clocks:

"Einstein's repudiation of Newton's belief in the uniqueness of distant simultaneity consists in (1) rejecting clock transport as a basis for absolute metrical simultaneity and (2) asserting the nonuniqueness of the \textit{absolute relation of topological simultaneity}.

"The behaviour which Einstein attributed to transported clocks thus does not make for relations of absolute simultaneity among spatially separated events.

... Einstein ... had postulated that the transport of adjacently synchronized clocks does not furnish a physical basis for the obtaining of relations of simultaneity."

"It also led Einstein to conclude that even within a single inertial system the simultaneity of two spatially separated events E and E* cannot be based physically on the criterion that the numerical readings of two clocks U1 and U2 be the same for event E occurring at the location of U1 and event E* occurring at the place of U2, the clocks having previously been transported to these separate places from a common point in space at which they had identical readings."\textsuperscript{13}
Although Gruenbaum fails to tell us where precisely Einstein ever expressed such a rejection, the meaning, the import, and the validity of these claims shall be dealt with in a separate chapter on intra-systematic clock transport synchrony.

Thirdly, Gruenbaum very strongly commits himself to the thesis that choices of \( \varepsilon \) other than 1/2 can in no conceivable way ever be shown to come in conflict with possible empirical determinations of OWVs and that such 'choices' are, apart from mere symbolic simplicity, all equally viable and justified.

"... must we not admit "at least among friends!" that the value 1/2 is more 'true'? The contention that either the isotropy of space of Occam's 'razor' is relevant here is profoundly in error, and its advocacy arises from a failure to understand the import of Einstein's statement that 'we establish by definition that the 'time' required by light to travel from A to B equals to 'time' it requires to travel from B to A.' For, in the first place, since no statement concerning a oneway transit time or oneway velocity derives its meaning from mere facts but also requires a prior stipulation of the criterion of clock synchronization, a choice of \( \varepsilon = 1/2 \), which renders the transit times (velocities) of light in opposite directions unequal, cannot possibly conflict with such physical isotropies and symmetries as prevail independently of our descriptive conventions.

And, in the second place, the canon of simplicity, which we are pledged to observe in all of the inductive sciences, is not implemented any better by the choice \( \varepsilon = 1/2 \) than by any one of the other allowed fractional values; for no distinct hypothesis concerning physical facts is made by the choice \( \varepsilon = 1/2 \) as against one of the other permissible values... On the contrary, it is the postulated fact that light is the fastest signal which assures that
each one of the permissible values of $\varepsilon$ will be equally compatible with all possible matters of fact which are independent of how we decide to set the clock at $P_0$. Thus, the value $\varepsilon = 1/2$ is not simpler than the other values in the inductive sense of assuming less in order to account for our observational data, but only in the descriptive sense of providing a symbolically simpler representation of the theory explaining these data."

Fourthly, Gruenbaum may have been the first to explicitly derive the CS-formulas for 'moving lengths' measurements. After all, one of the standard methods of measuring OWVs with the help of only one clock consists in timing the passage of, e.g., a rod of known length over one point of the ground frame. Therefore, it must also be assumed that length be direction dependent in order to 'produce' the imputed velocity differences. As Gruenbaum stated in the Encyclopedia of Philosophy article:

"More generally, as yet unpublished computations which Gruenbaum has carried out show that the dependence on the length $\lambda$ of the moving rod in $S$ on the criterion of simultaneity (value of $\varepsilon$) chosen in $S$ is given by

$$\lambda = l \cdot \sqrt{1 - \beta^2} \cdot \frac{\varepsilon}{c + \sqrt{2\varepsilon - 1}}$$

an expression which reduces to $\lambda = l \cdot \sqrt{1 - \beta^2}$ for $\varepsilon = 1/2$, as required by the Lorentz transformations of the STR."\(^{15}\)

As we shall see, however, it was Edwards and Minnie who carried out the program of $\varepsilon$-generalizing the Lorentz transformations.
Edwards

In 1963 Edwards published what amounted to the first detailed, technical elaboration of the CS-thesis. He derived some 'generalized Lorentz transformations' which he claimed were equally 'correct' since "corresponding with the infinity of possible clock synchronizations". All of them were said to "give the same results when applied to observable effects". His conclusion was (and I would like to emphasize the restrictive caveat implied in his final assessment) that "using electromagnetic means alone, the measurement of the one-way speed of light or the establishment of an absolute simultaneity cannot be established." (Italics supplied).

There are, however, some flaws in Edwards' derivation of the 'generalized Lorentz transformations' which, to my knowledge, have gone unnoticed. I shall quote here from Edwards' introductory section entitled "Definitions":

"The two-way speed of a light pulse traveling from point O to point A and by reflection, back to O, is an observable quantity that does not depend upon the definition of simultaneity for only the clock at O need be used. Using the definition of time lapse, the following is seen to be an identity:

\[ t_{OA0} = t_{0A} + t_{AO} \]  \hspace{1cm} (1)

where the subscripts indicate the path of the light signal and the first and last subscripts identify the clocks that are read when the signal reaches those points."
Fig. 1. The 'circulation' path of a light signal emanating from the origin and traversing path OABCO by reflection from mirrors. It is assumed that an experiment would show that the time taken in traversing such a path would give the value \( c \) for the average speed. This thought experiment is used to find the velocity of light as a function of direction.

Using the definition of speed and letting \( c \) indicate the two-way speed of light, we find that

\[
\frac{2x}{c} = \left( \frac{x}{c_{OA}} \right) + \left( \frac{x}{c_{AO}} \right)
\]

or

\[
\frac{1}{c_{OA}} + \frac{1}{c_{AO}} = \frac{2}{c}
\]

\[ (2) \]

The choices of \( c_{OA} \) and \( c_{AO} \) are restricted in such a way that the sense of 'cause' is preserved. In other words, a light signal starting at 0 cannot reach A before it leaves 0. Since \( t_{OA} \) must be positive for such signals connected events, so much \( c_{OA} \) be positive; and since this also holds for \( c_{AO} \), we see from Eq. (2) that neither can be smaller than \( c/w \). Therefore, the speed of light in any direction indicated by the direction cosines \( \alpha, \beta, \gamma \) has the restriction

\[
\frac{c}{2} \leq c(\alpha, \beta, \gamma) \leq \infty
\]

\[ (3) \]

It should be clear that the choice of synchronization of clock A with respect to clock 0 determines the value of \( c_{OA} \), or vice versa, and \( c_{AO} \) follows
from Eq. (2). Thus, we can use the ideas of clock synchronization or one-way speed of light interchangeably.

Further limitations on the arbitrariness of clock synchronization are found by imagining an experiment where a light signal from O reflects from mirrors appropriately placed so that the path is OABC0, as in Fig. 1. One can easily imagine light traversing each of the straight paths in such a way that the two-way speed for each path is equal to c but the 'circulation' speed, \( c_{OABC0} \), is equal to \( \infty \). This would occur if \( c_{OA} = c_{AB} = c_{BC} = \infty \) and \( c_{OC} = c_{CB} = c_{AO} = c/2 \), so that conditions (2) and (3) are met.\(^{18}\)

First of all we notice that Edwards, unlike Reichenbach and Gruenbaum, includes the boundary values 0 and 1 for \( \varepsilon \). In other words, he is using \( 0 \leq \varepsilon \leq 1 \) instead of \( 0 < \varepsilon < 1 \). That in itself presents us with the conceptual oddity of finding it conceivable to speak of an 'infinite velocity' at all. This already was an oddity in the context of Newtonian mechanics; it becomes an outright absurdity in the context of electromagnetic field theories where the signals or 'disturbances' involved do not just 'cross space' but transfer precisely defined quantities of energy and momentum. These concepts lose their sense and foothold if we want to re-admit 'infinitely fast' signals in a field-theoretical context.

In addition, Edwards' reasoning is beset with other technical errors. A 'circulation' speed \( c_{OABC0} = \infty \) is not 'easily imaginable' because it would mean that any signal is 'received' in 0 the very same instant it is 'emitted'. Any
true physical emission, however, would be accompanied by a recoil-momentum vector in the negative x-direction, while a reception would be accompanied by a recoil-momentum vector in the CO-direction. Thus, if such a simultaneous 'emission-reception' did occur, the resultant recoil would be in the direction of the vector sum of these two vectors while the opposite emission along the path OCBAO would result in a different displacement, i.e., along the OC-direction. The two 'equivalent' assumptions would therefore clearly be distinguishable. Edwards goes on to say that such completed round-trip or 'circulation' speeds will have to be equal upon reversal since any difference would otherwise by measurable using only one clock. However, Edwards' illustration would already violate the principle of the preservation of open-ended triangular time differences. What this requires is that the time interval (as measured on one clock) between the arrival in B of a direct signal OB and a redirected signal traveling along the paths OA + AB must always be preserved. This imposes the following restriction on signals traveling in a triangular fashion:

\[ \Delta \tau = \Delta T_{OB} - (\Delta T_{OA} + \Delta T_{AB}) = n \left( \frac{1}{c} + \frac{m}{c^2} \right) \]

\[ \Delta \tau^\kappa = \frac{n}{c^\kappa} - \left( \frac{1}{c^\kappa} + \frac{m}{c^\kappa} \right) = \Delta \tau \]
OWVs in mutually perpendicular directions can be varied independently of one another. Let us therefore introduce, in analogy to the $\xi$-factor of the x-axis a $\xi$-factor for rays along the y-direction with $0 < \xi < 1$ and, in general, \( \xi \neq \xi \). Then the following time difference, as 'classically' measured will have to be preserved under all choices for $\xi$ and $\xi$:

\[
\begin{align*}
C_\alpha &= \frac{c}{1 - \cos(2\xi - 1) + \sin(2\xi - 1)} \\
C_\alpha &= \frac{c}{1 + \cos(2\xi - 1) - \sin(2\xi - 1)}
\end{align*}
\]

Edwards, however, not only includes the boundaries $0 \leq \xi, \xi \leq 1$, he is assuming that, e.g., $c_{OC}$ could be set equal to $c_{CB}$. (He apparently forgot the interval $c_{CB}$ in his enumeration of the 'infinitely fast' elements.) But co-planar velocities equal to one another and not along mutually perpendicular directions would violate the principle of the least time path for the propagation of light as well as entailing direct empirical discrepancies. For example, for $c_{OC} = c_{CB} = c/2$, assuming $\xi \neq 45^\circ$ and $OC = CB$, the triangular time interval

\[
\Delta t^\kappa = (\Delta t_{oc} + \Delta t_{cb}) - \Delta t_{oa} = \left(\frac{2a}{c} + \frac{2a\sqrt{2}}{c}\right) - \frac{a}{c} z_c = \Delta t = \left(\frac{a}{c} + \frac{a\sqrt{2}}{c}\right) - \frac{a}{c} = \frac{a\sqrt{2}}{c}
\]

could only be 'saved' if $\xi = (1 + \frac{a\sqrt{2}}{z_c})$, i.e., $\xi > 1$. 
In 1968 John A. Winnie in two papers entitled "Special Relativity without One-Way Velocity Assumptions" developed the kinematics of the STR in an \( \varepsilon \)-generalized form. His most important contribution seems to consist in extending the 're-metrizations' to all UWVs, not just that of light signals. The pertinent formulae turn out to be:

\[
\begin{align*}
\vec{v} &= \frac{c \nu}{c + \nu (2\varepsilon - 1)}; \\
\nu &= \frac{c \nu}{c - \nu (2\varepsilon - 1)}
\end{align*}
\]

In the process of deriving the Lorentz transformations in an \( \varepsilon \)-generalized form, Winnie also dealt with the one other commonly referred to method for achieving intra-systematic synchronization, i.e., the clock transport method. These arguments, however, of his as well as others, shall be collectively dealt with in chapter IV.

Giannoni

Following in the footsteps of Edwards and Winnie, Giannoni has recently undertaken to generalize the \( \varepsilon \)-formalism even further, so as to include relativistic mechanics and electrodynamics. Since this is a straightforward extension of the principles and formulas already encountered we need not reduplicate here the technical details of that 'generalization'. We shall only keep in mind the overall purpose of that program as stated by Giannoni himself:
The philosophical import of this extension is twofold: because a number of physical magnitudes become dependent on the one-way velocity of light in this generalization, it provides a very useful framework from which to discuss the question of the correct philosophical interpretation of physical magnitudes, and because this generalization will be seen to be part of what is involved in a generally covariant formulation of physics, an additional argument can be adduced in favor of the thesis that the one-way velocity of light is indeed a matter of convention. We shall have very little to say about the claim that somehow these allegedly equivalent 're-synchronizations' of clocks will allow us to discern which physical magnitudes are 'real' or 'more real' than others. Rather than spending much time discussing such vaguely general, possibly confused contentions in equally abstract terms we shall instead concentrate our efforts on taking a closer look at the very basis upon which such 'philosophical' claims are made in the first place.

Similarly, with respect to the second contention, we shall not engage in a lengthy discussion as to what, if anything, the General Theory of Relativity (GTR) has to do with the CST. Why we need not do so becomes obvious from Giannoni's own line of reasoning:

Granting that nonstandard synchronization simply yields an alternative coordinatization of space-time, then it follows that it is permitted by General Relativity, according to which no space-time coordinate system has preeminence over any other (except perhaps for descriptive simplicity). While it has been shown that the Principle of General Covariance is a methodological rule, rather than a law of nature, nevertheless there would be no reason to follow this rule unless it was the case that in nature there were no
preferred coordinate systems. Now if the one-way velocity of light is in fact unique in nature (although not necessarily measurable as such), then certain coordinate systems are indeed preferable, that is, those which are true. In our case it would be those coordinate systems which represent standard synchronization. The coordinate systems which represent nonstandard synchronization would be false. But surely to say that certain coordinate systems are false is to run into conflict with the principle of General Covariance. The use of alternative synchronizations is not only permitted by General Relativity, but one is enjoined to formulate the laws of nature so that they hold with any synchronization. The generalization is therefore seen to be part of the program of General Relativity.

Whether or not this is intelligible in itself or whether or not this involves a fundamental misunderstanding of the Principle of General Covariance is irrelevant for the aim and purpose of my discussion. I am out to undercut the very first move of this attempted argument: if I succeed in showing that we need not and indeed cannot grant that "nonstandard synchronization simply yields an alternative coordinatization of space-time", then it follows by the very same token that it is not permitted by General Relativity. The antecedent of this conclusion is all that I want to establish in this essay. What follows from it (or fails with it) shall be of no concern to me (here and now).

Once again, this is all that I shall have to say about the 'general covariance' argument in this essay. I do want to point out, however, that Earman\textsuperscript{23} has already responded
to similar claims of that nature advanced earlier by

Gruenbaum:

Now there may be considerable latitude in defining
a projection of space-time onto a three-space or
reference frame, but once the projection has been
chosen, there is no latitude in the measure of
infinitesimal spatial intervals: According to
one of the basic correspondence postulates
linking the GTR to the special theory of relativity
(hereafter, STR), the spatial distance $d\sigma$, as mea-
sured in $K$, from a point $P_1$ of $K$ to a point $P_2$ of
$K$ infinitesimally distant from $P_1$ is at a given
moment equal to the distance as measured in a local
inertial frame $K_0$ which is at rest with respect
to $P_1$ at the given moment. It follows that is
the length of a space-time displacement which is
orthogonal to the world line of $P_1$ and which lies
between the world lines of $P_1$ and $P_2$.

Gruenbaum bases his claim that the GTR supports
his thesis of the conventionality of spatial and
temporal congruence on what he calls the non-
invariance of the measures of spatial and temporal
intervals under coordinate transformations. But
the above measure of spatial intervals is defined
in a geometric, and, therefore, invariant fashion.
Of course, the value of this measure as computed
with the help of some special coordinate system will
not in general remain invariant if we switch to
another coordinate system. For example, let $\{x^i\}$,
i=1, 2, 3, 4, be a coordinate system which is adapted
to $K$ in the sense that the world lines of the
points of $K$ have the equation $x^\alpha=constant,
\alpha=1, 2, 3$. Then the spatial distance as measured
in $K$ from $P_1(x^\alpha)$ to $P_2(x^\alpha+dx^\alpha)$ is

$$d\sigma=\gamma_{\alpha\beta}dx^\alpha dx^\beta \quad \gamma_{\alpha\beta}=\delta_{\alpha\beta}-\frac{\partial x^\alpha \partial x^\beta}{\partial x^\gamma \partial x^\gamma} \quad (2.1)$$

where the $g_{ij}$ are the components of the space-time
metric tensor relative to $\{x^i\}$ (see [9], p. 238). This expression does not remain invariant under a
general coordinate transformation, but it does remain
invariant under a transformation which represents
a recorodnization but not a change of the frame $K$ (see [9], p. 236 and appendix 4), i.e.,
under a transformation of the form

$$x'^\alpha = x'^\alpha (x^\beta) \quad x'^4 = x'^4 (x^\beta, x^4)$$

(2.2)

And from the above distinction between the relativity of congruence and the conventionality of
congruence, it is clear that it is only the
invariance or non-invariance of the expression
(2.1) under transformations of the form (2.2)
and not the invariance or non-invariance under
general coordinate transformations which is relevant
to the conventionality issue. It is hard to see,
therefore, how the GTR can be said to allow for
alternative congruences of infinitesimal spatial
intervals within the same frame.

In summary, Gruenbaum obscures a fundamental
point. A relativistic space-time may not be such
as to allow a projection onto a three-space $K$;
and even if it does, there may be no accompanying
projection onto a time $T$ such that space-time is
split into space $K$ and time $T$. Whether or not there
is such a $T$ is not a matter of convention but a
matter of physical fact about the structure of the
space-time manifold. When there is such a $T$, there
is only one, and, therefore, there is no room for
the conventionality of simultaneity or temporal
congruence. When there is no such $T$, the question
of the conventionality of temporal congruence for
the intervals between events occurring at different
points of $K$ does not even arise.

We have now before us the basic technical and conceptual
tenets of GCC so far as they directly relate to the CST. We
shall disregard, as mentioned earlier, any of the secondary
arguments which purportedly tie in the CST with the GTR.
The CST can and must be divorced from this attempted higher-
level support and we shall simply proceed to examine and judge
it on its own merits.

There is one other realm of 'supporting' arguments, however, which I have not mentioned so far and which will also have to go largely untreated. It is this the 'ontological' penumbra with which the CST discussion has cloaked itself, mainly due to Gruenbaum's efforts. Only briefly, at the very end of my thesis, shall I touch upon the 'intrinsic metric amorphismness' of physical space and time claim and the 'continuity' argument advanced by Gruenbaum. Once more, even if these 'supporting' philosophical arguments were perfectly clear and intelligible in themselves, as they are not, they could only be of importance or assistance to the CST if the latter were, contrary to fact, able to stand on its own. That it does not even possess that kind of self-enclosed 'consistency', or 'irrefutability', in the narrowest possible sense, is precisely what I hope to show in the chapters ahead of us.
REFERENCES FOR CHAPTER II


2 Ibid., p. 81.


4 Grunbaum, A., "Geometry, Chronometry, and Empiricism", in Geometry and Chronometry in Philosophical Perspective...


7 Ibid., pp. 125-127.

8 Ibid., p. 162.


12 Ibid., p. 135.

13 Ibid., p. 136.


15 Grunbaum, A., op. cit., note (11), p. 139.

16 Edwards, W., op. cit., note (10).

17 Ibid., p. 483.
18 Ibid., p. 483.

19 Winnie, J. A., op. cit., note (1).


21 Ibid., p. 1.

22 Ibid., p. 9.

III.

THE PHYSICAL CONCOMITANTS OF THE CST

In discussing step (B) one must keep in mind the conventionalist's basic contention to be merely 're-describing' a given set of facts in different, unfamiliar 'languages' of sorts. GCC claims to be merely altering the time coordinate assigned to certain spatial coincidences, whereby directions perpendicular to each other can be 'epsilonized' independently of one another. Such deliberate efforts to dispense with the 'unverifiable assumption' of spatial isotropy are believed to yield a consistent, sensible, and fully equivalent description of the same set of facts. The latter term, in this context, is generally regarded as comprising no more than the bare set of basic, irreducible and apparently 'directly perceived' recordings of the 'local coincidences' of certain geometrical points. Anything going beyond these basic data must expressly be relegated to the realm of definitional (and freely alterable) 'assumptions'. The immediate consequence of 'altering these assumptions' is, however, that of introducing arbitrary, non-isotropic propagation velocities into our calculations and whether we could still understand the physics in question is something
GCC has yet to prove. Almost anything can generally be 're-described' on an ad hoc basis. Whether we could still construct a coherent, sensible system of physics on this basis, which alone would give us a truly equivalent and equally viable alternative, is, however, an entirely different matter.

We have so far been following the proponents of the CS thesis in talking about "light" as if it were simply a string of moving geometrical points, created at some instant by a fixed point and passing over, or 'triggering', a distant clock at some later instant in time. However, the proposed 'alternatives' must not only be able to 're-describe' a very abstract type of 'geometrical optics' but genuinely physical optics as well, if it is to be a fully equivalent alternative. This condition is in part a reminder that physical accounts must be explanatory as well as descriptive. Yet the distinction between the two is not a hard and fast one. For example, "physical light" exhibits a phenomenon called "polarization". That particular phenomenon cannot be said to be explanatorily related to the propagation speed of light, yet it must, as a concomitant phenomenon, be correctly describable within any 'alternative' constructed on the basis of the alleged CS introducing differing OWVs. Hence, e.g., the quarter-wave plate, the Faraday rotation of linearly polarized light, and the energy and angular momentum absorbed
by an atomic oscillator in response to circularly polarized
light have yet to be incorporated into the 'alternative
descriptions' operating with anisotropic propagation velo-
cities and wavelengths.

On the other hand, the velocity of light is also directly
related to physical concepts of 'explanatory' character.
For example, in free space, Maxwell's field theory yields the
relation \( c^2 = 1/\mu_0 \cdot \varepsilon_0 \). Hence, if \( c \neq c \) in vacuo, the
permittivity and the permeability of free space would have
to be different for incoming and outgoing waves (in addition,
non-isotropically so). Before we continue here let me
interject that it is indeed the permeability and permittivity
of the restframe itself which would have to differ. The
conventionalistic values for \( c_{0w} \) could not be accounted for
kinematically, say in terms of some uniform, unidirectional
'etherwind'. Even in one direction no one "w" can be found
which would satisfy the two equations \( c = c/2\varepsilon = c-w \) and
\( c = c/2(1-\varepsilon) = c+w \), since \( w_1 = c(2\varepsilon-1)/2\varepsilon \neq w_2 = c(2\varepsilon-1)/2(1-\varepsilon) \).

Even if conventionalists could manage to produce a
're-description' of Maxwellian field theory that could acco-
modate electromagnetic propagations in vacuo, their task be-
comes even more formidable for propagations through physical
media. Since all locally measurable time differences between
the arrivals of simultaneously emitted signals must be
preserved, the time delay between the arrival of a light ray
traveling through vacuum (or air) and another one traveling
the same distance in some refractive medium must remain
invariant under different choices of ε.

Hence, is \( c_{OW} \) is presumed to vary with direction, so must
the refractive index \( n \) of the medium:

\[
\hat{\eta}_\alpha = \frac{n + \cos(2\varepsilon - 1) + \sin(2\varepsilon - 1)}{1 + \cos(2\varepsilon - 1) + \sin(2\varepsilon - 1)}
\] with 
\[
\hat{\epsilon}_\alpha = \frac{c}{n + \cos(2\varepsilon - 1) + \sin(2\varepsilon - 1)}
\]

However, the value of the speed of light in, i.e., the
refractivity of, optical media is physically directly related
to synchronization independent quantities, namely molecular
structure and density. For example, the Gladstone-Dale law
and the Lorentz-Lorenz formula \(^1\) 'explain' the OWV of light
in terms of these properties (rest distances!) which would
somehow have to be 're-defined' on the basis of the CS-thesis.

The point I want to drive home in this entire section is
not a technical one, however; rather, I would like to show
the variety of countless physical ('descriptive' as well as
'explanatory') consequences entailed by a 'mere' re-definition
of simultaneity. These concomitants of the CST are
apparently the ones conventionalists have forgotten, chosen
to neglect, or are simply ignorant of.

Conventionalists traditionally want to maintain a sharp
distinction between 'explanations' and 'descriptions'.
Where they concede that their account is devoid of explana-
tory power, they insist on it being able to provide at least
an 'alternative description', fully equivalent in genuinely 'empirical content'. Yet there are concepts intimately and inextricably connected with the magnitude of OWVs which are not in themselves 'explanatory' but rather constitute an integral part of what it is to give a physical description. To put the point differently: physical descriptions already operate with a 'momentum space' not merely an 'event space'. Relativistic mechanics is not explanatory but genuinely 'descriptive' and whether the CS-thesis can indeed provide viable alternatives to 'standard' STR in that (momentum) 'space' is yet an open question.

Let us assume for the moment that there are in fact no direct kinematical discrepancies forthcoming which would immediately rule out such hypothetical anisotropies. Even then, however, cogent arguments against such 'assumptions' can be made on other grounds. It must be remembered that the signalling processes involved here are more than just operational vehicles for introducing a particular time metric. Looked upon as full-fledged physical phenomena it is concepts like "mass", "charge", "momentum" and "energy" that come into play when OWVs are arbitrarily assigned different magnitudes. Since all physical phenomena are affected by these 'assumptions' such arbitrary changes in OWVs will in effect topple the entire system of genuinely physical concepts. We shall merely list now a few of the most pertinent physical consequences which conventionalists will have to deal with.
1. Dynamical concepts and the metric of OHVs.

1.a. Most obviously, of course, though hardly ever noted, the CST will come into conflict with the concepts of "mass", "momentum" and "energy" associated with all physical signals. As is acknowledged in Giannoni's elaboration of the CST, there are only two ways to formally generate 4-\(\xi\)-vectors from ordinary 4-vectors. One of these ways will make mass, the other one momentum itself directionally dependent quantities. He goes on to conclude: "Therefore, one must either give up the classical definition of momentum or the notion that both momentum and mass are real quantities. In general this issue will arise everytime we generate a 4-\(\xi\)-vector from a 4-vector."

Both mass and momentum, however, are 'real quantities' in the sense that any imputed directional dependency of either would entail ramifications which could be verified or falsified directly, locally, and unambiguously. The criterion for momentum isotropy, for example, is given by the absence of changes in the rest positions of physical sources after the emission of identical particles in opposite directions. Invariance of momentum, on the other hand, would entail directional variability of \(m_0\). That in turn is directly falsifiable by exposing identical sources to the same acceleration (or gravitational field) perpendicular to a given axis after they have emitted identical large numbers of
particles (with all individual $m_{\text{particle}} \ll m_{\text{source}}$) in opposite directions along that given axis. Similar objections apply to Giannoni's remarks concerning the concept of "kinetic energy".

The other 'alternative' mentioned by Giannoni of "giving up the classical definition of momentum" is merely an oblique way of conceding that the CS claim cannot ('as of yet', if you please) provide a sensible, equivalent description of physical kinematics. The precise meaning or detailed formulation of that proposed 'alternative' is nowhere clarified or worked out in the conventionalistic literature. Similar objections apply to Giannoni's remarks concerning the concept of "kinetic energy".

Dynamically the CST runs even deeper into troubled waters. As we saw earlier, when introducing their non-standard linear light-clocks, conventionalists were careful to avoid suggesting that light was not traveling uniformly between the mirrors. It was simply the magnitude of these uniform linear velocities which was stipulated to be different for the to and fro motions. They apparently felt that for such 'differences', i.e., the 'mere' introduction of differing scale factors, no physical reasons would have to be given or could even legitimately be demanded. Such demands do arise, however, immediately and very naturally, as soon as continuously varying, noninertial speeds are
involved. While carefully avoiding such questions by 'merely' postulating discontinuous 'jumps' in inertial speed upon reflections, conventionalists were apparently unaware of the fact that such changes in scale of linear, inertial OHWVs already committed them to the introduction of non-uniform accelerations if even the minimal, pre-kinematical requirement of preserving pure space-time coincidences of 'moving points' is to be met. For example, a rotational 'clock' or, in general, any uniformly rotating masspoint P will coincide with a linearly uniformly moving point Q in certain points and intervals which must be preserved under different choices of $\varepsilon$.

Let us therefore derive here the conventionalistic expression for 'uniform' rotation since this, to my knowledge, has not been done before. The adjective 'uniform' in this context means any rotation commonly, i.e., under the standard $\varepsilon=1/2$ synchronization, 'assumed' to be continuously uniform in magnitude and continuously varying in direction, while according to the CST this only means uniform in the number of subsequent, completed revolutions per time (as measured on any one clock, e.g., at A). The average velocity is $\mathbf{u}$ and constant.
(a) $\vec{u}$ between A and D:
$$u_{\alpha} = \frac{c\,u}{c + u \cdot \sin(\varepsilon - 1) + u \cdot \cos(\varepsilon - 1)}$$

(b) $\vec{u}$ between D and B:
$$u_{\alpha} = \frac{c\,u}{c - u \cdot \sin(\varepsilon - 1) + u \cdot \cos(\varepsilon - 1)}$$

(c) $\vec{u}$ between B and C:
$$u_{\alpha} = \frac{c\,u}{c - u \cdot \sin(\varepsilon - 1) - u \cdot \cos(\varepsilon - 1)}$$

(d) $\vec{u}$ between C and A:
$$u_{\alpha} = \frac{c\,u}{c + u \cdot \sin(\varepsilon - 1) - u \cdot \cos(\varepsilon - 1)}$$

Hence the travel times between these points turn out to be:

(a) $\Delta T_{(A,B)} = \int_{\alpha = 0}^{\alpha = \frac{\pi}{2}} u_{\alpha} \, d\alpha = \frac{\gamma \pi}{2u} + \frac{\varepsilon}{c}(2\varepsilon - 1) + \frac{\varepsilon}{c}(2\varepsilon - 1)$

(b) $\Delta T_{(B,C)} = \int_{\alpha = 0}^{\alpha = \frac{\pi}{2}} u_{\alpha} \, d\alpha = \frac{\gamma \pi}{2u} + \frac{\varepsilon}{c}(2\varepsilon - 1) + \frac{\varepsilon}{c}(2\varepsilon - 1)$

(c) $\Delta T_{(B,C)} = \int_{\alpha = 0}^{\alpha = \frac{\pi}{2}} u_{\alpha} \, d\alpha = \frac{\gamma \pi}{2u} + \frac{\varepsilon}{c}(2\varepsilon - 1) + \frac{\varepsilon}{c}(2\varepsilon - 1)$
\[
(d) \quad \Delta T_{cA} = \int_{0}^{\pi/2} \frac{x}{\varepsilon} \mu_{\kappa} d\kappa = \frac{\varepsilon}{2} \mu + \frac{1}{\varepsilon} (2\varepsilon - 1) + \frac{1}{\varepsilon} (2\varepsilon - 1)
\]

Or,
\[
\Delta T_{A8} = \frac{\varepsilon}{\mu} + \frac{2r}{\varepsilon} (2\varepsilon - 1)
\]
\[
\Delta T_{B8} = \frac{\varepsilon}{\mu} - \frac{2r}{\varepsilon} (2\varepsilon - 1)
\]

What this means is that the \(\varepsilon \neq 1/2\) stipulations would introduce variable radial accelerations into uniform rotational motion as well as tangential accelerations where none existed before.

Hence it seems that the CS-type 'alternatives', unlike the standard STR, are not capable of correctly re-describing gravitational or Newtonian dynamics.

We need not think, however, of our rotating point P as just a gravitational masspoint. Things get even worse for the CST if P is pictured to be a pointcharge moving in an electrical and/or magnetic field. Here velocities of the magnitude of \(u \approx c\) are readily attained even in the laboratory and the effects of accelerations much more easily observable than in the gravitational case. If we think of P as an electron circling in a uniform magnetic field perpendicular to the plane of motion, even minute variations in
accelerations would ordinarily be accompanied by immense physical effects, i.e., electromagnetic radiation. Hence, the absence of "Bremsstrahlung", as well as the synchrotron radiation itself, in short electromagnetic phenomena, will all have to be newly accounted for if we 'merely' were to alter the 'assumptions' concerning the isotropy of OWVs relative to inertial frames of reference. On the other hand, the necessary 're-descriptions' cannot be produced in any way we please. Such concepts as "charge" must meet certain empirical restraints and cannot be made to 'mean' whatever we wish. The directional independence of "q", for example, verifiable 'statically' in a way similar to the one sketched above for determining \( \vec{m}_e = \vec{m}_e \), must surely be preserved.

Conventionalists keep insisting that the CST is merely a 'philosophical' theory, making a logical point rather than propounding a physical theory. Yet their 'alternatives', allegedly fully equivalent in 'empirical content' involve notions that do not in any intelligible way resemble the 'behaviour' of ordinary physical concepts connected with OWVs. To compile a table of space-time coincidences alone is not to give a physical description. There is more than just computational simplicity or complexity at stake here. The 'choice' of \( \varepsilon = 1/2 \) is 'customarily accepted' not because it happens to yield the mathematically simplest space-time
coincidence tables but for a myriad of conceptual reasons operating within the entire intricate web of genuinely 'physical' concepts connected with this 'choice'. The latter are only meaningful, coherent, and intelligible on the basis of that particular 'convention-without-sensible-alternative'. In what sense it might nevertheless be said to be 'merely' a 'convention' is largely a moot question at this point. We have not finished making our case, i.e., proving that conventionalists have in fact not presented even a minimally 'equivalent' alternative, i.e., an account unassailable at least within purely spatio-temporal 'kinematics'.

2. Difficulties connected with the wave character of physical signals.

We have so far only addressed some of the direct consequences of non-isotropic OWV 'assumptions', acting as if there were no direct way to test or falsify these 'scale-assumptions' themselves. We shall in separate chapters discuss at length the means and methods by which such direct tests could in fact be carried out. Since these arguments will be based on entirely different principles they shall not be anticipated here. However, even within the context of signal synchronization, i.e., utilizing discrete pulses or 'disturbances' propagating through space for establishing
distant simultaneity, I would like to point out at least two instances in which the OWV of such signal chains could unambiguously be measured without already presupposing knowledge of simultaneity.

For the first example let us recall what the ε-formalism does and does not preserve. The time differences between simultaneously emitted signals, e.g., one traveling with c and the other with v, are invariant. E.g.:

\[ \Delta \tau = \frac{\Delta x}{\sqrt{v}} - \frac{\Delta x}{c} = \Delta x \left( \frac{c-v}{c} \right) \]

\[ \Delta \tau' = \frac{\Delta x}{\sqrt{v'}} - \frac{\Delta x}{c} = \frac{\Delta x}{vc} (c + v(\lambda \epsilon - 1)) - \frac{\Delta x}{c} (\lambda \epsilon) = \Delta x \left( \frac{c-v}{c} \right) = \Delta \tau \]

But the ratio of the two OWVs is not preserved: \[ \frac{v}{c} \frac{\sqrt{v}}{\sqrt{c'}} = \frac{2 v \epsilon}{c + v(2 \epsilon - 1)} \]

However, the angle of the Cerenkov radiation cone is directly proportional to the ratio \( v/c \). This indicator is one of the primary means of measuring the OWV of particles in high-energy physics and seems clearly incompatible with the \( E/1/2 \) 'assumption'. (This, like the following example, involves the notion of empirically establishing the true measure of angles relative to a given inertial frame, discussion of which shall be postponed until a later section).
The other example is simply utilizing the 'serial' or wave character of the signals involved. Even though their OWV and mutual separation, i.e., their 'wavelength', cannot immediately be determined locally with the help of only one clock, their frequency or temporal separation can. Furthermore, we ought to keep in mind that we need not necessarily measure the OWV of light to begin with. Any OWV that we can truly and unambiguously establish empirically will do as a 'first synchronization vehicle' subsequent to which the OWV of light can also be determined empirically. Let us then take an inertially moving mirror and see whether or not we can determine its OWV without begging the question. The relativistic law of reflection from moving mirrors predicts a precisely defined and highly confirmed change in frequency of the reflected light that proves to be a direct measure of the OWV of the mirror relative to the frame. For a plane mirror reflecting rays perpendicular to its direction of motion we obtain $\nu_s/\nu_r = (c-v)/(c+v)$ if the mirror is approaching the source. Hereby the outgoing and return frequencies $\nu_s$ and $\nu_r$, respectively, can be measured locally and directly on one and the same clock. (Once more, we shall later discuss how the 'true' perpendicularity of the mirror relative to the frame in question can empirically be established beyond reasonable doubt and without being open to alternative interpretations). This locally determinable ratio $\nu_s/\nu_r = n$
would then give us a direct measure of the 'true' UWV of the approaching mirror with the help of which we could synchronize all clocks in its path.

It must be stated here without proof that all these 'physical' objections to the proposed nonstandard synchronizations will in fact present unsurmountable problems for the CST. This should, however, not merely be construed as forced upon us by lack of space but rather as an indication of the proper burden of proof. If there were room for 'assuming' such arbitrary values for OWVs then, so the evidence suggests, we would no longer understand the physics involved. The criteria at stake, then, are not merely those of descriptive complexity or simplicity, as conventionalists want us to believe, but rather those of sense and non-sense, comprehensibility or incomprehensibility. A rational account of physical phenomena need not necessarily be the mathematically simplest one as well; however, a 're-description' that involves tearing apart the entire web of physical concepts without either justification or a viable substitute is an idle game based only upon the narrow foundation of an alleged 'empirical irrefutability' of its initial premises. If conventionalists claim to be able to produce a fully equivalent alternative account of everything that 'isotropic assumptions' or 'time-orthogonal' reference frames can do by way of recording, interrelating, and making sense out of physical
occurrences, then we must ask them for proof.

In the last analysis all the objections to be raised against step (B) of the conventionalistic chain of reasoning boil down to this: (i) The CST introduces \textit{ad hoc} geometrical and temporal differences without any physical distinctions or reasons -- aside from the alleged bare fact of 'empirical irrefutability', i.e., logical permissibility. (ii) The CST leads to 'descriptions' which can be made to 'fit' only by a complete rejection and 're-definition' of genuinely physical concepts and reasoning. The possibility of such 'revisions' is nowhere shown in the literature, nor even attempted, mainly because of an astonishing lack of insight into all the rami-

fied logico-physical consequences entailed by the $\epsilon \neq \sqrt{2}$ 'hypothesis'.

Let us leave now this field of dynamical and physical objections and return to idealized 'pure' signals. We shall now discuss the one other recognized method for intra-systematic synchronization, i.e., the clock transport procedure.
REFERENCES TO CHAPTER III


3 Ibid, p. 18.
IV.

THE CLOCK TRANSPORT ARGUMENT

In order to provide distant places with synchronized clocks one can either start with initially unrelated fixed clocks and then establish a temporal connection between them via some physical linkage or one can start with initially i.e., locally, synchronized clocks and transport them to the places in question. At times it seemed that conventionalists wanted to deny the feasibility of the second method altogether. Two quotes may suffice to buttress this contention. Thus, Reichenbach for example spoke of "only one method" (italics his) of establishing distant simultaneity when he first introduced the $\varepsilon$-factor into the one-way signal connections. Gureenbaum later wrote:

"Is there an actual physical basis for relations of absolute simultaneity among spatially separated events? Suppose that the behaviour of transported clocks were of the kind assumed by the Newtonian theory - that is, that if two clocks are initially synchronized at essentially the same place A, this contiguous synchronism will be preserved after they have been separately transported to some other place B independently of the lengths of their respective paths of transport and of whether or not their arrivals at B coincide. In that case a physical basis for absolute simultaneity would exist in the form of the coincidence of physical events with suitable identical readings of transported clocks of identical constitution. But according to the STR this physical-clock basis for absolute simultaneity does not exist. Indeed, the STR makes the following assumption: transported material clocks fail to define unambiguously obtaining
relations of simultaneity within the class of physical events because relations of simultaneity yielded by clock transport depend on the particular clock used. This dependence on the particular clock used prevents transported clocks from defining relations of absolute simultaneity within the class of physical events. It also led Einstein to conclude that even within a single inertial system the simultaneity of two spatially separated events E and E* cannot be based physically on the criterion that the numerical readings on two clocks U₁ and U₂ be the same for event E occurring at the location of U₁ and event E* occurring at the place of U₂, the clocks having previously been transported to these separate places from a common point in space at which they had identical readings.

The behaviour which Einstein attributed to transported clocks thus does not make for relations of absolute simultaneity among spatially separated events. Einstein had postulated that the transport of adjacently synchronized clocks does not furnish a physical basis for the obtaining of relations of simultaneity. He therefore thought to ground the temporal order on physical foundations which are independent of any synchronism of spatially separated clocks.¹

It would be interesting to know where exactly Einstein makes these intra-systematic 'assumptions' and 'postulates'. But we shall not get preoccupied with scholarly exegesis. The fact is that there are at least two successful attempts in the recent literature to vindicate the slow clock transport method, proving that Einstein neither intended nor needed to exclude this as a viable intra-systematic criterion. Bridgman² and later Ellis and Bowman³ showed that slow clock transport and Einstein's E=1/2 'assumption' are in fact equivalent. Both of these attempts have been
taken note of by the proponents of the CS-thesis and we shall consider their responses in a moment. There is, however, a third transport method which has largely or totally been ignored in this dispute. Since 1948 Herbert E. Ives has published a series of papers concerning a synchronization procedure which makes no stipulation about the magnitude, i.e., slowness of the transport velocity. Arzelies has summarized this procedure in the following way:

"We can synchronize B with A by transporting a clock from A to B with a negligible velocity. . . . Ives has suggested a variant upon this method the underlying idea of which is as follows. Instead of moving the synchronizing clock from A to B with negligible velocity, we shift it from A to B with an arbitrary velocity. The result is that B is pseudo-synchronized. We denote by $\Delta t$ the difference between the times shown by the synchronizing clock when it is in coincidence with A and B respectively; we denote the times shown by the clocks A and B (the latter pseudo-synchronized) when a light flash passes by $t_A$ and $t_B$. Ives shows that the velocity of light c along the path AB is given by

$$c = \frac{2Q(Q-q)}{(2Q-q)}$$

where $Q = AB/(t_B-t_A)$  

$$q = AB/\Delta t$$

He therefore obtains c by means of readings which do not require previous synchronization. His calculations, which employ relativistic formulae (Ives speaks of a medium of propagation, but in fact, only Galilean systems are involved), seem at first sight to form a circular argument; in fact, they contain the assumption that the velocity to be measured and the relativistic constant c are identical. One might reply that his formula provides a means of verifying the premises, by performing the operation in different directions, AB."
We need not go into the technical details here because we really need no 'improvement' upon the slow transport method. Ever since Ellis and Bowman's paper conventionalists essentially concede the feasibility of slow clock transport. They merely deny its philosophical import as having rendered the CS-thesis either trivial or false. Since they concede correctness and uniqueness without drawing the same conclusions, it is to their reasoning rather than their calculations that we must turn our attention.

This acknowledgment-and-denial process takes the following form: Gruenbaum recently conceded that "Ellis and Bowman are perfectly correct in emphasizing that the slowly transported clock presents us with a unique time coordinate $t_S$ for the optical event $E'$, whereas a light ray itself delivers no such coordinate unless we stipulate it" (italics his). Therefore it is admitted that "the adoption of slow transport synchrony confers factual truth on the assertion of [the to and fro] velocity equality ...". Yet he wants to deny the conclusion that therefore the CS-thesis is falsified or trivialized: "... [O]f course, if we do adopt slow transport synchrony to stipulate simultaneity, then the equality of the to and fro velocities is a mere definitional consequence of the synchronization rule" (italics his).

The point of the whole transport counterargument, I
take it, was the following:

1. Here we have a procedure which is internally free of circularity. It is, at this point, the only sensible and natural starting point for establishing intra-systematic simultaneity since its contender, i.e., the signal procedure, is admittedly presupposing the very condition which we are trying to establish. Hence, 'externally', as between the two competing methods, we really have no sensible choice to make. One is not only 'simpler' than the other; it is, at this stage of the game, the only non-circular procedure at hand.

2. Given that we do proceed on the basis of this transport procedure, any suggestion as to the 'arbitrary' magnitude of OWVs becomes importantly and verifiably false.

Far from agreeing to this line of argument, conventionalists try to simply turn it upside down: If we nevertheless insist on starting with the logically defective and circularly 'arbitrary' signal procedure, then slow clock synchrony becomes also 'conventional' and ε-infested.

Wesley Salmon, in a sequel to the above quoted paper by Gruenbaum, brings out this attempt at a conventionalistic 'rebuttal' of the clock transport argument very succinctly. After elaborating on the Ellis and Bowman method and, in fact, adding a new version of his own to this procedure, he concludes:
"This definition of simultaneity is tantamount to the stipulation that the oneway velocities of the pair of clocks with minimal retardation are equal in magnitude. If the factual assertions of the special theory are correct, this defines a unique simultaneity relation on the basis of clock transport, and the resulting synchrony is identical with standard signal synchrony and with slow transport synchrony of either the Bridgman or the Ellis and Bowman variety. Given that we can recognize the inertial motion of transported clocks, this method of establishing a simultaneity relation does not make use of oneway clock transport velocities of any sort, literal or fictitious, but relies entirely upon the time lapse and retardation observable at one particular place A.

... Acknowledging, then, that there are various methods of establishing a unique relation of distant metrical simultaneity through the use of transported clocks, we must now consider whether this fact does show that the alleged conventionality of simultaneity has been overcome. I shall argue that the answer to this question is emphatically negative."9

The reasons for reaching this verdict are twofold. After conceding that the clock transport procedure is indeed 'possible' within the STR (and not either implicitly or explicitly rejected by Einstein) and 'unique' (corrigible as to path and velocity dependence and, as such, yielding only one value for the directional scale factor invoked by the signal method), he goes on to argue: (1) that clock transport is not the only procedure for establishing intra-systematic synchrony but only one among two equally qualified contenders, and (2) that within the rules defined
by this other, 'alternative' method (which we could
initially adopt with equal justification) simultaneity is
not unique but subject to a range of alternate, arbitrary
'choices'.

Thus, it is argued, there are two kinds of convention-
alistic 'freedoms': one external degree of freedom as to
the initial choice of adopting one among at least two
competing, alternative, and equally sensible procedures,
and one internal degree of conventionalistic freedom,
exercisable within at least one of these methods, as to
subsequent choices of velocity metrics. Most importantly,
if the second method is chosen from the start, the unique
settings achieved by the first procedure are retroactively
deprived of their uniqueness and can be reproduced with
varying choices of oneway signal speed values:

"We may begin by synchronizing the two clocks
U_A and U_B at B by the slow transport method. Having
synchronized these clocks which are spatially
separated, we may use them to verify that the
light signal reaches B at t_2 = t_1 + 1/2(t_3 - t_1), where t_2
is measured on U_B which is in slow transport synchrony
with U_A. It is logically possible, of course, that
such a procedure would yield the result that \( \varepsilon \neq 1/2 \),
but this would be inconsistent with a factual claim
of the special theory.

Suppose, however, that someone were to begin by
synchronizing U_A and U_B by the method of light
signals, choosing an \( \varepsilon \neq 1/2 \) instead of the standard
signal synchrony. He could then investigate the
transport behaviour of clocks, using his nonstandard
definition of simultaneity. The result - if the
special theory is correct - is that, in the limit
of slow transport, clocks suffer an advancement or
retardation depending on whether the choice is $\varepsilon < 1/2$ or $\varepsilon > 1/2$. Suppose, for the sake of definiteness, that $\varepsilon > 1/2$, so that the slowly transported clock suffers a retardation on that particular nonstandard synchronization. Since the difference $t_2 - t_1$ is given by $\varepsilon(t_3 - t_1)$, the difference $t_3 - t_2$ must be $(1 - \varepsilon)(t_3 - t_1)$. This is the amount of time it takes for light to return from B to A; hence, the $\varepsilon$-value for that direction is 1/2, so the slowly transported clock experiences an advancement upon being transported in the opposite direction. In the limit of slow transport, the net retardation of the clock for the entire round-trip is zero; that is, if $C_1$ is synchronized locally with U and is transported slowly from A to B, and slowly from B back to A, in the limit it is in synchrony with $U_A$ when it returns to A. But, if the choice of $\varepsilon \neq 1/2$ is made, it is not in synchrony with $U_A$ throughout the journey.\textsuperscript{10}

So far, however, this is at best a very strange kind of 'argument'. Apparently, conventionalists are able to save their claim as to the allegedly structural, inherent 'non-empiricalness' of simultaneity only by deliberately ignoring a truly alternative method, nay one which at this point, i.e., compared only to the signal procedure, is the only sensible, nondefective starting procedure. After all, the main objection advanced by conventionalists against the clock transport method, i.e., that it was ruled out by Einstein's STR because of the velocity dependence of the transport retardation, had been shown to be untenable by Bridgman and Ellis and Bowman. Since these retardations can either be made negligible or be corrected for without begging the question, Reichenbach was completely mistaken
as to this, his initial assessment of the transport method:

"Among the definitions of simultaneity, those based on the infinite limiting velocity or the transport of clocks might turn out to be the simplest. Whether or not they do is an empirical question; both possibilities are denied by the special theory of relativity."[11]

(I shall later show that some instances of the other 'explicitly rejected' method, i.e., the one involving 'infinitely fast connections' are indeed - if properly understood - also compatible with STR).

Hence, as long as the one-way signalling method was believed to be the only possible procedure imaginable, its apparent shortcomings, i.e., the circularity of its basic assumption, might indeed have seemed inextricable and structurally inherent. In that case the apparent, superficially 'unobjectionable' feasibility of alternate OWV 'assumptions' might indeed have seemed to constitute prima facie proof of the 'non-empirical nature' of the concepts of OWV and distant simultaneity. Even though that 'proof' was already operating with a rather strange, simple-minded dichotomy of empirical vs. non-empirical 'assumptions', all the overwhelming physical objections against the introduction of anisotropic OWVs (cf. the preceding chapter) seemed to be no deterrent in the eyes of such champions of 'descriptive purity'. The emergence of the clock transport procedure as an admissible and unambiguous intra-systematic synchronization
method must, however, have posed a much more immediately felt threat to the CST. In some sense, Reichenbach already anticipated this potential threat and tried to deal with it in the apparently only way possible: conventionalists per force will have to deny that we have here an essentially different, unique alternative. It will be mandatory for them to argue that somehow these two are essentially on the same footing or even fundamentally one and the same. This is attempted in two ways, one of which is a very broad general (and extremely unconvincing) rebuttal by analogy; the other attempt, carrying somewhat greater initial plausibility, tries to draw heavily upon the undeniable fact that the 'new' method, when not adopted as the initial standard, can in fact be incorporated into the other, signalling procedure even with the stipulation of arbitrary OWVs. From this an all-out reduction argument is thought to be derivable.

The first response, then, is no more than a very general assertion of similarity: both procedures are merely attempts to define simultaneity. Even if one yields truly unique, unquestionable, and unambiguous settings, it need not be preferred over its 'more complex' but also acceptable 'rival':

"However, if relativistic physics were wrong, and the transport of clocks could be shown to be independent of path and velocity, this type of time comparison could not change our epistemological results, since the transport of clocks can again offer nothing but a definition of simultaneity."
Even if the two clocks correspond when they are again brought together, how can we know whether or not both have changed in the meantime? This question is as undecidable as the question of the comparison of length of rigid rods. Again, a solution can be given only if the comparison of time is recognized as a definition. If there exists a unique transport-synchronization, it is still merely a definition of simultaneity\textsuperscript{12} (italics his).

This is strange reasoning indeed. As both Gruenbaum\textsuperscript{13} and Salmon are quick to point out, this puts transported clocks in the same general category as any and all 'coordinate definitions'. Words, a la proper names, are apparently thought of as being 'ostensively tied' to objects only once and at one place requiring separate stipulations concerning 'sameness' of the referent in question at later times and other places. The analogy commonly drawn is that to the 'definition' of the standard of "length" as understood by conventionalists:

"... I am inclined to regard the Ellis and Bowman result as a demonstration that the status of simultaneity in a single inertial frame of special relativity is strongly analogous to that of congruence in physical geometry. Just as one can adopt a standard definition of congruence, according to which a solid body that has been corrected for deforming influences is taken to remain self-congruent under transport, so too can one adopt the definition of simultaneity according to which a clock that has been corrected for the retardation due to transport can be taken to be everywhere self-congruous... However, it is likewise possible to adopt a definition of congruence according to which the solid body that has been corrected for perturbing influences (Reichenbach's
differential forces) changes its length as it is moved about from place to place . . . Similarly, even if we wish to define simultaneity in terms of clock transport, we are still able, without contradicting any empirical fact, to maintain that clocks are subject to 'universal forces' which metaphorically describe the retardations and compensating advancements experienced by slowly transported clocks. Of course, in the case of clocks as in the case of the measuring rods, no forces in any literal sense are involved. 14

This type of argument may well be said to be characteristic of the central philosophical confusion underlying most conventionalistic epistemologies. In its crudest form it amounts to some very vague form of Cartesian doubt asserting that anything that logically can be said to have changed can never be 'known' not to have changed. While arguably not necessarily wedded to the most simple-minded version of this argument, i.e., to 'assuming' changes where- and whenever no 'direct perceptions' (comparisons) do take place (e.g., while two standards are undergoing mutual separation), this school of philosophers is merely propounding a slightly more sophisticated variation of the same theme: even when 'observations' are possible again, are in fact carried out, and yield the same results or 'coincidence behaviour', we nevertheless cannot ever justifiably rule out the possibility that, for no reason whatsoever, both object and standard, or standard and 'environment', have imperceptively changed in the same degree. Hence, we can never really know that they are still 'the same', i.e., unchanged, even though
nothing in their relative relationships has noticeably changed.

This seems to me to be a particularly irrelevant kind of 'doubt' in cases where no 'relative comparisons' whatsoever are involved. We do not compare here the readings of the slowly transported clock with the readings of another clock already stationed at B, we simply move the clock from A to B in an appropriate way and position it there. It seems to me that this type of argument is based on a deep-seated confusion regarding the use of criteria for "sameness" and "identity". Restricting 'knowledge' to the ascertainment of 'local coincidence', a pointlike, momentary, 'absolutely certain' judgment of sorts, it seems indeed natural to argue that nothing else can be 'known' in that same degree. It is only by additional, in the above sense 'unverifiable', assumptions that we can ever speak of X and Y to be "essentially the same" on later occasions or in a different environment. There seems to be an abysmal logical hiatus between the two primary usages or senses of "equal", i.e., between equal₁ = "identical" ('coincident') and equal₂ = "unchanged". Conventionalistic analyses of this kind would completely obliterate distinctions between "essential" and "inessential" changes and sameness as if we had no valid criteria for these distinctions.

All we need to note at this stage is that this 'general
rebuttal' would not save the day for conventionalists in the clock transport debate. It would at most leave them with an extremely uninteresting sense of 'conventional', one that would in no way distinguish spatio-temporal concepts from any other 'definition' or concept. Distant simultaneity and distant congruence would be no more conventional than, say the continued sameness of two "masses" or two "charges" which had been 'identified' locally. Although my objections to this kind of 'conventionality' go much deeper than this, I can at least partially agree with Ellis and Bowman's conclusion that if this is what conventionalists have in mind they are belaboring at best a rather 'trivial' kind of 'conventionality':

"It has often been claimed that there are no logical or physical reasons for preferring standard signal synchronizations to any of a range of possible nonstandard ones. In this paper, the range of consistent nonstandard signal synchronizations, first for any one inertial system, and second for any set of such systems, is investigated, and it is shown that the requirement of consistency leaves much less room for choice than is commonly supposed. Nevertheless, consistent nonstandard signal synchronizations appear to be possible. However, it is also shown that good physical reasons for preferring standard signal synchronizations exist, if the Special Theory of Relativity yields correct predictions.

The thesis of the conventionality of distant simultaneity espoused particularly by Reichenbach and Gruenbaum is thus either trivialized or refuted."
The trivial type of allegedly all-pervasive philosophical 'definitional conventionalism', meaning the allegedly inextricable stipulational element inherent in (but only in) all non-local, non-instantaneous 'judgments' seems particularly unavailing in the case of "simultaneity" defined by transport clocks. If slow clock transport does indeed have no effect upon the rate of a clock, or if all such effects can be shown to be noncircularly corrigible and eliminable, then we have all the criteria and justification we could possibly need for saying that reading off a clock at A is at the same time "knowing the time at B". Such a common time needs to be established, to be sure, e.g., by transporting a clock in such a manner from A to B, but subsequently "distant simultaneity" is uniquely defined. The happening of an event at B cannot be instantly known at A since any such transmission of information will require signals and hence take some finite time. But this truism does not detract from the important fact that the time of its happening can unambiguously become known, i.e., that 'distant simultaneity' is and can be known apart from this transmission delay. Given the criteria for clock transport constancy, any instant $t_i$ on clock $U_A$ at A will also mean that a clock $U_B$ at B shows, at that same instant, the same pointer position $t_i$. UVMs can be measured unambiguously and distant events can be triggered or influenced simply by an advance signal of
known delay, i.e., travel speed (A and B are fixed, of course, in the same frame). In short, the intra-systematic feasibility of clock transport synchronism must be particularly damaging to the conventionalistic 'doubt' argument if, contrary to Reichenbach and Gruenbaum's initial assumption it is not incompatible with, or definitely ruled out by, STR. If we can in fact know or make sure that nothing physically 'happens' to clocks 'in between' as long as they are being moved in a certain way, then nothing short of some mysterious, all-powerful Cartesian demon could still force us to assume a stance of epistemological agnosticism regarding distant simultaneity.

If this were the only rebuttal advanced by conventionalists they would not escape the charge of being left with merely a trivial thesis of sorts. Surely to say that it is only a method among others to physically 'transport time' to distant places is not saying very much. However, if that particular method consists in continuing to 'read' a clock at B as indicating $t_i$ if we have in fact every reason to say that it still shows $t_i$ at that instant, then there simply seems to be no punch left to either the 'one among many methods' objections, or to the claim that we have a genuine choice to make here. To continue to claim that after all nothing forces us to 'call' $t_i$ "$t_i$" is to say no more than that nothing 'forces' us, in that sense, to call blue "blue". Anything
that could possibly have had any perceptible effect has carefully been eliminated -- and yet we want to refuse to accept \( t_i \) as the 'true time' at that place and instant? Keep in mind that this is not like the signal case where the clock at B was entirely unrelated. No matter what it actually showed when the signal from A arrived, that arrival event was taken to first of all 'set' the clock \( U_B \). It was only at that instant that we assigned \( t_2 \) to the clock \( U_B \), even though it may actually have shown, say \( t_0 \). Here it seemed to be at least superficially plausible to suggest that we had no better reason for changing \( t_0 \rightarrow t_2 = t_1 + 1/2(t_3 - t_1) \) than any other assignment \( t_0 \rightarrow t_2 = t_1 + \varepsilon(t_3 - t_1) \) with \( 0 < \varepsilon < 1 \). But now we have a clock at B which, according to every justifiable criterion, has remained unaffected by its separation from A (where \( U_B \) and \( U_A \) were in synchrony). Hence the question of 'assigning' any time to clock \( U_B \) does not even arise. We need no translational function to properly 'read off' \( U_B \) in its relation to \( U_A \). There is no synchronizing arrival event on the basis of which we interpret for the first time the readings actually portrayed by clock \( U_B \). If it shows \( t_i \) we know that it also 'is' \( t_i \) -- if by that we mean no more than that \( U_A \) (and all other clocks positioned by that kind of transport and now at rest relative to one another) will also show \( t_i \) at that instant. We have more than just 'every reason to assume' this to be the case; we
have indeed every right to say that we know this to be the case. If this is still merely due to a 'decision' on our part, then the 'choice' involved here can be none other than the 'freedom' to 'call' blue things "blue". Surely nothing can force us to speak intelligibly (and not 'privately') or to reason rationally. To point out that it is only within a realm of shared agreements that arguments, doubts, and reasons can function at all is not to 'prove' that the entire game is therefore conventional and, in principle, as good as any other 'alternative'.

Even those who take the above sketched 'threat' seriously still have a point in classifying it as merely a trivial kind of epistemological 'freedom'. The conventionalistic rebuttal, if it wants to avoid the charge of triviality, must go further than this. It is, after all, most emphatically denied that the CST is only an instance of such a general, allegedly all-pervading type of conventionalism, the kind Gruenbaum termed "Trivial Semantical Conventionalism". Naturally, he thinks this to be not an inherently unintelligible philosophical position but rather a tenable, though trivially true, observation about 'all language' in general. The CST, however, has somehow uncovered a far more special and interesting kind of conventionality, one that is ontologically inherent in the structure of space-time itself. As he puts it:
"... there is an important respect in which physical geochronometry is less empirical than all or almost all of the non-geo-chronometric portions (ingredients) of other sciences." 17

It is this somehow 'ontologically grounded' kind of conventionality, inherent in any type of intra-systematic synchronization procedure that we must try to deal with. The merely trivial element of external choice (as to different methods) must now be eliminated altogether. The clock transport synchrony must somehow be shown to be essentially the same as signal synchrony in another, more important sense than just being another method of equal status.

This is the 'reduction' task which Gruenbaum and Salmon face in their symposion papers. 18 In so far as the transport method does not presuppose initial knowledge of OWVs or distant 'times' it apparently presents us with an entirely different, independent procedure against which the first response, noted above, proves to be trivial and ineffective. Therefore conventionalists want to go on to argue that the two methods are ultimately reducible to being merely two species of one kind in a more fundamental sense. Salmon tries to establish that point in the following way:

(i) "The first question is whether the use of light signals is indispensable in establishing a simultaneity
relation."¹⁹

(ii) "The answer to the first question is patently negative, as the existence of the clock transport synchronies shows."²⁰

(iii) However, this does no damage to the CS-thesis, since fundamentally transporting clocks is some kind of signalling procedure and 'signalling' IS the only method:

"When Reichenbach said that time comparison of distant events requires the use of signals, he was contrasting local comparison with distant comparison. He was not contrasting signal-synchrony with transport-synchrony. He was maintaining that there can be direct perceptual comparison of neighboring events, but not of widely separated events. Time comparison of distant events requires causal processes that transmit information from one place to another in order that a local comparison can be made . . . Ellis and Bowman crucially assert that the 'physical relationship' on which slow transport synchrony is based is 'independent of any signaling procedure, and does not require for its determination any prior measurements of velocity'. The first part of this assertion is evidently unsound as an answer to Reichenbach, for in the sense that is relevant to Reichenbach's view that signalling is indispensable, the transport of a clock from one place to another is a signalling process. In this sense, therefore, slow transport synchrony is not 'independent of any signalling procedure'"²¹ (italics his).

What we have here, obviously, is a confusion of "causes" and "reasons". We certainly need reasons and criteria for saying (i) that A and B are 'physically equivalent' places, i.e., that there are no differential
inhomogeneous fields affecting the rate of stationary clocks at A and B and (ii) that the rate of a slowly transported clock is not affected by moving it from A to B. Given this knowledge, however, the time comparison of distant events does not depend on causal transmission of information from A to B. If an observer at B signals to A (causally) that something took place at B at time $t_0$ (as shown on $U_B$) then we at A also know what point in time (on our clock) this refers to, namely $t_0$ on our clock $U_A$. That we only know this 'afterwards', so to speak, and the fact that we at A cannot directly (causally) influence that particular event at B (unless we did so beforehand) does in no way prevent us from knowing when it happened, i.e., from comparing 'its time' to 'our time'. We cannot immediately know it when it happens but we can unambiguously come to know when it happened. Given the above criteria (i) and (ii), which form the rational basis for clock equivalence according to the transport method, we can come to know the OWV of any and all causal links between A and B: if A sends a signal carrying the message "emitted at $t_0"", its arrival time $t_1$ as recorded on B's clock will give us the intermediate travel time $t_{AB}$, and vice versa for signals emitted by B carrying information regarding their B-emission time. Subsequently, we can even influence or 'trigger' distant events causally, simply by taking the necessary time
lag into account. Hence, given the above criteria, the finite causal propagation speed of all signals becomes a complicating, but epistemologically irrelevant, factor for intra-systematic time comparisons and physical measurements. All we are deprived of in the case of distant happenings is the psychological immediacy of experiencing X when it actually happens, as opposed to an 'image' or signal of X after some (empirically well defined and known) time delay $t_{A\rightarrow B}$. The inevitable transmission delay precludes us from 'knowing', i.e., experiencing, distant events instantly, so to speak, but not from truly knowing when they happened. Therefore, if conventionalists concede the two slow transport criteria (i) and (ii) referred to above, the kind of 'knowledge' we are allegedly still deprived of can only be a Lockeian kind of immediacy and somehow superior, infallible 'certainty' of 'presently sensed perceptions'. When Salmon insists that

"[t]he fact that slow transport provides a definition of simultaneity which does not involve velocities - Ellis and Bowman's 'intervening velocities' are not genuine velocities - does not matter, for apart from a convention we do not know that the infinitely slowly transported clock retains its synchrony,"

he can only be talking about a further sense of 'knowledge' in which we do not know, e.g., that chairs do not disappear while they are not ('locally') perceived. He has granted that the only rational and physically intelligible reason
for doubt in this context, namely the fear of a possible distortion of the rate of clocks due to the transport velocity itself, has been carefully eliminated or is kept in check -- and yet we are precluded from knowing that, after coming to rest, two such clocks will show identical times? All that we are still 'excluding' here are only those 'imperceptible acts of God' which we have literally no reason or evidence for either 'assuming' or 'denying'.

Again, we need not deal here with these broader issues concerning the fundamental confusions underlying conventionalistic epistemologies (and/or ontologies). Suffice it to say that I would wholeheartedly agree with Salmon if by the alleged indispensability of 'signals' he means no more than the indispensability of criteria: time comparisons of distant events require (a) physical signals as evidentiary carriers of information and (b) criteria or justifications for taking this evidence, e.g., the message "emitted at t₀" at, e.g., face value. What is indispensable, then, are reasons justifiable within the constraints of rational discourse, not fundamentally arbitrary 'conventions' in the midst of a primeval epistemological vacuum.

Because of its importance it may well be worthwhile to bring out this reduction argument (claiming transport synchrony to be only another kind of signalling method), in yet another way. After all, it is the 'discovery' of the finite speed
of causal propagation which allegedly forms the ultimate basis for the epistemologically and ontologically unique, nontrivial 'conventionality' of distant simultaneity.

The CST was originally believed to somehow follow from these two premises as summarized by Gruenbaum in his introduction to the panel discussion on simultaneity by slow clock transport:

"Two important physical considerations provide the foundation for the claim that distant simultaneity is nontrivially conventional:

(1) Clocks originally synchronized with one another at some place and then transported along different paths or at different velocities are, in general, found to be out of synchrony when brought together again.

(2) There is a finite maximum round-trip velocity of transmission of signals.

On the basis of these two fundamental facts it has been concluded that neither clock transport nor the sending of signals if sufficient to determine a relation of metrical simultaneity."

After conventionalists had to concede the feasibility of slow clock transport, i.e., after the Ellis and Bowman paper, the alleged foundation for the CST had to be somewhat readjusted. In Salmon's words:

"The situation we now confront may be stated as follows:

(1) It is physically possible to establish a unique relation of distant metrical simultaneity on the basis of transported clocks.

(2) There is a finite maximum round-trip velocity of transmission of signals."

...
Premise (2) is now the sole pillar of conventionalism. It is this 'structural property of space-time' which somehow distinguishes the CST from other merely 'trivially conventional' propositions.

The logical implication of (2) seems to be that two distant events cannot ever be said to be known to be simultaneous because they must always be linked via some physical process between them and that inevitably 'takes time'. That, however, is both false and criterially irrelevant. This is apparent in the limiting process of 'infinitely slow' clock transport and will become even more obvious in the third kind of intra-systematic synchronization procedure which shall be outlined shortly. To anticipate the prime example: if we can know, i.e., come up with independent and unambiguous criteria for determining the intra-systematic angle of inclination of a rod L moving with respect to the frame in question, then we know independently of any inter-clock signalling procedure or signal velocities that some point B is passed or 'triggered' at the same instant point A is passed or 'triggered' by L. The only reason why the conventionalistic reduction argument seems to have had some residual plausibility in the clock transport procedure has now altogether vanished: clock transport still seemed to involve some kind of signalling process from A to B. This last semblance of any cogent 'signal similarity' is
eliminated once we realize that what we now have is not any kind of physical signal from A to B but rather a purely kinematical connection 'passing on information' to A and B simultaneously. If we can empirically establish the perpendicularity of L relative to its line of motion with respect to our inertial system S then, given the instantaneous position of one point P of L, we can truly claim to know where, in S, some other point Q of L will be located at that same instant of time in S. The true OWVs of any causal propagations between these points in S can then be measured and the isotropy of non-isotropy, i.e., the 'assumption' ħ = 1/2 or ħ ≠ 1/2, be verified empirically. The conventionalistic reduction argument will lose all cogency if we can show the existence of not two but three logically independent synchronization methods which are equivalent to one another only for the 'standard assumption' of ħ = 1/2 and one of which at least is not at all dependent upon, nor compatible with, the 'assumption' of arbitrary magnitudes of OWVs.

In that very special sense, then, an 'infinitely fast', i.e., unambiguously and truly simultaneous linkage of (not between) distant points is in fact possible intra-systematically, notwithstanding the finite limit upon all causal propagation velocities. A simultaneous or instantaneous transfer of momentum and energy by L to A and B is possible and unobjectionable because momentum does not thereby flow from
A to B. The event "A'=A at t'_0" does not cause the event "B'=B at t'_0" but we can truly claim to know that the two are referring to the same instant "t'_0" in S because L moves as it does due to some common (to both A' and B'), external condition. If we can empirically establish the continued perpendicularly of L relative to its line of motion through S, then a truly simultaneous connection between distant points in S can easily be established.

Before we go on to discuss how this might be carried out let me add here that much more could be said about the conventionalistic clock transport argument. I merely hope that the untenability and irrelevance of their ultimate reduction attempt becomes more conspicuous in the context of our P-simultaneity discussion, an example of a method free of any inter-clock signal connections.
REFERENCES TO CHAPTER IV


   "Extrapolation from the Michelson-Marley Experiment", J. Optical Soc. of America, 40, 185, 1950.
   "Revisions of the Lorentz Transformations", J. Optical Soc. of America, 95, 725, 1951.


7 Ibid., p. 698.

8 Ibid., pp. 698-9.


10 Ibid., p. 51.


12 Ibid., p. 133.


19 Salmon, W., op. cit., cf. (9), p. 57.

20 Ibid., p. 57.

21 Ibid., pp. 57-8.

22 Ibid., p. 58, footnote 7.


In this chapter I shall attack directly the fundamental presuppositions and operational restrictions underlying the CST. I shall argue that the initial premise (A.3) of the CST is wholly misconceived and simply false. In order to show this we must now attack the conventionalists' fundamental point of departure and ultimate refuge: the logical impossibility of synchronization procedures not subject to, or affected by, the finite delay inherent in all physical signalling procedures. Unless we can do this there is little hope that we will ever be able to effectively dismantle GCC since any objections one might raise against the consequences of its nonstandard synchronizations will always be met by the (undelivered) promise that further and further 'redefinitions' will solve all apparent paradoxes. Proponents of the CST will readily admit that they 'complicate' matters but they do not believe that they are suggesting something the logical conclusions of which lead to physical non-sense and the premise of which is simply false. To show the former will most likely involve us in endless disputes with neither side able to wholly convince the other. Showing the premise to be false, however, will hopefully prove to be the most effective way to permanently discourage conventionalistic
interpretations.

Einstein's famous remark\(^1\) alluded to earlier should not be read as propounding an all-embracing inexorable principle but merely as a recognition of the narrowly limited logical restrictions imposed by the kind of stationary set-up he is considering at that moment. The 'deep' ontological status commonly granted to this passage by conventionalists is simply untenable as we shall see. A different, perfectly legitimate approach to synchronizing distant clocks is feasible through the use of moving links or objects providing a simultaneous connection between distant points in space. Any extended reference frame moving at some constant but not necessarily known rate of speed will in principle provide such connections.

Assume for example two orthogonal frames \(S\) and \(S'\) in standard configuration. Whenever 'good local clocks' are passed or triggered by \(S'\) let them emit light signals toward each other. Each will then record the time interval between the emission of its own and the arrival of the other's signal.

\[
\begin{array}{c}
\begin{array}{cc}
S & S' \\
A & A' \\
B & B' \\
\end{array}
\end{array}
\]

\[y, x, x' \]
If proponents of the CST still want to maintain their freedom to set either clock to more or less any value of "t" they please then it will be incumbent on them to explain the discrepancy between the non-identical 'velocities' calculated from the stipulated readings of the separated clocks and the identical velocities calculated from the identical time intervals recorded on each clock separately.

If conventionalists want to question the soundness of the initial 'assumption' made at either point, i.e., that the instant of its own emission is identical with the point in time of the emission of the other signal, what they must deny, in effect, is the possibility of establishing 'standard configurations'. In other words, they must claim that the angle between the y and y' axes is somehow affected by the ways we decide to set clocks in S. To this move our response is twofold:

(i) Conventionalists cannot wish to claim that the y'-axis is really changing its inclination relative to the x-axis to such a degree that it now touches A and B with just the right delay that will always cover up the alleged difference between ↑c and ↓c. This kind of 'solution' would fail for the following three reasons. First of all, such a claim is patently absurd if we remember that GCC finds nothing wrong with admitting two, three, or any number of different
'synchronizations' of the same frame at any time. One and the same rod would then have to possess any number of inclinations with respect to a given inertial frame at the same time.

Secondly, any arguments for different inclinations from 'start-up' considerations are doomed to fail. By 'start-up' considerations we mean the claim that one must always consider this rod as originally having been set in motion from within our reference frame, starting from a state of relative rest. If we then tried to start pushing at A' and B' simultaneously, the 'simultaneity' of our first impact and hence the final inclination of the rod would be affected by our conventions regarding the OWV of causal propagation in the y-direction. However, we need not push a material rod at more than one place. Any distortions introduced by the finite speed of causal propagations during the initial acceleration stage will have subsided as soon as the rod reaches an inertial state since it is then moving free of stress. Moreover, the previously mentioned objection must again be raised in this context.

Thirdly and lastly, we need not think of the rod A'B' as being or having been influenced by our system of reference in any way. For all we know it is just rushing by and we must take it as we find it. Our alleged freedom of choice in setting or reading certain clocks in certain ways cannot possibly have any causal influence whatsoever upon S' and its
geometrical relationship of perpendicularity with respect to some given axis x of S. Even GCC must take this type of purely kinematical 'perpendicularity synchronization' as we commonly find it and then attempt to somehow reconcile this with their differing, allegedly non-contradictory time stipulations.

(ii) What proponents of the CST will therefore want to deny, of course, is the logical possibility of empirically discovering the true inclination of a system in relative motion with respect to another inertial system. This claim, if taken up as the last resort, can also very easily be refuted. The 'true inclination' referred to here is, of course, not some absolute entity but only the inclination with respect to certain axes of the particular rest- or groundframe in question. It must be emphasized once more that these determinations are intra-systematic not inter-systematic affairs. We are not interested in what other 'observers' in the other system would 'see' or what time they would assign to certain coincidences. We merely want to determine the intra-systematic angle of inclination of a moving rod with respect to its given direction of motion. We shall prove this to be unambiguously feasible in at least one special case, i.e., with the rod being perpendicular to its axis of motion. We shall need to prove no more than that in order to vindicate 'perpendicularity synchronization' as a legitimate,
non-circular and logically independent synchronization procedure, wholly incompatible with the fundamental tenets of the CST. We shall only cite two of the simplest criteria by which the perpendicularity of a moving reference system with respect to another may be said to be empirically determinable without requiring prior knowledge of distant simultaneity.

For example, think of the rod A'B' as a moving reflector or mirror of sorts. We believe its inclination to be perpendicular to the line of motion but we have no proof of it. Assume then a source (and a receiver) positioned somewhere along the x-axis. If we now emit lightflashes or discrete particles with \( v < c \) from this source towards the mirror, then the particles reflected from the approaching mirror will return to the same source point that emitted them if and only if the front plane of the mirror is perpendicular to its line of motion. Assume further, if you will, that a number of such emitter-receivers are placed along the y-axis, say within the interval AB. No matter what the temporal connection between their respective emissions might be, we can determine for each and every one of them whether or not their reflections return to their point of origin. Even according to the relativistic law of reflections\(^2\) this is possible if and only if the mirror is and remains perpendicular to its line of relative motion. Given that to be the case, we may truly
non-circular and logically independent synchronization procedure, wholly incompatible with the fundamental tenets of the CST. We shall only cite two of the simplest criteria by which the perpendicularity of a moving reference system with respect to another may be said to be empirically determinable without requiring prior knowledge of distant simultaneity.

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claim to know that our points A and B will be triggered simultaneously by the passing mirror, regardless of the 'time' our clocks stationed there are stipulated to show at that instant. Rather than relying on these totally irrelevant and irrational 'readings' it is sound physical sense and procedure to first use this moving mirror plane in order to establish simultaneity between distant points within a given inertial frame of reference.

Another criterion can be cited for empirically determining the perpendicularity of certain reference axes which is not dependent upon physical reflection laws. Instead it makes use of the fact that the moving y'-axis, if indeed inclined in just the ways required by the imputed $\xi \neq 1/2$ differences in the O WVs of signals along the y-axis of S, would necessarily have to be stretched as well as inclined. Let us set out, then, to empirically refute that possibility.

To retain our moving mirror example from before, assume that a certain rest distance $a$ has been marked off on y' before it was set in motion, e.g., by drilling a hole at B'. Assume further that we have arranged a set-up such that 0 emits light flashes or particles which precisely pass through the opening B' as it passes points E, D, etc., of S, all of which possess a y-coordinate of $a$ (cf. Fig.2a and 2b).
If the mirror axis $y'$ were in fact inclined, contrary to our expectation or 'standard assumption', as required by the CST, i.e., such that (assuming $\varepsilon=1/2$ for the moment)

$$\tan \delta = \nu (1-2\varepsilon)/c$$

then the passage of signal $P$ through opening $B'$ (in principle a uniquely localizable event in $S$) could no longer take place at the previously established points $E$, $D$, etc., along line $g$. To claim that somehow the opening at $B'$ stretches or moves along $y'$ is patently absurd if we remember that this would have to follow directly from mere stipulations about clock synchronizations in frame $S$. Anytime we decided to change the setting of rest clocks in $S$, the angle of inclination between $x'$ and $y'$ (a rest angle in $S'$) and the $y'$ coordinate of certain physical marks (rest distances $a$ in $S'$) would immediately have to change with such 're-synchronizations' -- at will and in any number of ways, since "... no
distinct hypothesis concerning physical facts is made by the choice $\xi=1/2$ as against one of the other values. That claim must surely be abandoned once we confront such arbitrary 're-synchronizations' with the criteria just discussed. The simultaneity 'defined' by 'perpendicularity synchronization', which we might call $P$-simultaneity for short, therefore constitutes an unequivocal refutation of the fundamental tenets of the CST.

We now have three sets of arguments against the CS-thesis: the arguments from physical reasoning, the slow clock transport argument and the $P$-simultaneity counterexample. Let us first briefly review the gist of the 'physical objections': to put it in a nutshell one might say that the introduction of anisotropic propagation velocities, without any physical reasons for such differences, has literally nothing that speaks for it while everything that is truly reflective of the way physicists actually proceed and reason will militate against it.

Even if it were in fact feasible to introduce such hypothetical 'differences' in a purely mathematical fashion without contradictions, this would at best be a mildly interesting mathematical game. To prove that certain accounts can be rendered more complex in certain ways is not necessarily to uncover an epistemologically interesting or essential
underlying structure. Conventionalists generally tend to be oblivious of this fact: even if it were, in a purely formal fashion, possible to replace and alter some 'assumption' within one very special and isolated context, it might very well turn out that this seemingly 'freely variable assumption' is in fact securely held in place by a multitude of connecting ties to the entire surrounding web of physical concepts.

To find 'the most general formulation' of a given set of relationships may well be of esthetic or mathematical interest; its philosophical or epistemological importance is thereby by no means established or self-evident. When it comes to render accounts 'about the world' rather than perfecting a mathematical game to its outmost, self-contained generality, paucity of assumptions and 'descriptive', in the sense of 'conceptual', simplicity assume a very different role. The importance of these 'initial assumptions' must then be illuminated from the entire (and extrinsic) point or purpose of such 'descriptions', rather than from their 'axiomatic replaceability'. On the whole, conventionalists are deploringly insensitive to the variety and intricacies of actual reasoning in scientific as well as in ordinary discourse. What they commonly fail to appreciate is something that is best summed up in the much-heralded 'philosophical revolution' contained in Wittgenstein's later work:
"I do not explicitly learn the propositions that stand fast for me. I can discover them subsequently like the axis around which a body rotates. This axis is not fixed in the sense that anything holds it fast, but the movement around it determines its immobility."

I shall try to make more of this metaphor in the closing sections of our discussion. At this point, however, let us continue to bring into focus some of the arguments that have led to the CST in the first place.

The concept of distant simultaneity seems especially suitable for a conventionalistic interpretation. One reason for the ease with which people have succumbed to a conventionalistic interpretation in this case may well have to do with some of the early popular or semi-popular remarks made by Einstein himself concerning this topic. Although he never went to the extreme of regarding the assumption of isotropic propagation velocities as merely one among many equally plausible alternatives, he nevertheless seems to have believed in a single-criterion method for establishing intra-systematic simultaneity. He at times spoke as though there were in fact no other ways to legitimately synchronize distant clocks than by transmitting light signals between them. Such temporary blindness to the variety of criteria and modes of reasoning actually employed or employable is not uncommon, however. Vexed by a narrow focus on one particular
paradigm, bent on achieving a distant goal, the initial
premises easily become blurred by a myopic concentration
on this ultimate goal. What Einstein really has in mind
from the very start is inter-systematic comparison. There
his focus on interconnecting OW signals is fully justi-
fied. He overshoots his goal, however, by inadvertantly
omitting legitimate alternative criteria for the intra-sys-
tematic procedure. Slow clock transport, for example, or,
for that matter. P-synchronization, seems to have completely
slipped his mind -- perhaps just because all three methods
are fully equivalent when the one and only one 'standard
assumption' that Einstein is prepared to suggest is 'adopted'.
The exclusive focus on the latter criterion, however,
distorts the entire picture by creating the appearance as
though there really was an initial 'dilemma' or logical
abyss which could only be bridged by a stipulation, no matter
how 'natural' the only one 'choice' actually exercised might
be. Einstein does not seriously propose or consider 'alter-
native choices', since he was undoubtedly fully aware of the
myriad of physical consequences militating against such
'alternatives' because of other, 'non-metrical' physical
concepts and principles. Nevertheless, stipulate we must --
or so at least he himself seems to have thought at times and
this is what conventionalists invariably adopt as their most
basic credo.
"There is only one demand to be made of the definition of simultaneity, namely, that in every real case it must supply us with an empirical decision as to whether or not the conception that has to be defined is fulfilled. That my definition satisfies this demand is indisputable. That light requires the same time to traverse the path A - M as for the path B - M is in reality neither a supposition nor a hypothesis about the physical nature of light, but a stipulation which I can make of my own free-will in order to arrive at a definition of simultaneity". 5

It is certainly true that, so long as I restrict my 'choice' to $\varepsilon=1/2$, this is a kind of 'stipulation' which I can make, but (a) I need not make this or any 'stipulation' in order to define intra-systematic simultaneity so as to satisfy the Einsteinian 'demand', and

(b) I could not rationally make any other if I want to be consistent and coherent.

The remarks quoted above just evidence the pitfalls of a typical one-sided diet form of reasoning.

"A main cause of philosophical disease - a one-sided diet: one nourishes one's thinking with only one kind of example." 6

Such undue restriction to a single-criterion method easily creates the illusion of some inexorable dilemma from which we will only be able to extricate ourselves by 'faith' and fiat. Such self-created dilemmas usually dissolve once things are put back in proper perspective. The whole debate
ought to serve as a lesson for keeping in mind three important reminders:

1. It is unreasonable to commit the identification fallacy of identifying one single criterion with THE method for establishing intra-systematic simultaneity when there are in fact a number of different, independent standards possible and commonly employed.

2. It is even more unreasonable to pick as THE standard the one which has initially and intra-systematically the least credentials and plausibility to offer. Of all three methods discussed, the OW signal procedure is the only one which does seem to paradoxically presuppose the very thing it is employed to establish. Disregarding the more theoretical, higher-order contexts of properly physical considerations which might ultimately justify the 'vacuum assumption' RTV=OWV, the OW signal method is the one with the least 'directly empirical' credentials. It is the conceptually most complex of all three methods and, admittedly, does seem to be viciously circular on a superficial kinematical level. Hence, it stands all the more to reason that, restricting ourselves to this first superficial level of analysis, one ought not to start with the only one method for which we have no 'direct empirical justification'.

Going along for the moment with the underlying suggestion that we truly know nothing about the logico-physical connections
between RTVs and OWVs, it seems that slow clock transport and P-synchronization are the more fundamental, simpler ways to first establish intra-systematic simultaneity. However, I do not want to press for any rigid scheme of hierarchical order among them. Keeping the proper and wider context of customary physical reasoning in mind, the signal method based on the principle RTV=OWV (in vacuo) is just as legitimate as the others. In different contexts, one or the other of these methods will naturally recommend itself. However, in the very special context of 'crystalline purity' in which conventionalists want to operate, selecting only one and, in addition, only the signal method, may very well not be a matter of 'mere convenience' but rather the first step towards a petitio principii form of argument.

3. The remarks about intra-systematic simultaneity to which Einstein (for whatever reason) sometimes lent his authority are actually irrelevant to his real argument. 'Convention' only enters the picture in inter-systematic comparisons. I do not, at this point, want to get entangled in a discussion as to whether Einstein's only 'convention', i.e., the retention of identical form of all the laws of nature with respect to different inertial frames, is really a convention in the sense of arbitrary stipulation (among a host of equally sensible contenders), or whether, and if so, to what degree, this invariance of form might actually
be said to be non-conventional or at least suggested by strong empirical evidence. Suffice it to say that the famous "relativity of simultaneity" has only modified, not altogether abolished inter-systematic simultaneity. It is simply wrong to express it in the strong form in which it is most commonly stated, i.e., "Two events simultaneous in one inertial frame are necessarily not simultaneous in another frame". See, for example, the formulation employed in the opening sentence of Winnie's article: "... the fact that distant events which are simultaneous in a given inertial frame of reference are not simultaneous events in any other inertial frame in constant (nonzero) motion with respect to the first" (emphasis supplied). That is simply false. Events perpendicular to the axis of relative motion continue to be simultaneous in all other inertial frames related by standard configuration. Hence, \( P \)-simultaneity survives inter-systematic transformations and 'absolute simultaneity' has, within the context of the STR, only been abolished in the weaker sense of requiring the qualification "Events simultaneous in one inertial frame are necessarily simultaneous in another inertial frame".

One must keep in mind here the simple but important fact that in the Einsteinian paradigm we did not intend to measure velocities in any way. In that example we set out to synchronize distant clocks and were at that moment seemingly 'forced' to 'assume' something about the OWVs of
the signals employed for that purpose. That in this particular context the velocity of a signal cannot itself be measured at the same time that it is being used to set distant clocks is, of course, trivially true. That, however, is all Einstein meant by his oft quoted remark. He never, either implicitly or explicitly, wanted to rule out the possibility of empirically establishing the magnitude of OWVs in other contexts. The E-formalism is therefore not merely a 'generalization' of Einstein's Special Theory of Relativity. Conventionalists of this kind are not explicating the true meaning of Einstein's 'conceptual revolution'. They plainly distort it by stretching one particular paradigm out of all proportions.

Throughout his lengthy discussion of the 'various attempts to determine absolute simultaneity' Reichenbach never even mentions the synchronization provided by a rod extended in the direction perpendicular to its line of motion. Proceeding from this truncated survey of 'logical possibilities' he presented us with the following false dilemma: Since we must first establish distant simultaneity in order to measure OWVs we must either first synchronize all clocks locally and then move them about or we must begin by sending out signals of which we can then logically never claim to know the true OWV. Since conventionalists thought the first method to be either inadmissible or at least fully reconcilable with their
If one half stipulations, they wanted to argue that we must forever rely on 'assumptions' concerning intra-systematic simultaneity and the true magnitude of OWVs relative to that system. They simply overlooked the plain fact, however, that in order to synchronize distant clocks we need not resort to transport nor send out any kind of signals towards these other clocks. They can also be linked kinematically, so to speak. Any moving rod or 'frame' in general will synchronize all points along all lines perpendicular to its direction of motion in a unique, unambiguous, and invariant fashion.

Without wanting to press for any rigid logical hierarchy among the three procedures for establishing intra-systematic simultaneity, P-synchronization seems to naturally recommend itself for conceptual simplicity. The one-way signal procedure, on the other hand, is the most complex and indeed the only one which, if adopted ab initio, does seem to paradoxically presuppose the very thing it is meant to establish. It only stands to reason, then, not to identify the latter procedure with the method and criterion for intra-systematic simultaneity.

In P-synchronization we have therefore a very simple way to establish distant-simultaneity relationships which does not presuppose inter-point signalling procedures or prior knowledge of OWVs. That, however, is precisely what Reichenbach claimed cannot be done as a matter of 'logical principle'. With the fall of its central premise the heavy 'ontological'
superstructure of GCC will eo ipso be deflated. It is always an earmark of a genuinely philosophical confusion that, once the spell is broken, the proverbial fly in the bottle is seen to have been kept alive solely by a special kind of one-sided diet. In this chapter, I hope to have shown the way out of one particularly complex and attractive type of logical labyrinth.
REFERENCES TO CHAPTER V


OTHER METHODS FOR ESTABLISHING INTRA-SYSTEMATIC SIMULTANEITY NOT INVOLVING LINEARLY UNIFORM PROPAGATIONS: R-SIMULTANEITY

In this chapter I shall describe another method which yields the relationship of intra-systematic simultaneity in an unambiguous fashion. This procedure will again be seen not to be dependent upon any inter-point signal procedure and, hence, be independent of any limitations placed upon the magnitude of all physical, causal signal propagation. I shall show this by relying upon certain purely geometrical relationships, such as those of angular or circumferential separations between certain points on uniformly rotating material discs or cylinders. Since it involves rotational motion, this method shall be abbreviated as R-synchronization, yielding an intra-systematic metrization called R-simultaneity.

With R- and P-synchronization we shall then possess two sets of criteria which are essentially non-signalling methods. There is, of course, the complimentary set of signalling procedures used to establish or verify distant simultaneity. Within that class we have so far encountered the light signal method, henceforth abbreviated as LS-synchronization, and the clock transport method, henceforth referred
to as CT-synchronization. The latter really is a hybrid method, being a genuine signalling procedure for finite (meaning: arbitrarily close to \( c \)) velocity transport but, at least arguably so, not a true signalling process when limited to infinitely slow clock transport (SCT) between distant points. At least in its genuinely 'signalling' version the CT-synchronization can, so it is argued, be fully subsumed under the so-called non-standard, epsilon-ized reformulations of LT-simultaneity.

Far from either attempting to exhaust the possibilities of sensibly permissible intra-systematic synchronization methods, or from attempting to isolate one or the other as 'THE' synchronization procedure per se for all purposes and circumstances, the upshot of this collection is simply the following: regardless of one's particular metaphysical preconceptions or predilections regarding the concepts of "time", "space", and "simultaneity", it is simply an undeniable fact that all four intra-systematic methods, i.e., \( P_- \), \( R_- \), \( CT_- \), and LS-synchronization are in full agreement with one another if and only if we 'choose' \( \varepsilon = 1/2 \). It is an equally undeniable fact that, even though LS-synchronization can, standing by itself and regarded in complete isolation, be made artificially 'internally consistent' for non-standard choices of \( \varepsilon \), the 'synchronization' thus introduced will invariably clash with \( P_- \), \( R_- \), and at least SCT-synchronization
procedures. By anyone's interpretation of "rational justification" this situation ought to justify us in rightly considering one and only one value for $\epsilon$, i.e., $1/2$, as the empirically correct one since it is vindicated by a number of logically independent operations and criteria.

Our 'discovery' of additional legitimate synchronization procedures will therefore fulfill these three vital functions:

(a) It will permanently break the myth of the 'original dilemma': there is in fact no logical predicament which perennially forces us to use OW signals to first introduce distant simultaneity within a given inertial frame, while at the same time forcing us, paradoxically enough, to make unverifiable 'assumptions' concerning the magnitude of such OW propagation speeds.

(b) Choices other than $\epsilon=1/2$ are not fully equivalent, in Reichenbach's sense, regardless of the extent to which we care to carry out the 'epsilonization program', if they in fact clash with one (or all) of the other equally permissible synchronization procedures.

(c) It helps to break a certain deadlock which, in the conventionalists' opinion, the discussion of the CST had reached in the recent literature. After the slow clock transport argument was raised, conventionalists were still able to retreat to this position: granted that there is one other method (SCT-synchronization) which renders OWVs non-
conventional, empirically determinable, magnitudes, we are still left with a genuine choice between these two methods themselves. As between these two, our 'original choice' was still unrestrained -- a perfect stalemate, so to speak. Declaring them to be on completely equal footing with regard to conceptual clarity, permissibility, and sense, we may apparently opt with equal justification for one or the other as 'THE' ultimate (original) standard. Nothing about SCT-synchronization alone, so the argument continues, could possibly be said to distinguish it as being any better, simpler, more sensible, or 'more true' than LS-synchronization with its paradoxical initial step. Even though they agree for only one value of $E$, since no further independent agreement or disagreement with another, third 'umpire' method seems to be anywhere in sight, we may still exercise our conventional freedom of choice in elevating one or the other of the two to the position of THE one and only original and ultimate criterion.

This deadlock - if it ever was one - I hope to have broken by the introduction of not one but three additional legitimate and independent intra-systematic synchronization procedures. The unanimous agreement of all four for one and only one value of $E$ will surely render the degree of 'choice' involved here trivial and vacuous. It should also undercut the temptation to succumb to the mono-criterial
identification fallacy. It makes no sense to 'reduce' all the various criteria actually employed to 'essentially' one and only one 'ultimate' standard which is somehow epistemologically 'prior' to all the others. If in fact all these different methods are in full agreement for only one particular value of $\varepsilon$ then it must, so it seems, be admitted that the CST, taken at face value, has literally been refuted.

Let us now proceed to consider the new method in somewhat greater detail. On pp.55-56 we saw how uniform circular motion must be 're-described' if distant simultaneity is determined via the LS-method, stipulating values for $\varepsilon$ other than $1/2$ as done in the standard isotropy 'assumption'. The average uniformity of circular rotation can, of course, be determined unambiguously with the help of any one good local clock. The instantaneous circumferential speed of 'on-the-average' uniformly rotating points must, however, become a complicated nonuniformly accelerated and decelerated kind of motion under the $\varepsilon \neq 1/2$ stipulation, as derived earlier in our discussion. I have also pointed out already that such motion should lead to tremendous and easily discoverable effects if the rotating 'points' in question are taken to be electric charges moving in a magnetic field. Conventionalists have yet to face the task of 'explaining away' the expected effects of such accelerations and
decelerations by suitably 're-describing' the electromagnetic laws and concepts involved. Up to this date they have merely dealt with the much simpler linearly uniform 're-descriptions', i.e., the context in which, after all, the E-notation was first conceived and developed. Leaving aside all questions as to the degree of success, completeness, and convincing plausibility with which that task has been carried out so far, it seems to have completely escaped their attention that intra-systematic accelerations, linear as well as angular (and all their various physical 'effects' and 'causes'), would also be directly affected and become dependent upon their arbitrary $E \neq 1/2$ LS-synchronizations. These immediate and obvious difficulties have nowhere been discussed or even been acknowledged in the existing conventionalistic literature. However, for the purposes of this discussion I would like to move away from the more remote and complex concepts of electromagnetic fields and charges moving within them. Suffice it to say that conventionalists have yet to realize and, of course, deal with, their introduction of directionally variable accelerations in such contexts.

For the purpose of deriving simple kinematical relationships from rotational motion which can serve as unambiguous criteria for justifiably speaking of distant simultaneity connections we may restrict ourselves to less complex
mechanical models, such as rotating solid material discs or beams, rather than isolated masspoints or charges. While it may seem less formidable as task to postulate the appearance (and 'explain away' the effects) of variable circumferential velocities and varying spatial separations between isolated points, such 'assumptions' quickly become absurd if we consider continuous media such as a solid material disc. As I understand the conventionalistic interpretation of 'moving lengths', no one wants to claim that material objects really change their lengths upon various $\xi$-dependent 're-definitions' of simultaneity; rather, it is the 'interpretation' given to the ratio $L/v = \Delta T$ (a time span directly measurable with the help of only one clock) that changes. Ontologically speaking, if I may use this rather awkward expression, the permanence of one and only one 'real' extension $L$ and one 'real' propagation rate $v$ (relative to $S$) is both undisputed and necessary for the very formulation of the $\xi$-'hypothesis'. The element of conventional freedom creeps in merely through the alleged lack of independent procedures or additional criteria permitting us to 'metrize' either $L$ or $v$ separately and in advance. It is therefore the alleged lack of independent epistemological access or agreement which introduces epistemological uncertainty and ambiguity, forcing us, as well as permitting us, to proceed by irreducibly unverifiable and unjustifiable fiat.
This crucial lack of independent access to the admittedly real (unaffected by stipulation) state of affairs constitutes the essential core of conventionalism. If this fundamental epistemological restriction can be shown to be illfounded, all elements of 'doubt' and 'choice' regarding empirical access to, and justifiable agreement upon, one (correct) magnitude (metric) ascribed to these phenomena will \textit{eo ipso} be dissolved.

For the purpose of focusing in on these allegedly fundamental epistemological restrictions it might be helpful at this point to briefly review the conventionalistic 'moving length' interpretation. This will permit us to better discern what is and what is not 'real' and 'variable' according to the conventionalistic interpretations. We shall then apply these distinctions and reminders to physical marks placed upon rotating material discs in order to see whether the periodic appearance of such marks in certain places will yield a justifiable criterion for claiming knowledge as to the simultaneous position of another distant but related mark at that same instant. If that is the case, we will have another way of determining distant simultaneity that does not already presuppose knowledge of the magnitude of OW signal propagations.

For uniformly rectilinearly moving rods the conventionalistic 'expansions' and 'contractions' seem to follow quite
naturally from the stipulated OWWs. A rod of length \( b \) and speed \( v \) will pass over any given point in \( S \) in a time interval \( \Delta T = \frac{b}{v} \). Since this is a one clock measurement, it must be preserved under the LS-'re-definitions' of distant simultaneity. We therefore will be forced to interpret this passage time as being that of a rod moving with a different OWW of \( v^* = \frac{vc}{(c \pm v(2\varepsilon-1))} \) and possessing a length of \( b^* = \frac{b \cdot c}{(c \pm v(2\varepsilon-1))} \), i.e., \( \Delta T = \frac{b^*}{v^*} = \frac{b}{v} \) (omitting the standard lorentz 'contraction' factor \( 1 - \frac{v^2}{c^2} + \frac{1}{2} \)).

Even without questioning the initial plausibility of this 're-interpretation', however, we must keep in mind that only one rod of constant (though allegedly 'indeterminable') length is actually moving about in this case. Thus even if we were to grant intelligibility to the notion of different observers at given points in \( S \) 'setting their clocks' according to their own different choices of \( \varepsilon \), we are not thereby physically altering the real extension (or speed) of that one rod. In particular, physical marks placed upon it, e.g., one distinguishing mark at its midpoint, will still be 'read off' or interpreted as being the center-point of rods of variable 'lengths' as 'measured' by these different observers. Regardless of the measure assigned to the entire length, however, all would agree that the center-point still marks the real center of the rod. No one wants
to claim that it actually, physically moves about on the rod as we decide to 'set our clocks' in whatever way we please.

Now assume that we have one continuous material disc of circular shape whose circumference we mark (when at rest) with some indenture A and another mark B at exactly the opposite point halfway around the entire perimeter. If placed under ('on the average') constant uniform rotation, the fixed relative position of these two marks will provide us with a criterion which logically supersedes any attempt to 'assign' different times to their respective instantaneous positions relative to the embedding inertial frame. The 'interpretation' to which the conventionalist takes refuge when dealing with isolated moving masspoints, i.e., the claim that points A and B travelled at different speeds and therefore covered different distances, is no longer possible when A and B are continuously connected with one another. No one can seriously want to claim that these marks really (physically) move along the otherwise undistorted perimeter. Given that connection, however, we can justifiably claim to know where, at any given instant, a number of distant points in S are located momentarily. Thus, locating A at some instant $t_0$ will eo ipso permit us to claim knowledge of the factually correct simultaneous position of B at that instant (or, for that matter, of any number of
to claim that it actually, physically moves about on the rod as we decide to 'set our clocks' in whatever way we please.

Now assume that we have one continuous material disc of circular shape whose circumference we mark (when at rest) with some indenture A and another mark B at exactly the opposite point halfway around the entire perimeter. If placed under ('on the average') constant uniform rotation, the fixed relative position of these two marks will provide us with a criterion which logically supersedes any attempt to 'assign' different times to their respective instantaneous positions relative to the embedding inertial frame. The 'interpretation' to which the conventionalist takes refuge when dealing with isolated moving masspoints, i.e., the claim that points A and B travelled at different speeds and therefore covered different distances, is no longer possible when A and B are continuously connected with one another. No one can seriously want to claim that these marks really (physically) move along the otherwise undistorted perimeter. Given that connection, however, we can justifiably claim to know where, at any given instant, a number of distant points in S are located momentarily. Thus, locating A at some instant $t_0$ will ipso ipso permit us to claim knowledge of the factually correct simultaneous position of B at that instant (or, for that matter, of any number of
other points along the perimeter).

If conventionalists want to cast any 'doubt' on the epistemological legitimacy of such intra-systematic claims, they will first have to construct intelligible theories as to how and why continuous media 'behave' in the way required to cover up their allegedly indistinguishable stipulations. Until that task is completed, however, we do possess in R-synchronization a genuine and unambiguous simultaneity connection between distant points which is logically independent of prior knowledge of OW signal speeds.

In addition, it is not even necessary to resort to dynamical arguments and to invoke the theoretical description of the properties of continuous media. There are simpler means of assuring the empirical legitimacy of the arguments and 'assumptions' involved in R-synchronization. Instead of a solid circular disc let us merely take an extended straight rod AB and rotate it around its centerpoint M. Think of AB as a physical beam possessing vertical thickness too. The straight line AMB may then be thought of as the line of intersection of a perpendicular plane and the horizontal plane of rotation. The continued straightness of our rotating rod AMB could then be empirically verified in a fashion exactly analogous to the methods employed for establishing perpendicularity in general. In other words, in the plane of rotation the rod could be 'tested' as a
reflecting mirror or in the other manner described on p. 121.

Given that we can unambiguously establish the instantaneous perpendicularly of AM and MB relative to any given line through M in the plane of rotation, we have verified the continued straightness of our rotating rod AMB. Hence, R-synchronization logically follows from the possibility of empirically establishing P-simultaneity. Given R-simultaneity, then, the correct synchronization of the entire plane of rotation is established.

In discussing R-synchronization we have seen how the seemingly innocuous 're-metrization' of linearly uniform OWVs would invariably entail the 'prediction' of non-uniform angular accelerations of rotational motion if it is to be carried out consistently. The latter 'predictions', however, can in turn be dismissed out of court or be empirically falsified in the ways described above. R-synchronization is, however, not confined to the plane of rotation. We can also 'transmit' the simultaneity connection provided by rotational motion. For that purpose let us consider a solid three-dimensional cylinder.
Since $\xi$ and $\eta$ are independent of $\varepsilon$, they can always be 'chosen' to be equal to $1/2$, such that we have to contend only with the alleged free variability of $\varepsilon$. One version of this type of argument has been published previously by Feenberg\textsuperscript{1}:

Consider a long, straight shaft of circular cross section mounted on frictionless bearings at rest in an inertial reference frame. A narrow, straight yellow line is painted on the shaft. Counting devices A and B are placed at two different positions along the shaft (on a straight line parallel to the axis of the shaft). Each device records the passage of the yellow line and displays the number of passages on a dial. The shaft is initially at rest with A and B dials set at zero. A torque is applied for a short time, setting the shaft into rotation. Eventually all torsional vibrations damp out and the shaft is rotating uniformly with angular frequency $\omega$. The number $N_A$ and $N_B$ on the dials at A and B can be translated into clock readings

$$T_A = 2\pi N_A / \omega, \quad T_B = 2\pi N_B / \omega$$

Thus the counters serve as clocks and, in fact, as synchronized clocks. With supplementary clocks at each position the possibility of a dependence of synchrony on the direction of rotation can be tested. Arzelies lists early references dealing with versions of this system. He also shows that the description in a moving reference frame does not require a relativistic torsion of the rotating shaft.
Once again we can simplify the argument by supplying some additional empirical criteria for verifying the presence or absence of distorting longitudinal torsion. To that end let the cylindrical shaft be equipped with a protruding straight edge or think of the entire device as consisting of a rotating planar cross section only.

Once again, then, P-simultaneity, i.e., momentary perpendicularity of EFGH and the x-axis can be established unambiguously with either of the two methods mentioned previously. Thus, on the simplest kinematical level R-synchronization and P-synchronization are seen to be intimately connected. There is therefore no immediate need to even invoke the more complex dynamical arguments which conventionalists will have to overcome if they wish to discredit R-simultaneity within a given inertial frame. Hence, on a number of different levels, R-synchronization as well as P-synchronization will directly and decisively reduce ad absurdum the proposed ('empirically indistinguishable') epsilonized LS- 'synchronizations'.
REFERENCES TO CHAPTER VI

VII.

THE EPISTEMOLOGY OF THE CST

1.

We have now reached the point where we may summarize the philosophical core of our discussion. As we saw, the one central tenet common to all brands of conventionalism is the purported existence of truly equivalent alternative accounts for some given state of affairs. Regardless of their idiosyncratic differences, all conventionalists will agree on this one 'acid test'. In fact that expression itself was coined by Gruenbaum who claims to differ from Reichenbach in many other respects. In the context of rejecting the notion that continuity be deemed just as 'conventional' an assumption as isotropy, Gruenbaum invokes this principle¹:

... these considerations lose much of their force, it would seem, as soon as one applies the acid test of a convention to the conventionalist conception of continuity in physical geometry: the availability or demonstrated feasibility of one or more alternate formulations dispensing with the particular alleged convention and yet permitting the successful theoretical rendition of the same total body of experiential findings, such as in the case of a particular system of units of measurement.
It is this assumption of 'total equivalence' that I have attacked most vigorously because I believe this to be the jugular vein of conventionalism. I have attempted to show that this fundamental and necessary condition is in fact not satisfied with regard to the 'alternative' E-re-metrizations for intra-systematic simultaneity. To that end one only needs to point up the limits of the self-imposed confines within which the equivalence contention arose and where it is thought to apply. Once we recognize the patent artificiality of these tacit restrictions, the question of 'choosing' the correct description is really a moot one. At that stage it becomes difficult to even perceive the initial plausibility which may have lead philosophers of a certain inclination to propound the CST in the first place.

It might be helpful, therefore, to sketch once more what I believe to be the basic assumptions of conventionalism. In many ways conventionalism is merely a philosophical reaction to another attempt to understand the 'essential nature' of all human knowledge in general, categorical terms, namely rationalism. In trying to escape the reputed inexorability of apriori principles, conventionalists stressed (and overstressed) the purportedly ever-present element of human choice as the ultimate cornerstone of certainty and knowledge. As is so often the case at this
level of generalities, it is difficult here to disentangle
the real problem amidst the proposed cures. Both 'answers',
it seems to me, suffer from the same defect, i.e., that of
aiming to come up with 'justifications' for knowledge in
general rather than analyzing in detail the specific contexts
and circumstances in which particular concepts function as
and when they are actually employed. Conventionalists have
merely redirected the rationalistic quest for 'ultimately
grounded' certainty to a different level. The general thrust
in that direction is, however, still alive, still unquestioned
and presumed to be legitimate and answerable. The role of
apriori principles is now being played by a specifiable
set of somehow basic, epistemologically privileged statements
loosely called 'the observation basis'. Anything which
goes beyond these immediate 'observations', whatever connects
them and relates them to one another, is deemed to be a
'theoretical statement'. These latter kinds of statements
are believed to be the quintessence of all ordinary as well
as scientific descriptions and accounts, thus rendering all
discourse 'theoretical' and 'assumptionladden'. The 'assump-
tions' inevitably involved in these 'theoretical statements'
ought to be freely replaceable (though never wholly dispens-
able) since they are not 'directly grounded' in first-order
observational 'reports'. Hence these 'assumptions' are
really forever 'unverifiable' on the 'ultimate' level of
observation-basis statements. Given this clear division between, so to speak, two levels of language, it is deemed to be an important philosophical discovery that one and the same observation basis can be 'described' and systematized consistently by any number of different 'theoretical assumptions'. The model of language which lies behind this reasoning is apparently the logico-mathematical paradigm of a closed deductive system. All language, ordinary as well as scientific discourse, is inadvertently patterned to fit that model. We merely lay down a number of 'axiomatic assumptions', derive certain observational consequences from them and check whether the predictions thus derived square with the irreducible basis of 'bare observational facts'. As long as two different systems of fundamental 'definitions' or 'axiomatic assumptions' yield exactly the same set of 'observational consequences' they are declared to be totally indistinguishable with respect to sense and truth. They may well differ greatly in syntactical (and computational) complexity, as well as in paucity of 'unverifiable assumptions', but identity of observational consequences is the sole criterion for granting them equal epistemological status. Hence semantically they are deemed to be indistinguishable and equally valid, sensible, and 'true' accounts of the world. Both sets being 'alternative descriptions' of one and the same irreducible observation basis, we are
said to have no empirical grounds for preferring one set over the other, save for reasons of syntactical, computational, or 'descriptive' simplicity. (It is quite obvious that this reasoning puts the cart before the horse). There can, in principle, be no other criteria to help us choose the better or 'the true' account. Even those 'assumptions' requiring a total revision of ordinary concepts and familiar modes of reasoning cannot be ruled out as absurd, bizarre, or nonsensical. As long as they 'entail' (in a strictly deductive fashion) the same observational consequences they must necessarily be deemed to be equally permissible and equally 'true to the facts'.

This, roughly, seems to be the kind of conventionalism which Reichenbach so vigorously espoused in his seminal works. Many variations upon this theme have since been introduced; the basic principle and fundamental core, however, remained the same: the allegedly total observational equivalence of any and all LS definitions of simultaneity (within the limits $0 < \varepsilon < 1$). There have also been numerous attempts to work out a consistent nomenclature for such variations and the entire controversy surrounding this area of the philosophy of science. Sklar, for example, has recently proposed a taxonomy for the more general problem area of the relationship between physics and geometry which should also be applicable to the specific topic of this
discussion. Although I find his systematization helpful I am not altogether satisfied with the distinctions as he draws them. I shall elaborate on some of my reasons for not being able to find 'my position' within his scheme shortly, i.e., in the course of outlining what I perceive to be the crucial pitfalls of conventionalism. First, however, let me briefly describe Sklar's "taxonomy of the controversy"³.

According to Sklar the general thesis that our ascriptions of geometric structure to space or space-time are underdetermined by all observational data can (and has) elicited the following possible responses:

- accept the observational/theoretical
- deny the observational/theoretical

```
   distinction
   |                       | distinction
   |   deny the reductionist |   deny the reductionist
   |   account of theories   |   account of theories
   \--------\              \--------\             \--------\              \--------\             \--------\              \--------\             \--------\              \--------\             \--------\              \--------\
   scepticism               conventionalism           a-priorism
```

Reductionists, as Sklar uses that term, differ from anti-reductionists in holding scientific theories to be ultimately reducible to their observational consequences. If two theories yield the same predictions they can only
differ trivially as but different formulations of one and the same theory. There is no room for any genuine, nontrivial epistemological 'doubt' or 'choice' as to which of the 'two' is (or is to be selected as being) the true account, both really being identically one and the same theory.

Anti-reductionists, in Sklar's usage, apparently deny this trivial kind of equivalence. The two theories, built upon the same observational basis, are making genuinely different claims or 'assumptions' about the world. The only remaining problem is how to decide between them. Skeptics are said to be those who deny the existence of any legitimate grounds, empirical or apriori, for deciding between these supposed 'alternatives'. Apriorists would be termed those who maintain that general, overriding principles such as prior plausibility, rationality, simplicity, and general explanatory power are available which allow us to determine which of the alleged 'alternatives' we accept as the only true account. Conventionalists, according to Sklar's characterization, are those who, basically agreeing with the skeptic's position, will nevertheless choose one of the theories as 'true by decision'\(^4\). As he himself concedes, this kind of conventionalism would imperceptibly reduce back to either skepticism or reductionism\(^5\).

Whether or not Sklar is right on this point as well as in his characterization in general is, however, not my
main concern. The reason I do not find his taxonomy very helpful is a more fundamental one. It may appear at first sight from much of what I have said earlier in this paper that my position must be that of Sklar's 'apriorism'. That, however, would also commit me to 'accept the observational/theoretical distinction' as well as the (tacitly presupposed) condition that the 'theories' in question are in fact intelligible (rather than 'observationally equivalent') contenders for the title of alternative descriptions.

For quite a number of reasons Sklar's kind of 'analysis' and what I deem to be called for have little, if anything, in common. Sklar himself seems to be steeped in a tradition of 'philosophizing' which has largely been abandoned in virtually all areas except perhaps the more technically oriented niches of the philosophy of science. Some of these traditional misconceptions will shortly be enumerated. One of them, however, may as well be cited at this point: Sklar nowhere takes the time to spell out what precisely either of his two fundamental dichotomies, i.e., the 'observational/theoretical distinction', or the 'denial' or 'acceptance' thereof amounts to. He apparently takes it for granted that such a distinction can be drawn or denied in general, regardless of the context and the purpose which any such inquiries might have. He never seems to feel a need to tell us what in his opinion 'the' (THE?) observational/
theoretical distinction consists of; yet he quite summarily
disposes of the 'alternative' which 'denies' the coherence
of 'the' observational/theoretical distinction with only a
few remarks. He advances a rather curious argument to the
effect that 'the distinction' must be 'accepted' because the
present scientific community firmly recognizes the complete
equivalence of two particular physical theories, the
Schroedinger and the Heisenberg formulations of quantum
mechanics. To examine the details of that particular
example as well as its possible relevance to Sklar's curious
'anti-denial' argument would take us far beyond the scope
of our present inquiry. Just what the particular equivalence
and the observation basis (bases?) of these two specific
theories amount to is itself the subject of a lively and
ongoing debate. Suffice it to say at this point that I
am in no way denying the feasibility of distinguishing
between theoretical and empirical components of particular
theories in particular contexts. It is my contention,
however, that conventionalists, empiricists, and others have
traditionally played fast and loose with such distinctions.
The general thrust of their 'analysis' is misconceived and
confused in numerous details.

To substantiate these charges I shall sketch part of
the philosophical background out of which the CST developed.
Many of the confusions operative here are shared by
conventionalists, empiricists, and logical atomists alike. Rather than trying to pin the ultimately proper label upon the man or his school, I shall let Reichenbach's writings speak for themselves.

2.

The general type of tacit assumptions which I believe to be fundamental to Reichenbach's philosophy have been eloquently summarized in J. L. Austin's "Sense and Sensibilia"\textsuperscript{7}. Following his quote I shall attempt to isolate these same three elements in Reichenbach's work.

In a nutshell, the doctrine about knowledge, 'empirical' knowledge, is that it has foundations. It is a structure the upper tiers of which are reached by inferences, and the foundations are the data on which these inferences are based. (So of course - as it appears - there just have to be sense-data). Now the trouble with inferences is that they may be mistaken; whenever we take a step, we may put a foot wrong. Thus - so the doctrine runs - the way to identify the upper tiers of the structure of knowledge is to ask whether one might be mistaken, whether there is something that one can doubt; if the answer is Yes, then one is not at the basement. And conversely, it will be characteristic of the data that in their case no doubt is possible, no mistake can be made. So to find the data, the foundations, look for the incorrigible.

Reichenbach, as I will show, operates on these three basic assumptions:

(1) There is a clear-cut hierarchical structure of
'knowledge' with inherently different 'levels' and modes of discourse.

(2) We must strive to find and isolate the basic, indubitable elements of knowledge.

(3) We can then isolate the inferred, dubitable, or 'theoretical' elements and vary or replace them - within certain limits - at will.

Before I get into the details of Reichenbach's search for the 'basis' and his search for the 'freely variable' elements of knowledge I would like to spend some time characterizing the general thrust of this kind of approach to philosophizing. It is a typical specimen of the kind of 'analysis by reduction' which Wittgenstein, Ryle, Austin, et al., have shown to be hopelessly muddled and confused.

The tacit assumption that the ordinary statements, criteria, and modes of reasoning employed by laymen and working scientists are always in dire need of refinement and purification first leads us to 'discover' (postulate) hidden but seemingly inextricable logical dilemmas, such as the one allegedly connected with the notion of a 'one-way velocity'. Certain procedures and certain criteria, viewed in isolation and taken out of their fullblooded context, are suddenly made to appear paradoxical and selfcontradictory. In our quest for refining standard procedures to their 'purest form' with outmost 'epistemological rigor' we tend
to lose sight of the forest for the trees. We have literally created the very dilemma from which conventionalists are claiming to rescue us by fiat. 'Analyzing away', disregarding, or simply overlooking all of the various other criteria by which we commonly justify the 'isotropy assumption', conventionalists manage to push themselves beyond the point where the ice will support them:

The more narrowly we examine actual language, the sharper becomes the conflict between it and our requirement. (For the crystalline purity of logic was, of course, not a result of investigation: it was a requirement.) The conflict becomes intolerable; the requirement is now in danger of becoming empty. We have got on to slippery ice where there is no friction and so in a certain sense the conditions are ideal, but also, just because of that, we are unable to walk. We want to walk: so we need friction. Back to the rough ground!

...The preconceived idea of crystalline purity can only be removed by turning our whole examination round. (One might say: the axis of reference of our examination must be rotated, but about the fixed point of our real need.)

Contrast this with Reichenbach's avowed methodological goal:

The axiomatic method is the only method that will reveal the logical structure of the theory with perfect clarity. The distinction between axiom and definition leads to the separation of empirical content from arbitrary concept formation, and the derivations of particular statements clearly reveal the empirical and the logical components of every assertion. The rigor of demonstration leads at the same time to exhaustive formulations of the assumptions and automatically excludes the formation of vague concepts.
For Reichenbach, philosophical analysis is literally a chemical process: one must 'boil down' customary concepts and modes of reasoning (being unable to either note or cope with their true complexities and subtleties) and 'distill' their basic 'conceptual atoms'; having successfully completed this reduction we can then proceed to play the variational game of synthetically reconstructing combinatorially different but 'effectively equivalent' logical complexes out of the same basic elements. What we have before us here is the well-worn belief that through this kind of 'reductive analysis' we can effectively perform the traditional metaphysical task of arriving at the ultimate structure of reality. By recasting ordinary language, and standard inferences or forms of reasoning in their 'purest' and only 'logically adequate' form we will somehow be able to read off the structure of reality from that (e.g., that - and to exactly what degree - it is 'supplied' by us 'by convention'.)

The CST seems to me to be a perfect example of one of those 'bumps of the understanding' that result from blowing out of proportion a certain aspect of the logic of certain kinds of statements while being preoccupied with this aspect to the exclusion of all others. The only effective cure here consists simply in restoring a proper perspective. That kind of shift in focus is all I have tried
to bring about in the previous chapters. No general philosophical counter-theory is either called for or helpful. Instead we must deal with each philosophical perplexity as it arises, illuminating the particular 'reduction', restrictions, and confusions to which it leads or out of which it arose.

The chief 'method' is therefore simply a collection of reminders about the sorts of uses which the various expressions and concepts of the language-game typically have.\(^{11}\) Only then will we be able to resist the temptation to fall into the trap of facile generalizations encountered in 'reduction analysis'; e.g., "It is impossible to know whether two events at different space points are simultaneous"\(^ {12}\). Instead of carefully noting the various ways and forms in which 'knowledge' really operates in this context, Reichenbach, by 'analyzing' it down to one narrow aspect, is lead to pronounce philosophical tenets of ultimate 'rigor' and generality. Of course, in a trivial but viciously circular sense, he is right, since he has implicitly assumed or stipulated 'knowledge' to fit his particular preconceptions: if one can only 'know' that which is 'local' and 'immediately' perceived, "distant simultaneity" becomes indeed a puzzling or even outright paradoxical notion. There simply 'must be' a definite and basically very simple hierarchy to all of human knowledge: there are
certain invariant "elementary facts" and probable but
dubitable "theoretical inferences":

Every factual statement, even the simplest one,
contains more than an immediate perceptual
experience; it is already an interpretation
and therefore itself a theory. To infer the
existence of a light source from the perception
of light, or the existence of an external mass
from the perception of pressure, is an extrapola-
tion which only seems to be a matter of course
because its consistency has been tested innumer-
able times . . . In this sense, some facts are
more elementary than others, namely, those whose
interpretation within certain limits does not
depend upon theoretical conceptions. This is why
the facts of daily life appear so certain to us.
Because of their imprecision they are invariant
relative to all scientific theories.

Accordingly, this investigation starts with
elementary facts as axioms; all are facts whose
interpretation can be derived from certain
experiments by means of simple theoretical
considerations. (Italics in the original)

Many of the themes that we encounter at the bottom of
Reichenbach's philosophy are all too familiar from the flood
of recent studies spawned by Wittgenstein's later work,
i.e., the naming theory of language, the meaning of concepts
being grounded in somehow privileged 'ostensive definitions',
the concomitant 'inferential nature' of all objective know-
ledge, etc. In fact, I believe the 'distant simultaneity
paradox' to be exactly analogous to the alleged puzzle
concerning the existence of 'other minds'. I will also try
to point out the striking analogies between Reichenbach's
'local time' or 'private clock' notion and the familiar
'private language' argument.

Starting with the unquestioned presumption that all language-games can or must be modeled upon the paradigm of mathematical axiomatic systems, the task of finding the base level at which these self-contained deductive schemes 'link up' with the world becomes of paramount importance. It is only this 'foundational problem' that distinguishes mathematical propositions from empirical propositions:

...[S]ince the physical axioms also contain the whole theory implicitly, they must themselves be justified: they must not be arbitrary but true. "True" refers again to a factual judgment ultimately tested by perception.

It is not easy to arrive at such a judgment with respect to the axioms of a theory. Usually the axioms, representing higher levels of abstraction, are quite remote from direct sense perception. It is frequently much easier to confirm a particular statement of a theory than its axioms. Therefore, only an indirect confirmation of the axioms will be possible...

In order to avoid this difficulty, it is advantageous to approach the axiomatization in a different fashion. It is possible to start with the observable facts and to end with the abstract conceptualization. A certain loss in formal elegance will be balanced by logical clarity. The empirical character of the axioms is immediately evident, and it is easy to see what consequences follow from their respective confirmations or disconfirmations. Such a constructive axiomatization is more in line with physics than is a deductive one, because it serves to carry out the primary aim of physics, the description of the physical world.16

Having already subscribed to an ultimately solipsistic sense-data theory, Reichenbach must struggle to somehow save...
the apparent objectivity of at least some empirical propositions, i.e., scientific statements. Strictly speaking, Reichenbach has all but abolished the latter distinction already since apparently all statements must be 'theoretical' and 'hypothetical'. There still are (or rather: there 'must be') purely private, absolutely certain 'experiences' of sort; and, at least in principle, we could always 'name' these privately and create our very own little 'conceptual schemes'. Yet for apparently no other reason than communicative simplicity we do arbitrarily choose one 'code' over another. By conveniently agreeing on some one particular set of 'coordinative definitions' correlated to 'elementary facts' we manage to arrive at 'objective empirical statements'. Naturally enough, such a 'naming' of 'things' appears to be, at bottom, entirely arbitrary:

Definitions are arbitrary; they are neither true nor false . . . The mathematical definition is a conceptual definition, that is, it clarifies the meaning of a concept by means of other concepts. The physical definition takes the meaning of the concept for granted and coordinates it to a physical thing; it is a coordinative definition. Physical definitions, therefore, consist in the coordination of a mathematical definition to a 'piece of reality'; one might call them real definitions . . .

The difficulty concerning coordinative definitions is similar to that concerning elementary facts: the physical thing that is coordinated is not an immediate perceptual experience but must be constructed from such an experience by means of an interpretation . . . We eliminate this difficulty in the same way as
we did above: for elementary interpretations we use coordinative definitions whose degree of precision is not important and which, in particular, do not make use of relativistic definitions. This device leads to a restriction in the arbitrariness of coordinative definitions: they must not contradict elementary definitions. For instance, a cube must not be defined as a straight line. Such a definition would require a reorganization of the elementary theory, even the 'theory of everyday life'.

As Wittgenstein has shown in numerous examples, Reichenbach would be hard pressed to say just what either 'sameness' or 'degree of precision' could possibly mean in such contexts. Furthermore, it is by no means clear in what sense Reichenbach's 'elementary definitions' can possibly be 'contradicted'. His own example attests to the vacuousness of that very notion. I, for one, have not the vaguest idea what it would really mean to "define a cube as a straight line". Either this amounts to no more than a trivial renaming game in which X idiosyncratically substitutes (but uses correctly) the term "cube" wherever Y and others use the term "straight line". No confusion arises as long as we remember the correct translation of this strange way of speaking and so long as X speaks sensibly, i.e., uses his pet term in accordance with the logic of "straight line". On the other hand, if anything more is to be involved in this 'redefinition', i.e., if anything 'about the world' is supposed to follow from it, then it is simply a nonsensical
piece of utterance. Therefore, the exhortation that one 'must not' engage in this 'redefinition' because of its complexity is also nonsensical. The required 'reorganization' of 'the theory of everyday life' is not just extremely cumbersome, it is wholly unintelligible. One does not prove conventionalism, i.e., the claim that ordinary language could, in principle, be revamped arbitrarily, by citing an example of an unintelligible 'statement'. We have thereby 'proved' no more than that this example in fact is unintelligible.

If one were really attempting to 'use' a cube as the 'coordinate definition' i.e. the physical embodiment of a straight line, one would not even know how to begin 'using' it. What, for example, would now be meant by the intersection of two "straight lines"? And what of the angle between them? What is the 'length' of a certain segment -- measured 'in a straight line'? Can we still speak of the sides of this cube as themselves being 'straight'? Etc., etc. There is a myriad of operational procedures as well as conceptual questions which are not just 'more complicated' but which have altogether lost their point. Far from being merely slower in answering, we are simply at a total loss as to what we are asked to conceive of here.

3.

Before we proceed to examine the last and 'variational'
stage of Reichenbach's epistemological hierarchy let us first focus on the 'foundational stage' a bit more closely. Since the CST presupposes (or claims to prove) that the notion of the simultaneity of distant events is in principle 'unverifiable', if not utterly meaningless, Reichenbach must attempt to build up an iron-clad dichotomy between 'local simultaneity' and 'distant simultaneity'. The latter, then, must appear completely 'non-empirical', always inferred, highly theoretical, and 'arbitrary'. By the same token, the inherent logical superiority claimed for local simultaneity must be 'grounded' in our ultimate 'logical atoms', i.e., 'elementary facts', which must themselves be set one level above our ultimate 'epistemo-logical atoms', i.e., direct and private (pre-linguistic) 'experiences':

Coincidence is frequently described as an immediate experience of simultaneity. This assertion is not quite correct. Such an experience furnishes only subjective coincidence; ... This is a psychological phenomenon because the merging of the two events in a coincidence takes place in the mind of the observer ... In addition, there are objective coincidences in which, independently of the observer, two events coincide. The mind merely records such a point event ... Both methods - the observation of subjective and of objective coincidences - are used in physics. But there is a great difference between them. With respect to the second kind, coincidence is inferred, not perceived. Perceptually speaking, a totality of qualities is given, such as dark and bright spots or sound impressions; from these qualities the objective coincidence of things is inferred ... Only
subjective coincidence has the character
of the immediately given; subjective
coincidence alone is independent of all
interpretations and is a necessary condition
for all physical observation. . . . The assertion
that all physical observations are reducible
to coincidences holds, therefore, only for
subjective coincidence. . . . One must be
careful, therefore, not to transfer the
evidential character of the subjective
experience of coincidence to the use of
the concept of coincidence for a description
of objective physical events. This coordination
of the concept of coincidence to the physical
event has merely the certainty of all empirical
knowledge and depends upon certain definitions.
The decision to base the theory of space and
time, nevertheless, upon objective coincidence
as an elementary fact is justified by the
consideration that objective coincidences are
elementary facts. . . . which remain relatively
invariant with respect to a great variety of
interpretations. . . . Even here there is a
hierarchy. There are objective coincidences
that are 'directly observable'; they are
coincidences whose space points lie above the
threshold of perception. We shall call them
objective coincidences of the first degree.
One usually thinks of them when speaking of
coincidences as intuitively given elementary
facts. Examples are furnished by the
coincidence of a light ray with a cross wire, or
of a pointer with a mark on a scale. Even they
are merely inferred as objective coincidences,
but for their interpretations concepts of daily
life and simple theoretical principles suffice.19

For all its 'objective' second-level patina Reichenbach's
epistemology is clearly based upon the myth of ultimately
private ostensive 'definitions'. It cannot possibly be my
task here to dispell that notion in all its varieties and
ramifications. I have attempted to exhibit some of the
absurd consequences to which it leads in the specific case
of understanding intra-systematic simultaneity. The purpose of the present chapter, then, is merely to provide some background for a few of the more hidden epistemological assumptions that are connected with, or are part of, the CST in various ways.

The literature on most of these more familiar problem areas is extensive and I cannot hope to summarize all arguments found therein. What I hope to indicate, however, is the kind of treatment this type of epistemology is calling for. At the same time I hope to be able to show how hopelessly muddled and antiquated, so to speak, the 'philosophy' behind the CST really is. Most of these pitfalls have long been exposed in other areas of philosophical discourse, whereas the CST has survived primarily because the $\mathcal{E}$-formalism seems to be fortified with mathematical rigor, consistency, and unassailability.

Reichenbach's 'private clock' argument, as we have seen, occupies a central position in his scheme. He must attempt to render plausible the notion that ultimately 'reality' could be built up without the notion of a given surrounding, a context or, in the physical sense, a 'material' reference frame. We should then be able to 'construct the world' solely with 'signals' issuing from, and returning to, one strictly local 'observation point'. In that fashion all 'global' contexts and relationships (including, of course,
P- and R-simultaneity) will necessarily appear 'inferred', hypothetical, and to an important degree 'arbitrary'.

Arguing along such lines, Reichenbach hopes to maintain a strict distinction between 'matter axioms' and 'light axioms' and, lo and behold, derive a conclusive proof for "the logical priority of time over space"20:

Let us imagine an empty space in which material points are traveling at random, similar to molecules in a gas. At every point sits a small observer who is equipped with physical instruments. He can send forth light rays, reflect them, etc.

The observers have the task of constructing a temporal and spatial order; they are told to determine temporal succession, to synchronize time for all material points, to determine when two points are at rest relative to each other, and to measure spatial distances. They are supposed to solve all these problems solely by means of measurements on the various material points without the use of rigid rods. Signals are the only connecting links at their disposal.21

... The observers now have the task of choosing a system of points that can be called 'at rest relative to each other' in order to define a space-time metric in this 'rigid system'. The choice of such a rigid system is again arbitrary and the state of relative rest is a matter of a coordinative definition. However, one definition is distinguished by its simplicity and leads to Newton's inertial system. ... The construction of the rigid system, or system of points at relative rest, is accomplished by steps which we shall describe in terms of certain operations performed by the observers on the various points. The observer at A knows what is meant by temporal order at A, but he does not as yet know what is meant by "two equal successive time intervals". Provisionally, he lays down a completely arbitrary rule, i.e., he chooses a measure of time that differs by some monotonically increasing function from the "uniform time" which is to be defined later.
He now tries to reach by signal a point B which has the property that the time interval $ABA$ of a light signal reflected at B will always have the same length when measured repeatedly at A in the arbitrarily chosen metric. $ABA$ is therefore constant. To make this possible the point B must have a specific state of motion relative to A, which depends on the time metric chosen in A. Using this method, the observer at A looks for a number of such points B, C, ... which form a "system related to A". Using the points of the system S obtained in this manner we now perform the following experiment. We send one light signal along the triangular path ABCA, another in the opposite direction along ACBA, and test whether the time interval ABCA equals the time interval ACBA. Again, we do not yet employ the concept of simultaneity for distant points. We send the two signals simultaneously from A and observe whether or not they return to A at the same time. Generally this condition will not be satisfied for an arbitrary system S. Therefore we add a further restriction if we demand that a system S' is to be chosen among the systems S which satisfies the roundtrip axiom. That such a system S' exists is again a matter of experience. Having made a choice among the masspoints and having combined a system S' of these points into a spatial coordinate system, we can now ask whether a special simultaneity can be defined for this system. This simultaneity definition is given by setting $\epsilon$ equal to 1/2. Its advantages consist in the following properties. These properties make the time order in S' particularly simple and justify the definition of simultaneity used by Einstein in the special theory of relativity where $\epsilon=1/2$. This should not mislead us into believing that this definition is 'more true' because of its simplicity. Again we are concerned with nothing but descriptive simplicity. The choice of a more complicated definition of simultaneity does not present any difficulties for our imagination. In addition to the simplicity of their time order, the systems S' possess a second important property: they permit us to make spatial measurements. This fact is of extraordinary significance because it proves that space measurements are reducible to time measurements. 

Time is therefore logically prior to space.
In criticizing such 'parables' it is often very difficult to draw the dividing line between legitimate 'idealization' and literal nonsense. One always ought to keep in mind the overall point of the fable when grappling with this distinction. Given Reichenbach's preconceived ideas about the 'non-empirical status' of distant simultaneity, he must push for a literal reduction of physical reference systems to collections of entirely unrelated isolated points. Only through this complete reduction will R- and P-simultaneity automatically fall by the wayside, so to speak. Hence, the literal problems connected with Reichenbach's account of the origin and 'primordial genesis' of extended physical reference systems cannot be brushed aside as merely metaphorical expressions.

When we take a closer look, however, the superficial plausibility of this piece of 'science fiction' soon evaporates: the example we are asked to envisage is simply unintelligible. An 'observer' on a point could be an innocuous figure of speech, but not in this context and not in the way in which such an 'abstraction' is to be used in this kind of 'proof'. To ascribe to A "knowledge" of "what is meant by temporal order at A" is on the same level as ascribing a knowledge of Greek to a rock. It is like asking us to 'imagine' a mathematical straight line to be reciting Shakespeare or a point to answer questions. What is it like
to imagine something like this?

Could someone have a feeling of ardent love
or hope for the space of one second - no matter
what preceded or followed this second? - What is
happening now has significance - in these
surroundings. The surroundings give it its
importance. And the word "hope" refers to a
phenomenon of human life. (A smiling mouth
smiles only in a human face.)

To speak of a 'temporal order' in Reichenbach's primordial
isolation is simply vacuous and meaningless. He has already
thrown out the child with the water: the logic of concepts
such as "measuring something" has been stripped of its
ordinary features to the point of idle vacuousness. How
exactly is A to choose a private, arbitrary 'measure of
time' (and if he were 'given' one to begin with, what is he
to do with that string of words)? I do not just mean 'how'
in the operational sense but: what could that mean here?
How does he arrive at the necessary criteria for 'sameness'
and 'repetition' -- how is he to pick a 'monotonically
increasing function' or anything at all?

We are told that A is to build his entire 'universe'
with the help of 'light rays' alone. But how is he ever to
establish these intervals \( \overline{ABA}, \overline{ABC\overline{A}}, \overline{ACBA}, \) etc.? If these
'light signals' are single point signals he would already have
to know in advance the position and subsequent motion of B
(and the speed of his signals) at the time he emits his
signals, lest he will miss B altogether. Similarly, B will
have to 'reflect' these signals in just the right way or A will never receive them back. Yet even if he does -- how is he to discriminate angles and directions so as to be able to tell from whence they came? Similarly, if these are spherical wave-signals, how will he be able to locate B from the incoming (reflected) wave alone? These are not just operational restrictions, rather the concept of 'locating' something in space, establishing 'angles', 'directions', etc., simply have no sense at this level. To that minimal extent one might say that an extended, common, physical reference frame is logically necessary and prior to any notions of 'signals', and 'measuring', and establishing a 'light geometry'.

Reichenbach's fundamental presupposition is simply nonsensical. It is this presupposition of 'signal priority', however, which forms the basis for the alleged logical 'impossibility' of ever being able to 'know' distant simultaneity:

The possibility of measuring with rigid rods and natural clocks depends on these axioms the 'matter axioms'. This geometry is not complete, however; 'straight line' and 'simultaneity' must be defined by means of light.24

This, surely, is a double non sequitur. Not only does Reichenbach not tell us why this must be done (without begging the question), in attempting to show us how it is done he has literally made no sense of his own preconceptions. Instead of carefully analyzing how "simultaneity" and
and "one-way velocity" are actually used in physical theory (or ordinary language), his 'analysis' has rendered these concepts totally unintelligible.

4.

We shall now turn to Reichenbach's characterization of the 'theoretical' structure of knowledge built upon the basis of 'elementary empirical facts'. As we know, this structure is deemed to be essentially 'arbitrary' and freely variable in certain ways. Such claims, I hope to show, are deeply confused; either they amount to a trivial point about natural languages or they are literally nonsense.

Conventionalism essentially starts with a very (and overly) simplistic model of language: words are 'coordinated' to 'things' in the world. Logical relations such as "identity" or "change" of the thing-named are also part and parcel of this christening procedure. They are explicitly defined, or, in their parlance, 'assumed', in such coordination rituals and are therefore also essentially arbitrary. Hence, in a trivial sense, it seems perfectly selfevident to declare 'concepts' to be 'conventional', given such a model of language. This may indeed be a trivial and innocuous thesis if it asserts no more than that the particular words, signs, or noises used are 'arbitrary' in a certain sense. Conventionalists, however, want to assert more than that.
The core of their thesis is not that individual words could possibly be changed at liberty, it is the logic, use, and meaning of such words that is alleged to be essentially variable within 'the conceptual system' that we have, while leaving the 'factual content', so to speak, unaltered. Again, operating under the presumption that 'conceptual system' itself is a clearly delineable, primordially 'constructed' notion, it seems obvious that there are possibly ('originally') many separate, entirely unrelated and self-enclosed systems of that sort and that all 'conceptual systems' possess the deductive, hierarchical structure earlier described. On such a basis the proposed systematic 'variational calculus' for ordinary concepts seems plausible. We are to alter the use of words not ad hoc, qua mere symbols, but systematically, so we are told, according to a 'unitary principle' which guarantees the 'factual content' to remain intact.

Let us try to keep two things in mind here: first of all, we shall have to examine whether that admonition itself makes any sense, and secondly we shall have to discuss whether, even if such a complete and systematic 're-description' could be had, it would be of any genuine philosophical import upon our notions of "knowledge" or "uncertainty".

As to the first point one would have to know at precisely what point the 'ad hoc-ness' of this 'unitary principle'
ends and where its genuinely systematic, independently specifiable unitarity begins. There are, however, only mysterious, sweeping declarations to be found here, with a surprising absence of systematic detail. Let us take a look at three specific examples:

... we, in our simple world, are also permitted to choose a definition of congruence that does not correspond to the actual behavior of rigid rods. Thus we could arrange measuring rods, which in the ordinary sense are called equal in length, and, laying them end to end, call the second rod half as long as the first, the third one a third, etc. Such a definition would complicate all measurements, but epistemologically it is equivalent to the ordinary definition, which calls the rods equal in length.25

Again we ought to note two things here: first the misconception that 'definitons' of standards are essentially identifications of tangible pieces of matter with a certain 'piece of language' at one 'extensionless point' in space and time and not already the outgrowth of an infinitely richer background of human behavior, knowledge, and objectives; secondly, it is, almost paradoxically, acknowledged that there is only one 'actual behavior' of rigid rods. Hence, there ought to be plurality (of 'descriptions') without actual uncertainty, confusion, or impossibility of understanding. On the other hand, how can that 'epistemological equivalence' ever be established? We are given one seemingly simple 'rule' and are forewarned that it will 'complicate
all measurements'. But how are we to have confidence in the equivalence of the results of measurements if we do not even know how to apply this 'rule' so as to measure in the first place? Let us try to stretch our imagination to see whether we can actually make any sense of that notion.

The first difficulty we would encounter in such attempts stems from the infinite variety and divisibility, so to speak, of measuring rods. If we were to measure a piece of cloth of a length of, say 5 meters (measured in the ordinary way), that same material, when measured with meter rods 'counted' in the Reichenbachian way would turn out to be \((1 + 1/2 + 1/3 + 1/4 + 1/5)\) 'meter', i.e., approximately 2.28 'meters' long. So far so good, but now assume that I am using a ruler which happens to be only 50 centimeters long. Hence, laying it down 10 times instead of 5 times, that same piece of cloth would now 'measure' \((1 + 1/2 + 1/3 + 1/4 + 1/5 + 1/6 + 1/7 + 1/8 + 1/9 + 1/10)\) 'meters', or approximately 1.46 'meters' in length. Imagine then, the poor carpenter who wanted to figure the lumber needed to build a carport with the help of a yardstick, a little 10 inch pocket ruler, and a 50 foot measuring tape! It thus seems obvious that our new 'rule' for 'measuring length' requires many more ad hoc 're-adjustments' before we could even begin to know how to use it in practice. On the other hand, arbitrarily settling on one and only one 'standard ruler',
e.g., one precisely one meter in length, does not make much sense because we would then always require an additional, special set of rules for measuring lengths below that limit.

The latter point leads us to the other side of the coin: the problem of the infinite divisibility, so to speak, of distances to be measured. Assume our carpenter has a dividing line running across the middle of his floor plan, say where the concrete ends and the hardwood floor begins. Having measured one side of the room once but eager to check things thoroughly he sets out to remeasure the room from the far end where he just arrived (or, alternatively, assume he starts measuring back towards the front side along the opposite wall of the room). Lo and behold, even though the total length of the room remains the same, the distance from the front wall to the dividing line and the remaining distance from the line to the rear wall are totally different! Yet a piece of carpet cut to size without measuring numerically will fit either section!

Under such circumstances, how could we seriously maintain that we have any idea what 'measuring' in such a world would be like? Assume, for example, that our carpenter has become more sophisticated. He is even trying to master some elementary concepts of physics. Can anyone tell him how he is to measure velocities in this 'system'? What kind of
laying-down-an-arbitrarily-picked-rod might suggest. The notion that we would only be 'complicating things' if we decided to 'pick' a different operation or to 'count differently' is simply without sense. Instead of having provided us with fullfledged alternatives of 'how to reason (measure) differently', conventionalists have only provided us with sketchy examples of 'behavior' for which we have no imaginable justification for calling it reasoning or measuring to begin with. Their 'unitary principle' is no more than an empty magical formula: IF one could give a general (nontrivial) advance recipe for 'how to do things differently' which could truly be applied consistently (without infinitely many ad hoc readjustments) for all kinds of purposes and contexts, in such a way that all 'observable results' remain the same, then one will have found an example of a different but totally equivalent 'conceptual scheme'. That would indeed make 'the one we have' look like it is floating in thin air, ready to be 'grounded' differently at any time.

No one will ordinarily see this last proposition that 12 inches = 1 foot as an empirical proposition. It is said to express a convention. But measuring would entirely lose its ordinary character if, for example, putting 12 bits each one inch long end to end didn't ordinarily yield a length which can in its turn be preserved in a special way.26 We should presumably not call it 'counting' if everyone said the numbers one after the other anyhow; but of course it is not simply a question
of a name. For what we call 'counting' is an important part of our life's activities. Counting and calculating are not - e.g. - simply a pastime. Counting (and that means: counting like this) is a technique that is employed daily in the most various operations of our lives. And that is why we learn to count as we do: with endless practice, with merciless exactitude; that is why it is inexorably insisted that we shall all say 'two' after 'one', 'three' after 'two' and so on. 27 The agreement of ratifications is the pre-condition of our language-game, it is not affirmed in it. 28

It is only a slight modification of the Reichenbachian example just discussed which suggests that we could simply 're-name' uniformly the standard measure of all lengths. This particular variation has been proposed in the form of a so-called overnight doubling hypothesis. 29 Instead of 'laying off' standards according to a special rule we now declare that all standards and all objects (distances) measured by them have simultaneously doubled in length. Hence all our observable results will allegedly remain the same, i.e., the 'doubling' will be totally 'hidden', while we are merely drawing a different 'inference' or are making a different 'assumption' concerning the 'real' measures of length involved. Such an 'assumption' should therefore be a very elementary, even trivial, example of a differing but 'empirically equivalent' 're-description'. As Schlesinger has pointed out, however, there are in fact numerous physical contexts in which such an 'assumed change' would lead to
clearly observable differences. Again, this seemingly straightforward, simple paradigm of a totally indistinguishable 'alternative' could only be saved by a myriad of simultaneous ad hoc 'adjustments' of a number of wholly unrelated physical quantities. The 'unitary principle' invoked again soon dissipates into an endless string of totally unrelated conjunctions: "If A ..., and B ..., and C ..., and also D ..., then X is truly undiscoverable and empirically indistinguishable from Y".

When we said that everything has doubled in size we meant that simultaneously other appropriate compensating changes have taken place in all the relevant features of nature so as to camouflage this event and make it undetectable. Thus simultaneously with the expansion of the earth's radius it received an impulse increasing its angular momentum so that its angular velocity did not change; the law of gravity has undergone change so that now the pull at a distance 2R is the same as it was yesterday at distance R; the velocity of light has doubled, and so on.

Under these conditions, we would indeed have to admit that the universal expansion will go undetected. But this would be no help to our opponent. For now it would also have to be admitted that there is nothing unique about the statement "Overnight everything has doubled in size". It would not differ from the statement "A pink elephant has just entered the room", the latter being equally unverifiable if it is joined with additional propositions stating that at the same time appropriate changes to mask completely this strange event have also taken place. It is unquestionably true that the proposition "Event E has taken place in a manner never to be detected" - no matter what E stands for - is unverifiable and we contradict ourselves if we say it is verifiable. But then all we have done is to make the extremely uninteresting point that to claim that something unverifiable is verifiable is a contradiction.
The third example is, of course, the best developed of them all: the CST itself. Here we seem to have a 're-description' which appears entirely unassailable on the simple-minded empirical/theoretical dichotomy which conventionalists have presupposed as basic datum. All 'empirical observations' are completely preserved, all differing 'assumptions' seem completely unverifiable. Little does it matter that practically all physical concepts would fall by the wayside -- and hereby I mean not just that they would have to be 'altered' in a quantitative sense but that their logic would have to changed beyond all recognition. And yet, given sufficient hedges, a myriad of 'proper' ad hoc 'readjustments' might eventually render the change-over complete and entirely 'consistent', might it not? Assuming that we ever reach the end of that rainbow, what justification will we then have for claiming the standard account to be 'right' and the 'E-observers' to be 'wrong'? Must we not admit then that they merely 'reason differently' but are basically 'describing the same thing' (performing the same operations)? That surely must prove that 'our physical conceptual scheme' (as though it were something closed and clearly delineable) is in fact 'arbitrary' and subject to wholesale alteration (again, wholly within the 'clearly defined' boundaries of its applications). Compare:
People pile up logs and sell the, the piles are measured with a ruler, the measurements of length, breadth and height multiplied together, and what comes out is the number of pence which have to be asked and given. They do not know 'why' it happens like this; they simply do it like this: that is how it is done. ...

Those people - we should say - sell timber by cubic measure--but are they right in doing so? Wouldn't it be more correct to sell it by weight - or by the time it took to fell the timber - or by the labour of felling measured by the age and strength of the woodsman? And why should they not hand it over for a price which is independent of all this: each buyer pays the same however much he takes (they have found it possible to live like that). And is there anything to be said against simply giving the wood away?

Very well; but what if the piled the timber in heaps of arbitrary, varying height and then sold it at a price proportionate to the area covered by the piles?

And what if they even justified this with the words: "Of course, if you buy more timber, you must pay more"?

How could I shew them that - as I should say - you don't really buy more wood if you buy a pile covering a bigger area? -- I should, for instance, take a pile which was small by their ideas and, by laying the logs around, change it into a 'big' one. This might convince them - but perhaps they would say: "Yes, now it's a lot of wood and costs more" - and that would be the end of the matter. We should presumably say in this case: they simply do not mean the same by "a lot of wood" and "a little wood" as we do; and they have a quite different system of payment from us.

(A society acting in this way would perhaps remind us of the Wise Men of Gotham.)

Frege says in the preface to the Grundgesetze der Arithmetik: "...here we have a hitherto unknown kind of insanity" - but he never said what this 'insanity' would really be like. 32

The reason one cannot adequately describe this kind of insanity is that it would mean to 'describe' something
unintelligible. Wittgenstein here attempts to show how our ordinary modes of reasoning, without being 'absolutely justified' or 'grounded' themselves, through various alterations gradually change into something that we can no longer understand. Certain things we can easily imagine as being done differently; the more these fictitious people depart from ordinary reasoning, however, the more perspicuously the concepts they are wont to retain begin to 'idle' and slip out of joint. They are gradually pushed beyond the bounds of sense because we no longer see any system behind their application and the uses to which they are now put. Even though these people are still going through the motions, so to speak, we can no longer follow them. Are we to say then that they reason differently or must we not rather say that we have no idea what kind of rituals they are performing here?

In 'describing' nature in the way the 'ε-observers' propose to proceed, how can we speak of, say charge A "acting on" charge B, event X "causing" event Y, etc., as we normally do? (Remember, of course, that linear accelerations, e.g., between interacting masspoints or point charges, are also directionally variable now!) What justification do we have for using these terms when the 'behavior' described is so totally alien to what is usually associated with these concepts? What possible justification do we have for saying
that these observers 'infer' Y from X, or 'predict' F 'on the
dbasis of' E, if all we are given are ad hoc patches of seemingly
similar verbiage but of totally unfamiliar use? What makes a
"transition" a valid inference is neither a particular formula
or a particular string of words, nor an interior process
which somehow guarantees its validity; rather, the justifica-
tion lies in its surroundings and in the multivariable uses
to which these words are commonly put. Unless we can un-
derstand the 're-description' in this sense we need not and
cannot give it the status of a competing description to
begin with. By the same token unless the 'z-descriptions'
can be shown to be coherent in that sense (i.e., not in the
mathematical-deductive sense, but as squaring with an open-
ended ultitute of pedestrian as well as high-powered uses
and applications), we should not know what it means to call
'our conceptual scheme' essentially 'arbitrary'.

The steps which are not brought in question are
logical inferences. But the reason why they are
not brought in question is not that they
'certainly correspond to the truth' - or
something of the sort, - no, it is just this
that is called 'thinking', 'speaking', 'inferring',
'arguing'. There is not any question at all here
of some correspondence between what is said and re-
alilty; rather is logic antecedent to any such
correspondence; in the same sense, that is, as
that in which the establishment of a method of measurement is antecedent to the correctness or incorrectness of a statement of length. 33

The pitfall of conventionalism consists in precisely this confusion: individual 're-adjustments' of particular statements or measurements are mistaken for the invention of a systematic new method. Their 'method', however, is totally oblivious of the infinitely richer range of applications of the actual method which they purport to replace. They manage to reshuffle various classes of particular outcomes, or, in a manner of speaking, even 'submethods', but overlook the many more numerous contexts in which their 'method' grinds to a very unmethodologically abrupt halt or even clashes head-on with criteria employed within, or with results derived on the basis of, the same (correct) method of measurement. Far from having uncovered 'truths' of the highest epistemological order, e.g., that we really have no basis for ever speaking of "the correct" or "the true" measures of particular magnitudes, they have merely reduced ad absurdum their own misunderstandings and preconceived metaphysical 'theories'.

The temptation to succumb to the myth of a 'total re-description', purporting to prove the essentially 'conventional' nature of all human knowledge, is, of course, a longstanding philosophical tendency. Descartes' famous
principle of universal doubt comes to mind here, based as it was upon the seemingly complete indistinguishability of 'dream reports' from waking accounts. (Even though Descartes himself was not a conventionalist, he apparently felt a genuine need to 'save' epistemology from this real 'threat'.)

I cannot, however, but remind myself that on many occasions I have in sleep been deceived by similar illusions; and on more careful study of them I see that there are no certain marks distinguishing waking from sleep. 34

That particular 'example' of a different but 'totally equivalent description' of 'appearances' can actually be traced all the way back to Plato's Theaetetus. 35 Reichenbach's kind of conventionalism may well be said to be an offspring of that same general type of temptation. Naturally enough, I cannot hope to adequately deal with a traditional movement such as conventionalism on the whole in the present paper. In any event, the only proper approach to treating such 'theses' is precisely not to fall into the trap of countering with an equally general contrary 'thesis'. The exact meaning of such analogies or 'examples' must first be spelled out in minute detail because they invariably suffer from a built-in myopic vagueness. Usually the latter characteristic proves to be fatal to the very thesis pro- pounded in the skimply drawn 'examples'. 
In the case of Cartesian mental-report 're-descriptions' such detailed examinations have been carried out by people like Wittgenstein\textsuperscript{36}, Malcolm\textsuperscript{37}, Ryle\textsuperscript{38}, Austin\textsuperscript{39}, et al. Their particular results need not concern us here. Their over-all import, however, has been a salutary attention to detail and multitude of contexts rather than a facile resort to cryptic, sweeping formulas and mysteriously hidden 'unitary principles'. This so-called revolution in philosophy has been somewhat slower, however, in pervading the circles of logical positivists and philosophers of science. Even Wittgenstein himself is often misinterpreted as an all-out conventionalist in this area, e.g., in his treatment of mathematical deduction and logical inference (cf. Dummet\textsuperscript{40}, Chihara\textsuperscript{41} et al.). Barry Stroud\textsuperscript{42} has, I believe, excellently rebutted these misconceptions and eloquently characterized the ordinary meaning of the 'inexorability' of 'the logical must'. Again, I cannot hope to deal with all these wider topics in the present paper. Any such kind of 'positive' account would require far more depth and detail than I can hope to give within the limits of this discussion. My primary aim here has been destructive rather than constructive.

In one way, however, one need not at all be apologetic for such a restriction in scope, at least not in this instance. Strategically speaking, we must keep in mind that we are not
really called upon to measure conventionalism on any other criterion than its very own 'acid test' principle (cf. p. 125). The wider implications and the historical-philosophical background of conventionalism are only of incidental importance if the central example given by this school should in fact fail to meet the very principle which they themselves declared to be decisive. As it turns out, in the instance of the CST conventionalists have committed one fatal error: they have, for once, elaborated their own thesis in sufficient enough detail to render it clearly empirically falsifiable. Reichenbach et al., have very explicitly set forth the context within which the CST is claimed to be operative. Speaking in relativistic terms, a Minkowskian flat space-time structure is presupposed, the only variation being the use of 'skew' rather than time-orthogonal inertial frames. It is within this given context that P- and R-synchronization decidedly refute the freely variable light-signal 'synchronizations' proposed by the conventionalist. Hence, the CST is either wholly confused philosophically, hopelessly vague, and ultimately unintelligible, or, if specified in sufficient technical detail, plainly untenable and simply false.
REFERENCES TO CHAPTER VII


3 Ibid., p. 120.

4 Ibid., p. 121.

5 Ibid., p. 129.

6 Ibid., p. 144.

7 Austin, J. L., Sense and Sensibilia, Oxford 1962, p. 105.


11 Ibid., §§ 23, 27, 304, 89, 109, 126, 127.


13 Ibid., p. 6.

14 Ibid., p. 5.

15 Ibid., pp. 5-6.

16 Ibid., pp. 4-5.

17 Ibid., pp. 7-9.


